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What Heinrich Hertz discovered about electric waves in 1887–1888

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Abstract

Among the most influential and well-known experiments of the 19th century was the generation and detection of electromagnetic radiation by Heinrich Hertz in 1887–1888, work that bears favorable comparison for experimental ingenuity and influence with that by Michael Faraday in the 1830s and 1840s. In what follows, we pursue issues raised by what Hertz did in his experimental space to produce and to detect what proved to be an extraordinarily subtle effect. Though he did provide evidence for the existence of such radiation that other investigators found compelling, nevertheless Hertz's data and the conclusions he drew from it ran counter to the claim of Maxwell's electrodynamics that electric waves in air and wires travel at the same speed. Since subsequent experiments eventually suggested otherwise, the question arises of just what took place in Hertz's. The difficulties attendant on designing, deploying, and interpreting novel apparatus go far in explaining his results, which were nevertheless sufficiently convincing that other investigators, and Hertz himself, soon took up the challenge of further investigation based on his initial designs.

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In the late fall and winter of 1887, a young physicist at the Technical University of Karlsruhe worked to determine whether electric and magnetic forces propagate at finite speeds. Heinrich Hertz was well aware that James Clerk Maxwell's theory not only claimed as much, but that his system located electromagnetic effects in a field that was considered to be the state of an all-encompassing medium called the "ether". No distance forces of any kind were present in Maxwell's system. Hertz's own mentor at the University of Berlin, Hermann von Helmholtz, had essayed a variant form of electromagnetics that also yielded finite speeds for measurable electric and magnetic effects but without abandoning distance forces between the ether's elements. He did so by treating the ether as electrically and magnetically polarizable. This implicated a physical distinction between the electromotive forces due to charge and those due to changing currents since Helmholtz's scheme did not envision a unitary electric field. While at Berlin, Hertz developed ways to test the two fundamental requirements that Helmholtz's and Maxwell's systems both required, namely that changing polarization currents act electromotively like changing currents of conduction, and that a changing conduction current can electrically polarize a dielectric. He found that the available apparatus to do so could not generate detectable effects, and so Hertz had abandoned the attempt while nevertheless going on to probe theoretically the foundations of Maxwell's theory. This changed in 1887 as a result of experiments that he undertook in light of a serendipitous discovery.[1]

While demonstrating electromagnetic induction to a class in the spring of 1886, Hertz observed what seemed to be an inordinately powerful effect that seemed to involve spark discharges. Over the next months he discovered that he could control the effect by discharging a small induction coil across an air gap bridging the arms of a narrow, metal cylinder terminated by capacitive end-loads. Hertz discovered that the spark discharge was sufficiently regular that he could use a device known as a "Riess micrometer"—a pair of wires connected to the termini of a micrometer—to probe what takes place along the arms of the discharge circuit. That device shortly evolved into a fully independent loop of wire bridged by a spark-gap that, Hertz found, sparked most under the influence of the discharging circuit's oscillations when adjusted to a particular length that, he conceived, put the two circuits in resonance. These experiments themselves, he wrote, illustrated "for the first time" the "mutual action of rectilinear open circuits which plays such an important part in theory" (Hertz 1893, 42).

In early September, 1887 he began a series of successful experiments to observe resonant induction at these extraordinary—and previously unexplored—frequencies. The discharger and resonator also, and critically, enabled Hertz to succeed in answering the second of Helmholtz's two Berlin questions. To do so he placed a dielectric block in such a position that the changing charges on the discharger's capacitive loads imaged themselves in the block, generating rapidly-changing polarization currents. Hertz found that his resonant detector sparked according to its position relative to

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[1] For short accounts of Maxwell's work and its consequences among his followers see, respectively, Siegel (2014) and Yeang (2014). Details are in Buchwald (1985), while Buchwald (2001) discusses Hertz's early work with Helmholtz's system. Darrigol (2000) provides an excellent overview of 19th-century electrodynamics.

both discharging circuit and dielectric, demonstrating that the changing polarization currents acted electromotively.

The Berlin questions still on his mind, Hertz could not see his way to producing a powerful enough effect directly to detect dielectric polarization by a rapidly-changing current. Hertz soon realized that he could indirectly determine whether such an effect exists by examining whether electric waves occur in air, since on either Helmholtz's or Maxwell's theories air waves will arise only if it does. That original motivation led Hertz in the late fall and early winter of 1887 to the first demonstrations that air waves can indeed be produced and detected. His account of the original discovery experiments reached the *Sitzungsberichte* of the Prussian Academy of Science on the second of February. It was reprinted the following May in the widely-read *Wiedemann's Annalen*, which was where what we'll call the "discovery paper" first became broadly available. In March Hertz had used his apparatus to examine air waves by reflecting them. His account of these reflection experiments appeared in the *Annalen* issue following the discovery paper.

For nearly 3 months, only Hertz's discovery experiments were available in print and so known in detail beyond those with whom Hertz spoke or corresponded. Although his first results did provide evidence for propagation, they were nevertheless inconsistent with Maxwell's theory. For they seemed to show that the wave in air travelled at a speed over 50% greater than a wave of the same frequency in an accompanying metal wire. This ran counter to the apparent implications of Maxwell's theory, which seemed to require the speeds to be the same since, according to it, waves properly speaking do not travel *in* wires but rather slip over their surfaces through the surrounding air. However, Helmholtz's polarizable ether, with its elementary distance forces, incorporated a constant k in its fundamental equations that did not appear in Maxwell's theory. As a result, proper longitudinal waves, but not transverse ones, could propagate in unbounded, purely conducting spaces with speeds inversely proportional to the square root of k. If explained using Helmholtz's theory, Hertz's discovery experiment would have measured speed differences between the electromotive effects of a longitudinal wave within the wire and those of a transverse wave in air.[2]

Hertz had not developed this possibility in his publication describing the discovery experiment, remarking there only that his "results might even suffice to decide between the various conflicting theories, assuming that at least one of them is correct" (Hertz 1893, 123). His next experiments, on the reflection of electric waves, were intended to provide further discriminatory evidence. Yet, according to his account, we shall see that Hertz found these further experiments sustained his initial results in that the reflected waves apparently had the same wavelength as those of the discovery experiments, again implying that wire waves travelled at much slower speeds than air waves.

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[2] There was another potential issue, which Hertz would certainly have known though he did not mention it. Namely, Helmholtz's theory requires *both* transverse and (non-dispersive) longitudinal waves in a non-conducting medium with finite susceptibility, with the longitudinal waves travelling faster than the transverse ones at $\sqrt{1+4\pi\chi_{\mathrm{e}}}/\left(A\sqrt{4\pi\chi_{\mathrm{e}}}\right)$. Later in 1888 Hertz examined the polarization of the air wave, obviating any possibility that he had detected a longitudinal air wave: "From the mode in which our ray was produced we can have no doubt that it consists of transverse vibrations and is plane-polarized in the optical sense" (Hertz 1893, 177). For the structure of Helmholtz's theory and its application to wire waves see Buchwald (1994, 375–388).

On further consideration later in 1888 Hertz decided that, in the limit of an infinitely small wire radius and perfect conductivity, even on Helmholtz's theory longitudinal waves within the wire would be accompanied by purely transverse ones along the wire's surface at air speeds, and their effects would have been the ones he should then have detected in the discovery experiments. He accordingly decided that it "seems rather doubtful whether the limiting condition [vanishing wire radius and infinite wire conductivity] is correct for rapidly alternating forces" (Hertz 1893, 158). But Hertz did not essay a deduction relaxing the limiting conditions, which would have been very difficult, so this remained, at best, a remote possibility (Hertz 1893, 159).

And so Hertz's first published experiments did not provide evidence for Maxwell's (or, in the end, Helmholtz's) electrodynamics given the apparent difference between wire and air speeds. In retrospect, it seems impossible for a well-constructed experiment to have produced such a result for, after all, in the simplest circumstances there can be no difference in speed between the waves measured by Hertz's device. Within little more than a year these results were set to the side as physicists concentrated principally on the properties of the reflected waves and as more precise measurements of wire waves did not yield a speed substantially different from that of light. Some pathological factor must have affected Hertz's measurements, but precisely what that might have been remained a matter of conjecture as investigators moved ahead with their own explorations of electric waves—despite the fact that it was precisely Hertz's retrospectively-flawed experiments that had first convinced physicists that propagation in air does take place.

To unpack what occurred in Hertz's laboratory we turn first to his discovery experiments and then to the subsequent ones on reflection. Our method comprises examination of primary historical sources, a material replication of Hertz's air-wire and reflection experiments, and mathematical modeling of the physical configurations pertaining to the two trials. We will show that the variation Hertz observed in his discovery experiment that implied a difference between wire and air wave speeds was likely a result of factors that he did not consider at the time, and that, even if he had, he would not likely have been able to take into account. Based on our model for such factors together with an inspection of Hertz's experimental procedure, we argue further that these same factors had lesser effects on the results of his reflection experiments in 1888, and that his putative conclusion of consistency between the wavelengths in the two sorts of experiments was due to an entirely different cause embedded in the very nature of his observational method.

