FRANÇOIS MAGENDIE: FROM DOGMATIC EMPIRICISM TO THE PRACTICE OF EXPERIMENTAL REASONING

Ruth García Chico and José Luis González Recio

ABSTRACT

The historiographical studies focused on French nineteenth-century physiology have eventually enshrined the thesis that the need to resort to hypotheses was assumed and proclaimed for the first time within the works and scientific practice of Claude Bernard (1813-1888). His teacher, François Magendie (1783-1855), is presented as a figure that fights against vitalism and that, devoted to an absolute empiricism, only admits the bare facts as constitutive elements of science. He accepted generalizations -as long as they were not premature- from what he called *materials collected within experience*, but rejected that ideas or hypotheses could lead the scientific path. Although this image of Magendie is widely shared -and was even prompted by some of his statements- this paper aims to show precisely that it is an image that does not correspond to reality. Magendie did know the crucial role of hypotheses within physiological research. Not only that: he used them extensively in his scientific work and in his activity as a researcher committed to the implementation of experimental physiology.

KEY WORDS: Magendie, observation, facts, hipotheses, experimental method, 19th century scientific physiology.

The historiographical studies focused on French nineteenth-century physiology have eventually enshrined the thesis that, in France, the methodological requirement –the absolute necessity of hypotheses- was assumed and proclaimed for the first time within the works and scientific practice of Claude Bernard. His mentor, François Magendie, appears as a figure that fights against vitalism and that opens the door for a mature theory of organic functions based on experience. Nevertheless, the prevailing opinion is that, even though Magendi had led the way, the culmination of the experimental method in French physiology corresponds to Bernard, who also has to be granted the paternity of the idea that, without hypotheses, scientific research cannot be deemed possible. There are numerous studies which accept this dual level of commitment with experience –discarding hypotheses (Magendi) or, on the contrary, considering them indispensable (Bernard). Master and disciple were responsible for such account, as both of them favored with their frequently categorical statements this description of their respective own roles. According to Bernard, while he worked with his mentor, he heard every day Magendie's impassioned justification of a militant and radical empiricism. Bernard claims that his master advocated and practiced an uncompromising empiricism, opposed to any theoretical systematization whether it was the prompter or the result of observation. He admitted generalizations -as long as they were not premature- from what he called *materials collected within experience*, but rejected that ideas or hypotheses could lead the scientific path. Facts should speak for themselves, without being associated to any preconceived notion, as the truth would eventually show itself in them. Bernard recalls hearing Magendie comment: "each of us tends to compare himself in his sphere with someone or something more or less magnificent, with Archimedes, with Michel Angelo, with Newton, with Galileo, with Descartes. Louis XIV compared himself to the Sun. I am much more humble; I compare myself with a rag and bone man, with my hook on one hand and my basket on my back, wandering the domain of science and collecting what I find".

Magendie himself contributed repeatedly to consecrate his image of an empiricist obsessed with the condemnation of hypothesis. Let these words serve to confirm it; "once thrown into the field of hypothesis, imagination loses its way, unable to focus; human pride hates so to be stopped!, even if nature says: you will go no further". Many other examples could be given. All are of the same sort and have served to make indisputable this belligerent attitude towards hypothesis that is attributed to the physiologist from Bordeaux. Even though this way of seeing things is broadly shared, we intend to show just the opposite.

Magendie did know the crucial role of hypotheses in the experimental method. Not only that: he used them extensively in his scientific research and in his work as a physiologist. And what is more, he did this using those hypotheses with the same goals that Bernard later reserved for them: to guide observation, to serve as the basis for the explanation of the facts and to be susceptible to verification. As we shall see, you only need to read Magendie's texts to confirm this beyond any doubt. Experimental physiology, equipped with all the necessary methodological resources, began its journey in nineteenth century France through the works carried out by François Magendie. It is true that Bernard succeeds in revealing every component of research, in clarifying every step in the path of scientific inquiry, in coining the terms that name those elements, but none of them is absent in the practice developed by his master. Both of them worked using the same method; nevertheless, it is also true that only Bernard had the will to express in a systematic way these methodological guidelines they both followed. We shall meet a double goal: 1) to make it clear that purely speculative hypotheses, linked to doctrines or systems, were rejected by both physiologists, given their lack of empirical support; 2) to verify that, understood in their other sense, that is, as ruling ideas of the experimental practice, hypotheses were not used only by Claude Bernard but were repeatedly formulated and used by Magendie to articulate his research activity and to find an answer to the many problems he turned his attention to when he devoted himself to experimental reasoning. Programmatic declarations are not as important as facts and, in this respect, Magendie should be judged for what he did, not for what he said he did: *you are what you do not what you think or say*.

