Radio Waves – Part IV: On the false Electric Waves of delusional Heinrich Hertz

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Abstract

After writing a paper critical to Maxwell's electromagnetic theory, it is only natural to continue with an article critical to Hertz's work. This is because every physics textbook today claims that Hertz demonstrated experimentally Maxwell's theory. This claim amounts to saying that Hertz demonstrated experimentally that radio waves and light are electromagnetic, i.e. that they are made up of entangled electric and magnetic fields that oscillate and induce one another. In this work it will be shown that Hertz's claim of having verified experimentally Maxwell's theory is an exaggeration simply not true. Although Hertz did confirm the *existence of a certain wave propagating in air*, it cannot be said that his verification that the waves were composed of *magnetic and electric oscillations* is correct. And Hertz explicitly stated that he did not offer a direct verification that light itself is electromagnetic. I only wish more physicists read Hertz's works before believing Maxwell's theory or that it has been confirmed experimentally through Hertz's works or otherwise.

Introduction

The present article is addressed to the same group of people interested in developing a mechanical theory based on the liquid aether that explains interactions at-a-distance, be they magnetic, electric, gravitational, light, or radio waves. The work sticks to the same guidelines as the other articles of the series on Radio Waves aiming to show that *it makes sense* to look for such a mechanical theory of the aether, and to provide *motivations* for pursuing this path of research by showing the flaws in the present aetherless theories and dogmas on light, magnetism, electricity and gravitation.

Specifically, the present article addresses the electromagnetic dogma according to which the electromagnetic nature of light and radio waves was proved correct by the experimental work of Heinrich Hertz; it argues that a careful look into Hertz's works reveals that this is an exaggeration and that Hertz was delusional and strongly influenced by Maxwell's theory both during his experiments and when drawing conclusions from his experiments.

Hertz's researches were directed towards showing whether the electric force acts at-a-distance or it takes time to reach from one point in space to another. During these researches Hertz produced, together with electrical discharges (electric sparks in air), waves that propagated away from his apparatus and which he believed were oscillating electric fields detached from his apparatus. It is hard to see what experimental evidence led Hertz to believe that electric fields detached from his apparatus and existed as waves of electric fields in space. It is also difficult to agree with Hertz that the existence of the waves is enough to proclaim Maxwell's theory correct, since describing the waves as mechanical waves in the liquid aether would have been equally acceptable. Hertz ended up using Maxwell's theory to delusion himself into believing that the waves he was producing and detecting were not simple waves in the aether but complicated oscillating electric and magnetic fields mutually inducing one another (a.k.a. "electromagnetic waves").

If one were to redo Hertz's experiments without having any knowledge about Maxwell's theory, he would simply conclude that the electrical discharges in Hertz's apparatus produce in fact powerful *electric currents* in his apparatus that propagate further through the aether as longitudinal waves of compression (aether wakes) and that the detection of the waves so produced occurs through Faraday's celebrated effect of electromagnetic induction. So Hertz's experiments can be used equally well to show that the waves propagating through space are not electromagnetic but waves in the aether propagating at the speed of light. If we were to describe these waves in terms of what is known to today's physics, the closest to the truth would be to call them just magnetic waves; this is because the velocity $\mathbf{v}$ of the moving aether is the same physical quantity as what in theory has been called (without any insight into its real physical significance) magnetic potential $\mathbf{A}$: since $\mathbf{B} = \nabla \times \mathbf{A}$, it follows that what we call magnetic field $\mathbf{B}$ is related to the velocity $\mathbf{v}$ of the flowing aether as

$$\mathbf{B} = \nabla \times \mathbf{v} .$$

Background

In an article[1] published a few years ago I mentioned in passing that Hertz's researches on radio waves do not necessarily prove that Maxwell's electromagnetic theory is true, that radio waves (and light) propagating through space are not necessarily electromagnetic in nature, i.e. they are not a system of entangled and mutually inducing electric and magnetic waves.

In another more recent work[2] I have showed that Maxwell's theory itself is not flawless, being based on an *unfounded assumption* and on a *faulty method of theoretical investigation* with the consequence that the whole picture of light (and radio waves) as a system of oscillating electric and magnetic fields inducing one another is objectionable to say the least.

In this article I return to Hertz's work and argue that Hertz has not verified experimentally the *entire* electromagnetic theory proposed by Maxwell. It can be seen from Hertz's works that he confined himself to confirming the existence of waves propagated in air with a finite velocity [3] (without even actually measuring the speed of the waves):

> I felt that the third hypothesis contained the gist and special significance of Faraday's, and therefore of Maxwell's, view, and that it would thus be a more worthy goal for me to aim at. I saw no way of testing separately the first and the second hypotheses for air;[1] but both hypotheses would be proved simultaneously if one could succeed in demonstrating in air a finite rate of propagation and waves.

I will also argue that the theoretical claim that radio waves are composed of oscillating electric and magnetic fields *has never been verified by experiment* – the truth is that Hertz interpreted his experiments to agree with Maxwell's theory.

In Maxwell's theory the finite velocity of propagation is calculated with the formula

$$c = \frac{1}{\sqrt{\varepsilon_0 \cdot \mu_0}} .$$

To set the record straight, this formula was not discovered by Maxwell, but it was known to him in the form of the ratio of the electromagnetic unit (emu) to the electrostatic unit of electricity (esu), having been discovered during the intense researches related to the establishment of electrostatic and electromagnetic units initiated by Carl Friedrich Gauss, Wilhelm Weber and Rudolf Kohlrauch[4]. The latter two were the first [5] to find in 1856 that the ratio (emu) to (esu) has a value close to the

1. Ionel DINU, *On an Experimentum Crucis for Optics*, General Science Journal (http://gsjournal.net/Science-Journals/Research%20Papers-Mechanics%20/%20Electrodynamics/Download/2371), November 18, 2010, (p.4).
2. Ionel DINU, *Trouble with Maxwell's Electromagnetic Theory: Can Fields Induce Other Fields in Vacuum?*, General Science Journal (http://gsjournal.net/Science-Journals/Research%20Papers-Mechanics%20/%20Electrodynamics/Download/4219), February 19, 2013.
3. Heinrich HERTZ, *Electric Waves*, Dover Publications Inc., New York 1962 (MacMillan, 1893), p.7.
4. William HALLOCK, Herbert T. WADE, *Outlines of the Evolution of Weights and Measures and the Metric System*, The MacMillan Company, London 1906, p.200.
5. E. MASCART, J. JOUBERT, *A Treatise on Electricity and Magnetism*, Volume II – Methods of Measurement and Applications, Thos. De la Rue and Co., 1888, p.553-567. (see Appendix A)

speed of light. Seeing this coincidence and without even understanding its significance (this fact is still true today), Maxwell started to mix things up and speculate [6] in 1865 that light itself might be "electromagnetic"; a safer approach would have been to just say that, since magnetic and electric interactions act across vacuum (i.e. through the aether), it is only natural that their speed be equal to that of light, although the particular type of motion which they impart to the aether might be different from each other and from that of light. It should be mentioned that it was much later (in 1868) that Maxwell verified the ratio (emu)/(esu) by an experiment of his own [7,8], as if trying to grasp the physical significance of this famous ratio.

