DID NIEPCE DE SAINT-VICTOR DISCOVER RADIOACTIVITY?

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Abstract: It is usually accepted that Henri Becquerel discovered radioactivity in 1896. However, up to the beginning of 1898, nobody (including Becquerel himself) interpreted the phenomenon he studied as anything similar to our current concept of radioactivity. Becquerel did observe effects due to uranium radiation, but he interpreted it as due to an invisible phosphorescence. His early works are full of mistakes concerning the properties of the phenomenon he studied. This article compares Becquerel's work to the researches of Abel Niepce de Saint-Victor. Forty years before Becquerel's investigations, Niepce had detected a persistent radiation emitted by uranium nitrate in the dark. Niepce's interpretation (an invisible phosphorescence) is similar to Becquerel's and different from our concept of radioactivity. It is likely that he made experimental mistakes and intermingled different phenomena. However, if mere contact with effects of a phenomenon, without a clear understanding of its nature and properties, can count as the discovery of that phenomenon, one could argue that it was Niepce – not Becquerel – who discovered radioactivity. The paper uses this example to discuss the concept of discovery of a scientific phenomenon.
Keywords: radioactivity; scientific discovery; history of physics; Becquerel, Henri; Saint-Victor, Niepce

MARTINS, Roberto de Andrade. *Historical Essays on Radioactivity*. Extrema: Quamcumque Editum, 2021.

1. INTRODUCTION

In 1903, Antoine-Henri Becquerel[1] (1852-1908) was accorded the Nobel Prize, together with Pierre and Marie Curie, "in recognition of the extraordinary services he has rendered by his discovery of spontaneous radioactivity" (Wasson, 1987, p. 70; Samuelsson & Sohlman, 1967, p. 45). At that time, as today, it was generally accepted that Henri Becquerel discovered radioactivity in 1896, when he observed that a compound of uranium was able to darken photographic plates wrapped in black paper, kept in a drawer. Historical accounts sometimes stress that this was a lucky chance discovery, or discuss the background of the discovery to show that Becquerel was looking for something and found it (Jauncey, 1946; Romer, 1964; Romer, 1970; Badash, 1965a; Badash, 1965b; Badash, 1966). But the discovery itself is never challenged.

It seems that Henri Becquerel can be regarded as the discoverer of radioactivity because:

a) He was regarded as the discoverer of radioactivity by himself and by the scientific community in the early 20th century;

b) Nobody else claimed to be the discoverer of radioactivity, at that time;

c) Historians of science did not find anyone else who discovered radioactivity before Becquerel.

There is, however, a dangerous implicit assumption in this argument: the premise that *someone* must have discovered radioactivity. Contrary to the usual belief, this article will claim that in the common-sense meaning of the word "discovery", Becquerel did not discover radioactivity: the discovery of this phenomenon was the result of a gradual and collective effort beginning with Röntgen's work on X-rays and culminating with Rutherford and Soddy's theory of transmutation of the elements. If, however, one wishes to interpret "discovery" in a way that would allow us to maintain that Becquerel discovered radioactivity, the same criterion could be applied to maintain

[1] For a biography, see Romer (1981).

instead that radioactivity was discovered in 1857 by a gentleman called Abel Niepce de Saint-Victor.

This article is not intended as an example of the historiographical vice of "priority chasing" (May, 1975). The main body of this paper will be devoted to the elucidation of Henri Becquerel's and Niepce de Saint-Victor's researches, and their scientific context, in order to elucidate the long path and the difficulties in discovering (and understanding) a new phenomenon.

2. BECQUEREL'S EARLY CONTRIBUTION TO THE STUDY OF RADIOACTIVITY

In 1896 and in the early 1897, Henri Becquerel published a series of papers that nowadays are regarded as the first description of radioactivity. The word "radioactivity" did not exist at that time, and Becquerel's interpretation of the observed phenomena was different from our current interpretation; however, for simplicity, those articles will be hereafter called Becquerel's "radioactivity" papers.

Let us summarize the conclusions of those publications:

2.1. *A penetrating radiation – 24th February 1896*

Becquerel detected with a photographic plate penetrating radiation emitted by phosphorescent samples (double sulfate of uranyl and potassium) under sunlight (Becquerel, 1896a). The experiment was a variant of those reported by Charles Henry (1896) and Gaston Niewenglowski (1896), who had detected penetrating radiations emitted by other phosphorescent substances (zinc sulphide and calcium sulphide). The motivation of those investigations was Poincaré's conjecture that luminescent substances could emit X-rays (Poincaré, 1896).[2]

[2] For a description of the context of Becquerel's research, see the previous paper in this volume: MARTINS, Roberto de Andrade. A pool of radiations: Becquerel and Poincaré's conjecture.

2.2. *Emission of radiation in the dark – 2nd March 1896*

Becquerel observed that the radiation emitted by his samples was able to pass through thin metal plates. He also observed that his samples of double sulphate of uranyl and potassium emitted penetrating radiation even when kept in the dark. He remarked that the visible phosphorescence of those samples is very short (1/100 s). He proposed the hypothesis that the phenomenon was produced by radiation similar to X-rays emitted by phosphorescence, with a persistence much larger than that of visible phosphorescence of the same substance (Becquerel, 1896b).

2.3. *Properties of the radiation and test of other substances – 9th March 1896*

Becquerel reported that the crystals of double sulphate of uranyl and potassium were able to discharge an electroscope. He did not interpret this phenomenon as due to gas ionization and supposed that it was due to the direct action of radiation upon the charged bodies. He noticed that the emission of penetrating radiation persisted after a few days in darkness and conjectured that the phenomenon was similar to calorescence. He also reported that the radiation was reflected by a polished metal mirror and by glass; and that it could be refracted by glass. In this paper, Becquerel described a series of tests with different phosphorescent bodies, searching for other substances that emitted similar penetrating rays. According to Becquerel, all phosphorescent uranium salts, and two calcium sulphide samples, emitted penetrating radiation (Becquerel, 1896c).[3]

2.4. *Emission of radiation by all uranium salts – 23rd March 1896*

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[3] With this paper, Becquerel started the publication of a series of wrong results. A discussion of Becquerel's errors is presented in: MARTINS, Roberto de Andrade. Becquerel's experimental mistakes, in this volume.

In this paper, Becquerel confirmed the refraction of uranium radiation by glass, using a prism. He reported that his calcium sulphide samples did not emit penetrating radiation in a second series of experiments (an effect, he remarked, similar to what had been observed by Troost with zinc sulphide). Becquerel observed that all uranium compounds he tested emitted penetrating radiation – even those compounds that were not luminescent. The emission of radiation seemed to increase when the samples were illuminated with a strong light.[4] Becquerel concluded that the phenomenon seemed due to an invisible phosphorescence that was not directly linked to visible luminescence (Becquerel, 1896d).

2.5. *Comparison between radiation of uranium compounds and X-rays – 30th March 1896*

Both X-rays and the radiation emitted by uranium compounds produce photographic effects, discharge electroscopes and pass through matter. Becquerel noticed, however, that their penetrating powers were different. Besides, X-rays could not be polarized, but Becquerel reported that the radiation emitted by uranium compounds could be polarized by tourmalines.[5] He measured a decrease of intensity of radiation emission a few hours after exposure to light. Becquerel suggested that X-rays and the radiation emitted by uranium compounds are different and that both might be emitted by X-ray tubes (Becquerel, 1896e).

2.6. *Emission of radiation by metallic uranium – 18th May 1896*

In this paper, Becquerel first summarizes his previous results: uranium salts emit radiation for several months, with a

[4] According to current knowledge, the emission of radiation by radioactive bodies cannot be increased by light.

[5] That was another mistake, that confirmed Becquerel's opinion that the radiation emitted by uranium compounds was similar to ultraviolet radiation.

small decrease of intensity; emission can be excited by strong light; the radiation can be refracted and reflected; this radiation can pass through thin metal plates and discharges an electroscope; and all uranium salts emit this radiation, irrespective of their visible luminescence properties. Due to this last property, Becquerel was led to test metallic uranium. He observed that it also emitted the penetrating radiation and the emission was stronger than that of uranium compounds. He concluded that uranium was the first example of a metal that presented a phenomenon of invisible phosphorescence (Becquerel, 1896f).