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Even though our study is based on the analysis of four works by the French physiologist, −*Examen de l'action de quelques végétaux sur la moelle épinière* (1809); *Précis élémentaire de physiologie* (1816-17); *Recherches physiologiques et cliniques sur l'emploi de l'acide prussique ou hydro-cyanique dans le traitement des maladies de poitrine, et particulièrement dans celui de la phthisie pulmonaire* (1819), and *Leçons sur les fonctions et les maladies du système nerveux* (1839)− in order to adjust to the time given for presentations, we are going to focus exclusively on what can be established following the *Précis élémentaire de physiologie*. It is a work that will sufficiently illustrate François Magendie's use of hypotheses within experimental research, although reading the other three papers strengthens even more the same conclusion. We leave then the detailed and complete exposition of the evidence supporting our thesis for a later publication.

Magendie organized several public demonstrations of physiology at the amphitheater Saint-Nicolas-du-Chardonnet, turned into a Medical School during the Revolution. The brilliance of the vivisections he performed had a great echo and earned him the admiration of young students. This success encouraged the revered professor to present his *Précis élémentaire de physiologie* to the *Académie des sciences*. Despite the implicit modesty of the adjective "*élémentaire*", it is a great contribution to the physiology of the time, developed in two volumes. According to Mazliak:

Magendie intended, then, to abstract any *a priori* reasoning, focusing entirely on experience, hoping that continuous repetition of the same experiment would provide, by simple induction, the key to the physiological problem he faced. Without any previous reasoning, i.e. without any initial hypothesis corresponding to an *a priori* interpretation born out of the spirit of the researcher…

Had this goal been achieved by Magendie, the introduction of *a priori* ideas or guidelines in experimental reasoning by Bernard would undoubtedly have marked the difference between them concerning how to build a scientific physiology. But regardless of Magendie's own statements, as said before, what is true is that in the Précis we can find tens of examples in which a great variety of hypotheses are assessed, some of them are confirmed while those that do not pass experimental verification are rejected. We shall begin with the experiments about voice production. Addressing this issue, Magendie asks himself: why is no sound similar to the human voice produced when blowing through the trachea of a corpse? Why is paralysis of the muscles in this organ followed by loss of voice? The proposed hypothesis is that the ligaments of the glottis only have the power to vibrate, as reed strips, when the thyroarytenoid muscles are in contraction. Therefore, in all the circumstances in which those muscles are not contracted, there will be no voice production. The hypothesis argues, in short, that vibration of the glottis ligaments requires muscles to be in contraction. What we thus have is a phase of familiarization with the facts deemed relevant with regards to phonation and, then, the abduction of a conjecture from them. It could be argued that verifying the absence of voice production whenever the muscles are not contracted is just an inductive generalization. However, the comparison of the glottis ligaments with reed strips clearly shows that the explanation given goes beyond mere observation. It makes it evident that observation data are being examined in a reflexive way in order to arrive at a hypothesis that explains them. But, if this were not enough, Magendie immediately suggests that the hypothesis must be experimentally confirmed. "The experiences with animals perfectly agree with these doctrines. Cut both recurrent nerves, which, as said before, are distributed towards the thyroarytenoid muscles and the voice is completely lost. Cut only one and the voice is reduced to a half". Note that the procedure serves to verify the hypothesis, and that it is not a mere blind exercise in which structures or anatomic elements are being cut at random in order to establish inductive associations. The starting point is the hypothesis that contraction of the muscles is necessary for voice production; acting within the framework of this hypothesis, the recurrent nerves are cut off; and it is confirmed that, without the action of the mentioned nerves over the muscles, these cannot be contracted and voice emission does not take place. So, ultimately, all the steps that define the experimental method are being used. We shall examine many other examples in which the explicit application of the experimental method offers the solution to the problem at hand. But even more interesting are those cases in which we can see that Magendie rejects a hypothesis, not because it is a preconceived idea, as it could be expected according to the way his work is usually interpreted, but because it is a hypothesis that did not reach an experimental verification. Had the case been the opposite, there would be no objection to its validity. When he points out that a certain conjecture has not been backed up by experience, and that, therefore, has to be abandoned, the rhetorical question we should ask ourselves is: what would he say about that same hypothesis if it had been verified?