1 The discovery experiments

The device that Hertz developed in 1886 and 1887 to investigate his newly-produced million-cycle-per-second oscillations (Fig. 1), and which he used to demonstrate that rapidly-changing polarization currents act inductively, comprised an induction coil (A) with an interrupter as source whose secondary connected to a pair of copper wires about 2 mm in radius and 120 cm in length separated by an air gap (B) and terminated at their other ends by metal objects (here the oblong C and C$'$ but soon spheres) to provide significant capacitance. He combined this "discharger", as Hertz called it,

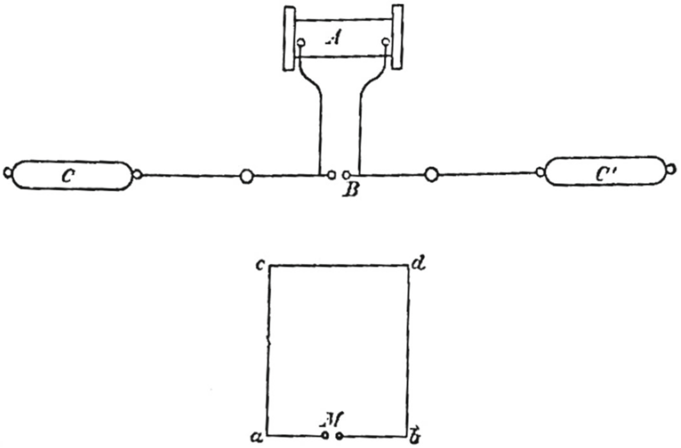

Fig. 1 Hertz's device to explore the inductive action of high frequency oscillations. [Hertz, *Electric Waves*, p. 40]

with a circular or rectangular "resonator" (M) to explore the inductive actions of these extraordinarily rapid oscillations—ones whose frequency, he wrote, "estimated, it is true, only by the aid of theory—is of the order of a hundred-millionth of a second."[3]

One source of the issues that would soon envelope Hertz's discovery experiments concerned just this calculation of frequency. To obtain a value Hertz calculated the discharger's self-induction with the usual formulae due to Franz Neumann, while admitting that the value would be different if he were to incorporate in it the effect of Helmholtz's extra constant k. The metal spheres for these experiments had equal radii of 15 cm, and Hertz took the discharger's capacitance to be precisely that. In 1891 Henri Poincaré pointed out that the two-sphere discharger had a capacitance of half that value, 7.5 rather than Hertz's 15. Instead of the frequency that Hertz in effect obtained for a discharger comprised of spherical terminators, namely about 28 MHz, he should here have calculated instead the much higher frequency of about 40 MHz. This error in estimating the capacitance of a system comprised of two equal metal objects joined by a wire continued in Hertz's discovery and reflection experiments, in which the capacitance-producing spheres were replaced by rectangular plates (giving, according to his incorrect estimation of the period, a frequency of about 36 MHz instead of about 50 MHz), though it was not responsible for the observations that produced a difference between wire and air wave speeds. It affected only calculations of what the speed of the wire wave in his first experiments might in fact be, but not its ratio to that of the air wave.

Once Hertz decided that he could answer the second of Helmholtz's Berlin proposals, having confirmed the first, by investigating whether electric waves in air exist, he deployed the apparatus in a way that set off a putative air wave against a wave that he could show directly to exist, and that no one would question: waves in conducting

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[3] Hertz (1893, 29) used a definition of period different by half of what became common later, writing that the period of a circuit with capacitance C and induction P is $\pi\sqrt{PC/A}$ where A is the speed of light. Hertz's units set capacitance in meters and induction in seconds.

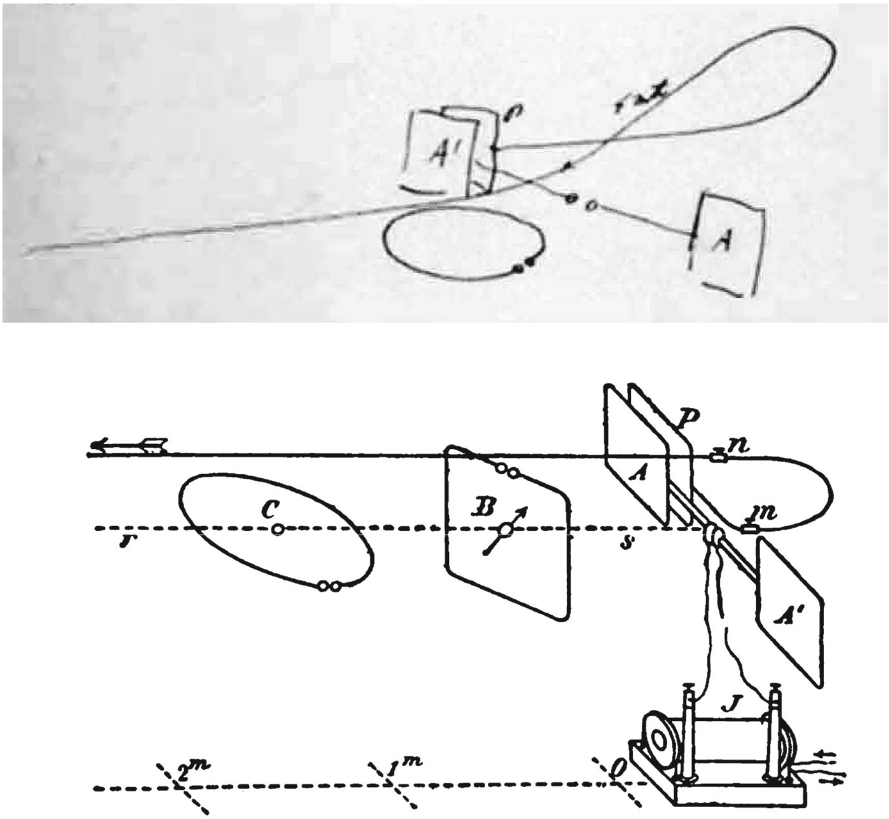

Fig. 25.

Fig. 2 Hertz's device adapted to examine air waves (from his notebook, top) and a published version (bottom). Doncel (1995, 212) for Hertz's notes and Hertz (1893, 108) for the published version

wires. The laboratory notes that Hertz took during his discovery experiments exist today, and from them we can present his drawing of the device that he used, a drawing that he reproduced in his published account (Fig. 2).

The diagram from Hertz's notes depicts one of the two configurations in which he placed his resonator, while the published diagram depicts both. Referring to the latter for clarity, we see that Hertz connected an induction coil *J* to his discharger by thin wires. The discharger itself was terminated by a pair (*A, A*′) of square brass plates 40 cm to a side to provide the requisite capacitance. Behind plate *A* Hertz placed an equal brass plate *P* to which he attached a copper wire (nm and beyond) that extended over 12 m across the room in which Hertz conducted his experiment. The oscillating charge on A will be imaged in P, thereby generating a wave along the wire, which was earthed, at the frequency of the discharger. The idea behind the experiment was to set off the wire wave's action on the resonator against that of the discharger.

Two factors underpinned Hertz's analysis and procedure here. First, he treated the effect as though whatever element of the wire that is nearest the resonator acted alone, carrying a current of magnitude determined by the value of the wave at its locus. The action of the discharger was treated similarly, namely as a wire element carrying a current of given magnitude. Second, to detect these differential actions he placed the resonator with its plane either vertical (B) or horizontal (C). To Hertz's way of thinking, which was substantially determined by Helmholtz's at the start of his experiments, these two positions could disentangle the distinct effects of electrostatic from electrodynamic *emfs*, since Helmholtz's theory implicated the possibility of different propagatory velocities (and different types of waves) for these two forces through his constant k. In position C the resonator responded entirely to electromagnetic induction through its area, while the near region to the discharger (position B) implicated both electrostatic and electromagnetic effects because of the charges on plates A and A'.

Place the resonator in position B. If it faces the discharger AA' then whatever occurs at its spark gap is due entirely to the discharger, while if it faces the wire then only the wire acts to effect sparking. Hertz observed as much, in agreement of course with all theories. In intermediate positions both discharger and wire act. Set the resonator so that it faces plates AP and observe its sparking. Then turn it to face plate A' and observe the sparking in that position. If the sparking visibly increased in the second position designate the change with '+', if it decreased with '–', and if it stayed the same with '0'.

It's easy to see the consequence. Suppose that the wire (W) and air (A) waves have different speeds but the same angular frequency ω – and so different wavelengths, respectively λ_w, λ_a.[4] Turn the vertical resonator so that its plane first forms an angle $\emptyset$ with respect to the wire. Then turn it to angle $-\emptyset$. With the wire stretched along Oz just above the origin at the discharger's spark-gap, the change in the effective sparking will track the difference (ΔM^2) between the squares of the magnitudes of their sum at each angle:

$$W_{\text{wire}} = \cos(\emptyset)\cos\left(\frac{2\pi z}{\lambda_{\text{w}}}\right) \quad \text{and} \quad W_{\text{air}} = \sin(\emptyset)\cos\left(\frac{2\pi z}{\lambda_{\text{a}}}\right)$$

$$M^2 = (W_{\text{wire}} + W_{\text{air}})^2 = 1 + 2\sin(\emptyset)\cos(\emptyset)\cos\left(2\pi z\left(\frac{1}{\lambda_{\text{w}}} - \frac{1}{\lambda_{\text{a}}}\right)\right)$$

$$\Delta(M^2) = \sin(2\emptyset)\cos\left[2\pi z\left(\frac{1}{\lambda_{\text{wire}}} - \frac{1}{\lambda_{\text{air}}}\right)\right]$$

If the air wave has infinite speed, and so infinite length, then the spark change tracks the wire wave. Suppose instead that the air wave has a finite speed different from that of the wire wave. Suppose further that at some locus z the spark change is at a maximum and that at a distance Δz further along it nearly vanishes. In that case the phase of $\Delta(M^2)$ must have changed by π, and consequently the ratio of wavelengths (and so speeds) must be

$$\text{speed ratio} = \frac{\lambda_{\text{a}}}{\lambda_{\text{w}}} = \Delta z/(\Delta z - \lambda_{\text{w}}/2)$$

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[4] The amplitude of the discharger's radiation would of course decrease with distance, but Hertz always adjusted the size of his spark gap so that the wire wave did not overwhelm the direct action since he observed only the interference change on rotating the resonator, not the magnitudes of the waves at different distances.