2.7. *Persistence of uranium rays – 23rd November 1896*

For six months, Becquerel published no new research on this subject. In his last publication of 1896, he stressed that the radiation he had studied was different from X-rays because the former could be reflected and refracted as light, and proposed the name "uranium rays". He reported that emission of radiation was still intense after several months, although there was a small weakening. The article called attention to the difficulty in explaining the source of energy in the phenomenon. In the same paper, Becquerel confirmed for uranium rays Joseph John Thomson's finding that X-rays produce electric discharge by ionizing air (Becquerel, 1896g).

2.8. *Electric discharge produced by uranium rays – 1st March and 12th April 1897*

In the two last articles of this series, Becquerel described further studies of the electric discharge produced by uranium radiation (Becquerel, 1897a; Becquerel, 1897b). There is no new remark about the nature of the radiation or about the process of its production. Becquerel described that the samples kept in darkness for one year continued to emit penetrating radiation "with a just decreasing intensity" (*avec une intensité à peine décroissante*) (Becquerel, 1897b, p. 803).

2.9. *After 1897*

It is clear that up to 1897 Becquerel's work is a mixture of correct and wrong observations, together with usually wrong interpretations. At that time, he was far from understanding the phenomenon he was studying as we understand it.

After the publication of those "radioactivity" papers, Becquerel turned his attention to the study of Zeeman's effect.[6] Only one year after Marie Curie's and Gerhard Schmidt's discovery of the radioactivity of thorium (Curie, 1898; Schmidt, 1898) Becquerel returned to the study of radioactivity (Becquerel, 1899).[7] Meanwhile, most of Becquerel's mistakes had been corrected, the name "radioactivity" was coined (by Marie Curie), and radioactivity was recognized as a new and important phenomenon.

3. THE STATUS OF BECQUEREL'S WORK TOWARDS THE BEGINNING OF 1898

What did the scientific community think that Becquerel had found, before the discovery of the radioactivity of thorium in 1898?

Several review papers[8] on uranium (or Becquerel's) rays were written from 1896 to 1898. The first report that dealt exclusively with Becquerel's rays was written by Georges Sagnac (1896). It was written in April and it was published in May 1896. Short reviews appeared in articles and books primarily concerned with X-rays (Thomson, 1896; Thompson,

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[6] Becquerel published five papers on this subject in the *Comptes Rendus*, from 1897 to the beginning of 1899.

[7] The first paper of the second series of radioactivity studies was presented to the French Academy of Sciences on the 27th March 1899.

[8] We are including among review papers only those articles that are primarily aimed to the description of previous work and not to publication of original research results.

1897; Guillaume, 1897; Poincaré, 1897). A detailed review was published by Oscar Stewart (1898).[9]

All those reviews told essentially the same story. They accepted without discussion Becquerel's experimental results and described his discovery of an *invisible phosphorescence*, or *hyperphosphorescence*:

a) All uranium compounds (and even metallic uranium) and some other phosphorescent substances (calcium sulphide, zinc sulphide) emit an invisible radiation that can pass through thin plates of metal and other substances; this radiation can be detected by its photographic effects.

  a.1) Some other substances (metals, wood, paper) had also been found to emit invisible penetrating radiation.[10]

  a.2) Uranium radiation can discharge an electroscope, as X-rays do.

b) The radiation studied by Becquerel can be reflected, refracted and polarized; it is therefore a kind of invisible light (similar to ultraviolet rays).

  b.1) This was the main distinction between those rays and X-rays, because the latter could not be reflected, refracted and polarized.

c) The penetrating radiation is emitted during a long time when the active substances are kept in darkness, but the intensity of radiation increases when the substance is excited by sunlight.

  c.1) In the case of blende and calcium sulphide, the effect gradually disappears and cannot be revivified; in the case of substances containing uranium, the radiation could be slightly increased by exposition to sunlight and other radiations.

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[9] Published in February 1898, this article was written without awareness of the recent discovery of the radioactivity of thorium. The same author also published another review two years later (Stewart, 1900).

[10] At that time, there was not a clear distinction between all those phenomena. For details, see my previous paper in this volume.

d) Because of all the previous properties, the phenomenon is similar to common (visible) phosphorescence and can be called *invisible phosphorescence* (or *hyperphosphorescence*).

Of course, that does not correspond to our present-day knowledge of radioactivity. It was not just an *incomplete* knowledge of the phenomenon we call radioactivity – it was a *erroneous* partial account of the phenomena. Indeed, nowadays we accept that:

a') All uranium compounds (and even metallic uranium) *but no other common phosphorescent substances* (calcium sulphide, zinc sulphide) emit an invisible radiation that can pass through thin plates of metal and other substances; this radiation can be detected by its photographic and electrical effects.

> a'.1) Metals, wood, paper, etc. *do not* emit invisible penetrating radiation similar to the radiation emitted by uranium compounds.

b') The radiation studied by Becquerel *cannot* be reflected, refracted and polarized (at least not in the way Becquerel said it could); therefore, the correct conclusion at that time was that it is *not* a kind of invisible light similar to ultraviolet rays.

c') The penetrating radiation is emitted during a long time when the active substances are kept in darkness, *and the intensity of radiation does not increase* when the substance is excited by sunlight or other kinds of radiation.

d') Because of all the previous properties, the phenomenon is *not* similar to common (visible) phosphorescence and therefore it *should not* be called *invisible phosphorescence* (or *hyperphosphorescence*).

The previously described wrong account (a-d) was accepted as true by Henri Becquerel, in the early 1898, and most evidence for that mistaken view had been provided by himself. All the above described corrections (a'-d'), together with a lot of new information, were produced in the period 1898-1899, as the result of contributions of many different researchers, specially Gerhard Carl Schmidt, Julius Elster, Hans Geitel, Marie and Pierre Curie, and Ernest Rutherford, without any important contribution coming from Becquerel.

In the early 1900, Oscar Stewart published a second review paper on Becquerel rays (Stewart, 1900). He remarked that "The importance of the subject of Becquerel rays has increased almost beyond expectation during the past year or two" and then proceeded to describe the completely new view on radioactivity that had been reached at that time. In 1900, at the International Congress of Physics, Marie and Pierre Curie presented an essentially correct (although incomplete) description of radioactivity:

> We shall therefore only recall that uranium rays, or *Becquerel rays*, are characterized by the following properties: they have rectilinear propagation; they act upon photographic plates as light, but in an extremely weak degree; they can pass through screens of different materials, but only if they are very thin; they are neither reflected, nor refracted, nor polarized; when they pass through a gas, they render it a weak electrical conductor.
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> Uranium radiation is spontaneous and constant; it is not maintained by any known exciting cause; it seems insensible to changes of temperature and illumination. (Curie & Curie, 1900, p. 79)

Becquerel, however, had not arrived to this view of radioactivity.

4. IF BECQUEREL DISCOVERED RADIOACTIVITY, WHEN DID HE DO IT?

It is generally accepted by historians of science that Becquerel discovered radioactivity in the first months of 1896. Some of them associate the discovery with the second "radioactivity" paper and the observation of emission of radiations by an uranium salt kept in darkness:

> On Sunday, 1 March 1896, Henri Becquerel developed a set of barely exposed photographic plates – and discovered the phenomenon of radioactivity. (Badash, 1966, p. 267)

Jean Becquerel (Henri's son) has also pointed out Becquerel's second "radioactivity" paper as the relevant turning point:

> Therefore, it was useless to expose the flakes to sunlight and to produce their phosphorescence; the emission of the new radiation was produced in the drawer, protected from all known exciting radiation; that emission was *spontaneous*, and seemed to defy the principle of conservation of energy because at that time there was no reason to think that matter could be an energy reservoir. Radioactivity had been discovered. (Jean Becquerel, 1924, p. 20)

Other historians prefer to describe the discovery of emission of radiations by metallic uranium as the culminating point of Becquerel's contribution:

> With this last announcement, on 18 May, Becquerel's discovery of radioactivity was complete [...]. (Romer, 1981, p. 559) [11]

At another place, Romer also associated Becquerel's paper of 18th May 1896 to the completion of the discovery of radioactivity:

> Seven weeks more went by and the discovery was complete. Uranium always gave out the penetrating rays, whether it was in fluorescent or non-fluorescent crystals, whether in the light or in the dark, whether dissolved in water or isolated in Moissan's pure and uncombined metal. (Romer, 1964, p. 18)

[11] Notice that this was not Becquerel's last paper.

