

WHO INVENTED THE SUPERHETERODYNE?¹

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INTRODUCTION

The birth of the superheterodyne gave rise to bitter controversy in the years 1925 and 1926. Our friend Robert Champeix has been good enough to sum up in a few pages the principal aspects of the problem. Thanks are due him, for this subject is a delicate one.

Our purpose in publishing this work is not to rekindle quarrels and emotions which have long lain quiet, but to show via this example just how difficult it is to establish a historical truth in science or technology.

The origin of any invention, the terms used in articles and in patents to define the purposes may vary with the authors who may be so full of their subject as to forget to give a precise definition, thereby inadvertently bringing new meaning to an old term, not having foreseen all the possible applications. One can also imagine the distortions of meaning introduced by translations.

Between the conception of a new idea and its special vocabulary, and its final acceptance as truth, many years may pass. In between, there are often dialogs between deaf persons, even persons of good faith, especially those who are touchy on the subject.

FOREWORD

One day in January 1968, the author happened to be at the Maison de la Radio, where he was taking a part in setting up the French Radio and TV's museum. Pierre Sabbagh had spoken some time earlier during a special broadcast to people who might have old radio apparatus, asking them to make donations or loans to the museum. This call was heard by many former "wireless" men.

This is how we saw the arrival of a gentleman, elderly but elegant, who introduced himself as Paul Laüt. This name was not unfamiliar to me, but the memories were not clear. It was then that he said to me "I was part of Colonel Ferrié's team during the 1914-18 war." Overjoyed to meet one of the few survivors of this heroic time, I asked him about many things. He was glad to answer me.

By chance during the conversation he made an astonishing statement: "I was the first to have the idea of reception by frequency conversion." I already knew, like most who are interested in the history of radio, about the dispute which had opposed Lucien Lévy and Edwin Armstrong over priority in the invention of the superheterodyne. Was there a "third man"? I asked Mr. Laüt to explain, which he did enthusiastically. Obviously he was very wrapped up in the subject.

"I was mobilized in August, 1914. I was 24 years old. Since I was an electrical engineer, I was assigned to the team of Colonel Ferrié at the

1. Originally published in the *Liaison des Transmissions*, No. 116 (March-April 1979) and No. 117 (May-June 1979). Reprinted in English by permission.

Eiffel Tower. I took an active part as early as the beginning of 1915 in the development and fabrication of the first apparatus using the TM tubes which had just been developed by Professor Abraham's group in Lyon.

In 1916 I fell seriously ill with tuberculosis. The Army sent me to the Berck Sanatorium, where I spent a year. As my illness did not impair my intellectual activity, Colonel Ferrié assigned various subjects to me. It was thus that I established the theory of the function of three-element vacuum tubes.

The military telegraphy [organization] had just developed the first heterodynes designed to render audible, thanks to beats, telegraphic transmissions in continuous waves. While studying reception using heterodynes, I had the idea of using these to obtain, by beating them with the wave to be received, oscillations at lower but still inaudible frequencies which could then be easily amplified. On the first of February 1917, I wrote a note that I sent to Colonel Ferrié, who, as it was required in the service, sent a copy to all his assistants.

Once back on my feet and back at the Eiffel Tower in the end of 1917, I was not a little surprised to learn that Lucien Lévy, who was also a member of the Ferrié team, had taken out a patent, during August, on an invention that was nothing less than the one that I had described in my note of February 1.

Naturally I lodged a protest with the Colonel, but he did not like difficulties. He reminded me that we were at war and that any disagreements between French scholars or engineers would be out of place. I did not press the matter, but later, in 1926, I had the satisfaction of seeing my priority recalled by the review *L'Antenne*.²

PART ONE: THE IDEAS

THE PRINCIPLES

To retrace the history of the superheterodyne,² one must first review the principles. We beg the indulgence of readers who are already familiar with what is to follow.

Between 1910 and 1920, wireless used virtually nothing but wavelengths greater than 1000 meters: their propagation being nearly without anomalies meant that they were reliable. Quickly, however, the crowding of the airways led to research into the use of shorter wavelengths, since the number of channels that can be squeezed into a given bandwidth is proportional to the mean frequency of the band. At the same time attempts were made to increase the sensitivity of receivers in order to augment the range of radiotelegraphic communications.

2. Let us outline a point of grammar here. The word "superheterodyne" is formed from *super* (above) and *heterodyne* [heteros = other, dyne = force]. The word "force" being feminine (the same as the unit of measure of force, the dyne), radioelectricians have naturally given the feminine gender to *heterodyne*. Logically, they should have done the same thing for *super heterodyne*, and some people have actually done so, but, when mention was made of a *superheterodyne*, the understanding was "receiver." The result was that the masculine gender was finally accepted as usage. The *20th Century Larousse* ratified this tradition.

The first idea, of course, was to use the newly created triodes (see [3] for the invention of the triode) and to multiply the number of stages of amplification using these triodes. Progress in this direction soon ran into a snag: the fact that the amplifier began to oscillate internally because of the built-in capacities either between tubes or between the grid and the plate of an individual tube. This nullifying factor was the more bothersome as the frequency of the wave to be received was higher.