To put things into perspective, it must be said that both Maxwell and Hertz belonged to the nineteenth-century school of *plenum*, i.e. they both believed in the existence, and tried to find a theory, of the *aether*. They both failed in finding such a theory, and the question I have always asked myself is: why have they failed?
Another case in point is that of William Thomson, Lord Kelvin, who wrote the preface to the English edition of Hertz's *Electric Waves*, and tried all his career to find an acceptable theory of the aether. William Thomson famously declared himself a *failure* at his Jubilee speech in 1896. Look at this memorable excerpt [9]:

> One word characterises the most strenuous of the efforts for the advancement of science that I have made perseveringly during fifty-five years; that word is FAILURE. I know no more of electric and magnetic force, or of the relation between ether, electricity, and ponderable matter, or of chemical affinity, than I knew and tried to teach to my students of natural philosophy fifty years ago in my first session as Professor.

The cause for which all of these otherwise famous scientists of the past failed is that they were unsuspecting victims of another dogma of their day, one the most vicious and damaging dogma that has been perpetuated in the whole history of science, and still existing in today's physics: that light is a *transverse* wave. Since only solid substances can sustain transverse waves, the dogma was in obvious contradiction with the fact that solid matter could move through the aether freely, without any resistance.

The dogma of light as a transverse wave was not proper a dogma when Thomas Young first proposed[10] it reluctantly in 1817 – it became a dogma in time, due to the fact that nobody could find an explanation of the phenomena of double refraction and of polarization of light within the framework of the *longitudinal* wave theory of light; Young's hypothesis was accepted quickly as it saved the wave theory of light itself

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6. James Clerk MAXWELL, *A Dynamical Theory of the Electromagnetic Field*, Philosophical Transactions, 1865, p.459-512 (p.465, 499).
7. E. MASCART, J. JOUBERT, *A Treatise on Electricity and Magnetism*, Volume II – Methods of Measurement and Applications, Thos. De la Rue and Co., 1888, p.560.
8. James Clerk MAXWELL, *On a Method of making a Direct Comparison of Electrostatic with Electromagnetic Force; with a Note on the Electromagnetic Theory of Light*, Philosophical Transactions, 1868, p.643-657.
9. Silvanus P. THOMPSON, *The Life of William Thomson Baron Kelvin of Largs*, Vol. II, MacMillan and Co., Ltd., London 1910, p.984.
10. George PEACOCK, *Miscellaneous Works of the Late Thomas Young: Letter from Dr. Young to M. Arago, 12th January 1817* (John Murray, London, 1855), p.383. (see Appendix B)

from the competitor it had at that time – the corpuscular theory. This opportunistic and desperate attitude of accepting such a bad idea as that of light as a *transverse* wave in the aether cost physics the eventual abandonment of the aether itself.
Reverting this and directing physics research towards finding that wave theory of light that considers light both a *longitudinal* wave and capable of the effects observed in *double refraction* (polarization) should be a **top priority of science now**.
A possible path of investigation, which I have called "wavefront dynamic fragmentation theory of polarization", was suggested in another paper[11]. It should also be noted that the term "polarization" itself is a misnomer because a light wave does not have any "poles" nor any other "polar" quality - this term has been handed down to the present science from the time when the corpuscular theory of light supported by Newton wrought havoc in physical optics to the detriment of the wave theory.

Maxwell and Hertz themselves, being overwhelmed by the contradictions apparent in an aether that should sustain transverse waves -which only a solid substance can do-, but would allow objects to move through it freely -which only a fluid substance can do-, found refuge in "electric and magnetic fields" existing in the aether. Hertz's honest acknowledgement of this policy of hiding behind "fields" is remarkable.
Look below at an excerpt to see the problem described by Hertz in his own words [12]:

> What, then, is light? Since the time of Young and Fresnel we know that it is a wave-motion. We know the velocity of the waves, we know their length, we know that they are transversal waves; in short, we know completely the geometrical relations of the motion. To the physicist it is inconceivable that this view should be refuted; we can no longer entertain any doubt about the matter. It is morally certain that the wave theory of light is true, and the conclusions that necessarily follow from it are equally certain. It is therefore certain that all space known to us is not empty, but is filled with a substance, the ether, which can be thrown into vibration. But whereas our knowledge of the geometrical relations of the processes in this substance is clear and definite, our conceptions of the physical nature of these processes is vague, and the assumptions made as to the properties of the substance itself are not altogether consistent. At first, following the analogy of sound, waves of light were freely regarded as elastic waves, and treated as such. But elastic waves in fluids are only known in the form of longitudinal waves. Transversal elastic waves in fluids are unknown. They are not even possible; they contradict the nature of the fluid state. Hence men were forced to assert that the ether which fills space behaves like a solid body. But when they considered and tried to explain

11. Ionel DINU, *A New Theory of Polarization of Light*, General Science Journal (http://gsjournal.net/Science-Journals/Communications-Mechanics%20/%20Electrodynamics/Download/4207), June 20, 2012.
12. Heinrich HERTZ, *Miscellaneous Papers*, Mac Millan and Co. Ltd., New York 1896, p.314-315.

the unhindered course of the stars in the heavens, they found themselves forced to admit that the ether behaves like a perfect fluid. These two statements together land us in a painful and unintelligible contradiction, which disfigures the otherwise beautiful development of optics. Instead of trying to conceal this defect let us turn to electricity; in investigating it we may perhaps make some progress towards removing the difficulty.