Generally, historians do not associate the discovery of radioactivity with Becquerel's *first* experiment. Indeed, at that time he observed for the first time the photographic effect of a radioactive substance, but he was far from understanding that he was facing a new phenomenon. For that reason, a date is usually chosen that seems related to some fundamental insight on the nature of the new phenomenon.

Once it was even claimed that Becquerel discovered radioactivity *several years before* 1896. According to Bertrand (1946), towards the end of 1893 or beginning of 1894, Henri Becquerel had already observed that a piece of pitchblende was able to darken a nearby photographic plate wrapped in black paper, kept in a drawer. Becquerel was not able to understand the phenomenon, and Bertrand suggested to him a chemical explanation: maybe the photographic plate had been affected by vapours emanated from the mineral. He suggested that Becquerel should try whether pitchblende could act upon the photographic plate through a thin tin sheet or paper impregnated with lead acetate.

> Some days afterwards, Henri Becquerel came again to see me and informed that my explanation was not correct, but, he added, *I have found it*. I was not indiscreet and did not ask him what was it; the following discoveries gave me the elucidation that I wanted and, at the same time, they revealed us that the fortuitous impression of a spot produced by pitchblende upon a photographic plate was the first observation of radioactivity made by Henri Becquerel. (Bertrand, 1946, p. 699)

Becquerel himself never claimed that he observed this radiation effect with pitchblende before 1896. It could happen that Bertrand's recollection was not correct.

Even those who do not attach a precise date to the discovery accept the Henri Becquerel was the discoverer of radioactivity. After all, that was the reason why he was nominated for the Nobel Prize:

The Royal Academy of Sciences of Sweden decides, on the 12th November 1903, to confer the Nobel Prize in *physics* of this year to

HENRI-ANTOINE BECQUEREL

for his *discovery of the spontaneous radio-activity*

and also to

PIERRE CURIE

and to

MARIE-SKLODOVSKA CURIE

for their *works concerning the radiation phenomena discovered by* HENRI BECQUEREL.[12]

When – and in what sense – did Becquerel discover radioactivity? The two questions are deeply interrelated.

The phenomenon we now call radioactivity is a process of spontaneous transformation of some unstable atomic nuclei, with emission of specific kinds of radiation (α, β and γ). The discoverer of the phenomenon we call radioactivity should therefore be someone who found out that:

a) some atomic nuclei can be transformed into other different nuclei;
b) this transformation is spontaneous (that is, the source of energy is internal, and no external phenomenon triggers the transformation);
c) the transformation has a speed that falls exponentially with time, with a characteristic half-time for each kind of atomic nucleus, that cannot be changed by external influences (temperature, chemical reactions, etc.);
d) there are different kinds of radiation emitted by those nuclei (alpha particles, equal to Helium nuclei; beta particles, equal to

[12] *Les Prix Nobel en 1903* (Stockholm, 1906), 2. Notice that Becquerel's name was Antoine-Henri, not Henri-Antoine. Also, the hyphen between Marie and Sklodowska is wrong.

electrons; and gamma rays, an electromagnetic radiation similar to high-energy X-rays).[13]

Not a single one of those components of our current concept of radioactivity can be ascribed to Becquerel. One can safely state that none of those aspects of radioactivity had been ascertained by Becquerel or anyone else before 1900 (except, perhaps, its spontaneity). Indeed, each of those aspects of radioactivity was established through a long line of experimental research combined with theoretical work. Many people contributed bringing together some parts of the puzzle – and sometimes with parts that did not fit the puzzle. Around 1903, on the contrary, the knowledge about natural radioactivity was close to what we accept nowadays. If some single name should be credited for this discovery, a likely candidate would be Ernest Rutherford, to whom is due a considerable fraction of our knowledge of radioactivity.

It would be very odd, however, to call Rutherford the discoverer of radioactivity. After all, he was not the first one to observe effects due to radioactivity. But what ground is there to give the title to Becquerel? Was his contribution a singular, necessary step in the development of radioactivity research?

Let us play with counterfactuals. Suppose Henri Becquerel had died before 1896. Would there be any delay in the discovery of radioactivity? Probably not. Independently of Becquerel, in the early 1896, other researchers were looking for penetrating radiations emitted by phosphorescent substances, and it was natural to try uranium compounds.[14] Without the contribution of Becquerel, it could even occur that other people – perhaps

[13] Nowadays we know, of course, that the emission of beta particles is accompanied by neutrinos. In the case of artificial nuclides, instead of the usual beta rays (electrons) the atom may emit positrons (positive electrons).

[14] Independently of Becquerel, Silvanus Thompson and Lea Carey searched for X-rays emitted by uranium compounds. For details, see my previous paper on this volume.

Silvanus Thompson – would make *correct* observations and notice that uranium radiation is not reflected, refracted or polarized. In that case, the development of the field would have been faster than it really was.

Yes, but it *was* Becquerel and no one else who observed the first effects of radioactivity, wasn't he? Not exactly. Before the discovery of the radioactivity of thorium, Becquerel (as most other scientists) believed that he had found a new kind of phosphorescence. That is: he supposed that some substances (anything containing uranium), after receiving energy from sunlight, slowly emitted an invisible, penetrating electromagnetic radiation similar to ultraviolet light. He did neither suggest that there was any subatomic transformation involved in what he studied, nor that the transformation was spontaneous, nor did he understand what kind of radiation was emitted by uranium. What he observed was not radioactivity, but hyperphosphorescence.

Someone might say: "Well, Becquerel didn't provide the correct *interpretation* of radioactivity, but after all he discovered the phenomenon".

Did he? If we analyse the details of Becquerel's work, it is possible to perceive that not only his interpretation, but even the *facts* described by him were not correct. The empirical properties of Becquerel's phenomenon do not correspond to the known properties of radioactive bodies – they correspond to his *opinion* about the phenomenon.

This case may be compared to Columbus' discovery of America: he had a wrong opinion about the size of the Earth, and for that reason he thought that it would be easy to arrive to India traveling westwards; he arrived to a place that we nowadays call "America", but he thought he had arrived to China or India (Mentré, 1905). Of course, empirical evidence was against his belief: the language, customs, dresses and physical appearance of the inhabitants was different from what was expected. Animals and plants were also different from those that were known to inhabit South Asia. Even when the accumulated evidence was clearly against his belief, however, he retained his interpretation. He never suspected he had discovered a new continent. In which sense did Columbus discover America? Only in the sense that he was the first European of his time to reach the place we call America and to announce his travels to the Old World.

Sometimes the "discovery" of a phenomenon is reduced just to this: a first contact or a first observation of something, even if the interpretation was wrong. Is it possible, in that sense, to ascribe to Becquerel the discovery of radioactivity? In that case, should we associate the discovery with his first experiment?

Let us suppose that Becquerel's first contact with radioactivity did happen as Bertrand told it. In that case, should we change the date of the discovery of radioactivity and say that Becquerel discovered radioactivity about the end of 1893 or beginning of 1894?

Finally, what is the *minimum* accomplishment someone must have done in order to be credited with the discovery of radioactivity? If we want to maintain that Becquerel discovered radioactivity in 1896, it will be necessary to reduce the "discovery of radioactivity" to this: some substances (specially those that contain uranium) emit invisible radiations that can pass through thin opaque bodies and produce photographic effects. Although Becquerel thought at first that this effect was produced by sunlight, he also noticed that the emission of radiation would continue for a long time in darkness.

Now, if that is enough to establish the discovery of radioactivity, then radioactivity was discovered 40 years *before* Becquerel, by Niepce de Saint-Victor.

5. NIEPCE DE SAINT-VICTOR'S EXPERIMENTS ON LIGHT STORAGE

Hitherto, historians of science have not seriously considered the possibility that Henri Becquerel had been anticipated in the discovery of radioactivity. Badash remarked that uranium compounds had been in use for decades, "but there had been no hints that uranium was steadily emitting unseen, penetrating radiations".