It was possible to use shielding to prevent inter-tube regeneration; there was nothing to be done about the inter-electrode capacities of the tube itself. It was not until much later, about 1924 or 1925, that the "neutrodyne" method was used to counteract this problem, and finally the screen-grid tube.

The fundamental principle of reception by frequency conversion ("superheterodyne") consisted of causing the oscillation F_1 received by the antenna to beat with a local oscillator (a heterodyne) of frequency F_2 , and thus to obtain a signal of intermediate frequency $F = F_1 - F_2$ much lower than the frequency F_1 of the incoming wave, this frequency being much more easily amplified than F_1 since the troublesome capacities had a relative influence on the frequency F that was much less than they would have had on F_1 .

After amplification, a classic detector revealed the audible frequencies which modulated the intermediate frequency F in the same way they originally modulated the incoming F_1 .

Let us recapitulate the characteristics and the advantages of reception by frequency conversion:

A) Very high sensitivity resulting:

- From the possibility of obtaining very high gain in the intermediate amplifier;
- From the possibility of a reasonable degree of amplification before the frequency changer without the risk of having the two amplifiers interact since their frequencies are very far apart;
- From the gain inherent in the injection of a heterodyne;
- From a greater efficiency of the final detection since this detector is working at a high signal level.

B) Simplicity of tuning due to the fact that the intermediate-frequency amplifier is tuned once and for all, while tuned radio frequency amplifiers require tuning of each amplifying stage for each frequency to be received.

C) Excellent selectivity because the method allows the augmentation of the number of tuned circuits as well as the use of a loop as a wave collector.³

We will not discuss the disadvantages of the first superheterodynes (numerous interference points at audible frequencies and thus whistles) because they were worked out later on. These disadvantages were judged to be serious enough at the time that the TRF circuit had fierce partisans for many years.⁴

3. It should be noted that, contrary to certain formerly accepted ideas, the improvement of selectivity does not come from the actual principle of frequency conversion.

4. In 1933, nearly ten years after commercialization of the first superheterodynes, Philips, which was manufacturing excellent TRF receivers, scoffed at frequency conversion in an advertising brochure entitled "What is the difference between a superheterodyne and a locomotive?" Answer: "None, both of them whistle at every station." The following year,

THE PATENTS

Having said all this, we will now have a look at the various patents involved whose principles can be connected with the method of frequency conversion defined above. We will be aided in this by de Bellecize [1] and Hémadinquer [2].

1. MEISSNER

On the 15th of January 1913, Meissner, who was an engineer at the Gesellschaft für Drahtlose Telegraphie, filed in Germany a patent whose French translation was filed the 21st of January 1914 under the number 467,747. The claims of Meissner are interesting for several reasons. (A) Among the first, he describes the beneficial effect of regeneration in a triode circuit, as well as the oscillating function which is an extreme case of regeneration.⁵ (B) He describes as well the reception of continuous waves using a local oscillator, insisting on the "reinforcement" of the signal thus obtained. (C) Then follow two paragraphs which are not entirely clear: "The interference circuit described can also be used several times in succession in such a way that the interfering oscillation produced at first can act conjointly with a second generator of oscillation to produce a current of a different frequency and thus come back to interfere again." Also: "Several relays⁶ acting as generators of oscillations and each coupled to a detector circuit are used in succession to provoke effects of interference, the indicator (the telephones) being connected only to the last relay circuit to render perceptible the sounds received." Finally: "There is a notable reinforcement produced, considering that one can make the amplitude of the oscillations produced by the relays considerably greater than that of the waves coming in."

That is all, and one must exert a great deal of good will to declare, as does de Bellecize, that in this patent "the method called later on superheterodyne is described with the greatest precision." In fact, Meissner describes primarily reception by heterodyning, about which we know that it not only renders unmodulated continuous waves audible but also that it increases by its nature the strength of the signal, the heterodyne adding its own energy.

It will be seen in any case that Meissner in no way describes the essential point of frequency conversion, that is, the possibility of amplifying at will the intermediate frequency while reducing the risk of provoking unwanted oscillations.

2. ROUND AND ALEXANDERSON

In all fairness we should call to attention the two patents which, albeit distantly, are connected to this study. H. J. Round of Marconi's Wireless Telegraph Company filed in England on the 28th of November 1913, and then in France on the first of July 1920, a patent whose French number is 518,396. Alexanderson filed in the United States on April 19, 1916, a patent which in France, will have on May 8, 1919 the number 499,212.

Philips would adopt frequency conversion in turn and would not drop it again.

5. On this point, Meissner found himself in competition with de Forest, Langmuir and Armstrong. From this came a huge trial which lasted not less than twenty years. In 1934, the Supreme Court of the United States accorded priority to de Forest.

6. It is thus that Meissner called three-electrode tubes.

These two inventors are picking up the idea of causing a local oscillator to interfere with the wave received by the antenna, while looking for a resulting inaudible frequency that will then be detected, but their intentions aim at virtually nothing more than reducing atmospherics, an idea which soon proved illusory.

3. LUCIEN LÉVY

With Lucien Lévy, we arrive at the heart of the problem. He filed two patents on frequency changing, the first number 493,660 on the 4th of August, 1917, and the second number 506,297 on the first of October, 1918.