Observe Hertz's candid acceptance of the hypothesis that light is a transverse wave! How beneficial it would have been for science if Hertz (or Maxwell) chose to remove the "unintelligible contradiction" by challenging the dogma that light is a transverse wave and trying -or, at least, attempting- to find an explanation of the polarization effects compatible with the theory of light as a longitudinal wave of compression in the aether!

Instead, Hertz and Maxwell chose the more comfortable path of employing "electric and magnetic fields" in the aether, which allowed them the freedom to not be very specific as to the nature of these two fields and to say whatever suited them, even that the two fields -forming a so-called "electromagnetic field"- can be transverse to the direction of the wave. This is how we have inherited a lack of clarity, and today we are left without any explanation, of what the electric ε_0 and magnetic μ_0 in the formula $c = \frac{1}{\sqrt{\varepsilon_0 \cdot \mu_0}}$ really mean as properties of the aether or of vacuum; nor do physicists today have any interest to pursue this field of research, being content with the non-mechanical picture of light and radio waves as "electromagnetic fields" flying in vacuum.

In relation to this, I cannot abstain from mentioning here a favorite quote from Poincare's *Electricite et Optique, La Lumiere et les Theories Electrodynamiques* [13], just to show how narrow-minded and prejudiced physical researchers the "electromagnetic theory" taught in schools has produced, compared to the researchers in the past who did not lose sight of the mechanical theory of light even when Maxwell's theory began to receive some acceptance:

Maxwell ne donne pas une explication mécanique de l'électricité et du magnétisme; il se borne à démontrer que cette explication est possible.

Roughly translated, the above means:
"Maxwell has not given a mechanical explanation of electricity and magnetism; he only showed that such an explanation is possible".

It may be interesting to close this part of the article with an unbelievable note about Hertz, namely his acknowledgement of the fact he did not completely understand

13. H. POINCARE, *Electricite et Optique, la Lumiere et les Theories Electrodynamiques*, Gauthier-Villars, Paris 1901, p.IV.

Maxwell's theoretical calculations and assumptions. Hertz considered only Maxwell's conclusions -the so-called "Maxwell's equations"-, which meant only that the waves are electromagnetic, and that they propagate through space at the speed of light.
The real reason that made Hertz agree with Maxwell's theory was that, among other theories existing at the time, Maxwell's was the only one which predicted the existence of waves that propagated at the speed of light.
Below are some excerpts in which Hertz makes such kind of statements [14] (others similar to these can be found elsewhere in his works):

> **And now, to be more precise, what is it that we call the Faraday-Maxwell theory? Maxwell has left us as the result of his mature thought a great treatise on Electricity and Magnetism; it might therefore be said that Maxwell's theory is the one which is propounded in that work. But such an answer will scarcely be regarded as satisfactory by all scientific men who have considered the question closely. Many a man has thrown himself with zeal into the study of Maxwell's work, and, even when he has not stumbled upon unwonted mathematical difficulties, has nevertheless been compelled to abandon the hope of forming for himself an altogether consistent conception of Maxwell's ideas. I have fared no better myself. Notwithstanding the greatest admiration for Maxwell's mathematical conceptions, I have not always felt quite certain of having grasped the physical significance of his statements. Hence it was not possible for me to be guided in my experiments directly by Maxwell's book.**

[…]

> **I therefore endeavoured to form for myself in a consistent manner the necessary physical conceptions, starting from Maxwell's equations, but otherwise simplifying Maxwell's theory as far as possible by eliminating or simply leaving out of consideration those portions which could be dispensed with, inasmuch as they could not affect any possible phenomena.**

[…]

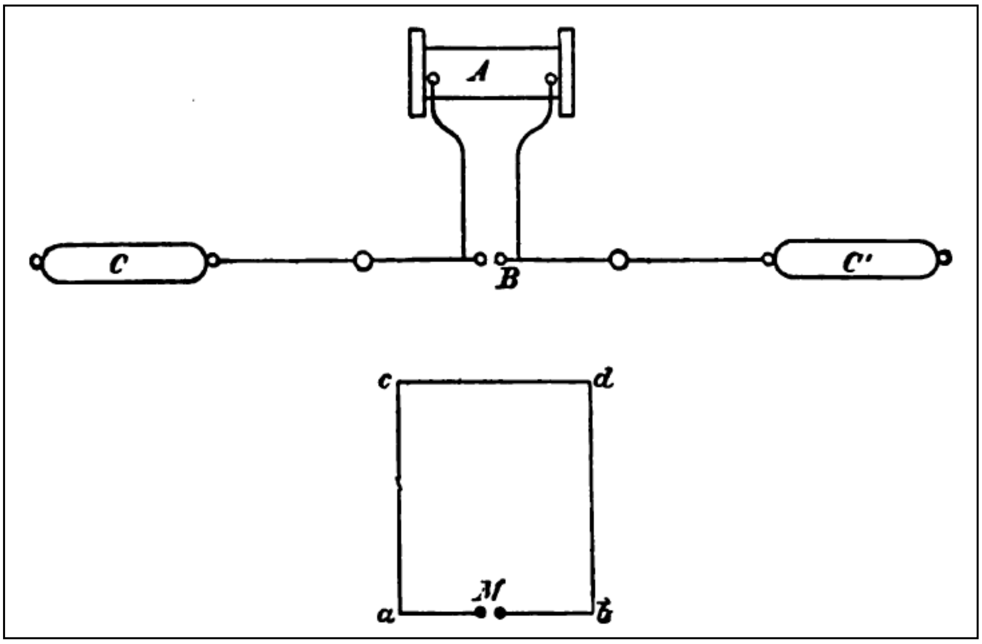
> **To the question, "What is Maxwell's theory?" I know of no shorter or more definite answer than the following: — Maxwell's theory is Maxwell's system of equations.**

14. Heinrich HERTZ, *Electric Waves*, Dover Publications Inc., New York 1962 (MacMillan, 1893), p.20-21.

Main

In order to prove that Hertz was heavily influenced by Maxwell's theory, ending up by interpreting his experiments to agree with it, I will show that Herz had in fact a few personal correct opinions about what was going on in his experiments, but that he later deluded himself into believing what Maxwell's theory propounded.