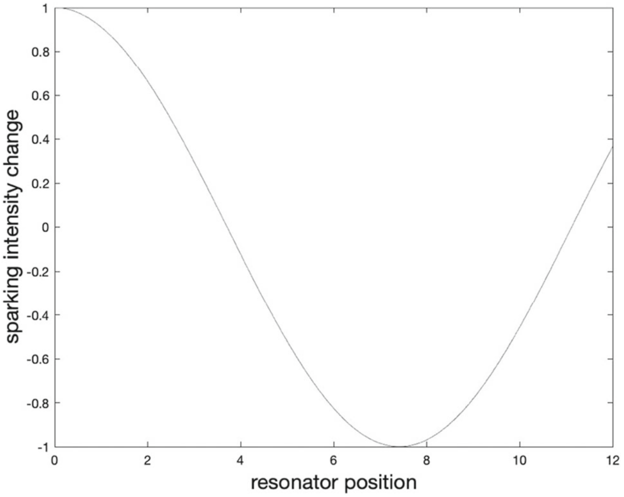

Fig. 3 The spark-change curve in the air-wire interference experiment according to Hertz's interpretation

Hertz had the wire wavelength at 5.6 m, and the shift from maximum to minimum sparking occurred after 7.5 m. Consequently he found that the air wave travelled about 1.6 times as fast as the wire wave (Fig. 3).

That was indeed Hertz's conclusion, but he arrived at it only after a considerable series of trials, all of which required skilled judgment. For he had to decide whether the sparking change on rotating the resonator was greater, less, or the same as it had been at the resonator's previous position. How to make such a judgment? The spark's visual intensity would hardly have been a good indicator because the eye does not respond linearly to intensity and because Hertz could not simultaneously compare sparking at two resonator positions.[5] He accordingly relied on the device with which his investigations had begun: the so-called "Riess micrometer" that formed the boundary of the resonator's spark gap. It comprised a nut and bolt pair, whose narrow separation could be adjusted by means of a screw. Hertz could adjust the width of the gap until, say, the spark vanished and use the gap size as a measure of intensity. Even that could hardly be exact because the sparks were weak, the micrometer adjustments subtle, the sparking of the induction coil loud and somewhat irregular, and so on. Furthermore Hertz's notes indicate that the discharger required continual recalibration by disconnecting and then reconnecting the wire to its drive plate.

Hertz's results for the ratio of air to wire wave speeds depended critically on his measurement of the wavelength in the wire. To be as accurate as possible, he drew his wire out to lengths of 8 m or 5.5 m, leaving it ungrounded. To measure the wave's nodes, he placed the resonator vertically below and parallel to the wire and moved it along until its sparking seemed to vanish. To do that required first calibrating the device by finding a location where the sparking was quite strong at some gap width and then maintaining that width as the resonator was moved along the wire. The

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[5] Hertz apparently never used more than one resonator at a time, probably to avoid differences in the responses of different detectors even if constructed as similarly as possible.

forward-traveling wire wave would be reflected at the end of the long wire, forming a standing-wave pattern with minimum intensity (nodes) appearing at every half-wavelength. In this way, he observed nodes for the 8 m wire at 0.2 m, 2.3 m, 5.1 m. For the 5.5 m one, the nodes were at 0.1 m, 2.8 m, and 5.5 m. Hertz accordingly estimated that the half-wavelength of the wire-wave "cannot differ much from 2.8 m" (Hertz 1893, 113).

Since Hertz had computed the frequency of his oscillator, terminated by its capacitance-producing square brass plates (40 cm to a side), to be about 36 MHz, he had the wire wave's speed at about 2×10^8 m/s. This seemed reasonable since what he considered to be the most reliable measurements of this kind for wire waves, done in 1875 by Werner Siemens, had produced 2.6×10^8 m/s and because he was not convinced that "the ordinary theory of electric oscillations gives correct results here"(Hertz 1893, 109). Making the aforementioned correction to the capacitance would have given Hertz about 51 MHz for the frequency and so about 2.8×10^8 m/s, still an insignificant difference because of computational uncertainty. In any case Hertz did not know about his error until 1891, when Poincaré pointed it out. By then he had set his wire and discharger experiments to the side as further experiments by himself and others produced different outcomes.

Turn now to Hertz's results. They were hardly unambiguous, and he had considerable difficulty producing a consistent set. For his first observations in mid November, 1887 Hertz set the resonator in the vertical, fixed the length of the wire, and moved the resonator every 50 cm. These observations seemed to indicate that the air wave did not propagate at all. He tried a different setup involving mutually-parallel oscillators, but again was unable to manifest a finite velocity. "Disheartened," he later wrote, "I gave up experimenting." But a month later he returned to the investigation, doggedly reflecting that "it would be quite as important to find out that electric force was propagated with an infinite velocity, and that Maxwell's theory was false" (Hertz 1893, 8).

The new experiments were more varied than the first ones, for he now added backlengths to his wire to shift its phase. For each such backlength he again moved his vertically-placed resonator every 50 cm at first to a distance of 8.5 m, with five backlengths from 100 to 500 cm. These results were not definitive, not least because the indications past about 4 m were hardly dispositive. Below 4 m or thereabouts, however, they did seem to indicate that the interference was tracking the wire wave. This "displeasing result," he wrote his parents, indicates that "the velocity [of the direct action from the discharger] is not that of light, but certainly much greater, perhaps infinitely great, at all events not measurable" (Hertz et al. 1977).

The experimental tables that Hertz published do not fully represent the sequence of his observations, including alterations in the data contained in his notebook. These changes were certainly based on further adjustments that Hertz had not bothered to record.[6] The notebook nevertheless contains most of his published results as well as providing the dates at which he made critical decisions.[7] From it and the remarks in

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[6] See, for one such example, Doncel (1991, 22).

[7] For full details of Hertz's results in the discovery experiments based on his notes see Buchwald (1994, 269–298).

his letters home we can reconstruct what occurred. He first explored the wire wave on November 7. On the 11th and 12th he undertook experiments to measure the air wave without finding any indication of a finite speed. These were likely undertaken with the resonator plane set vertically and extending out to no more than 4 or 5 meters, albeit likely with a sequence of wire backlengths. Five weeks later (Dec. 17) he began a careful series of numbered experiments, detailed in his notes, intended to confirm his November result that the air wave does *not* have a finite speed. These experiments used a sequence of backlengths and extended out to 8 m from the discharger. They did not seem to obviate his previous results, though interpretation of the data was not easy given that Hertz had to judge the resonator's sparking while adjusting the micrometer gap that governed it.

One week later (Dec.23), Hertz altered his observational method. He did so in two ways. First, he placed a circular resonator (35 cm in radius) horizontally so that its plane contained both the wire and the discharger. If its spark gap was turned so that the gap was perpendicular to the discharger, the actions of the large charges on the discharger's end plates had no net effect, and in any other position of the gap the static effect was at least weak in comparison with the electromagnetic induction through the resonator's plane. But that was just the first change, because of course the whole point of the experiment was to alter the mutual actions on the resonator of the wire and the discharger. Placing the resonator at a given position and now holding it there, Hertz flipped the wire from one side of it to the other. This did not change the induction through the resonator's plane due to the discharger but it did reverse the wire's action. Figure 2, top, illustrates one such position; note the orientation of the spark gap to minimize electrostatic action.

In this considerably different configuration, Hertz found something unexpected and exciting. In experiment 50 from his notebook the resonator was placed close to the discharger, and a strong increase in its sparking on flipping the wire recorded. In the next experiment that day, labeled 51, Hertz moved the resonator 3 m further down and repeated the wire flip. And this time he found, excitedly, that strong sparking still took place with the wire "on the same side as when close" to the discharger. He followed it further, moving the resonator along up to 4 m away and continued to find no significant alteration at any point. Only one conclusion was possible given Hertz's model: at least at distances up to about 4 m, the wire and air waves seemed to track one another reasonably well, though the observations were not sufficiently accurate to obviate any speed difference whatsoever.

But, of course, Hertz's experiments with the resonator placed vertically had provided no such indication of the air action's propagation. This must be, he concluded at the time, because those experiments, done up to 4 m or so from the discharger with the resonator vertical, remained preferentially susceptible to the discharger's electrostatic force. Consequently they indicated only that this force itself, though not necessarily the electromagnetic one, has a very high or even infinite speed, which was consistent with one implication, as noted above, of the constant k in Helmholtz's polarizable ether theory (provided that the ether's polarizability is not itself effectively infinite). Hertz had now to determine whether the speed of the electromagnetic force was reasonably close to that of the wire wave, as his experiment 51 and its follow-up to about 4 m distance seemed to indicate.

Table 1 Hertz's final table, collating his several results

	0	1	2	3	4	5	6	7	8	9	10	11	12
100	–	–	–	–	0	0	0	+	+	+	+	+	0
250	0	+	+	+	+	+	0	0	0	0	–	–	–
400	+	+	+	+	0	0	–	–	–	–	0	0	0

The entries in the leftmost column are the wire backlengths, those on the top row are resonator distances from the discharger (Hertz 1893, 120)

To find out, Hertz returned to the vertical resonator, now taking it as far as 12 m from the discharger to avoid the electrostatic action that seemed to predominate with such a resonator orientation below 4 m. He published his results—these new ones together with the previous set—in the first two tables of his article. From the first (which runs to 8 m), he wrote, "it might almost appear as if the interferences changed sign at every half-wavelength of the waves in the wire," though this was not quite clear because "we notice that the retardation of phase proceeds more rapidly in the neighborhood of the origin than at a distance from it"—that is, up to a distance of about 4 m (Hertz 1893, 118). But the second, which runs to 12 m, now indicated a different change in sparking past the 4 m distance, implying a finite speed difference between air and wire waves.

To be certain of his results, Hertz turned again to the horizontal resonator and measured *up to 4 m* using different backlengths. He combined these observations with those done *past 4 m* using a vertical resonator—both sets accordingly avoid the electrostatic force—to produce his final published results for the electromagnetic wave (Table 1). From it he drew two conclusions: that at "distances of every 7.5 m the sign of the interference changes from + to –", while adding backlengths to the wire wave (and so retarding its phase) shifts interferences toward the origin, which implies that "of the two different rates of propagation that through air is the more rapid."[8] Given the wire wave's length of 5.6 m, it followed at once that the air wave travelled about 1.6 times as rapidly as the wire wave, producing an air wavelength of 5.6 m × 1.6 ~ 9.0 m.