> This is perhaps unusual in itself. Once a significant discovery is made, numerous priority claims are often lodged. To my knowledge, there have been no public claims for the prior discovery of radioactivity, and only one simultaneous assertion by Silvanus P. Thompson. (Badash, 1965, p. 63, footnote 29)

However, not long after the publication of Henri Becquerel's early "radioactivity" papers, there was a claim that Niepce de Saint-Victor had discovery the invisible, penetrating uranium radiation in the period 1857-1861. In his very popular book *L'évolution de la matière*, Gustave le Bon charged Becquerel with plagiarism:

> It was at the same time that Mr. Becquerel published his first researches. Repeating the forgotten experiments of Niepce de Saint-Victor and making use of uranium salts, as he [Niepce] had done, he [Becquerel] has shown, as the former one had already shown, that those salts give off in the dark some radiations that can affect photographic plates. Continuing for a longer time than his predecessor the experiments, he observed that the emission seemed to persist indefinitely.
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> What are those radiations? Always under the influence of the ideas of Niepce de Saint-Victor, Becquerel initially believed that they amounted to what Niepce called "stored light", meaning a kind of invisible phosphorescence; and to prove it, he set up experiments which were described at length in the *Comptes Rendus de l'Académie des Sciences* and which led him to believe that the radiations emitted by uranium are refracted, reflected and polarized. (Le Bon, 1906, pp. 35-36)

Claude-Félix-Abel Niepce de Saint-Victor (1805-1870) was a cousin or nephew (Larousse, 1865-1876, vol. 11, p. 999) of the famous Joseph Nicéphore Niepce – one of the originators of photography. Influenced by his relative's work, he began to develop photographic experiments in 1846, that led him to the discovery of the first process for producing negatives on glass using a film of albumen to hold the sensitive compound (Gibson, 1923, p. 23; Potonniée, 1936, pp. 218-222).[15] Among his several researches on photography and light, there was a very interesting study on what could be called an invisible phosphorescence by common materials.[16]

> Does a body, after being strocken by light or put under the Sun, keep in the dark some impression of that light? That is the problem that I tried to solve by photography. Phosphorescence and fluorescence of bodies are well known; but, as far as I know, nobody ever did experiments such as those that I am going to describe.

In his first experiments,[17] Niepce de Saint-Victor used a printed paper. It was first kept in darkness for several days. Then, half of the paper was covered with an opaque screen, and the other half was exposed to sunlight during 15 minutes or more. Afterwards, in a dark room, the whole printed paper was applied to a photographic plate. After one day, the plate was developed and showed a negative copy of the part of the print that had received the light.[18]

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[15] His first publications: Niepce de Saint-Victor (1847; 1848a; 1848b, 1850a; 1850b).

[16] His results were first published as short communications to the Paris Academy of Sciences, and afterwards as a full report (Niepce de Saint-Victor, 1857-1867; Niepce de Sain-Victor, 1861). Some of his communications were also published (in full or as extracts) in other journals.

[17] Meeting of 16th November 1857 of the Paris Academy of Sciences.

[18] The observed effect could be partially explained by Colson's finding, four decades later, that contact with dry ink affects photographic plates: it produces an oxydation effect that turns the plate insensible to light, at the points of contact (Colson, 1896). A

Niepce de Saint-Victor reported that all kinds of papers produced the same effect, although in different degrees. Similar effects were produced by wood, ivory, parchment, marble, chalk, porous porcelain, cotton, and other substances. Metals, wood charcoal, vitrified porcelain and glass produced no effect.

The effect seemed not be produced by heat, since black or dark paper produced no effect upon the photographic plate, although dark surfaces become hotter than white paper under sunlight.

At a first sight, the list of substances that Niepce de Saint-Victor described as emitting invisible radiations seems very odd. Let us, however, recall the knowledge of his time about phosphorescence.

There are some phosphorescent substances that can shine in the dark for a long time – even several hours. This effect was first observed with some jewels, and in 1604 the so-called Bologna phosphor (barium sulphide) was first described (Edmond Becquerel, 1859, p. 9). Afterwards, several other strongly phosphorescent bodies were found, such as the Canton phosphor (calcium sulphide), strontium sulphide and a kind of calcium fluoride called *chlorophane*. In the 19th century, systematic search showed that many other substances exhibited a short lived phosphorescence (lasting from a few seconds to a fraction of a second). Among those substances, it is relevant to cite chalk, sugar, paper (Edmond Becquerel, 1859, pp. 10-11), and several other organic substances, such as tartaric acid, lactose, teeth, silk, etc (*ibid*., pp. 22-23). Therefore, most of the active substances described by Niepce de Saint-Victor were not entirely devoid of phosphorescent properties. Edmond Becquerel stated:

> The phenomenon of phosphorescence by insolation is much more general than is generally thought, [...] We shall see that a very large number of bodies give rise to effects of

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similar suggestion had already been made much before (Malone, 1862).

> the same order as the alkaline earth sulphides, and, as I have proved, certain substances that do not present emission of light after insolation, keep nevertheless the impression due to the action of the radiation, but for a time too short to allow the effect to be observed in ordinary circumstances. (Edmond Becquerel, 1859, p. 12)

It could happen, however, that those substances emitted some non-visible radiation for a long time after their visible phosphorescence had died out.

By interposing plates of different substances between the printed paper and the photographic plate, Niepce de Saint-Victor observed that the effect did not traverse glass, mica, crystal, etc. A print covered by gelatin or collodion could be reproduced by this process, but varnish or glue prevented the effect.

Niepce de Saint-Victor examined whether the effect was due to the direct contact between the print and the photographic plate. Even when they were separated by a few millimetres, the effect still occurred.

Among several curious observations, he described the following experiment. The inner surface of an iron tube was covered with white paper and exposed to the rays of the Sun for about one hour. If it was hermetically closed immediately after exposition to light, "it will keep during an indefinite time the power of radiation that was communicated to it by the sunlight" (Niepce de Saint-Victor, 1861, p. 37). Hence, it seemed possible to store light in a can.

Niepce de Saint-Victor also made some experiments with fluorescent substances.

> A drawing on white paper, made with a solution of quinine sulphate, one of the most fluorescent of known bodies, exposed to the Sun and applied to a sensible [photographic] paper, is reproduced in black with a greater intensity than the white paper that constitutes the basis of the drawing. (Niepce de Saint-Victor, 1861, p. 38)

He remarked that the effect was not due to chemical action: when the quinine drawing was not exposed to light, it produced no effect upon the photographic paper. In his second paper, Niepce de Saint-Victor remarked that the effect was stronger replacing the quinine by a solution of tartaric acid or uranium nitrate. The effect was also observed when there was a distance of two or three centimeters between the drawing and the photographic paper, if the line of the drawing was sufficiently thick.

In his second communication on this subject (1st March 1858), Niepce de Saint-Victor described a new kind of experiment.

> One takes a sheet of paper that was kept several days in darkness; one covers it with a photographic negative on glass or paper; one exposes it to the rays of the Sun during a longer or shorter time, according to the intensity of light, and bring it to darkness; one takes out the negative and treat [the paper] with a solution of silver nitrate. One sees, in a short time, an image that can be fixed by washing in pure water. If one wants to obtain a faster and stronger image, the paper sheet should be impregnated beforehand with a substance [...] with a stronger power of storing the light activity. One very efficient substance of this kind is a water solution of uranium nitrate, obtained by treating uranium oxide with diluted nitric acid, or by dissolving in water uranium nitrate crystals. (Niepce de Saint-Victor, 1861, pp. 39-40)

This observation, as will be seen below, led to the development of new photographic materials, at the time.

A similar, but weaker effect, was obtained replacing uranium nitrate by tartaric acid. A few other substances also produced similar effects: citric acid, oxalic acid, aluminum sulphate, iron citrate, etc.

The experiment of light storage in metal tubes was also reproduced with paper containing uranium nitrate and tartaric acid, and he obtained stronger effects than before.

> I expose to the light of the Sun a sheet of cardboard strongly impregnated with two or three layers of a solution of tartaric acid or uranium salt. After insolation I cover with this cardboard the inner part of a long and thin white iron tube. I hermetically close the tube and I notice that the cardboard impresses a sensible paper prepared with silver chloride after a very long time delay, the same as on the first day. [...] The experiment only succeeds once, that is, it seems that the light escapes completely from the cardboard, and, in order to obtain a second image, it is necessary to have recourse to a second insolation. (Niepce de Saint-Victor, 1861, p. 43)

Niepce de Saint-Victor remarked that the uranium salts are strongly fluorescent, but tartaric acid exhibits no fluorescence whatever. Therefore, the effect seemed independent of phosphorescence or fluorescence.