A critically important thing to notice is that Hertz, in his *very first* experiment whereby he succeeded to produce and detect radio waves, admitted that the reception of the waves at great distances from his apparatus took place through the *effect of electromagnetic induction*. The apparatus he constructed for this experiment in 1887 (shown below [15]) was a modification of earlier apparatuses of his which he designed for other experiments, and it would become the very first radio transmitter in the history of mankind: the spark transmitter that Marconi would use later to accomplish the first transatlantic communication[16] in December 1901. Before that, Hertz would use it in his researches on the reflection and refraction of radio waves:



The radio waves were produced by the circuit ABCC' through *spark discharges* at the gap B and were detected with the rectangular wire Mabcd. Hertz's acknowledgement that the *reception of radio waves takes place through the effect of electromagnetic induction* is extremely important because in electromagnetic induction an electric current is induced in a circuit through a time-varying magnetic field.

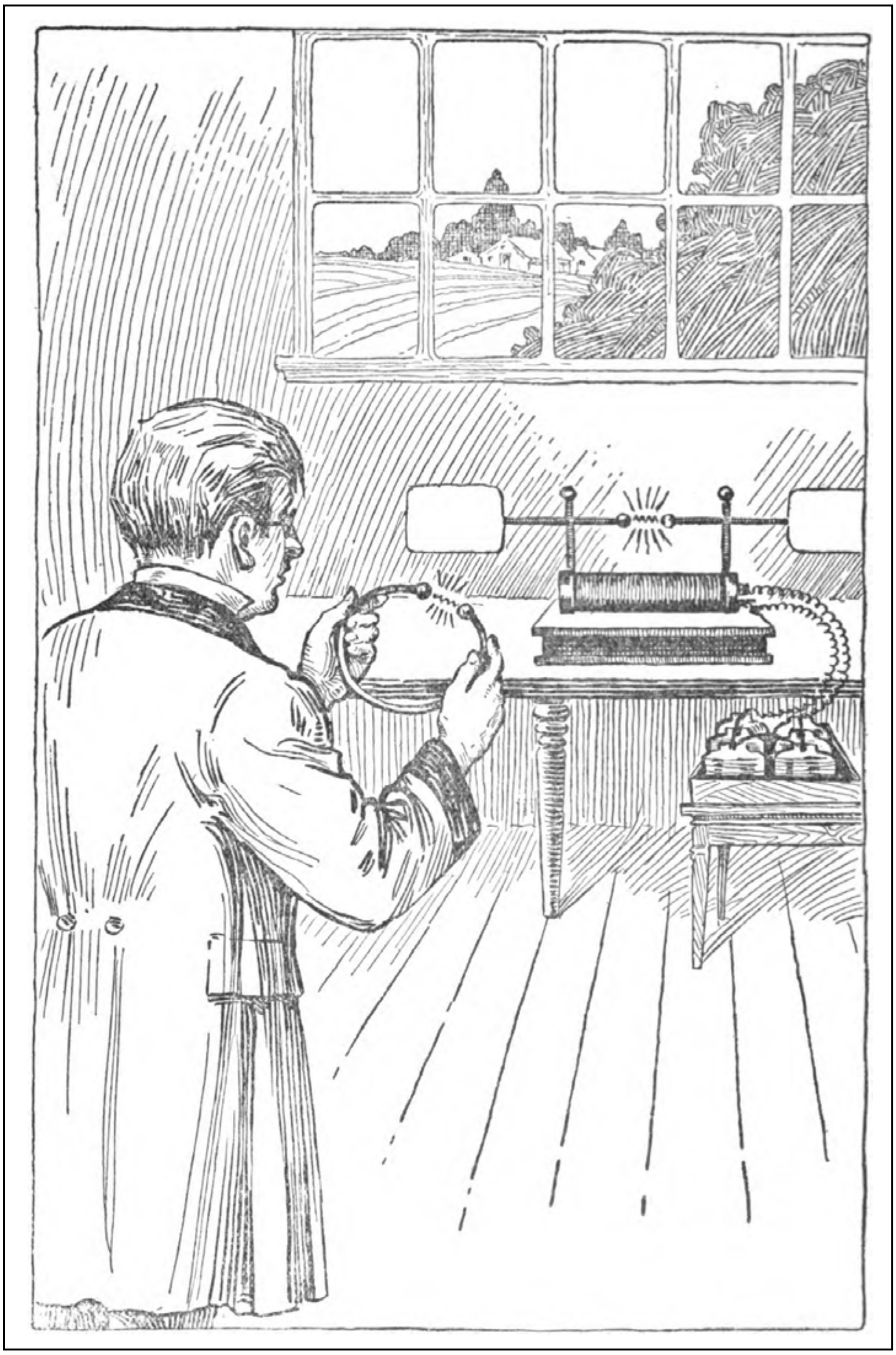
So if it is admitted that radio waves are detected in the rectangular wire Mabcd through electromagnetic induction, is not this sufficient for admitting that radio waves are just magnetic oscillations and waves? Is it allowed to introduce supplementary assumptions and suppose that radio waves also have an accompanying electric field?

We will see later that Hertz has never truly detected the electric field that supposedly exist in a radio wave, but suited the conclusions of his experiments to match Maxwell's theory that the waves are "electromagnetic" (i.e. composed of

15. Heinrich HERTZ, *Electric Waves*, Dover Publications Inc., New York 1962 (MacMillan, 1893), p.40.
16. Orrin E. DUNLAP, *Marconi, the Man and his Wireless*, MacMillan Company, New York 1937, p.96-97.

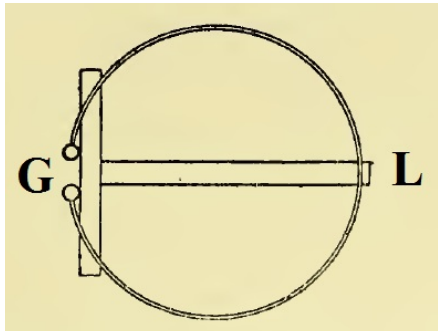
electric and magnetic oscillating fields); so it is fair to say that we believe today in the electromagnetic theory of radio waves not because Hertz's experiments offered any proof of that but because of Hertz's blind belief in Maxwell's theory.