In retrospect Hertz's experimental results are of course puzzling since the electric wave propagating in air should have the same speed as that propagating along the wire. Was there something about Hertz's apparatus, method of observing, or experimental space that, unbeknownst to him, could have produced such a difference? To explore the situation, we first undertook to reproduce Hertz's apparatus and method of measurement insofar as we were able.

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[8] Hertz (1893, 120–21) again provided no formula, but it's easy to see what he had in mind. Suppose that adding some amount b to the backlength of the wire reproduces the same interference at position $z + \delta z$ that it had at position z. Then any change to the phase of the interference must vanish, in which case δz must equal $\left(\frac{b}{v_{\mathrm{m}}}\right)/(1/v_{\mathrm{a}} - 1/v_{\mathrm{m}})$, where $v_{\mathrm{m}},\ v_{\mathrm{a}}$ are, respectively, the wire and air speeds. If the air speed is greater than the wire speed then the phase position shifts back toward the discharger.

2 Air-wire interference in Toronto and Florence

In recent years two researchers have endeavored to replicate aspects of Hertz's several experiments. Roland Wittje rebuilt Hertz's discharger and resonator. Working with resonator sparking, he was able to replicate Hertz's exploratory examination of electromagnetic induction between conductors at these high frequencies (Wittje 1996). Ted Simpson rebuilt the apparatus for Hertz's 1888 reflection experiment and measured the received signal intensity on a loop resonator using modern radio-wave measuring devices, though not in an environment similar to Hertz's (Simpson 2018). These works have demonstrated the plausibility of replicating at least certain aspects of Hertz's experiments.

At Toronto from the fall of 2014 through the winter of 2015 and then in Florence in May, 2018 we reconstructed and deployed apparatus that we attempted to make as similar to Hertz's as feasible. Following Hertz's specifications, our spark-gap discharger comprised a pair of primary conductors, a frame to support them, a Ruhmkorff coil (that is, an original or a facsimile of a late 19th century induction coil with interrupter), and a battery. This coil was not the typical inductor wound with a single wire, but was instead similar to a transformer with primary and secondary coils wrapped around an iron core. Each of the discharger's two arms consisted of a square brass plate 40 cm to a side, a copper wire 60 cm long and 5 mm in diameter connected to the plate, and a spherical brass knob 3 cm in diameter connected to the other end of the wire. A vertical, T-shaped wooden frame supported the discharger with the spark-gap oriented horizontally at 1.5 m above the ground. In February 2015, we conducted carpenter work outside the campus of the University of Toronto to build the wooden frame.[9]

The Ruhmkorff coil was crucial to the feasibility of the replication, and we were able to obtain one fabricated in Italy around 1950. According to its specifications the device could generate sparks of lengths up to 8.89 cm; its interrupter operated about 10–20 times per second. Our coil did certainly differ from Hertz's: ours had a length of 38.1 cm, a diameter of 22.86 cm, and used an electromechanical interrupter, while his had a length of 52 cm, a diameter of 20 cm, and used a mercury interrupter. Such a difference did not affect the frequency of the discharger proper, and these were set to Hertz's specifications. Still, the differences between our coil and Hertz's did undoubtedly result in a considerably lower spark intensity in our case, which likely accounted for the difficulty we had in reliably observing the resonator sparking.[10]

Our resonator consisted of a gapped 60 cm square loop made of 2 mm-thick copper wire fixed to a wooden frame. We attached the ends of the gap to two fine-threaded bolts to form a micrometer for adjusting the width of the resonator's spark-gap. We subsequently experimented with circular loops of different sizes, but did not fix them to wooden frames. Figure 4 is a photo of the apparatus for our Toronto replication.

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[9] Curtis Forbes, then a PhD student at the Institute for the History and Philosophy of Science and Technology, University of Toronto, offered a venue for the carpenter work and assisted the construction.

[10] We used a 12 V and 12 AH lead battery to power the coil. Hertz's power source comprised "six large Bunsen cells," a form of electrochemical battery with a zinc anode in sulfuric acid and a carbon cathode in nitric acid, with each such cell producing approximately 1.9 V. Our source was accordingly close in magnitude to Hertz's.

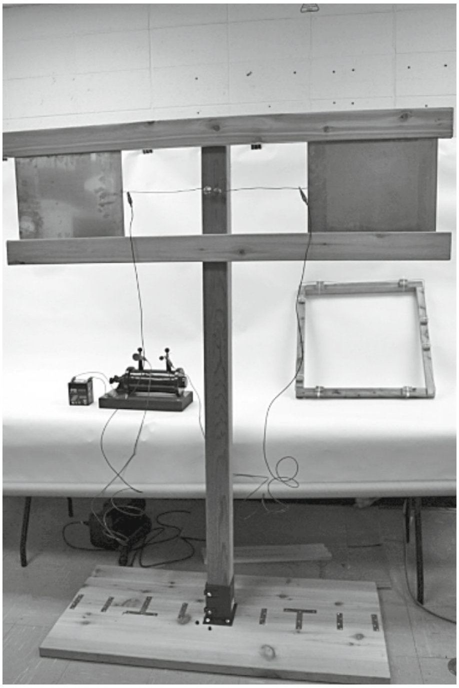

Fig. 4 The Toronto apparatus

It displays the finished device, with the Ruhmkorff coil at mid-left, the resonator at mid-right, and the discharger with its brass plates at the top.

The first trials took place in the storage room of the University of Toronto Scientific Instruments Collection (UTSIC) in the basement of the Sidney Smith Building. The room is about 10 m long, 5 m wide, and it is filled with compact shelf-stacks on one side. An aisle the length of the room and 2 m in width was available for our work. From June through November 2015 we worked to stabilize the device and to become familiar with its operation. We early discovered that sparks about a centimeter in length could be produced at a distance of 1 m. The resonator did respond up to an additional meter's distance but without reasonable stability and strength, appearing only when the micrometer in the resonator adjusted the gap to an extremely narrow width. Moreover, the room had to be absolutely dark for these faint sparks to be seen at all. This was certainly due to the insufficient strength of the impulse generated by our coil. Hertz had himself faced a similar difficulty, remarking that he and his laboratory assistant (otherwise unnamed) had to "consider sparks as being perceptible when they are a *few hundredths of a millimeter* in length" (Hertz 1893, 34, emphasis added) (Fig. 5).

The visual weakness of the resonator's spark was indeed the principal difficulty of these experiments, for Hertz as well as for us. Hertz and his assistant had to adjust their eyes to pitch-black conditions and use a magnifying lens, conditions we were not able precisely to emulate. He was nevertheless eventually able to observe sparking as far as 12 m from the discharger, though "this required rather an effort" (Hertz 1893, 119). We tried to improve the resonator's spark by altering the design of the micrometer,

Fig. 5 Bright discharger spark below weak secondary at the Toronto apparatus

adjusting the Ruhmkorff coil, fine-tuning the width of the primary spark-gap, and training our eyes in dark for an extended period. None of this provided sufficient improvement, and we remained unable to observe sparks beyond 2 m. In the end we decided that the only way to improve the situation would be substantially to increase the strength of the Ruhmkorff coil.[11] Unfortunately no stronger coil was available to us, and so to continue observing we decided in January 2016 to insert a small neon bulb (type NE-2) into the resonator's spark-gap since its luminous intensity would be a reasonable indicator of the *emf* across it.[12] This worked well, for the neon bulb lit even at distances past 10 m from the discharger. We could as a result use the bulb's visual intensity to produce sign sequences using Hertz's designators for stronger, weaker or no change on rotation of the resonator.

With a circular resonator 65 cm in diameter we observed consistent weakening of the signal as the loop size was increased and decreased in equal increments, with no observable response in the 50 cm or 80 cm loops, providing clear evidence of resonance. In his 1886 experiments on inductive coupling Hertz had found resonance for a loop 70 cm in diameter (Hertz 1893, 42–46). His loop was accordingly responding principally to a discharge oscillation of about 4.4 m wavelength, while ours corresponded to about 4.08 m.

In order to observe the resonator's oscillations as directly as possible, we borrowed a 100 MHz Tektronix TDS2012 oscilloscope from the Department of Electrical and Computer Engineering, University of Toronto, in April 2017.[13] We found, as expected, that the distance between the discharger and the resonator affected the amplitude of the damped waveform, but not its shape. This allowed us to provide a rough estimate of the central frequency. Upon an examination of the oscilloscopic display at two different time scales we determined the temporal separation between adjacent peaks in the waveforms to be roughly 15 ns, providing a frequency of 66.67 MHz for our discharger's central oscillation (Fig. 6).

From April through June 2016 we prepared to examine air-wire interference. To that end we installed a 40 cm square brass plate behind one of the discharger's plates.