The phenomenon described by Niepce de Saint-Victor may seem to us unbelievable, but it was not completely different from other known phenomena. Let us recall that when a phosphorescent substance is exposed to light and brought to a dark room, it will shine during some time, but its luminosity will decrease and after a longer or shorter time the substance will seem to have lost all its phosphorescent light. However, there are several phosphorescent substances that can shine again after becoming dark, if they are heated. They can store light energy during a long time. This phenomenon is, of course, different from the one described by Niepce de Saint-Victor, but there are some similarities: in both cases there is some kind of hidden phosphorescence that can remain for a long time in a substance that is not shining any more. Besides, as will be seen below, the active substances produced stronger and faster effects when they were heated – exactly as it occur in calorescence.

In a later communication, Niepce added that the effect of the tube with uranium nitrate or tartaric acid was the same as the first day, *even after several months* (Niepce de Saint-Victor, 1867). The conclusion of the second communication was:

> The experiments described in this Memoir prove, I think, in the most evident way, that light communicates a real activity to some substances stricken by it; in other words, that some bodies have the property of storing light in a state of persisting activity. (Niepce de Saint-Victor, 1861, pp. 44-45)

After many other experiments that cannot be described here, in his fifth communication (1st July 1861), Niepce de Saint-Victor concluded:

> From the whole of my experiments, it follows that this persistent *activity* given by light to all porous bodies, even the most inert ones, cannot be a phosphorescence, because it would not last so long a time, according to the experiments of Mr. Edmond Becquerel. It is therefore more likely that it is a radiation invisible to our eyes, as Mr. Léon Foucault[19] believes, a radiation that does not pass through glass. (Niepce de Saint-Victor, 1861, p. 59)

It was odd, of course, that some kind of radiation that could produce photographic effects could not pass through glass. But the same occurred in the case of ultraviolet light, and Niepce de Saint-Victor also remarked that the light emitted by the combustion of phosphorus in air did not produce photographic effects after passing through glass (Niepce de Saint-Victor, 1859b). At that time, there seemed to be no impossibility in the phenomenon, and it seemed a relevant discovery.

6. REACTIONS TO NIEPCE'S DISCOVERY

Niepce de Saint-Victor's researches were well accepted by the Paris Academy of Science. In 1861 he was unanimously declared the winner of the *Prix Trémont* for his works on photography and light (Chevreul, 1861). The report of the prize committee specially praised the above described experiments:

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[19] I have not been able to find any paper written by Foucault discussing Niepce's work.

> Mr. Niepce has proved the remarkable fact that some bodies receive from the rays of the Sun the faculty of acting afterwards in darkness upon substances that are sensitive to light, as if those bodies were luminous, in such a way that the Sun transmits to them an activity that they keep for months in darkness. (Chevreul, 1861, p. 1140)

The committee was so strongly impressed by Niepce de Saint-Victor's work that it was suggested that the Trémont Prize for 1862 and 1863 should also be given to him. The Academy accepted the exceptional suggestion. This is a strong evidence that his experiments were accepted as relevant and correct by the French scientific community of that time.

In France, François-Napoléon-Marie Moigno, editor of the journal *Cosmos*, gave ample publicity to the work of his friend Niepce de Saint-Victor (Moigno, 1858). Moigno reproduced in his journal several of Niepce's communications to the Academy of Sciences (Niepce de Saint-Victor 1857b; 1858a; 1859a; 1859c) and presented excited reports such as this:

> That is where we have arrived: we can collect light at the end of the world, carry it to any place we want, and bestow it to our great-nephews, who will be able to use it to reproduce our portrait painted by ourselves or by our own face. Wonder! Wonder! (Moigno, 1858a, p. 286)

Moigno was very well informed about the subject. He published several letters and articles upon Niepce's work, and usually added his own comments. For instance: he reproduced a letter written by a gentleman called Charles Piallat who described some facts that seemed to him similar to those observed by Niepce, but without previous excitation by sunlight. Moigno pointed out that the observed effect was a "Moser image" and was not related to Niepce's invisible phosphorescence (Piallat, 1857).[20]

[20] Ludwig Moser's work will be discussed below.

One of the aspects of Niepce's work that called the public attention was the application of his discovery to photography. The time when Niepce de Saint-Victor published his works was a period of fast development of photographic techniques. The use of nitrate of uranium in photographic paper or plates was soon discussed and adopted by many photographers (Hagen, 1859; Blanchère, 1858; Moigno, 1858b; Brébisson, 1858). It seemed to have several advantages: decrease of exposure time, direct acquisition of positive images, etc. It is curious to remark that Niepce was accused of plagiarism by Burnett (1860), who had already used uranium in photography.

In England, Niepce de Saint-Victor's work was also well received. William Grove, one of the early proponents of the "correlation of forces", described Niepce's work to the Royal Institution (Grove, 1858a). He presented it as evidence for his own views on the nature of light (Grove, 1858b). Robert Hunt, a photographic expert, stated that "The recently discoveries of M. Niépce de St. Victor are certainly the most important which have been made since the discovery of photography itself" (Hunt, 1858, p. 15).

Partial translations and comments on Niepce de Saint-Victor's two early papers on "a new action of light" were published by William Crookes, who was the editor of the *Journal of the Photographic Society of London* (Niepce de Saint-Victor, 1857a, 1858c).[21] In editorial announcements of Niepce's works, William Crookes called the attention of the readers, with words of praise:

> We must also direct especial attention to the marvelous discoveries of M. Nièpce de St. Victor, an account of which will be found in our columns. A boundless field for experimental research is therein opened, and we hope that the

[21] Both memoirs were published while William Crookes was the editor of that *Journal*. Crookes then founded a new periodical, where he published a translation of Niepce de Saint-Victor's third communication (Niepce de Saint-Victor, 1858b).

> columns of the Journal will soon show that the path of discovery, so grandly pointed out by M. Nièpce, has been quickly followed up by our home experimentalists. (Crookes, 1857, p. 101)

> It will, doubtless, be remembered that in a recent Number of this Journal we published a memoir by M. Nièpce de St. Victor, revealing, among other highly important scientific facts, the singular property which light possesses of communicating to the bodies which absorb it its chemical action on the salts of silver. [...] The sensation excited by these remarkable discoveries throughout Europe has induced M. Nièpce de St. Victor to continue his researches [...]. (Crookes, 1858, p. 169)

The President of the Photographic Society, Sir Frederick Pollock, was deeply affected by Niepce de Saint-Victor's first paper. In the Annual General Meeting of the Society, a significative part of the President's speech celebrated Niepce's achievement (Pollock, 1858). At the same meeting, Niepce de Saint-Victor was elected a honorary member of the Society.

After a short time, however, Niepce's discovery was challenged. It was suggested that the effects observed by Niepce were due to chemical reactions produced by something emanating from the paper excited by sunlight (Laborde, 1858).[22]

In England, doubts were raised on the very *facts* described by Niepce de Saint-Victor. Pollock, who had received so enthusiastically Niepce's researches, was now skeptical:

> I regret to have to inform you that the hopes I gave you last year have not been realised, and that the experiments of M. Nièpce de St. Victor have not been repeated with success by any English experimentalist. I have heard that Mr.

[22] An anonymous Italian author also claimed that Niepce's images could be produced by vapours: *Cosmos. Revue Encyclopédique Hebdomadaire des Progrès des Sciences et de leurs Applications aux Arts et a l'Industrie* **13** (1858), 335-339.

> Hardwich[23] and several others have tried the experiment and failed. (Pollock, *apud* Crookes, 1859, p. 277; cf. Moigno, 1859a)

Pollock did not doubt the integrity of Niepce. He conjectured that the failure of those duplications could be due to insufficient light intensity, weak sensibility of the photographic paper or some other unknown circumstance.