For the sake of having a better idea of what Hertz did in his experiments, I have added below a very illustrative rendering of his experimental set up [17] (Hertz eventually adopted a circular wire as a detector for his radio waves instead of the rectangular wire Mabcd used initially):



17. Raymond Francis Yates and Louis Gerard Pacent, *The Complete Radio Book*, The Century Co., New York 1922, p.32.

Observe Hertz's method of detecting his radio waves: by looking at the sparks being produced in the small space (gap) between the knobs of his circular wire; and because the sparks were in fact very faint Hertz had to work in a darkened room in order to observe them.



Also, to enable him to easily manipulate the wire circle detector and place it in different positions in space, Hertz mounted it on a T-wooden frame, as shown on the left. The gap (G) and the opposite side (L) are labels to be referred to later as Hertz used them to describe the orientation of the detector in space and thus to claim to be able to detect the electric and magnetic components of the wave.

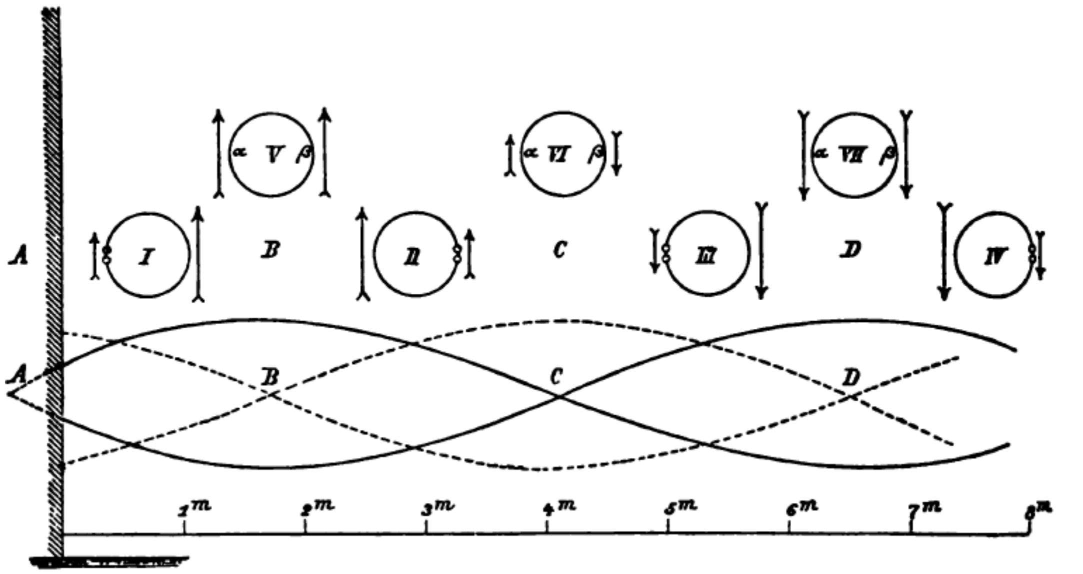
And now comes the big question:

Just HOW could Hertz, with such an extremely simple and primitive detector as that shown above, HOW could he possibly detect the *electric* and the *magnetic* oscillations in the radio wave?

To answer this big HOW question, we have to go to the articles in which he described his experiments and stated his conclusions, and observe how biased Hertz's reasoning was towards accepting Maxwell's theory.

Probably the most important article is that in which Hertz announced the observations of stationary radio waves in air formed by the interference of the direct waves produced by his sparks and the waves reflected from a conducting sheet of zinc (2m broad x 4m high) placed 13m in front of his apparatus.

The diagram below shows the geometry of this experiment.

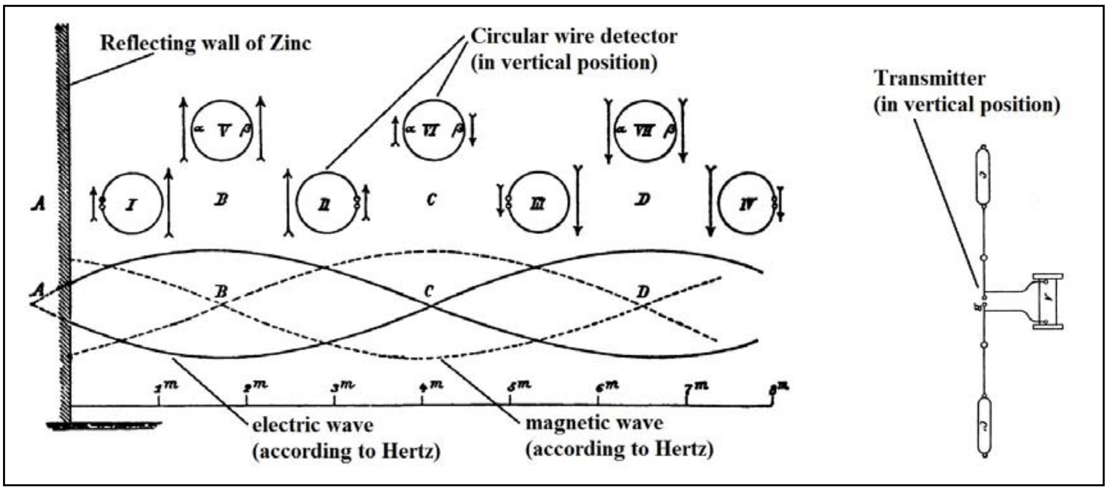


This experiment has been characterized by Oliver Lodge as Hertz's "greatest achievement" [18] and it was published in an article called "On electromagnetic waves in air and their reflection" [19].

The circle in the figure is the circular wire with a small gap used as a detector for the waves and shown on the same page. Observe that the gap (G) is sometimes towards the wall and sometimes away from the wall; these are the positions in which Hertz observed the strongest sparks.

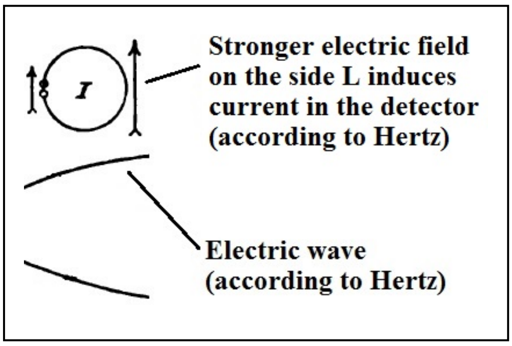
While the figure shows the circle in vertical plane, the transmitter is omitted; regarding the transmitter, Hertz mentioned that in these experiments it was placed vertically, so he expected the "electric forces", which he showed in the diagram by the arrows, to oscillate up and down in a vertical direction.