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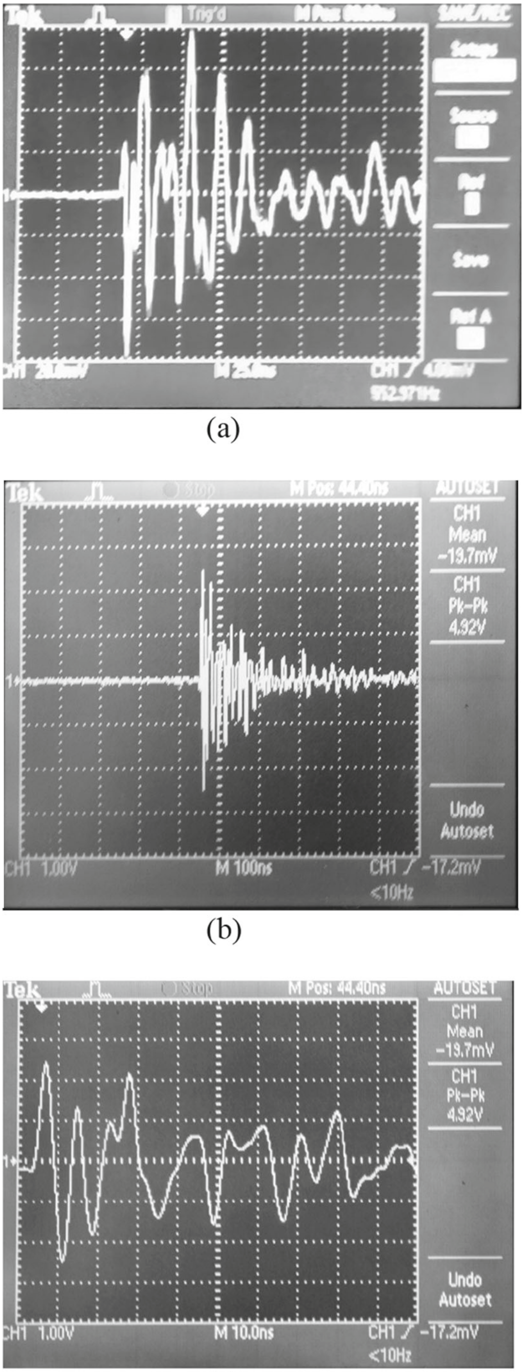
[11] Hertz's coil was 52 cm long and 20 cm in diameter, while ours was only 38.1 cm by 22.86 cm. If the number of winds per unit length were similar, then ours would have been much less effective than Hertz's.

[12] This was done at Buchwald's suggestion, who, with Naum Kipnis, had essayed a qualitative version of the air-wave experiment on June 3–4, 1993 in Minnesota (Buchwald 1994, 286–288).

[13] We were enabled to do so through the assistance of Bruno Korst and Professors Sean Hum and Willy Wong at Toronto.

Fig. 6 Waveforms of the received spark-gap radiation on the loop resonator. The panel **a** was taken on May 12, 2017. The panels **b** and **c** were the same waveform taken on May 31, 2019, but displayed at different time scales



(a)

(b)

(c)

We then attached a long copper wire 1 mm in diameter to the back of the rear plate, passed it directly above the discharger's spark-gap (which now contained our neon bulb) and extended it in parallel to the baseline through the room. When the plane of the resonator in the absence of the wire was parallel to the discharger the bulb lit brightly at all distances but dimmed out when the resonator plane turned away. With the wire installed, the bulb lit nicely when the resonator plane faced it as well. We could therefore be confident that the bulb would respond to both discharger and wire at intermediate orientations as required by Hertz's arrangement.

To measure the length of the waves in the wire, on May 13 we cut it at a distance of 5.5 m from the discharger to produce standing waves and placed the resonator vertically below it at a fixed distance so that its plane contained the wire but was perpendicular to the discharger. Although the bulb lit differently as the resonator was moved along the wire we found it difficult to be precise. We accordingly developed a different procedure.

Instead of always fixing the resonator at a given distance below the wire, we first placed it near the wire and then moved it down until the bulb's light was barely recognizable. We took that distance as a reasonable measure of the standing wave's intensity in its vicinity. Hertz did not specify just how he had made his measurements, beyond writing that he attached pieces of paper where he estimated the nodes to occur. Since we had a reasonably-quantitative measure at a sequence of points along the wire, it was possible to map the standing wave in two series of measurements from 100 cm to 450 cm distance, yielding Fig. 7. The standing wave accordingly had a length of about 5 m, which conforms reasonably well with Hertz's own result of 5.6 m given the difficulties in observation. We and Hertz were accordingly able to measure the length of the wire wave with reasonable accuracy by marking where the spark (Hertz) or bulb light (us) effectively vanished or nearly so.

Having stabilized the apparatus and succeeded in measuring standing waves, we turned to Hertz's air-wire experiments in October, 2016. The confines and paraphernalia of the basement were factors to be avoided, and so we moved to the foyer on the second floor of the university's Old Victoria College, a Victorian-style academic building (Fig. 8). The foyer provided a spacious hallway about 15 m long, 6 m wide, and 3 m high. Unlike Hertz's laboratory, which was bounded above, below, and on the sides by stone (as well as by metal pillars along the walls), this space had a wooden floor and concrete walls and ceiling. We continued our experiments at various times in this location until May 2019.

The length of the foyer enabled us to extend the wire through 14 m and from there to an electrical receptacle's ground. Hertz had continued his wire for another 60 m outside the lecture hall and then earthed it. To measure the air-wire interference we followed essentially Hertz's procedure. We placed our square resonator vertically with its neon bulb at the top. Starting at the discharger, we moved the resonator along the wire every half meter up to 12 m distance. At each position we estimated the bulb's comparative strengths when the resonator plane faced plate P (in Hertz's diagram, Fig. 2) and then when it faced A' at orientations of 45° and − 45° with respect to the wire. We could recognize reasonably well with our naked eyes whether the neon light became stronger

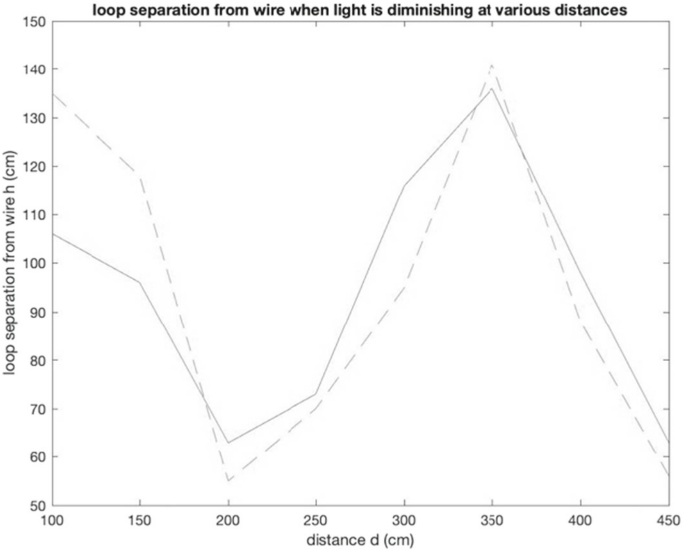


Fig. 7 The y-axis represents the vertical distance between the resonator spark gap and the wire, while the x-axis represents the resonator's distance from the discharger along the wire. The curves represent data from two distinct sets of measurements

Fig. 8 The foyer at Victoria College

('+'), weaker ('–'), or didn't change ('0') on rotating the resonator.[14] We performed a series of such air-wire interference experiments from June 2017 through December 2018. Table 2 provides samples of data taken on distinct dates during this period

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[14] One obvious difference distinguishes our experiment from Hertz's. He had to judge the point at which the resonator's sparking stopped on expanding the micrometric gap. This was certainly not very precise given sparking vagaries, but it did at least obviate having to judge the differences visually between stronger, weaker, and no change. Visual perception however varies logarithmically with optical intensity, whereas the latter is directly proportional to spark strength, and we did judge according to the visual strength of the bulb's light. Nevertheless the bulb responded quite sensitively to the current across it, and so it was not hard to perceive such qualitative changes in its luminosity despite the eye's logarithmic responsiveness.

Table 2 Selected data from our replication of Hertz's air-wire interference experiment in Toronto

(a) November 4, 2017

		1		2		3		4		5		6		7		8		9		10		11		12
100	0	0	0	0	+	+	0	0	0	0	0	0	+	0	0	0	0	0	0	0	0	0	0	0
250	0	0	0	+	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
400	0	–	0	0	0	0	–	–	–	0	0	+	0	–	+	+	0	0	+	+	–	+		

(b) June 13, 2018

	1		2		3		4		5		6		7		8		9		10		11		12
0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
100	0	0	0	0	0	+	+	+	+	+	+	+	+	+	+	+	+	+	0	0	+	0	+
200	0	0	–	–	+	+	+	+	+	+	+	+	+	+	+	–	0	0	+	+	+		
300	0	0	+	+	+	+	+	+	+	+	+	+	+	+	+	+	0	0	0	0	0		
400	0	0	+	+	+	+	+	+	+	+	0	–	–	–	–	–	–	–	–	–	0		
500	0	0	0	0	0	0	0	0	0	0	–	–	–	–	–	–	–	–	–	–	–	0	
600	0	0	0	0	0	0	0	0	0	0	–	–	–	–	–	0	0	0	–				

(c) December 7, 2018

		1		2		3		4		5		6		7		8		9		10		11		12
100	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
250	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
400	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

for comparison with Table 3, Hertz's observations over the same distances using a vertically-oriented resonator.

The difference between our results and Hertz's is quite obvious: where he detected considerable variation in the resonator intensity on rotation at different positions up to 12 m, we found comparatively little change throughout and certainly nothing like the alterations in Hertz's table. Unlike our results, past a distance of about 4 m or so Hertz's shows a clear retardation of the point at which the rotation has no effect (his '0') as the wire back length is increased. As we saw above (note 8), that's just what should take place if the air wave travels more rapidly than the wire wave.

To be certain of our results, in May, 2018 we journeyed to Florence to collaborate with Dr. Paolo Brenni and his assistant Anna Giatti. Brenni heads the physics section at the Fondazione Scienza e Tecnica, a history of science museum with a rich collection of nineteenth-century physics instruments, including functional nineteenth-century Ruhmkorff coils. One of the principal difficulties we had had in Toronto was our inability usefully to employ the resonator's spark to judge its intensity, hence our substitution of a neon bulb. Prior to our arrival in Florence, Brenni constructed a spark-gap discharger and two loop resonators that conformed to the specification in Hertz's papers, as had ours in Toronto. To hold the discharger, Brenni fabricated a wooden frame and stand (Fig. 9).