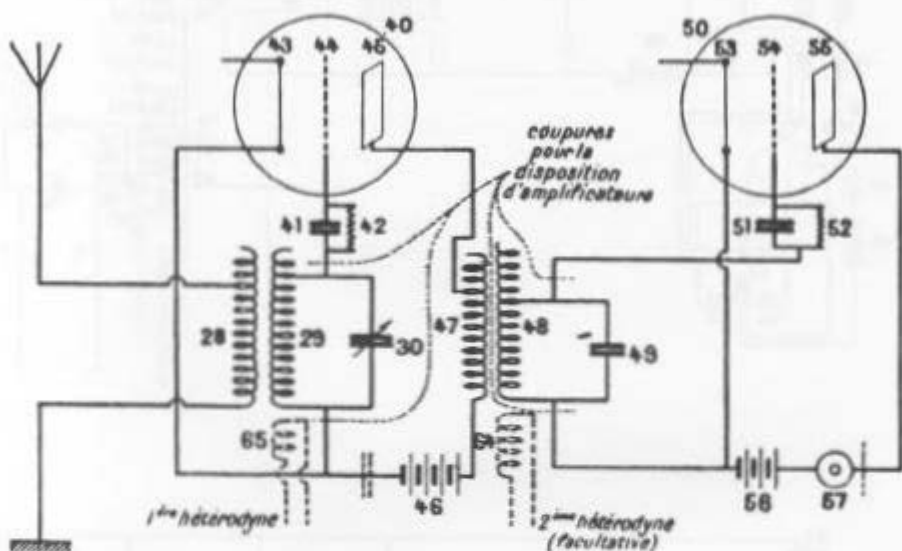


Figure 1. The first Lévy patent, No. 493,650, August 4, 1917.

Patent 493,660 covers a large domain since it begins by claiming the use "both in transmission and in reception of high-frequency currents, modulated at high frequency, the modulation frequency being itself modulated." Most of this patent covers the theme of a double modulation of transmission, and it is really only in a secondary way that he conceives of the use, in reception, of a "continuous local wave tuned in such a way as to produce high-frequency beats" with the wave received by the antenna.

Strangely, Lévy does not give much importance to the value of amplification combined with frequency changing either of the incident wave or of the intermediate oscillation. He declares in effect: "arrangements to amplify the high-frequency currents could be used interposed . . . without changing the spirit of the invention."

He sees as the value of this invention, moreover, just as did Round and Alexanderson, a means of eliminating interference and atmospherics, but prudently declares that "the selection cannot always be absolute," particularly when the interference results from damped waves.

Patent 506,297 is more explicit in that it bears this time upon a system of reception which includes:

- Selector circuits and detectors tuned to the wave to be received;
- A local generator coupled to these circuits and causing with the wave received "ultra-acoustic (above audible frequency) beats";
- Selector circuits and detectors tuned to this ultra-acoustic frequency.

Contrary to his predecessors and to his first patent, Lévy insists in his second patent on the value of amplifying this intermediate frequency. Moreover, his patent is illustrated with a schematic which is precisely the one which was to be adopted later in superheterodynes. On the other hand, as he had done in his first patent, Lévy sees as the principal value of his invention only the question of an "anti-static, high-selectivity reception system." The very great potential sensitivity of his system does not seem to attract his attention.

4. EDWIN H. ARMSTRONG

For the Americans, the inventor of the superheterodyne is E. H. Armstrong (perhaps because of nothing more than the fact that he made up the name). Nevertheless the first patent taken by Armstrong on this subject is a French patent number 501,518 filed on the 30th of December 1918, three months after the second Lévy patent. There never was, as far as we know, any prior American patent. We shall why see later on.

Armstrong claims "a method of reception of high-frequency oscillations of short wave-length and especially waves inferior to 10 meters in length, characterized by the fact that the incoming energy is joined by oscillations produced locally and that the combination is converted . . . to produce oscillations of a third high frequency which are then amplified . . ."

This is definitely the principal of frequency conversion which Armstrong is re-discovering, without bringing in any new element, and even then with the restriction of reserving its use to very short waves.

5. PAUL LAÛT

Although chronologically speaking it was written on February first, 1917, thus before the first patent of Lucien Lévy, we have kept for the end the study of the note that Laüt sent on that date to Colonel Ferrié, since it was never the object of a patent.

The receiver that he describes includes an oscillating circuit coupled to the antenna and connected to a detector tube. In addition, a heterodyne is also coupled to this oscillating circuit. The beat produced is amplified by a resistance-coupled amplifier, then "rectified" again. Laüt explains: "the frequency of the heterodyne is chosen so that the beat frequency has a rather large value (10,000 to 15,000 cycles per second). Thus one can have in the telephones, after rectification by the last tube, a current curve of audio frequency having the same form as the curve of the amplitude of the voltage of the oscillations received."

Finally, he notes two essential points. First: "One can set up in cascade any number of tubes and obtain an amplification as large as desired." Second: "The advantage of this arrangement is the presence of a frequency F (intermediate) independent of that of the oscillations being received; only the tuning of the heterodyne is variable."

Can we attempt a first conclusion?

Meissner certainly invented - with others - the oscillating function of the triode,

reception by heterodyne, and the use of regeneration. It is certain that the seed of the procedure later known as "superheterodyne" is rooted in all of that, but it is not fundamentally claimed nor is it obvious. Round and Alexanderson will go a bit further, since they speak of an inaudible "intermediate" frequency which will then be detected, but their arrangements only aim at eliminating atmospherics.