I think that showing the transmitter in vertical direction as it was placed by Hertz in these experiments is very important to understanding that the reception of the waves takes place through *electromagnetic induction*; so I have added the transmitter to the previous figure together with a few labels and I obtained the diagram below:



Hertz wrote that in the positions I, II, III and IV he observed sparks at the knobs (gap G) when his detector circle of wire was placed with the gap (G) as shown, sometimes towards the wall, sometimes away from the wall.

The key point here is how Hertz explains the production of sparks in his circular wire detector:



he says that the electric current responsible for the spark at the gap (G) is produced in the circular wire by the electric field of the stationary wave supposedly existing in air; the amplitude of the electric field of the wave is greater on the side (L) opposite to the gap and weaker at the side (G) of the gap, as shown by arrows.

The figure on the left shows in detail the situation at location I.

With this kind of reasoning, Hertz claimed that he detected a stationary electric

18. Oliver Lodge, *The Work of Hertz and his Successors*, 2nd Edition, The Electrician Printing and Publishing Company, London 1897, p.9.

19. Heinrich HERTZ, *Electric Waves*, Dover Publications Inc., New York 1962 (MacMillan, 1893), p.124.

wave like that drawn with full line. But, in my opinion, this is erroneous. Here's my claim:

What Hertz believed was an electric wave is in fact a magnetic wave; the electric current responsible for the spark at the gap (G) of the circular wire is induced through electromagnetic induction on the side (L) of the circular wire, the amplitude of the magnetic field of the stationary magnetic wave being greater at (L) than at (G) for the positions I, II, III and IV specified by Hertz.

This shows that the stationary wave drawn by Hertz in his diagram in full line (and believed by him to be an electric wave) is in fact a magnetic wave.

Then how about the stationary wave drawn in dotted line?

About it Hertz, completely mesmerized by, and under the total control of, Maxwell's ideas (as can be seen in the excerpt below [20]), said that it was a magnetic wave:

> We might, however, in another sense call *B* and *D* nodes, for these points are nodes of a stationary wave of magnetic force, which, according to theory, accompanies the electric wave and is displaced a quarter wave-length relatively to it. This

(Note Hertz's words "according to theory", which refer to Maxwell's electromagnetic theory. This illustrates the point made earlier that Hertz was under the influence of Maxwell's theory even when performing and explaining his experiments.)

So, to explain how Hertz obtained the stationary waves shown in dotted line (the magnetic wave, according to him) he looks again at the same sparks produced at the gap (G) of his circular wire detector, whom Hertz holds vertically, *like before*. Hertz moves the circular wire (this time with the gap G upwards) away from the wall towards the transmitter and finds that the intensity of the sparks at the gap (G) vary somewhat as shown by the dotted line; he then concludes that the electric currents responsible for the sparks at (G) must be produced through *electromagnetic induction* in the circular wire by the magnetic component of the wave, noting (in this second case only) that the magnetic field is perpendicular to the plane of the circle.

If you followed carefully Hertz's explanations, you will see that he employs a double standard for explaining the same experimental situation:
Thus, in spite of the fact that *the detector was in the same vertical plane in both situations* (only the orientation of the gap G being changed) Hertz invokes
(i) electromagnetic induction to show that there is a magnetic wave (shown in dotted line)
but
(ii) simple electric field to show that there is an electric wave (shown in full line).
Note that when the detector is in **vertical plane** the magnetic field lines from the vertical transmitter are perpendicular to the plane of the circle wire detector and in this situation **electromagnetic induction occurs most effectively**.

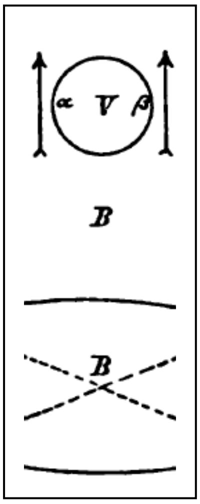
20. Heinrich HERTZ, *Electric Waves*, Dover Publications Inc., New York 1962 (MacMillan, 1893), p.130.

Since the wire was vertical in both cases, why not use electromagnetic induction to explain both observations?
Perhaps to force the experiments to fit Maxwell's theory?

We can see clearly now that the stationary wave drawn with dotted line is in fact a representation of the same magnetic wave shown in full line, because the sparks obtained when the dotted wave was found are fully explainable by admitting that the wave shown in full line is magnetic and that the currents in the circle wire detector are induced through electromagnetic induction in all cases.

In order to understand this better, let us take a few situations:

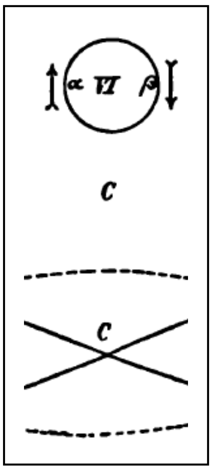
a) position V, where Hertz claims to have evidence of maximum amplitude (a.k.a. antinode) for the electric wave (full line) and zero amplitude (a.k.a. node) for the magnetic wave.



This situation can be explained simply by admitting that the full line is in fact the magnetic wave: when the gap (G) of the circular wire detector is towards the wall (labeled α in figure) or away from the wall (labeled β in figure) you have a spark because electromagnetic induction is stronger on the side L of the circular wire detector where it is not interrupted by the gap.

The dotted line need not be there as if a different kind of wave existed because when the gap (G) is upwards electromagnetic induction on one semicircle of the wire is balanced by the electromagnetic induction occurring on the other semicircle, preventing the production of a total current and the detection of a spark.

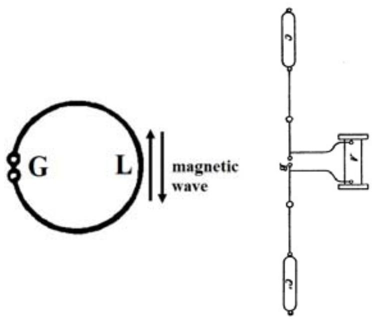
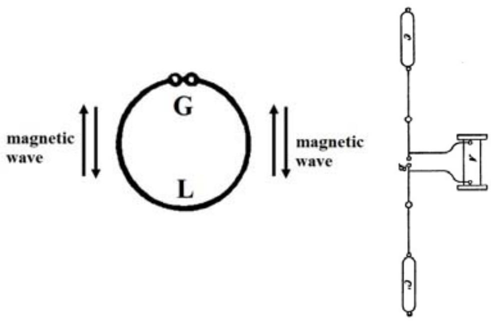
b) position VI, where Hertz claims to have observed minimum amplitude (node) for the electric wave (full line) and maximum amplitude (antinode) for the magnetic wave (dotted line).