Table 3 Hertz's results with a vertical resonator (Hertz 1893, 118)

	0		1		2		3		4		5		6		7		8	9	10	11	12
100	+	+	0	–	–	–	–	–	0	0	0	0	0	+	+	+	+	+	+	+	0
150	+	0	–	–	–	–	0	0	0	0	0	+	+	+	+	+	0				
200	0	–	–	–	–	–	0	+	+	+	+	+	0	0	0	0	0				
250	0	–	–	–	–	0	0	+	+	+	+	0	0	0	0	0	0	0	–	–	–
300	–	–	–	0	+	+	+	+	+	+	0	0	0	0	–	–	–				
350	–	–	0	+	+	+	+	+	+	0	0	0	–	–	–	–	–				
400	–	–	0	+	+	+	+	0	0	0	0	–	–	–	–	–	–	–	0	0	0
450	–	0	+	+	+	+	+	0	0	0	–	–	–	–	–	–	0				
500	–	0	+	+	+	+	0	–	–	–	–	–	0	0	0	0	+				
550	0	+	+	+	+	0	0	–	–	–	–	–	0	0	0	0	+				
600	+	+	+	+	0	0	–	–	–	–	–	0	0	+	+	+	+				

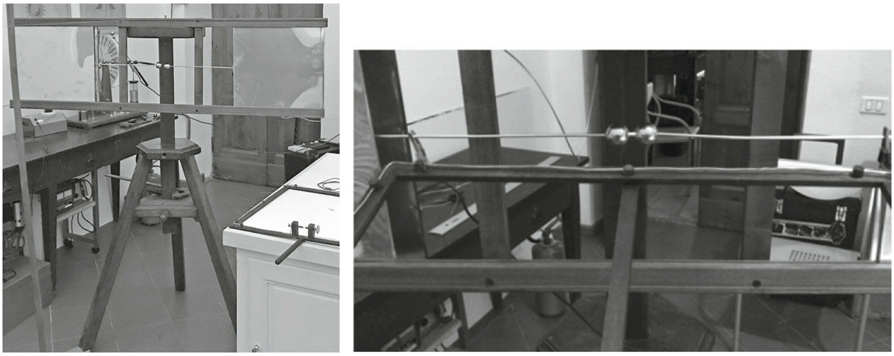

Fig. 9 Brenni's discharger (left) and the spark produced by it in his square resonator up to 2.5 m distance

Brenni obtained an antique Ruhmkorff coil from the museum collection 30 cm in length and 16 cm in diameter that produces sparks as long as 11 cm. This coil was comparable to the one we had used in Toronto and so was still considerably smaller than Hertz's. Nevertheless, we were now able to produce quite visible secondary sparks across the gap of a loop resonator. The secondary sparks were easily seen in a lit room when the distance between the discharger and the resonator was within 2.5 m. However, past that they could no longer be seen even in the dark. We tried a coil 33 cm long and 29 cm, greater even than Hertz's 20 cm diameter but still much shorter than his 52 cm in length. Still, we remained unable to see sparking past about 2.5 m. We therefore resorted to the neon-bulb alternative that we had deployed in Toronto. Though Hertz's coil must certainly have been considerably more powerful than the ones we had in Toronto and in Florence, nevertheless even he had considerable difficulty observing in a completely dark room.

We repeated the standing-wave measurements, obtaining a mean wire wavelength of about 5 m, as we had in Toronto. To observe the wire-air wave interference we proceeded in essentially the same way, the only difference being that in Florence we

Table 4 Typical wire-air interference results in Florence. Empty cells indicate the bulb did not light

		1		2		3		4		5		6		7
100	0	0	+	+	+	+	+	+						
150	0	0	+	+	+	+	+	+						
200	0	0	+	+	+	+	+	+	+					
250	0	0	+	+	+	+	+	+						
300	0	0	0	0	0	–	–	–	–	–	–	–	–	–
350	0	0	0	–	–	–	–	–	–	–	–	–	–	–
400	0	0	0	–	–	–	–	–	–	–	–	–	–	–
450	0	0	–	–	–	–	–	–	–	–	–	–	–	–
500	0	–	–	–	–	–	–	–	0	0	0	0	0	
550	0	0	0	–	–	–	–	–	–	–	0	–	–	0
600	0	0	0	0	0	+	+	+	+	+	+	+	+	

could not go past a wire length of 8 m. None of our Florence trials exhibited anything like Hertz's set of sign changes at a given back length. Neither did our data provide any consistent indication of a shift toward the origin as the back length increased. (Table 4).

Our results in both Toronto and Florence, then, show nothing like Hertz's changes, nor indeed any truly reliable change at all. That, after all, is not in principle surprising, since waves on such wires and waves in open air should indeed travel at the same speeds because wires actually guide waves travelling through the surrounding medium, with the waves slipping as it were along the wire's surface. It's worthwhile developing a model for the air-wire experiments on the assumption that each element of both the wire and the discharger radiates with appropriate phases to see what Hertz should have observed had the conditions obtaining in his experiment conformed to the model we'll now consider.

3 A simple model for Hertz's air-wire experiment

Hertz's discharger was quite strongly damped by radiation, while he had tuned his resonator by adjusting it to respond as strongly as possible. The behavior of the two devices is certainly not simple. Indeed, questions concerning the resonator's response in particular were discussed for some time after Hertz's work by, among others, J. J. Thomson and Henri Poincaré.[15] Nevertheless, for simplicity we'll model the Hertz discharger as a dipole antenna along which we'll consider the current to be uniformly distributed, while the wire has at each point a current magnitude determined by the phase that reaches it at a given time.[16] For simplicity we consider the dipole's elements

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[15] For a careful analysis of the behavior of Hertz's resonator and discharger see Smith (2016). For the questions posed by the resonator as perceived at the time, see e.g. Poincaré (1894).

[16] We accordingly ignore propagation along the dipole proper. Strictly, the burst at the dipole spark gap should be modeled as a damped wave using a Heaviside step-function for a given time and then the

all to have the same phase at a given time and then retard their respective distances to the field point. The wire elements have phases that depend upon the sum of the wire back length and their position along the wire, and we then retard the resulting effect to the field point.[17]

In general a radiating element bearing a unit current produces an **E** field in phasor representation at a distance r and with wave number k as:

$$\mathbf{A} = \mu_0 \frac{\mathrm{e}^{-ikr}}{r}$$

$$\mathbf{E} = (\nabla(\nabla \cdot \mathbf{A}) + k^2\mathbf{A})/i\omega\mu_0\varepsilon_0$$

Set the discharger along the y-axis with the wire stretched out along the *x-axis* above the discharger's center at a given height and allow a backlength (bl) to be added to delay the wire's phase beyond that due to an element's position (x) along it, thereby adding a factor $\mathrm{e}^{-ik(bl+x)}$ to the expression for the field associated with the wire wave. Set the leg containing the resonator's spark-gap in the xy plane at a given height above the discharger at some point along the wire. First rotate the resonator to form a 45° angle with the wire, measure the sparking at this orientation, and then rotate it to − 45°. With wire backlengths of, respectively, 0 cm, 200 cm, and 400 cm the values of the field at the spark gap in the two orientations of the resonator conform to the following curves plotted logarithmically for unit discharger current together with the direct differences in the intensities (Fig. 10).[18]

Close to the discharger with backlengths of 400 cm and 600 cm one change in the interference type would occur on this model using a Hertzian vertical resonator. At 600 cm backlength the change takes place at about 1.6 m from the discharger (Fig. 11). As expected, however, no subsequent alteration in the type of interference occurs thereafter: the sparking change on rotation of the resonator as it shifts along the wire remains unchanged in direction though naturally variable in magnitude.

Whether the intensity curve at + 45° lies above or below the one at − 45° depends upon the phase difference between the discharger and wire currents at their sources. Our example here sets that difference to zero. If it were instead 180° then the curves reverse their order. Consequently averaging over all such phase possibilities would eliminate the dip in the region closer to the discharger. The fact that Hertz did observe such an effect indicates that, to the extent the model captures this region, the discharger and wire currents do not differ in phase at their sources. Our model of course only approximates

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Footnote 16 continued
phase at a given point along the extended dipole calculated via retardation. Moreover, the large plates of the discharger are hardly point loci. In what follows we have nevertheless reduced the plates to points. And since the discharger is sufficiently short in comparison with most of Hertz's observation loci we can reasonably assume the phase at each of its loci at a given time to be effectively the same.

[17] In our model we consider only the **E** field at a point (i.e. the resonator's spark-gap or a point directly opposite it) and do not integrate the field around the loop directly or, equivalently, by computing the changing magnetic induction through it. Calculation indicates no significant observable differences between two such methods given the dimensions of Hertz's apparatus.

[18] We have used Hertz's parameters to the extent that he provided them: discharger length of 80 cm, total wire length of 300 cm, wire height of 30 cm, spark-gap height (estimated) of 25 cm, and wavelength of 5.6 m.