Laüt is the first to systematize frequency conversion whose advantages he seizes immediately: possibility of an amplification "as large as desired" and simplicity of tuning, but Laüt is only using a resistance amplifier. Lévy picks up these ideas, but meanwhile the tuned amplifier had been invented by the military telegraph [organization] (he had a hand in that, moreover). Lévy understands its value and adopts it, thus giving the superheterodyne its definitive form.

Armstrong finally reinvents the principle of the superheterodyne, chronologically after Laüt and Lévy, and he seems to be only interested in its value as a method of receiving short waves.

Who, then, invented the superheterodyne?

THE PROCESSES

However it may turn out, we shall see that Lévy claimed to be the first and the only inventor of the system of reception by frequency conversion - that his rival Armstrong had besides baptized "superheterodyne," and that out of this arose a sharp conflict between these two engineers on the subject of priority in this invention.

But this discussion remained courteous despite its bitterness. It was not the same as that which opposed, in 1925 and 1926, in the columns of the weekly wireless review *L'Antenne*, Lucien Lévy with the partisans of the "radiomodulateur Ducretet." What was the subject of this dispute? In both cases it was a method of reception by frequency conversion, but obtained by differing means.

1) The process of Lucien Lévy consisted of using two triodes, one functioning as an oscillator and producing a local signal; the other serving as a mixer, the two signals (the incoming and the local) being applied to the grid. The intermediate frequency was picked up in the plate circuit. Lévy expressly claimed that this triode was to be hooked up as a detector (in fact, a grid-leak detector).

2) The method used in the radiomodulateur Ducretet replaced Lévy's two triodes with a *bigrille* (double-grid tube)⁷. The method had been invented by the Englishman Scott-Taggart⁸ but was worked out in France by Jean de la Mare and René Barthélemy. The first grid of the tetrode constituted, with the plate, an oscillating triode which furnished the local signal, while the incoming wave was applied to the second grid. The intermediate frequency was picked up in the plate circuit.

Lévy filed a suit for infringement against the Ducretet company, stating that their double-grid system was not different from his triode method; in both cases, it was necessary that detection take place at the moment of mixing of the signals to cause the appearance of the intermediate frequency, or, in other words, that

7. The double-grid tube was invented and patented by Langmuir in 1913. The French patent bears the number 514,766. It was made and sold in France by "la Radiotechnique." See the article by H. Nozières and P. Giroud [4].

8. English patent 153,681. The Ducretet Company had bought this patent.

the two oscillations that were to beat with one another must act on a non-linear curve. His adversaries maintained that the use of a double-grid tube dispensed with the requirement for detection postulated by Lévy. Lévy retorted that there was indeed detection in the double-grid tube, but through the curve of the plate characteristic⁹.

The author of these lines tried to learn how this judicial contest finally ended. According to Mr. Francois de Gournay, who was at the time the managing director of Ducretet, it would seem that some bargain was struck, but we have not been able to discover its terms.

PART TWO - THE MEN

1. THE ANTENNE AFFAIR

The end of 1925 saw the beginning of a dispute between Lucien Lévy and several wireless personalities, an argument that one might say, at the least, was not infused with the basic courtesies. It was through the intermediary of *L'Antenne*, very much followed by "wireless adepts," that the words that we are about to read were exchanged.

It all began on November 3, 1925 in number 136 by a rather mild article signed "Commander Hourst" who told of having traced out the schematic of a radiomoteur Ducretet with a double-grid tube, and having observed that the incoming wave was applied to the first grid while the local oscillation was obtained by a coupling between the second grid and the plate. The intermediate frequency was picked up in the plate circuit.

Hourst put this diagram next to that of a superheterodyne (Lévy's) which used two different tubes to fill the same roles of local oscillator and detector-mixer, while emphasizing the fact that the double-grid tube system did very well without the first detection claimed essential by Lévy.

Up to that point, there were no surprises. It was only a scientific discussion which normally ought to have remained within the limits of the acceptable. But be patient, things will grow worse.

9. Lévy's idea was correct, but he and his adversaries used, with "detection," an improper term: here it is not really detection which we are talking about, a word which is reserved for the extraction of a signal at audio frequency modulating a radio-frequency signal, but really of a modulation of one signal by another, as happens in a radio-telephone transmitter, where no one would think of talking about detection when speaking of the modulation of the radio frequency by the audio frequency.

The confusion comes from the fact that, whether we are referring to modulation or detection, in both cases the signals in question must act on a non-linear characteristic: in the case of detection, to extract the audio signal from the carrier; in the case of modulation, to obtain the variations of amplitude in the radio-frequency oscillation at the rhythm of the audio signal.

In the case of the frequency changer, it is really a modulation of one signal by another that we have, for the purpose of engendering beats. It is therefore necessary that the characteristics of the tube used as a mixer (triode or double-grid) be nonlinear. If they were linear, the modulation would change the point of instantaneous functioning, without changing at all the amplitude of the signal to be modulated. We have to believe that this idea, which seems so obvious to us now, was not so at the time, judging by the lively discussion that we will quote later on.