In his studies about chyme formation within the same work, he reflects about the functions of lung, stomach and gastric juice. He recalls that for some time the effector role in chymification has been attributed to the vagus nerve pair. It was thought that this pneumogastric nerve, by means of respiratory and gastric stimulation, makes digestion possible. More specifically, it was believed that a breathing deficit had direct consequences on stomach activity. Indeed, if the nerve is tied or cut close to its roots, the substances introduced in the stomach do not suffer any modifications, i.e. the formation of chyme does not take place. This has served to back up the hypothesis that this nerve pair plays a fundamental role in respiratory activity and, through it, in gastric activity. It should be noted that such proceeding, the section or ligation of the above mentioned nerve pair, is the experiment that was carried out by other researchers in order to assess the hypothesis of the nerve's role in digestion. It is a strict implementation of the experimental method, as the assumption that the nerves play an important role in digestion gives purpose to the test carried out to verify it. However, Magendie criticizes the conclusions drawn, not for the use of a conjecture supposedly confirmed until then, not for the use of a hypothesis, but because the experimental reasoning followed is incorrect. Indeed, if these nerves are tied or cut at their root, the food introduced in the stomach is not altered, is not digested. But the consequence derived from this fact does not seem rigorous to him. The reason is that, based on another hypothesis -that the branches of the nerve found under the lung are responsible for chymification- he cut out the nerve branches that reach the lung, leaving intact those that continue to the stomach. In his own words: 'the food which is then introduced in the stomach is transformed into chyme and later on produces abundant chyle'. The specific hypothesis that Magendie wanted to confirm is that the inferior branches of the nerve pair, those which innervate the stomach, are the ones that stimulate gastric activity and take part in chymification. The appropriate experiment to test it involves cutting out the nerve at the level of its ramifications to the lung, so that only the branches that reach the stomach can act. The results corroborate the hypothesis and the suitability of the reasoning. Once again we are not facing a mere active observation. It is the reflection about the spatial arrangement of the nerve, about its routing through the thorax and abdomen, about the possible role of its ramifications throughout the body what allows Magendie to formulate his hypotheses with sense and what guides him to program a virtual verification.

Magendie also deals with the issue of venous absorption in several pages of the Précis. The idea was widely accepted in the physiology of the time. It was known that small venous roots receive blood from arterial capillaries, but they were also thought to have another very relevant feature or function: it was assumed that any gas or liquid that enters in contact with the different parts of the body passes through these small veins and soon reaches the lungs carried by venous blood. The same applied to solid substances that can be diluted by blood. However, this supposition or hypothesis must be tested to be accepted. It must submit to an experimental verification and, therefore, we have to devise an experiment that would clearly show its validity. The proposed experiment is as follows: let us introduce an aqueous solution of camphor in any mucous or serous cavity of the body, or just a solid portion of camphor in an organ. If the hypothesis is correct, when the blood reaches the lungs, the presence of camphor –absorbed by the veins and transported by the blood to the lungs- should be detected in exhalation. And so, a few moments after introducing camphor into the animal's body, the air leaving its lungs has a strong camphor odor. Administering a camphor enema has the same effects in human beings: most commonly in five or six minutes, breath will strongly smell of camphor. What initially was just a conjecture, a hypothesis, has been backed up by experimental verification.