In order to establish the reality of the phenomenon he had described, Niepce invited Wheatstone to his laboratory in the Louvre and showed him his experiment of light storage in a tube. The experiment succeeded, and Wheatstone took with him two tubes prepared by Niepce, to reproduce the test in England (Moigno, 1859a). Crookes reproduced Moigno's note in his own journal, and added that he had been surprised to see the picture shown to him by Wheatstone in England (Crookes, 1859a, p. 277). Crookes was not convinced, however, that the effect was produced by light, because Niepce himself had also shown that it was possible to produce similar effects using radiant heat.

Niepce's description of the best procedure for obtaining images using stored light led to that interpretation. He recommended that the iron tube should be heated to a temperature of about 60 to 70 degrees (Celsius) before applying it to the photographic plate (Niepce de Saint-Victor, 1859a).

One week after the publication of Niepce's method, Crookes reported that he had repeated the experiment of light storage *without exposing the paper with tartaric acid to light*, but heating it as recommended by Niepce. The hot tube produced the effect that Niepce ascribed to invisible phosphorescence, although the experiment was performed at night and all objects had been kept in darkness before the test. Crookes concluded: "[...] we think we are justified in expressing our opinion that this

[23] At that time, Hardwich had not published his experiments. A short report appeared afterwards (Hardwich, 1859).

heat, combined it may be with a chemical reaction between the bodies in the tin tube, is the actual producing cause of the effect he [Niepce] has described" (Crookes, 1859, p. 301)

Moigno defended Niepce:

> It is evident that Mr. Crookes wrongly interpreted his experiment and erroneously concluded that in Mr. Niepce's tubes it is not light that acts. What does this experiment prove, after all? That heat produces in Mr. Crookes' tube the effects that Mr. Niepce ascribes to light. Nothing more, nothing less. (Moigno, 1859b, p. 272)

Moigno's defense was, of course, very weak. Niepce himself provided a stronger answer: he presented new experiments (Niepce de Saint-Victor, 1859c). He used a sheet of paper without tartaric acid or uranium nitrate. He cut it in two pieces, then he kept one of them in darkness and put the other in sunlight. Both pieces were next put into iron tubes. They were afterwards placed upon a photographic plate, in a dark and cold place, for 24 hours. After that time, the photographic plate showed an image produced by the tube containing the paper that received light, and no image at the place where the other tube was applied. Hence, the effect seemed due to light, not to heat.

Niepce also commented that when tartaric acid or uranium salt is added to the paper, the effect is stronger and that it can be observed even when the tubes are not hot. He ascribed to heat a faster release of the stored light and remarked that the tube should not be heated to 100 degrees, to avoid the effect of radiant heat. He also stated that an alkaline photographic paper with silver salt is insensible to heat, and in this way it was possible to distinguish the effects of light and heat.

In France, too, Niepce was attacked. Gaultier de Claubry (1859) was able to reproduce some of his experiments using hot paper as the source of radiation. The temperature used in those experiments, however, was between 100 and 120 degrees – a temperature that should be avoided, according to Niepce.

Bouillon and Sauvage observed that Niepce's tubes produced a faster effect when they were moist and heated than in the case when they were dry heated. They observed that water vapour alone was also able to produce photographic effects. They also reported that Paul Thénard had been able to produce similar effects with a tube containing a paper sheet impregnated by ozone (Bouillon & Sauvage, 1859).

Niepce de Saint-Victor replied by a new experiment: he placed the iron tube containing cardboard with tartaric acid in an ice-box during 48 hours, and even in that case the tube was able to affect photographic paper (Niepce de Saint-Victor, 1859b).

The Abbot Edme César Laborde presented strong evidence for the action of vapours in Niepce's experiments. Niepce's "stored light" did not act through glass. Laborde showed that when a glass plate is put between the tube but at a distance from the photographic plate, allowing circulation of vapours, the plate was affected (Laborde, 1859). He concluded that the effect was produced by formic acid produced by oxidation of the paper used in the experiments. Another author, T. A. Malone, claimed that the effect observed in Niepce's experiments was due to the ink used by French newspapers (Malone, 1860) – an obviously inadequate explanation, since several of Niepce's experiments were made without any printed paper.

No consensus about the explanation of Niepce's phenomenon was reached. After a few years, the whole subject was simply forgotten. After his death, Niepce de Saint-Victor was reminded for his contributions to photographic technique (Larousse, 1865-76, vol. 11, pp. 999-1000; Dreyfus, 1886-1902, vol. 24, p. 1080). However, his researches on invisible phosphorescence sank into oblivion.

Nowadays it is very difficult to understand what happened in Niepce de Saint-Victor's experiments. No single explanation proposed at that time seems to satisfy all observed facts. It is possible that part of the effect he observed was due to the radioactivity of uranium – this would explain the long persistence of the effects and the lack of action through thick glass. On the other hand, according to Niepce, tartaric acid produced effects similar to those of uranium nitrate in several experiments (for instance, the storage experiments), and the experiments seemed to show that heat and excitation by light increased the emission of the invisible radiation by the uranium salt. If part of the effect observed was really due to the radioactivity of uranium, the situation is similar to what happened to Becquerel: the later also stated that another substance (calcium sulphide) emitted penetrating radiations of the same kind as the uranium compounds; and ascribed to the uranium radiation many properties that have not been confirmed afterwards.

7. WAS NIEPCE ANTICIPATED BY MOSER?

Niepce's work was sometimes compared to experiments on invisible light that had been made fifteen years earlier, by Ludwig Moser (see Hunt, 1858; Pollock, 1858, p. 157). A short description of Moser's researches is useful, as it provides another example of the difficulties of understanding an obscure phenomenon.

The period following the invention of the Daguerre photographic process was full of ingenious investigations concerning the effect of light on matter. In a series of papers, Ludwig Moser compared the effects of light, pressure and vapour condensation on several material surfaces. The three kinds of influence were able to produce latent images upon all tested surfaces (Moser, 1842a; Moser, 1843a). According to Moser, if some region of a surface is touched, acted by light or simply breathed on, it acquires the property of precipitating all vapours, which adhere to it (or combine chemically with it) on these spots differently to what it does on the other regions. In this way, any of those influences can produce latent images that can be revealed and fixed by reaction with suitable vapours.

> By these experiments I think I have proved that *contact, condensation of vapours, and light produce the same effect on all bodies.* The differences which appear may be referred to the varying intensity of the producing cause, and the greater or less depth to which the action extends. [...] The most general axiom that I can propose with reference to the influence of the above-mentioned causes is, *that by their means the affinity of all bodies for vapours is modified*, so that they are precipitated and adhere to them in a greater or less degree. (Moser, 1842a; Moser, 1843a, p. 456)

At the end of Moser's first paper there is an *Addendum* where he remarked that contact was not necessary to produce an action upon a surface: one body can act upon another even at a distance, in darkness. This is one of his experiments:

> A plate of agate with several engraved figures was covered with thin strips of mica, and upon these the silver plate was laid, so that the space between the two surfaces amounted to one-fifth of a line, and admitted of seeing through; when, after the lapse of several hours, the plate was introduced into the mercurial vapours, a perfect image of the engraved figures was produced. I have examined other bodies placed at a greater but not measured distance, and always found them depicted, and have thereby discovered the curious fact, that *when two bodies are sufficiently approximated they reciprocally depict each other*. (Moser, 1843a, p. 459)

How did Moser interpret the observed phenomenon? He did not ascribe it to vapours, light or heat. His opinion was '*that every body must be considered as self-luminous*' with emission of an invisible radiation. The phenomenon seemed independent of heat and different from phosphorescence, since, according to Moser, "it made no difference whether the bodies have been kept in the dark for a long time or exposed to the sun before the experiments are made" (Moser, 1843a, 459-460).

In his first paper, Moser described that the following substances were observed to produce effect at a distance upon a silver plate: silver, iodized silver, brass, iron, steel, violet and red glass, black polished horn, white paper, gypsum, mica, agate and cork. In a second paper (Moser, 1842b; Moser, 1843b), written one month later, he added the following to the list of active bodies: gold, copper, German silver, zinc, white transparent glass, bismuth, antimony, tin, lead, mirror metal, wood, mother-of-pearl, black pasteboard, black leather, black velvet, and lamp-black. He generalized his finding to all substances and stated that "it would consequently be a discovery if we could find any body which does not possess self-luminosity, or in which it is present in so small a degree as to escape our observation". Moser also varied the plate upon which the effect was observed, and reported that gold, silver, copper, brass, iron, steel, zinc, porcelain, mica and even liquid mercury were acted by nearby bodies and images could be detected by vapour condensation (Moser, 1843b, pp. 462-463). The images were sharper when the object was closer to the plate receiving the impression. It was possible to obtain images of black characters written on white paper, although Moser remarked that they were not very sharp.