On November 25th, 1925, in number 139, Lévy takes fire: he recalls that he had brought suit for infringement against Ducretet, and he adds that, whatever Commander Hourst may think, there is most certainly detection in the double-grid system: had Mr. Hourst never heard of plate detection? Thereupon, Lévy takes a jab at the Ducretet Company, which, he maintains, "never misses an occasion to inspire articles intended to bring confusion to technical questions of the simplest kind, but of a nature to be difficult to work out on a commercial level." This aroused the ire of Commander Hourst, who, in number 140 of the 1st of December 1925, feels that Mr. Lévy has unexpectedly "spit in his face" and that he has "told a lie multiplied by slander."

Getting back to the technical aspects, Hourst declares that the principle of the superheterodyne is found in the Meissner patent "prior by several years to all the Lévy patents," and which moreover are "property of the nation by application of one of the articles of the Treaty of Versailles."

In the same number, François de Gournay, managing director of the Ducretet Company, protests in his turn against the insinuations of Lévy, which he attributes "to the commercial success of the double-grid radiomodulateur sets."

Everything could have ended there but in number 142 of the 16th of December 1925, Lucien Lévy repeats that the radiomodulateur is nothing other than a superheterodyne. Such was not the opinion of Hourst who, in number 144 of the 29th of December, declares war while brandishing the Meissner patent and the work of Round and Alexanderson, all of which, he claimed, was prior to Lévy.

As we shall see, the new year does not see the abatement of passions. A newcomer, René Barthélemy, throws himself into the fray. Barthélemy, who was to become famous a few years later for his work in television, was an engineer for the Compagnie des Compteurs. In number 145 of the 6th of January 1926, he tries to establish a distinction between frequency conversion by addition (the Lévy system) and frequency conversion by modulation (the Ducretet system), the latter, according to him, not involving "anything like detection."

We have seen above that this opinion of Barthélemy's was erroneous. However, he throws a large rock into the pond . . . of Lévy, while avoiding naming him: "in answer to those who ceaselessly call themselves inventors of the method of frequency conversion, I feel it is my duty to shed some light upon the role of an engineer of great merit who is too often forgotten: that of Mr. Laüt. Mr. Laüt was assigned to the Radiotélégraphique Militaire, but was sick and undergoing treatment at Berck in 1916, at which time he proposed, by letters to General Ferrié, the system that Armstrong later named the superheterodyne."

In this number of *L'Antenne*, Lucien Lévy answered the preceding statement of Commander Hourst. Obviously he denies that his patents are plagiarized from Meissner, Round and Alexanderson, and he counter-attacks by opposing to the Ducretet radiomodulateur a circuit of a frequency changer using a double-grid tube that, seven months before the filing of the Ducretet patent, was apparently described by two engineers of "La Radiotechnique," H. Nozières and P. Giroud, during a meeting of the Society of the Friends of Wireless.

Lévy felt that under these conditions, speaking of the radiomodulateur Ducretet as involving new theory . . . could only provoke a most justified outburst

of laughter among the readers of *L'Antenne*¹⁰.

In the following number (146 of the 12th of January 1926), Commander Hourst states that as far as he is concerned, the discussion is over. Exit Commander Hourst, not without having shot one last barb at Lévy: "... it appeared during the discussion that the function of the superheterodyne was fathered by many men and we find each day a new name to add to the list (see the article of Mr. Barthélemy in the last number of *L'Antenne* on the subject of Mr. Laüt)."

In number 147 of the 19th of January 1926, Lévy takes the challenge. After having given Mr. Barthélemy a lesson in applied mathematics and having accused him of "superficiality," he challenges him to prove his assertions on the hypothetical priority of Mr. Laüt.

Barthélemy replies in number 148 of the 26th of January, that he is ready to remit to the magazine the "Laüt document." Then he launches himself (in Nos. 149 and 150) into a rather murky discussion which tries to prove to Lévy that the latter has misinterpreted his mathematical reasoning on the frequency changer.

In the same number 150, Lévy replies to some earlier technical notes of Barthélemy, stating that the latter has not understood any of his (Lévy's) ideas.

Sixty years later, what comes out of this rather trifling discussion for an impartial reader is that we are dealing with two engineers of equal merit who are fundamentally of the same opinion, but who, cordially detesting each other, wish to show:

- That the other is grossly mistaken on the theoretical level, which brings into question his professional qualities;
- That, moreover, he is mistaken (or pretends to be mistaken) on the ideas of his adversary, which brings into question his good faith.

Major wars have been declared for far less. Nevertheless, in number 151 of the 13th of February 1926, Lévy starts in again. He publishes a diagram which appeared in June 1924 in the English magazine *Experimental Wireless* under the signature of A. L. Williams and entitled "Superheterodyne Receiver with Six Tubes with a Four-Electrode Tube." It is obviously the same principle as the radiomodulateur Ducretet, which allows Lévy to state that "the radiomodulateur is not new. It is not due to Mr. de la Mare or the Ducretet Company. It was described in detail in a publication seven months before the filing of the Ducretet patent and designated by its author as the superheterodyne."