Another question that also finds and answer in an experimental test described in the work is that about the reason for the coloration of blood. Magendie enquires about the cause for venous blood changing color when it traverses the lungs. His hypothesis is that this change is due to blood's contact with the air in the lungs -more specifically, with the oxygen present in the air. Once again, we must devise a procedure that serves to test the conjecture. The experience is straightforward in this case: simply getting blood in contact with atmospheric air would be enough. Indeed, it does not take long until blood's surface turns red. Nevertheless, it is necessary to establish that the causal agent of the color change is oxygen and not any other component of air. Atmospheric air reaching the lungs contains other gases that could be responsible for the phenomenon. The experiment that would play the required discriminatory role is then suggested. We should introduce venous blood in a bladder and then submerge it in oxygen. When conducting the experiment, blood acquires a scarlet color all over its surface. The assumption has been validated.

A few pages latter, the text focuses on *pulmonary transpiration*, a phenomenon that the physiology of the time admits as a conjecture : 'this transpiration is considered [...] as the result of the passage through the bronchial vesicles of part of the liquid that runs through the pulmonary artery'. Once again, even though it is a hypothesis that most physiologists admit, it must submit to an experimental evaluation. One of its implications would be that water injection into the pulmonary artery should produce observable effects on airway cells. And that is what happens when the experiment is carried out: endless water droplets appear in the lung cells, mixing with the air the cells contain. Even more, once the main hypothesis is verified, we can deduce a second one, implied by the first, and an experiment may be carried out to confirm the former, forasmuch as transpiration is real, its amount should relate to the amount of injected liquid:

In living animals the amount of pulmonary transpiration is increased at will by injecting distilled water at a temperature similar to that of the venous system, as evidenced by the following experience: take a small sized dog; inject him repeatedly with a considerable volume of water. The animal will first reach plethora, a state in which its blood vessels will be so distended that it will be difficult for the dog to move. However, after a short while, respiratory movements will accelerate noticeably and from all over the mouth will flow in abundance a liquid whose origin is clearly pulmonary transpiration, which has increased considerably.

There can be no doubt, in sum, of the meaning of experimental design at the service of hypothesis verification in these two cases. In early 19th century physiology, experimental reasoning meant exactly following Magendie's procedure: getting familiar with the functions fulfilled by the organs of living beings, raising questions, proposing hypotheses to answer them and submitting the hypotheses to the experiment's verdict.

The issue of blood passing from the arteries to the veins gets a special attention in the Précis. Magendie wrote this work in a context still influenced by Bichat's theory, according to which, capillary action is a sort of oscillation or vibration of the vascular walls. Magendie wonders how such vibration can produce the movement of a liquid, blood, through a canal, the vessels. He immediately adds that there is another explanation, a more plausible hypothesis: that the main cause for blood passing from the arteries to the veins is the heart contraction. Then, he gives an account of the experiment that confirms this hypothesis. After carrying out a ligation around the thigh of a dog so that it does not compress the crural artery and vein, apply another ligation to this vein, near the groin, and make a small incision in the vessel. Blood will very soon leak out leading to a considerable loss. Next, press down the artery with the fingers to prevent arterial blood flow to the limb. The venous blood flow will not stop then and it will continue for some instants, yet it will progressively decrease and eventually come to an end. If the artery is examined while the induced phenomena take place, we would observe how it narrows gradually, until it is eventually completely empty. At this point, blood stops leaking out of the vein. If we no longer compress the artery, blood pumped by the heart will reach the artery's capillaries and it will leak again through the incision practiced in the vein. This set of operations, which the author calls literally *expérience sûr le passage du sang des artères dans les veines*, is meant as an experimental control for the initial hypothesis. It is important to point out again that this is not an active observation practiced in the absence of hypotheses. Experiments may be considered active observations, if you wish, but they are always programmed with the aim of verifying a conjecture. There remains a whole domain of possible active observations, alien to the formulation of hypotheses. However, the examples given so far show us a physiologist who is not confined to the observation of bare facts; on the contrary, he devotes himself to the design of experimental tests for the assessment of his hypotheses.