The situation can be explained in the same simple way, consistent with point a): when the gap (G) of the circular wire detector is towards the wall (labeled α in figure) or away from the wall (labeled β in figure) you do not have a strong spark because the amplitude of the magnetic wave (the full line) is small on the side L so the current produced through electromagnetic induction is too weak to produce a spark.

If the gap (G) is upwards a spark is observed because the currents induced in the two semicircles add up since the oscillations of the magnetic wave are **opposite** across point C; this shows again that the dotted line claiming the presence of a second wave, superposed on the wave shown in full line, need not be there.

In conclusion, the difference between my claim and Hertz's claim is summarized in the following table:

	the wave shown in **full line** in Hertz's diagram of the experiment	the wave shown in *dotted line* in Hertz's diagram of the experiment
Heinrich HERTZ says it is	an electric wave	a magnetic wave
Ionel DINU says it is	a magnetic wave (detected through electromagnetic induction prevalent on side L of circular wire detector) 	a magnetic wave (the same) (detected through electromagnetic induction on both halves of the circular wire detector) 

Summary
This article analyzed Hertz's claim to have verified experimentally that radio waves are "electromagnetic", i.e. composed of electric and magnetic waves.
Even if Hertz's experiments have been done more than one hundred years ago (in 1888), they are relevant today because they are considered important piece of evidence in favor of Maxwell's electromagnetic theory and mentioned as such in every physics textbook today.
Close analysis shows however that Hertz's experiments are not necessarily a confirmation of Maxwell's theory and that Hertz's observations can be explained by simply considering that the radio wave produced by his apparatus is magnetic only, (without an electric component), propagating in the aether as a mechanical wave, its reception taking place through the effect of electromagnetic induction.
It was demonstrated that what Hertz believed was an electric wave is in fact a magnetic wave, and that what Hertz believed was a magnetic wave is in fact the same magnetic wave but detected differently, with the circular wire detector turned through 90 degrees in vertical plane (gap G upwards).
It was shown that Hertz was profoundly influenced by Maxwell's theory to the point of delusioning himself into believing that he was detecting "electromagnetic waves" when in fact he was observing magnetic waves only.
As such, Hertz's experiments cannot be invoked as proof for Maxwell's electromagnetic theory.

APPENDIX A

1138. SUMMARY OF THE EXPERIMENTS.—Apart from the method of Weber and Kohlrausch, in which the value of a is taken solely from the numbers given by the experiment itself, the other methods bring in the numerical value of a resistance, which has often been determined by the British Association. If we assume (1128) that the most probable value of the ohm is represented by 106·25 cm. of mercury, the unit (B.A.U.) is equal to 0·98664. By correcting the results for the error made in estimating the resistances, we find as means of the numbers obtained by various experimenters:

Date and Observer.	Value of a Found.	Value of a Corrected.
1856. Weber and Kohlrausch ...	$31 \cdot 07.10^9$	$31 \cdot 07.10^9$
1869. W. Thomson and King ...	$28 \cdot 46.10^9$	$28 \cdot 08.10^9$
1872. Dugald McKichan	$29 \cdot 35.10^9$	$28 \cdot 96.10^9$
1880. Shida	$29 \cdot 95.10^9$	$29 \cdot 55.10^9$
1879. Ayrton and Perry	$29 \cdot 80.10^9$	$29 \cdot 60.10^9$
1883. J. J. Thomson	$29 \cdot 20.10^9$	$29 \cdot 20.10^9$
1884. Klemencic	$30 \cdot 19.10^9$	$30 \cdot 19.10^9$
Mean		$29 \cdot 52.10^9$

The most recent researches have given for the velocity of light:

1862. Foucault	$29 \cdot 80.10^9$
1874. Cornu	$30 \cdot 04.10^9$
1879. Michelson	$29 \cdot 98.10^9$

It will be seen that, in all probability, or at any rate with an error which is less than one per cent., the velocity of light in a vacuum, and the ratio of the electromagnetic and electrostatic units of electricity, are represented by the same number.

APPENDIX B

présenter les assurances de la profonde estime que vos travaux m'ont inspiré depuis long temps.

Votre très-humble et très-obéissant serviteur,

F. Arago.

Cette lettre vous sera remise par M. Dupin, l'un de nos ingénieurs les plus distingués. Mon excellent ami, M. de Humboldt, qui a eu l'an dernier l'honneur de faire votre connaissance, s'est chargé de vous le recommander.

41.—*From* Dr. Young *to* M. Arago.

My dear Sir, London, 48, Welbeck-street, 12th January, 1817.

I have long been intending to scold you for leaving England without performing your promise of paying me another visit with your friend Gay-Lussac at Worthing. I was the more mortified at the circumstance, because I fear that you left me under a mistaken apprehension that I had some engagement for the day which made your company inconvenient to me;—this was very far from the truth;—and when I expressed some regret that you had not written to give me notice of your coming, it was more from feeling how easily it might have happened that I might have been absent the whole of the afternoon without seeing you, than from any partial engagement which I had actually made. For the present, the obligation is all on my side. I am sensible how great a compliment you paid me in undertaking such a journey for such an object; and I am conscious that I was unable to repay you either by information or civilities of any kind. You were already acquainted with everything that I meant to have told you respecting my optical speculations; and you did not give me time to do the honours of the country by common hospitality. I am, however, most happy to find that you are to return in the spring, and then, I trust, that you will allow me to make up for the deficiency.

I was reflecting, after you left me, on the very important experiment which you made on the equality of the intensity of colours formed in reflected and in transmitted light: you seemed to regard it as forming a difficulty in my hypothesis; but in reality there is nothing in this fact at all unfavourable to that theory, although it requires some modification of the general law of interference, if we set out with considering the light as arriving at any given point independently of the action of this law; for instance, in the present case of transmitted light, *after two* internal reflections, which would leave it less intense than you actually found it. But it is equally consistent with the theory to consider the colour in question as being formed at the instant of the second reflection; and the analogy with elastic bodies fully justifies this mode of applying the law, so as to consider the whole light once reflected, as interfering with an equal portion of the transmitted light. (Supra, p. 160.)