Thereupon, six weeks pass and one might believe that the affair had finally been laid to rest, but on the 28th of March 1926 (number 157) it bounces back even harder. This time, it's the editor of *L'Antenne*, Henri Etienne, who adds to the fire by publishing the notes sent on the first of February 1917 by Laüt to Ferrié, notes of which we have already spoken in the beginning of this study, in the paragraph consecrated to patents and priorities. These notes, according to Etienne, were sent by former colleagues of Laüt at the Eiffel Tower station.

10. Lévy wanted to be right at all costs. If he had read more carefully the communication of H. Nozières and P. Giroud which we have already mentioned [4], he would have realized that these authors were not at all presenting a frequency conversion circuit, but simply a regenerative detector which had the advantage, thanks to the use of a double-grid tube, of working with a very low plate voltage.

Etienne doesn't mince words: "The notes mentioned contain the essential part of the later claims of Mr. Lévy, then assigned to the Eiffel Tower station . . . he (Laüt) indicated to the researchers a circuit allowing through double rectification preceded and followed by amplification the reception of short waves, and did it long before Armstrong and Lévy."

Lucien Lévy answers in the following number (159 of the fourth of April 1926), and one has to believe that he is somewhat embarrassed, judging by the specious arguments that he presents. He contests that Laüt was indeed an engineer at the Eiffel Tower on the date of his note, February 1st, 1917, indicating in fact that "he had been sick for months out in the provinces and sent by letter the result of his meditations to General Ferrié and to Commander Brénôt, who is now chief executive of the Société Française Radioélectrique." He states that it was in August 1917 that he (Lévy) was given the authorization to take out certain patents for "certain inventions that I had conceived a long time before." Captain Brénôt had, he said, forwarded his application with a most positive recommendation and had added: "The work in question is absolutely personal. No attempt has been made at verification on the practical level, but the arrangements suggested are

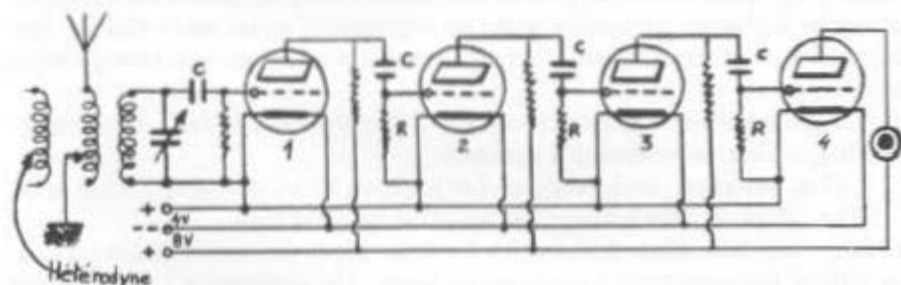


Figure 3. The diagram accompanying Laüt's note of February 1, 1917 (after *L'Antenne*, no. 157).

very interesting and may have some very important applications. It would be well to test them out as soon as possible."

And Lévy comments: "It certainly seems to me that at this moment, the 'remarkable' little note of Mr. Laüt had not attracted a great deal of attention." He adds that in October 1918, he had published on his own system of reception a note to which Ferrié had added an annotation: "The studies of the process which have just been briefly described have been in progress for about two years. October 15th, 1918," signed "Ferrié."

Technically speaking, Lévy criticizes the diagram accompanying Laüt's note, in particular the fact that he uses a resistance amplifier, which causes the loss of all the advantages of selectivity of the superheterodyne. Lévy ends by stating that "The little note of Mr. Laüt brings no moral proof at all of the priority of his invention" and he further states that "wireless in France has still a great deal to do to escape from the tyrannical imperialism which, in the wings, prepares its somber machinations [sic]."

The argument will continue for several weeks more. In the following numbers of *L'Antenne*, articles on the subject will be entitled: "on Mr. Lévy and secon-

darily on frequency conversion." Etienne gives us to understand that, more than the technical aspects of the problem, it is the personal behavior of Mr. Lévy which is in question.

In number 159 of the 11th of April 1926, Etienne launches a direct attack on Lévy and mounts an energetic defense of Mr. Laüt, and that, it must be said, in a rather hypocritical way: Etienne pretends to believe that Lévy had criticized Laüt for having been sick at the time of his famous notes. He deems "scandalous and unworthy" the manner in which Lévy spoke of the "meditations" of Laüt, whereas it was really a "work completed for its own reward." "That alone, Mr. Lévy, just these words, this little sentence lowers you in my esteem" and further on: "Have I ever reproached you for lining your pockets?"

The tone is obvious. And to top it all off, Etienne will use the same occasion to attack someone who seems to surpass even Lévy in his execration: "Don't imitate Marius Latour¹¹, believe me, you would cover yourself with ridicule. That seat is taken."

It is now that Mr. Brénat puts in his oar, in number 160 of the 18th of April 1926. Let us recall that Brénat was director of the military station of the Eiffel Tower during the war, with the rank of Commander. Then he became director of the Société Française Radioélectrique, with the title of colonel in the reserves. Brénat steers quite clear of having wished, by annotating Lévy's patent, to grant any sort of priority to the invention outlined in the application (the superheterodyne). He willingly recognizes that the note of Laüt, distributed among the technicians at the Eiffel Tower seven months before the patent application in question, did not hold his attention for, as he says, if this idea of Laüt's became later on important, it really had no application to military telegraphy. As for the work of Lévy, it was presented as aiming for the elimination of static, and it was from this point of view that Brénat had appraised it.