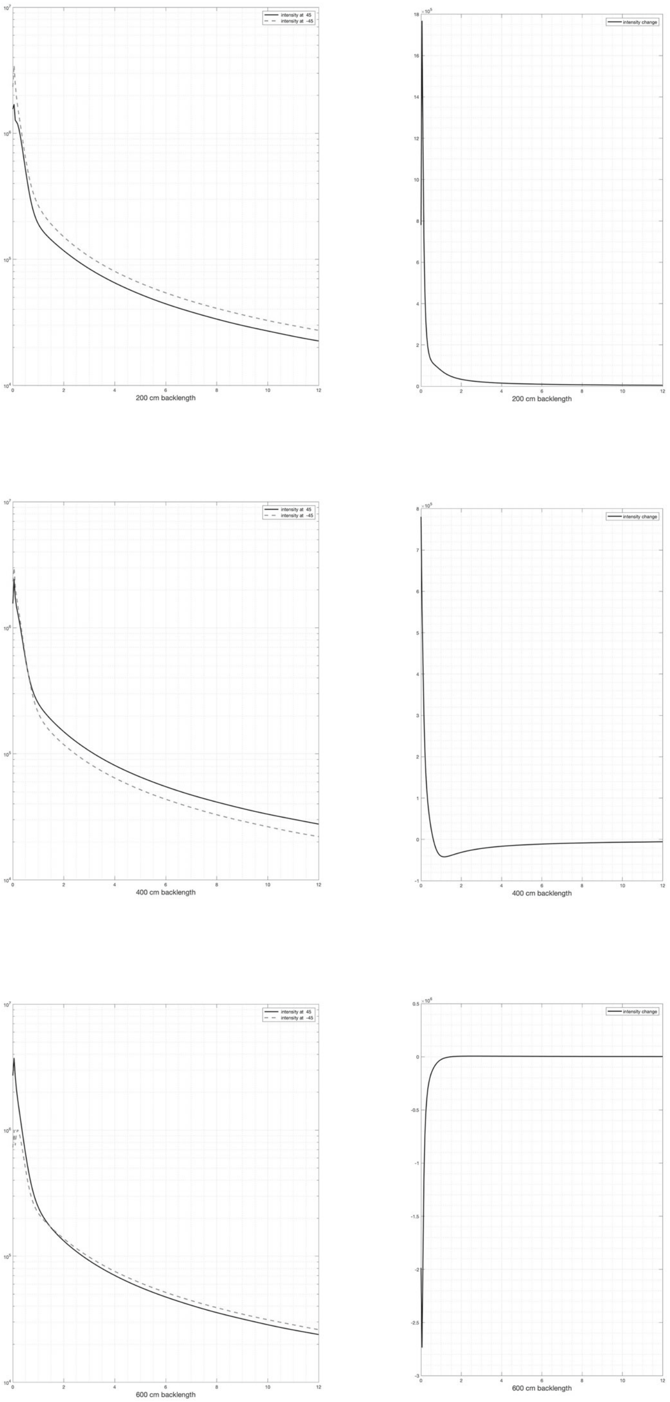

Fig. 10 Curves at 200 cm, 400 cm, 600 cm of wire backlengths. The left panels represent the calculated electric field intensity when the resonator orientation is at 45° (solid curve) and − 45° (dashed curve). The right panels represent the difference between the two intensities

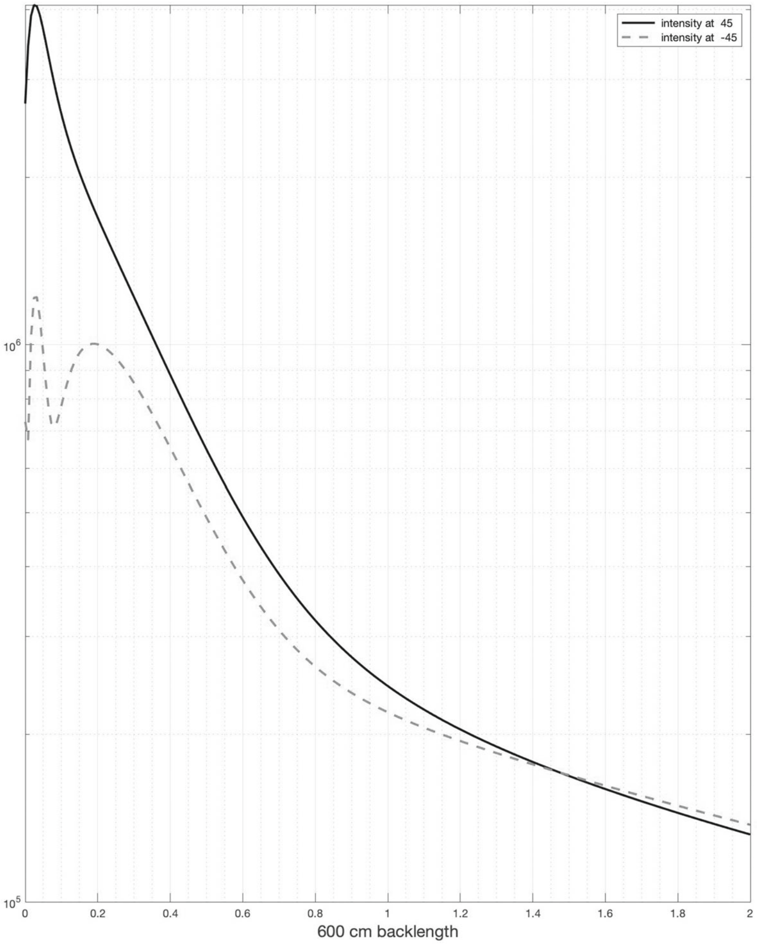

Fig. 11 Intensity curves up to 2 m with a 600 cm backlength

the behavior of Hertz's apparatus since, among other factors, we reduced the large rectangular plates that terminated his discharger to points. This downgrades the near-field influence that even in our simplified model produces a dip in the intensity-change curve and a reversal of its type close to the discharger. A more realistic approximation would extend such a region somewhat further, to, e.g., the 2 m or 3 m distance that Hertz himself observed with a 600 cm backlength.

The dip effect applies specifically to Hertz's vertical-resonator experiments which alter the *angle* of the resonator plane to the wire and discharger, thereby affecting both of their projected actions. In his horizontal-resonator experiments, on the other hand, the plane of the resonator remains fixed in position while the wire is flipped from one side of it to the other. That alters only the action of the wire field, leaving the action of the discharger field unchanged, thereby avoiding the alteration in the latter's vicinity that would be observed with a vertical resonator. And, indeed, it was precisely the *observational absence* of any change in the wire-flip experiments up to 4 m from the discharger that first convinced Hertz that he was on the track to finding that the air wave had a finite speed. These experiments were however insufficiently sensitive to decide whether no change persisted at further distances, which is why Hertz proceeded with a vertical resonator to 12 m. Doing so gave him evidence for a change after about 7.5 m, as we have seen, and hence for a speed difference between the wire and air waves.

Our model accordingly provides a reasonable basis for understanding what Hertz observed closer to the discharger with either a vertical or a horizontal resonator, but it certainly does not capture his observations further along. After all, Hertz found at every one of his chosen backlengths that the interference changed type past 4 m or so and that the pattern shifted *toward* the discharger as the backlength increased, indicating that the air wave travelled faster than the wire wave.

What then might account for Hertz's observation of an effect that, it would seem, should not exist at all in what has ever since been known as the first experimental detection of the finite velocity of electromagnetic radiation?

4 Hertz's experimental space

The city of Karlsruhe where Hertz performed his discovery and reflection experiments was a significant industrial center and so became a preferred target of Allied bombing during World War II. Most university buildings were reduced to rubble, including the one in which Hertz had worked. Only the building's ground floor and remnants of its walls remained. Ironically, the Nazi regime in the 1930s had removed any encomia to Hertz since he had Jewish ancestry. After reconstruction, a plaque recalling Hertz's breakthrough was placed next to the new physics building, memorializing an experimental discovery that, in its full dimensions and over time, proved to be among the most influential of the 19th century (Fig. 12).

Although we cannot return to Hertz's laboratory, the dimensions of its space suggest—as he intuited several years after the discovery—that the very walls, ceiling and floor in which he worked had something to do with the results that he had obtained in his air-wire experiments. Hertz described the physics lecture-room in which his investigations took place as "15 m long, 14 m broad, and 6 m high." Two rows of iron pillars ran parallel to the 15 m walls. Though he did not specify the pillars' separations, Hertz thought them close enough to one another "that the parts of the room which lie outside these cannot be taken into consideration" since each of the pillar rows "behaves much like a solid wall toward the electromagnetic action." In which case the 15 m sides of the space were in effect bounded by conducting walls forming a working region, Hertz wrote, 8.5 m wide. The ceiling was festooned with "hanging parts" of gas pipes as well as associated chandeliers that Hertz removed in conducting his reflection experiments. Some of these pipes no doubt remained, so we can reduce the room height a bit, say to 5.6 m. Moreover, the metal pipes that carried the illuminating gas still coursed just above the ceiling, so that it's reasonable to consider the ceiling also to form a conducting barrier, and Hertz likely had not removed the "hanging parts" of the pipes in his air-wire discovery investigations since there would at that stage have

Fig. 12 The memorial to Heinrich Hertz at the University of Karlsruhe

been no reason to do so. The front wall of the room "was a massive sandstone wall in which were two doorways, and a good many gas-pipes extended into it"(Hertz 1893, 125). Accordingly, that wall might also have behaved like a conducting barrier. Hertz did not specify the structure of the back wall, but it's unlikely to have been different from the front one, at least in respect to the presence of metal pipes. Neither did he specify the material of the room's floor. This was the only part of the original building left undestroyed, though it was subsequently removed for new construction. Photos taken after the war show thick concrete block construction. Such a floor has a fairly high coefficient of reflectivity (Holloway et al. 1995). In contrast, both in Toronto and Florence our spaces were nearly devoid of disturbing metal barriers, were much smaller in length and width than his, and did not have thick concrete flooring.

Consequently Hertz's experimental space may well have constituted a wave guide with dimensions only two or so times the 5.6 m length that he had measured for the wire's standing waves. We can model such a space to see what effect guidance might have had on these first measurements. Such a space will accommodate a discrete number of propagatory modes. With reference to Fig. 13, we set the lecture hall's length along x, its width along y (a), its height along z (b) and calculated the modes and their effects as follows.