In later papers, Moser published new results. Among other things, he claimed that invisible light (of different colours) was emitted when change of state (condensation of vapour) occurred. He compared the phenomenon to emission of latent heat in changes of state, and hence called that radiation "latent light" (Moser, 1842c; Moser, 1843c).

Moser's work called much attention during a short period. The phenomenon he described was confirmed, but his hypothesis was not accepted. Different explanations of Moser's images were offered by several authors. Erwin Waidele ascribed all effects to exchanges of vapours and gases between sensitive surface and acting body (Waidele, 1845). Hyppolyte-Louis Fizeau explained the images as due to exchanges of greasy organic matter (Fizeau, 1843) and he showed that images were not formed when a thin mica plate was put between the body and the polished plate. M. Knorr and Robert Hunt ascribed the effect to heat (Knorr, 1843; Hunt, 1843). The later showed that the effect occurred even when the surfaces were boiled to eliminate any layer of volatile organic matter. There was no agreement about the agent that produced Moser's images, but there was a consensus that Moser was wrong:

> The beneficial thread that will some day guide us in the tortuous maze of photography has not yet been established; instead of theories we only have more or less plausible hypotheses. We have believed for an instant that Mr. Moser would raise the veil; but he lost himself in the invisible light, as he moved forward guided only by touch [...]. (Moigno, 1847)

Moser images were soon forgotten – the same fate of Niepce's later work.

There are some similarities and many differences between the phenomena described by Moser and Niepce. Moser images were claimed to be produced by any substance upon any other body, without previous excitation by light or heating. In the case of Niepce's researches, some substances were described as able to produce invisible phosphorescence, other substances were apparently inactive; and two substances, in particular, were more active than any other tested by him. Both Moser and Niepce talked about invisible light, but in the first case the emission is supposed to be continuous and spontaneous; on the other hand, the emission of Niepce's invisible light seemed to depend on previous storage of light and could be called, therefore, an invisible phosphorescence.

8. NIEPCE OR BECQUEREL?

Is there any relation between Niepce's invisible light and Henri Becquerel's experiments? Some of Becquerel's contemporaneous scientists thought so. Several scientists recalled Niepce's investigations, after the publication of

Becquerel's researches. One of them was William Crookes. In 1910, he declared:

> Niépce de St. Victor had discovered that Uranium salts possessed the property of storing up light and giving it out in the dark, and in 1858 I took what was perhaps the first radium photograph in this country, by writing with solution of uranium nitrate on a card, insolating it, and then putting it face to face in the dark with a sheet of photographic paper; the image of the writing was reproduced on the paper. (Crookes, 1910, p. 252) [24]

The first mention of Niepce de Saint-Victor's experiments, shortly after Becquerel's experiments, was made in 1896 by Silvanus Thompson:

> It should not be forgotten that so far back as 1857 M. Nièpce de Saint Victor observed many cases in which an object, an engraving on paper or a figured piece of porcelain or marble, immediately after exposure to sunlight, was found capable of giving a photographic impression to a sheet of paper prepared with chloride of silver, with which it was placed in contact. He even used, after exposure to light, cardboard imbibed with salts of uranium or with tartaric acid, and found such to be capable of emitting rays that were photographically active. There was no attempt made, however, to investigate the possibility of transmitting these invisible radiations through opaque bodies. (Thompson, 1896d)

In the same way, Charles Guillaume, in 1897, compared Becquerel's work to that of Niepce de Saint-Victor:

> It is interesting to recall the old experiments of Niepce de Saint-Victor on the emission of radiations in darkness by a

[24] It is doubtful that Crookes did that experiment: he never reported it during the period when Niepce's researches were widely discussed.

> large number of substances, and particularly by tartaric acid and uranium nitrate. [...] Niepce de Saint-Victor found that those radiations did not traverse glass; but this result could be due to a lack of sensitivity of his plates. (Guillaume, 1897, p. 133, footnote)

One of the critics of Becquerel, that reminded him several times of Niepce de Saint-Victor's former work, was Gustave le Bon, who explicitly ascribed to Niepce the discovery of the emission of uranium rays (Le Bon, 1900, p. 299, footnote 1). In a later publication, Le Bon even accuses Becquerel of appropriation of Niepce's ideas without due acknowledgment (Le Bon, 1907, pp. 21-22, 424-426).

An anonymous (and not well informed) review published in December 1897 described Le Bon's "black light" experiments as repetition of Niepce de Saint-Victor's observations (Anonymous, 1897). The author recalled that one argument against Niepce de Saint-Victor's claims was the lack of action of the tubes containing uranium nitrate when a glass was interposed between the tube and the photographic plate; however, according to this author, this was not as conclusive as was thought, because there do exist radiations that would be absorbed by thick glass.

In his 1903 book, one finds the only comments Becquerel ever made on Niepce de Saint-Victor's work.

> When I published my first observations about uranium radiation, some people have tried to bring together those statements to experiences formerly made by Mr. Niepce de Saint-Victor with several papers, some of which were impregnated with tartaric acid or uranium nitrate [...].
>
> Although Foucault had issued the hypothesis of an unknown radiation to explain those phenomena, it was proved that this effect, that was not produced through glass or through a thin slab of mica, was due to chemical actions arising from the decomposition of organic or saline matter by light. It is true that uranium nitrate is among those substances. [...] On those papers, uranium is in such a small amount that in order

> to produce an appreciable impression on the photographic plates used by the author, several months of exposition would have been necessary. Therefore, Mr. Niepce de Saint-Victor has not been able to observe uranium radiation. (Becquerel, 1903a, pp. 51-52)

Becquerel added that the impossibility of reproducing Niepce de Saint-Victor's effects with mica or glass between the photographic paper and uranium proves that the observed phenomena were not produced by uranium rays. He also states that he tried to reproduce Niepce de Saint-Victor's experiments with tartaric acid and plain paper, with black paper between the active substance and a photographic plate or an electroscope, and observed no effect.

Was Henri Becquerel aware of Niepce de Saint-Victor's work when he began his studies of "radioactivity"? Probably not, because that work had long been forgotten and was not cited by his father.

In 1867, Edmond Becquerel published a monumental work on phosphorescence: *La lumière, ses causes et ses effects*. After publication of this book, no other comprehensive treatise on this subject appeared in any language until the next century (Harvey, 1957, p. 221). The effect observed by Niepce de Saint-Victor was not described in Edmond Becquerel's book.[25] It was not, however, completely neglected by other contemporary phosphorescence researchers. It was referred in a book published by Thomas Lamb Phipson (1870, pp. 72-76), who described the "invisible phosphoresce" discovered by his "ingenious friend" Niepce de Saint-Victor. Phipson reported that he had seen experiments with tubes containing cardboard imbibed with tartaric acid or uranium salt, that produced photographic effects a few months after their exposure to light. Phipson did not describe any criticism concerning Niepce's

[25] In this book, there is a passing mention of the use of uranium salts in photography, but no direct reference to Niepce's work (Edmond Becquerel, 1867-1868, vol. 2, 73).

experiments. If Henri Becquerel had read Phipson's book, that chapter could have suggested him the idea of invisible phosphorescence of uranium compounds. There is no evidence, however, that he ever read the book.

9. THE CONCEPT OF SCIENTIFIC DISCOVERY

The process of discovery is a very complex process. The following analysis is an intended contribution to the elucidation of the meaning of scientific discovery of new phenomena[26]. "Discovery" is not a technical term with well defined meaning. For that reason, although the analysis presented below conforms to some current uses of "discovery", it cannot conform to all different and mutually contradictory uses of the word.

I will call "discovery of a new scientific phenomenon" a complex operation that includes, among other things, the individual (or group) research work, communication and social acceptance of the following aspects:

1) To have contact with the phenomenon.

   The researcher might meet or to find a new phenomenon by chance or while looking for it. Prediction without actual detection cannot be called a "discovery".

2) To notice that it is a new phenomenon.