We will have to shorten this discussion, for the distinction is becoming mired in personal considerations between Lévy and Etienne, which have little to do with frequency conversion. Let's end up with number 162 of the second of May, 1926. Now Marius Latour, very upset with certain ironic remarks of Lévy, enumerates the inventions he claims to have fathered: the use of a common battery for all the tubes in an audio amplifier; the method of calculating the gain of such an amplifier; use of iron cores in radio-frequency transformers; combination in a single apparatus of radio-frequency and audio-frequency amplification. All these arrangements are used in Mr. Lévy's radios.

In his turn, Emile Girardeau, director of the SFR, takes Latour's defense, asking Lévy why he doesn't file suit? Lévy replies that shortly after the note of Laüt of the first of February 1917, the SFR tried to patent - following the idea of Laüt - "the use of a heterodyne for reception in wireless telephony." He deduces

11. Marius Latour, an engineer at the Société Française Radioélectrique, was an eminent technician but also an excellent businessman. Besides his own patents, he had bought several others of which the creators had not seen the interest, and had been able to exploit them very well. It was this way that he bought from Peri and Biguet their patent on the TM tube (with cylindrical and coaxial elements) and that he received fees from all the French manufacturers of vacuum tubes for 15 years. This *savoir-faire* wasn't to the taste of everyone.

from this that the SFR's whole interest here is to deny to him, Lévy, the invention of the superheterodyne because of "the money which the commercial exploitation of the superheterodyne would gain for the SFR."

The last line of this long and involved discussion (which lasted no less than six months), will be written in number 165 of the 23rd of May, 1926 in three open letters addressed respectively to the magazine by Latour, Girardeau and Barthélemy, in which each of these will rehash without adding anything new, the arguments already brought to bear against Lévy.

2. LÉVY AGAINST ARMSTRONG

It remains for us to discuss the dispute which arose about the same time (1926) between Lucien Lévy and Edwin H. Armstrong. The latter was then a professor at Columbia University in New York, but at the time he had developed his superheterodyne in 1917-1918, he was a major in the U. S. Signal Corps and was in Paris where the Americans had set up a laboratory which was working in liaison with the Military Telegraphy organization.

In his 1926 book The Superheterodyne and Superregeneration, P. Hémard-

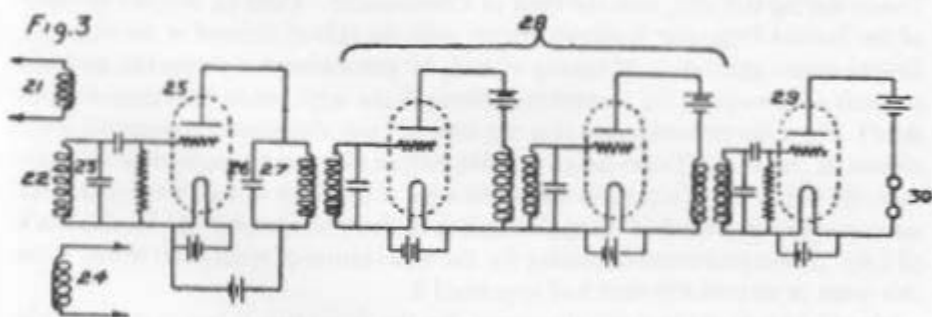


Figure 4. One of the circuits in Armstrong's French patent No. 501,511 of December 30, 1918.

inquer published the respective arguments of Armstrong and Lévy. The text of Armstrong was reproduced from an article which had appeared two years earlier in the *Proceedings of the IRE* [7].

As strange as it may seem, Armstrong announces in the opening lines of his paper that "the invention of the superheterodyne goes back to the early months of 1918." On the other hand, he seems entirely unaware of the work of Lévy: his name is never mentioned. However, it is difficult to believe that by 1926 Armstrong would be unaware that the first patent by Lévy dated from August 1917.

Armstrong details the following points. When, in 1917, the Americans entered the war, the American Expeditionary Force had to resolve the problem of receiving damped waves of frequencies going from 500,000 to 3,000,000 cycles¹², with the fewest possible tuning adjustments. Now, the tubes made in the United States at this time had internal capacities that were much too high for this work.

12. It could in no way be a question of receivers to be used by troops in the field, since continental and transcontinental communications were, as everyone knows, carried on in continuous waves of kilometric length.

At the beginning, the problem was solved "thanks to the good will and energy of General Ferrié and his staff"; but it could only have been a temporary solution, and it is then that Armstrong thought of changing the frequency through a heterodyne and of amplifying this intermediate frequency. There follow long considerations by Armstrong on the advantages of the superheterodyne and its commercial success, which we will not dwell upon.

Before moving to Lévy's response, we will cite the work of Armstrong's biographer L. Lessing [6] on the work of Armstrong. During an air-raid alert in the beginning of 1918, Armstrong, who was watching the raid from one of the bridges over the Seine and who noticed the poor efficacy of the anti-aircraft fire, wondered if it would not be possible to detect airplanes by receiving the weak electric oscillations emitted by their ignition systems. He estimated the frequency involved at "more than 10,000,000 cycles" but there existed at the time no receiver sensitive enough for such frequencies. It was then that he is supposed to have had the idea of frequency conversion. He filed a French patent on the 30th of December 1918, then an American patent on the 8th of February 1919, which was granted on the 8th of June 1920 under the number 1,342,885.