The same analogy is fully sufficient to explain the inversion of the undulation, or the loss of half an interval, when a direct partial reflection takes place from the surface of a rarer medium, as, I believe, you are yourself aware. But Mr. Fresnel, in his letter to me, mentions this fact as equally inexplicable with the inversion by extremely oblique reflection. I am sincerely delighted with the success which has attended Mr. Fresnel's labours, as I beg you will tell him; and I think some of his proofs and illustrations very distinctly stated; but I cannot fully adopt your expression in the letter you wrote by Mr. Dupin, that his memoir may be "considéré comme *la* démonstration de la doctrine des interférences;" for neither I nor any of those few who were acquainted with what I had written can find a single *new* fact in it of the least importance: nothing certainly half so important as your experiments on the colours seen in transmitted light, or on the non-interference of light polarised in opposite directions. Mr. Fresnel's words, in his letter, are "les franges extérieures se propagent aussi suivant des hyperboles comme je l'ai reconnu, et la courbure de ces trajectoires, qui est nulle pour les bandes intérieures, devient sensible au contraire dans les franges extérieures." Now you are all well aware that this was known to Newton himself, and that he attempted to elude the difficulty by saying that the light was not the same; and it was, therefore, unnecessary for me to repeat it in the same form. And the precise hyperbolical nature of the curves concerned is by no means a very strong point in the chain of evidences, partly on account of the difficulty of measuring the exact breadth of the fringes, and partly on account of the loss of the half interval, not hitherto explained. Mr. Fresnel has repeated some of Mr. Dutour's experiments on small cylinders, and has very truly observed that the spectra move with the cylinders. This was the reason that I never considered these experiments as of any value, the circumstance having been noticed by several authors, and, among the rest, by Mr. Brougham in 1796.

We have made but little progress in the measurement of the pendulum, except that Major Kater's experiments are nearly completed. Troughton is going on with his, but I am persuaded they can be of no use, from the nature of his suspension. I have been calculating the effect of the flexure of a spring in shortening the pendulum, and I find that it must be very sensible in all imaginable cases, even when the elastic force of the spring as an impelling power is wholly inconsiderable. I hope in a few weeks to get a clockmaker to make a scapement for my pendulum, which shall not have any influence on its rate; or if otherwise, to make the experiments without a scapement, as has been done in other instances; but in this case it would be necessary to fix the moveable weight at such points as would afford coincidences at convenient intervals, and the whole determination would be more laborious.

I have been reconsidering the theory of capillary attraction, and have at last fully satisfied myself with respect to the fundamental demonstration of the general law of superficial contraction, which I have deduced in a manner at once simple and conclusive from the action of a cohesive force extending to a considerable number of particles within a given insensible distance. This solution has very unexpectedly led me to form an estimate, something more than merely conjectural, though not fully demonstrative, of the magnitude of the ultimate atoms of bodies; of water, for instance, about a million of which would occupy a length equal to the diameter of one of the red particles of blood. This, however, you may possibly regard as a mere dream, and you are fully at liberty to do so.

I have also been reflecting on the possibility of giving an imperfect explanation of the affection of light which constitutes polarisation, without departing from the genuine doctrine of undulations. It is a principle in this theory, that all undulations are simply propagated through homogeneous mediums in concentric spherical surfaces like the undulations of sound, consisting simply in the direct and retrograde motions of the particles in the direction of the radius, with their concomitant condensation and rarefactions. And yet it is possible to explain in this theory a transverse vibration, propagated also in the direction of the radius, and with equal velocity, the motions of the particles being in a certain constant direction with respect to that radius; and this is a *polarisation.** But its inconceivable minuteness suggests a doubt as to the possibility of its producing any sensible effects: in a physical sense, it is almost an evanescent quantity, although not in a mathematical one. Its foundation is this: suppose two particles to reflect two portions of light, which interfere with each other, and form a dark fringe, the one being situated at the distance of several intervals from the other, in a direction transverse to that of the fringe: it is obvious that their interference can never be so completely effectual as not to leave some remains of the motions combined with each other; the direct motion of the one will destroy the retrograde motion of the other: but the transverse motions of each, with respect to the line bisecting their directions, will conspire with each other and will produce a single transverse vibratory motion. And who shall say that this motion will be too minute to produce any effect in any circumstances?

Pray give my compliments to Mr. Gay-Lussac, and tell him that I was much disappointed in not having some further conversation with him on elective attractions. Mrs. Y. begs to unite with me in kind remembrances both to him and to your-

* This suggestion was a capital step in the undulatory theory of light. See Dr. Whewell's 'History of the Inductive Sciences,' vol. ii. p. 417.

self. I am happy to hear that the work on Egypt is going on, and that Mr. Jomard has married a pretty widow.

Believe me, dear Sir,
Very sincerely yours,
THOMAS YOUNG.

I know that I need not apologize to you for writing in English, as you read it with so much ease. I write it so much faster than French, and of course better in a *duplicate ratio*, although this is of less consequence.

12.—Dr. YOUNG *to* M. ARAGO.

MY DEAR SIR, London, Welbeck-street, 22nd April, 1817.

I HAVE been preparing for you a few memorandums on the subject of the double internal reflection of light, which I promised you at Paris, but I have not had time to put them together in a very satisfactory form: having, however, an opportunity of writing, I do not like to let it pass without assuring you that I have not forgotten the many pleasant hours that I spent at Paris, and the many kindnesses that I received there, for which I am indebted to no one so much as to yourself. Pray tell me if I am not to expect the pleasure of seeing you here shortly; I understand that Mudge is ready to cooperate in everything of every kind that you can desire of him.

With respect to the reflections in question, you were very right in hesitating to admit the matter as self-evident, and I was a little precipitate in my way of stating it: for I find upon calculation, that if we considered the simple velocities as the measure of the intensity of light, the second reflection would be no stronger in the case in question than in any other case, and the transmitted light ought to be much less strongly coloured than the reflected, since the arrival of a new vibration at the first surface would not affect the *velocity* of the second internal reflection, as added to or subtracted from the velocity produced by its simple transmission. But the velocity alone is not the measure of the intensity of light; and it must in