   The researcher can only conclude that something is new by comparing it with other known similar phenomena, remarking relevant differences. If a researcher is in contact with a phenomenon, but does not notice that it is different from known phenomena, he made no discovery.

3) To identify the phenomenon.

[26] The concept of phenomenon used in this paper agrees in most aspects with Hacking's use of the term (Hacking, 1983, chapter 13, especially p. 225). However, I prefer to use "to discover" than "to invent" a phenomenon, even when it is an effect artificially created in the laboratory.

To identify a phenomenon is different from understanding it. In order to allow further investigation of some phenomenon, it is necessary to recognize when it is present and when it is not, noticing similarities and distinguishing the phenomenon from similar but different phenomena. The characterization of a new phenomenon is usually dynamic: it changes in time. However, *some* identification (even if temporary) is desirable. When several different phenomena are confounded, the specificity of the new phenomenon is lost.

4) To identify conditions that make the phenomenon and its main effects reproducible.

This step corresponds to the detection of relevant conditions and/or causes of the phenomenon. When this aspect of the investigation is fulfilled, the phenomenon becomes reproducible, if the conditions can be controlled. Of course, further research may show that some conditions that seemed irrelevant are important, and vice-versa; but it is desirable to *attempt* the identification of the relevant conditions.

5) To find and to provide adequate evidence for some properties (generalizations and exceptions) of the phenomenon.

The empirical investigation of the phenomenon provides data that must be analyzed in order to infer the properties of the phenomenon.[27] In this step, there is an interplay of facts and arguments – data and interpretation. The facts should be carefully collected and tested, in order to avoid wrong data; and the arguments should be sound. Whenever possible, quantitative aspects should be measured and empirical laws should be proposed.

6) To understand the phenomenon.

[27] Cf. the analysis of *phenomena* and *data* presented by James Woodward and James Bogden (1988).

It is desirable to suggest a plausible interpretation (one that does not directly conflict with known facts), to provide evidence for or against hypotheses, to present evidence to distinguish between alternative interpretations, etc.

7) To include the phenomenon in the domain of a broad scientific theory.

The integration of a new phenomenon to a wider scientific theory is the final *desideratum* of the discovery. The integration presupposes the empirical knowledge of a large number of properties of the phenomenon and the study of compatibility between them and consequences of the theory.

I propose to classify as an *empirical discovery* of a scientific phenomenon the fulfillment of desiderata 1 to 5. The scientific relevance of the phenomenon, however, depends on the fulfillment of desiderata 6 and 7, because an isolated, unexplained phenomenon, is of lower scientific value. We might call the fulfillment of all desiderata 1 to 7 the *complete* or *full discovery* of the phenomenon.

Scientific knowledge is never complete. It is always possible to find out new conditions that influence some phenomenon, or to find some new properties of an old phenomenon. The discovery is accepted as such after *some* relevant conditions and *some* properties of the phenomenon have been established – completeness is neither required nor possible.

Scientific knowledge is temporary. Therefore, at any given time, there might be accepted properties and hypotheses that are afterwards dismissed as wrong. At each time, the researcher who found and provided evidence for the accepted knowledge concerning the phenomenon can be regarded as its discoverer. If, however, at another time, the data and interpretation provided by some researcher are rejected, he will no longer be called the discoverer of the effect – except in ironic phrases, such as “Blondot was the discoverer of N-rays”.

According to the above analysis, Röntgen was the *empirical discoverer* of X-rays because he had contact with the phenomenon, noticed that it was a new phenomenon, identified it, established conditions that made the phenomenon and its main effects reproducible, and found and provided adequate evidence for some properties (generalizations and exceptions) of the phenomenon. All the properties of X-rays described by Röntgen were confirmed by later observers. Of course, many properties that were not described by him were found by other researchers, but Röntgen had already shown, in his first paper, the existence of a new, reproducible, recognizable phenomenon. Röntgen was unable, however, to understand the nature of X-rays and to link it to broader physical theories.

Full discoveries are usually preceded by prediction. The full discovery of electromagnetic waves was done by Hertz – the theoretical explanation was already available, of course, but Hertz was able to produce the waves, to study their properties and to exhibit an agreement between theory and experiment.

Discoveries can seldom be ascribed to a single person. Even in the case of empirical discoveries, contributions from different researchers might be necessary before the phenomenon is adequately described. For instance: in the case of the discovery of Brownian motion, there was a wide gap between the early observation and description of motion of microscopic particles in a liquid and the elucidation of the relevant conditions and properties of the phenomenon.

10. CONCLUSION: THE DISCOVERY OF RADIOACTIVITY

After all, can anyone claim that Niepce de Saint-Victor discovered radioactivity? Yes, if we reduce "discovery" to the first contact with a phenomenon. No, if discovery of a phenomenon also implies the discrimination between that phenomenon and other similar but distinct phenomena, the correct ascertainment of properties and the correct interpretation of phenomena. According to the analysis of "discovery" presented above, the empirical discovery of radioactivity cannot be ascribed to Niepce.

Can Henri Becquerel be called the discoverer of radioactivity? No. If we reduce "discovery" to the first contact with a phenomenon, Becquerel was preceded by Niepce. If discovery of a phenomenon also implies the discrimination between that phenomenon and other similar but distinct phenomena, the correct ascertainment of properties and the correct interpretation of phenomena, neither Becquerel nor Niepce discovered it.

Why, then, did the Royal Swedish Academy of Sciences ascribe the discovery of radioactivity to Becquerel? Did the Swedish Academy assume another different criterion for assigning the discovery?

When the Nobel Prize was given to Henri Becquerel, the President of the Swedish Academy presented a justification. According to this presentation speech, Becquerel

> [...] demonstrated that these substances [salts of uranium] emit rays of a special nature, distinct from ordinary light. Tests continued and he established an even more extraordinary fact, namely that this radiation is not in direct relation to the phenomenon of phosphorescence, that phosphorescent as well as those which are not can give rise to this radiation, that previous lighting is never necessary for the phenomenon to occur, and lastly that the radiation in question continues with invariable force to all appearances without its origin being traced to any of the known forces of energy. This was how Becquerel made the discovery of *spontaneous radioactivity* and the rays that bear his name. (Törnebladh, 1903, p. 14)

> Becquerel had already shown by the study of uranium radiation some of the most important properties of those rays.[...]
>
> Becquerel radiation resembles light in several respects. Propagation is rectilinear. [...] Yet it differs from light in certain essentials, for example by its property to pass through

> metals [...] and lastly by the absence of phenomena of reflection, interference and refraction, characteristic of light. In this the Becquerel rays are exactly similar to Röntgen rays and cathode rays. It has been found all the same that Becquerel radiation is not homogeneous, but is composed of different kinds of rays [...] (Törnebladh, 1903, pp. 15-16)

According to the analysis of "discovery" presented above, if Becquerel had done what the President of the Swedish Academy of Sciences described, he would have deserved the epithet of [empirical] discoverer of radioactivity. However, as a matter of historical fact, Becquerel did not do what was ascribed to him. It was not because of a different concept of "discovery" or due to the use of different criteria that the Nobel Prize was given to Becquerel – it was because the Swedish Academy of Sciences was ill informed about the real contribution of Becquerel and other researchers to the knowledge of radioactivity. This misinformation was not due to chance: it was due to Henri Becquerel's systematic propaganda strategy.[28]

As it often occurs, the full discovery radioactivity was the result of a gradual and collective effort. Its beginning was the search for penetrating radiations emitted by luminescent bodies, motivated by Poincaré's conjecture. It was completed by the development of Ernest Rutherford and Frederick Soddy's theory of transmutation of the elements.

ACKNOWLEDGEMENTS

This work was produced while the author was a visiting scholar of the Department of History and Philosophy of Science, University of Cambridge, and visiting fellow of Wolfson College (1995-1996). The author is grateful to the São Paulo State Research Foundation, Brazil (FAPESP) for supporting this research.

[28] See MARTINS, Roberto de Andrade. Becquerel's experimental mistakes, in this volume.

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Scientiarum Historia et Theoria Studia, volume 2

Roberto de Andrade Martins

Historical Essays on Radioactivity

Extrema: Quamcumque Editum, 2021

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Paperback edition: ISBN 978-65-996890-2-4
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