Lessing goes on to say that Armstrong received in February 1919 the ribbon of Knight of the Legion of Honor. Further on, he mentions the differences which arose between Armstrong and several inventors who allegedly had imagined systems "vaguely" recalling that of Armstrong. "The most surprising of these claims" was that of Lieutenant Lucien Lévy whom Armstrong had met a few times in France and whose system was virtually unusable, and which brought no solution to the problem of detecting weak enemy signals. Besides, Lessing adds, one cannot judge the claims of Lévy concerning the superheterodyne because of the fact that the French government recompensed Armstrong for his invention but never accepted the claims of Lévy¹³.

There is how history is written.

Let us get back to the book by Hémadinquer (2) written in 1926. Lévy answers the note by Armstrong. But the more Armstrong evinces calm detachment, the more Lévy is aggressive, as if on the defensive . . . from attacks that he has not yet had to face. Right out of the gate, Lévy speaks of the series of inexactitudes that Armstrong's exposé is supposed to include. He says that he first got the idea of frequency conversion in June, 1916; that in 1917, Armstrong was chief of the American research service in France, and that it was for this reason that as soon as Lévy took out his patent of the first of August 1917, he must have learned of it, the French services having communicated it to the various allied services.

"We should not, therefore, be surprised," concludes Lévy, "at the coincidence between the ideas of Armstrong and those of Lévy." This is a rather pointed re-

13. Armstrong had a tragic end. The three inventions of which he was the most proud - the oscillating function of the triode, the superheterodyne, and frequency modulation - were bitterly contested: the oscillating triode by de Forest and a few others; the superheterodyne by Lévy and others; FM by RCA. He lost most of his trials (he never had one with Lévy). Beaten by these failures, ruined financially, and in spite of the backing of all the great American scientific associations who had given him prizes and medals, he could not overcome his disappointments and took his own life in 1954.

mark by Lévy when one considers that it is likely that he himself was inspired by a certain note written by Laüt . . .

3. AN AUTHORIZED OPINION

We shall end this study by citing the opinion of the eminent German physicist Walter Schottky, to whom the early days of radioelectricity owe many discoveries. In October 1924, Schottky published in the *Proceedings of the IRE* [5] a short study of the origin of the superheterodyne. He states that in the end of 1917, the Siemens laboratory that he was in charge of had, like Lévy, worked on the problem of frequency conversion. Voluntarily or not, Schottky leaves room for doubt on the manner in which he had become aware of the value of the system. Is it by chance that he had undertaken this study three months after the application for the first patent was filed by Lévy (that of the 4th of August 1917)? Or did he already have knowledge of it (but how, since it was wartime?) and had he recognized its value? Both interpretations are possible. In any case, to add to the confusion, Schottky declares that Lévy had apparently already patented his invention in England on the 8th of April 1917 under the number 143,583. This point would be worth verifying.

Schottky sweeps away in this area the priorities of his compatriot Meissner and those of the Englishman Round. He keeps only the merits of the Lévy patent and the Armstrong patent. Of the first, he says that it is of "fundamental importance," and that we must consider Lévy as the true creator (originator) of the superheterodyne system. On the other hand, he accords to Lévy only the merit for the idea (the word), to give to Armstrong the advantage in the area of practical realizations (the deed).

On the date when Schottky wrote *that*, it was still true, since it wasn't until the same year 1924 that Lévy brought out his first commercial superheterodynes, while Armstrong [via RCA] had already done so in the U. S. two years before. But it is only right to recall that the superheterodynes made by the firm of Lévy (Radio L. L.) will know for fifteen years a well-earned commercial success, without mentioning the fact that frequency conversion will later on become virtually unique as the procedure for receiving Hertzian waves.

CONCLUSIONS

One can, without risking much error, thus describe the paths of thought which led to reception by frequency conversion.

At the beginning of the First World War, the idea of causing the incident wave to beat with a local oscillator and to amplify the resulting inaudible frequency was "in the air." However, Round and Alexanderson saw only the (debatable) value of reducing static. It was in fact Laüt - who had probably never seen their work - who thought of making frequency conversion a systematic method of reception of great sensitivity, and brought out a few of the most substantial advantages.

But Laüt was not able - and for good reason - to carry to the end by trying out his proposal. That Lévy knew of the note from Laüt is highly likely, and he would have brought honor to himself by recognizing it. At least he had the merit to use a tuned amplifier after his frequency changer and especially to develop

theoretically and experimentally this method, to the point of rendering it convenient to use and commercially viable.

As for Armstrong, in any case his patent is posterior to that of Lévy. We must give him credit for having carried out in the U. S. a work of development and of perfection similar to that of Lévy in France.

It is the same with the invention of the superheterodyne as with that, more general, of the invention of wireless telegraphy: if it is difficult to designate a unique inventor, one can at least say that there was a "main person responsible," the one who took on the unpleasant work of making an idea go from the almost fairy-land domain of theory to the much more down-to-earth domain of practise and realization. These "main persons responsible," we believe, are Marconi for wireless, and Lévy and Armstrong for the superheterodyne.

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