In this same work, after raising the question of the possible mechanism by which animals are able to adapt themselves to temperature increases, Magendie refers to the experimental test that served to confirm a hypothesis conceived by Delaroche. This hypothesis consists on the idea that the cause for animals' observed resilience to heat lies in their skin and lung evaporation. To verify this important assumption, Delaroche had placed animals inside a warm atmosphere so saturated with moisture that no evaporation could take place in it. The animals could not withstand a temperature that slightly exceeded the usual one for their body, and died. Their temperature raised, and they did not have any means to make it decrease.

Along with this comprehensive proposal of hypotheses subjected to experimental control, in the Précis we also find other conjectures that are rejected for failing the tests that would have confirmed them: that relating to the pathways teardrops follow, that referring to the function of the pinna, one formulated by other physiologists about air or gas production in the stomach during digestion or another one which states that blood coagulates upon cooling. Magendie does not criticize them for being hypotheses but for not passing their respective experimental validation tests.

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The legend of dogmatic empiricism, allegedly practiced by François Magendie, has been nurtured, in short, by three wrong assumptions: a) that he actually practiced what he advocated for. b) that Bernard judged objectively the method his master used in his research. c) finally, that the distinction between testable and untestable hypotheses was posed only by Bernard. To a great extent, Magendie is indeed to be blamed for the misunderstanding in the first assumption. Many excerpts of his writings can be cited where he emphasizes his conviction that a true scientist should banish from his work theoretical assumptions, preconceived ideas and hypotheses. Given the historical context, in which unverifiable idealist conceptions or vitalist postulates circulated so vigorously, it is not surprising that he wanted to underline that the new physiology he advocated for should firmly condemn the misleading consequences of speculation. His attitude anticipated that which Bernard maintained against doctrines and systems: there is no place for any of them in physiology. This is the generic rejection of any speculative element present in science that Magendie wanted to fulfill. From here, two different interpretations might follow: that in his explicit statements he only referred to the unacceptability of those hypotheses found in systems and doctrines, i.e. to hypotheses not susceptible of confirmation, or that he also had in mind the fully verifiable hypothesis. Should the latter be the case, we must say with complete confidence that his work was not faithful to his words. As we have clearly seen, he very frequently -and successfully- used hypotheses that were able to be tested, had full explanatory force and directed his experiments. If, when despising hypotheses, he was only thinking about unverifiable ones, his radical empiricism vanishes.

Given the unequivocal presence of hypotheses in Magendie's experimental research –something that Claude Bernard knew first hand-, it should be understood that the latter's persistent use of the idea that his master not only defended but also practiced a strict empiricism, turned into doctrine, was a means to earn all the credit for establishing the experimental method in the 19th century French physiology. This is what can be said about the second assumption. Magendie's grandiloquent statements in favor of empiricism could easily be used to this purpose. Still, nobody knew like Bernard did the content of his mentor's works and nobody witnessed so directly how he used hypotheses in the laboratory as well as in the clinic. Now, if we accept that Magendie used verifiable hypotheses in his work, it is him whom should have been credited with the main role in establishing the experimental method in French medicine.

Finally, regarding the third assumption, there is nothing as clear and illustrating as these words by Magendie himself:

The analysis [...] enabled by the microscope shows us that blood contains prodigious amounts of corpuscles of various shapes, designated by the general name of *globules*; although we completely ignore what their functions are. There is nothing as interesting as watching these globules walk through infinitely small tubes without producing any interference in the blood stream. There is such coordination in their movements that physiologists do not hesitate to consider them little intelligent beings following their instinct, without hindering the laws of hydrodynamics. We have proved that these are just mere hypotheses, which must be urgently replaced by theories based on experience.

Theories based on experience versus purely speculative hypotheses. And, above all, not just bare facts.