The **E**, **H** fields under these circumstances have the free-space wavelength λ reduced for each mode to yield

$$\mathbf{E} = \mathbf{E}_0(y, z)e^{i2\pi x/\lambda_{\text{eff}}}$$

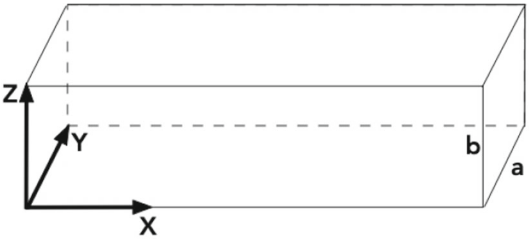

Fig. 13 Hertz's lecture hall rendered as a wave guide with breadth **a** of 8.5 m and effective height **b** of 5.6 m. In the air-wire interference experiments the discharge was placed centrally along the *y* axis 1.5 m above the floor

$$\mathbf{H} = \mathbf{H}_0(y, z)\mathrm{e}^{i2\pi x/\lambda_{\mathrm{eff}}}$$

where

$$\lambda_{\mathrm{eff}} = 1/\sqrt{\left(\frac{1}{\lambda}\right)^2 - \left(\frac{m}{2a}\right)^2 - \left(\frac{n}{2b}\right)^2}$$

The mode integers m, n must be small enough to ensure that the corresponding propagation does not simply evanesce, so we require $\left(\frac{1}{\lambda}\right)^2 - \left(\frac{m}{2a}\right)^2 - \left(\frac{n}{2b}\right)^2 > 0$. In general two types of propagation can occur. In one type, TM (transverse magnetic), the electric field has a component along the direction of propagation (here x), while in the other type, TE (transverse electric), it does not. In Hertz's air-wire experiment the discharger was horizontal (here along y), in which case it's reasonable for our purposes to assume that TE guidance predominated. To the positions along y, z we have respectively added 4.25 m, 1.5 m given the location of Hertz's discharger. The modes accordingly have E_y, E_z fields as follows (Marcuvitz 1986, 57, 60):

$$E_y = -\frac{V_i''\sqrt{\varepsilon_m \varepsilon_n}}{b}\frac{n}{\sqrt{m^2\frac{b}{a} + n^2\frac{a}{b}}}\cos\left(\frac{m\pi}{a}(y + 4.5)\right)\sin\left(\frac{n\pi}{b}(z + 1.5)\right)$$

$$E_z = -\frac{V_i''\sqrt{\varepsilon_m \varepsilon_n}}{a}\frac{m}{\sqrt{m^2\frac{b}{a} + n^2\frac{a}{b}}}\sin\left(\frac{m\pi}{a}(y + 4.5)\right)\cos\left(\frac{n\pi}{b}(z + 1.5)\right)$$

Here V_i'' is the effective voltage of the equivalent transmission line circuit corresponding to the TE mode, while $\varepsilon_m = 1$ if $m = 0$ and 2 otherwise. Since Hertz's discharger was horizontal in his air-wire experiments, only E_y enters here. We will need E_z when we consider his reflection experiments.

The free-space wavelength in Hertz's experiment would surely have been the one that he measured for the wire's standing wave—that much is unproblematic—and so about 5.6 m, corresponding to a frequency of about 53.6 MHz. We had measured 5 m, for a frequency of 60 MHz, which is well within the variances of the experimental parameters. Hertz was quite a careful experimenter, so we chose his value for the wavelength to see what the effect of guidance might have been. Hertz's wire lay fairly accurately along the x axis in our representation, so that both y, z are approximately

Table 5 The waveguide modes compatible with Hertz's experimental space for air-wire interference

m	n	λ_{eff}	E_y
0	1	6.47	0.22
1	0	5.93	0
1	1	6.99	0
2	0	7.44	0
2	1	9.96	– 0.13
3	0	36.6	0

zero. Calculating the resulting effective wavelengths and fields E_y for modes that do not produce simple evanescence yields the following results (Table 5).

Six modes are excited by Hertz's discharger, but only two among them have detectable amplitudes. These two correspond to effective wavelengths of, respectively, about 6.47 and 9.96 m. To find out how these two modes would have affected Hertz's air-wire measurement, we need to project them *together with the wire field* onto his resonator. Since Hertz placed his resonator very close to the wire itself, any reflections from the surrounds to the wire fields proper would have been negligible in effect compared to the fields received directly from the wire and from the discharger. Taking into account the wire wave, the time-field at the resonator due to it ($\mathbf{E}^{\text{wire}}$) and two air waves ($\mathbf{E}_1^{\text{air}}, \mathbf{E}_2^{\text{air}}$) with respective effective wavelengths $\lambda,\ \lambda_{\text{eff}1},\ \lambda_{\text{eff}2}$ would be

$$\mathbf{E}_{\text{resultant}} = \left(\mathbf{E}^{\text{wire}} e^{i(2\pi/\lambda)x} + \mathbf{E}_1^{\text{air}} e^{i(2\pi/\lambda_{\text{eff}1})x} + \mathbf{E}_2^{\text{air}} e^{i(2\pi/\lambda_{\text{eff}2})x}\right) e^{-i\varpi t}$$

The resultant signal intensity will be $|\mathbf{E}_{\text{resultant}}|^2$, producing three spatially-periodic terms in addition to the constant biases as follows:

$$\begin{aligned}|\mathbf{E}|^2 = {} & \left|\mathbf{E}^{\text{wire}}\right|^2 + \left|\mathbf{E}_1^{\text{air}}\right|^2 + \left|\mathbf{E}_2^{\text{air}}\right|^2 + 2\mathbf{E}^{\text{wire}} \cdot \mathbf{E}_1^{\text{air}} \cos(2\pi\lambda_{\text{I}}x) \\ & + 2\mathbf{E}^{\text{wire}} \cdot \mathbf{E}_2^{\text{air}} \cos(2\pi\lambda_{\text{II}}x) + 2\mathbf{E}_1^{\text{air}} \cdot \mathbf{E}_2^{\text{air}} \cos(2\pi\lambda_{\text{III}}x)\end{aligned}$$

where

$$\lambda_{\text{I}} = 1/\left(\frac{1}{\lambda} - \frac{1}{\lambda_{\text{eff}1}}\right) \quad \lambda_{\text{II}} = 1/\left(\frac{1}{\lambda} - \frac{1}{\lambda_{\text{eff}2}}\right) \quad \lambda_{\text{III}} = 1/\left(\frac{1}{\lambda_{\text{eff}1}} - \frac{1}{\lambda_{\text{eff}2}}\right)$$

and so here

$$\lambda_{\text{I}} = 41.79\,\text{m} \quad \lambda_{\text{II}} = 12.79\,\text{m} \quad \lambda_{\text{III}} = 18.43\,\text{m}$$

Two of the three components involve intermodulation between the air and wire waves, while the third represents the effect of guidance on the air wave proper—it would be present in the wire's absence. These three periodic components are not equally significant in the context of Hertz's experiment. The first (λ_{I}) involves a fairly long spatial period, taking about 20.8 m to change from a positive to a negative contribution in the sorts of measurements that Hertz undertook by moving the resonator along the wire. Since he was looking for reversals in the effect but did not go beyond 12 m, this component would not have affected his results. The third contribution (λ_{III}) produces a sign change after about 9.2 m, but this component involves intermodulation between the air wave's two modes, whereas the other two involve intermodulation between the air and wire waves. Hertz wrote that he had endeavored to maintain the same intensity between the air and wire effects by adjusting the spatial separation between the driving and receiving plates of his apparatus. In our reproductions we found this difficult to produce, for the wire wave always remained notably stronger than the air wave, and we think this likely to have been the case in Hertz's experiment. Consequently we do not think that the third component, which involves just the air wave modes, would have been comparatively efficacious in Hertz's observations.

These considerations leave us with λ_{II} as the only component that likely affected Hertz's observations under our guidance model. It produces a periodic sign change about every 6.4 m. Hertz reported that he had found such a change about every 7–7.5 m, resulting in the 9 m length for the air wave, as we have seen. Given the vagaries of the experiment, which depended on observations of changing spark strengths (or, in our case, of neon bulb intensities), we think it reasonable to conclude that Hertz did indeed observe the effect of intermodulation between the air and wire waves in a space controlled by guidance, at least for distances past about 4 m or so from the discharger. For closer distances, wave guidance would not have been so effective given proximity to both wire and discharger.[19] Indeed, we saw above that, on the basis of our direct-radiation model, a single change in interference type would generally be observable in those regions using Hertz's vertical-resonator method, while no change would be observable using a horizontal resonator. That is just what he had found.

In the introduction to the 1894 collection of his papers, Hertz wrote that in "performing the experiments, I never in the least suspected that they might be altered by the neighbouring walls." He hoped that his air-wire experiment might be repeated to yield, as he now thought "the result which it should at first have given" (Hertz 1893, 9–10). Hertz died at the very beginning of the year his collection was published. Eight years later the Adams Prize Essay at Cambridge was awarded to Hector Munro MacDonald for his *Electric Waves* in which he had worked his way to "a satisfactory explanation of the discrepancies in some of Hertz's own observations" (MacDonald 1902, 8). MacDonald did not fully reach the concepts of TE and TM waves, but he did essay an explanation for Hertz's air-wire experiments based on the effect of floor reflections. He did not provide a calculation beyond a formula based on sequential floor reflections to the resonator's position. These, he argued, would have the effect of producing interference with the wire wave "of the character observed by Hertz." This went little further than Hertz's intuition, but, in the end, both were in essence

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[19] Hertz's experimental space was not a simple wave guide because of the presence within the guide space of his discharger, in effect placing a radiating antenna within the guide proper. We have not attempted to model this situation given the complexity introduced by the presence of Hertz's capacitance-providing plates. However, a configuration involving an antenna placed within a cylindrical guide can be arranged so that "the waveguide field is nulled near the antenna" (Onofrei and Albanese 2017, 93). In this case, the antenna consists of a dipole array arranged around the cylinder's axis, with the dipoles parallel to the axis and occupying a significant cross-section of the guide. Hertz's radiator is much different, but given the large dimensions of his experimental space in comparison with the size of his discharger, it seems feasible that a direct antenna field might dominate near the discharger with guide modes coming into play further out.

correct: Hertz's discovery that air waves propagated faster than wire waves was due to the effects of what came to be called wave guidance.[20]

This was hardly the last of Hertz's experiments that gave such a large wavelength to the air wave for apparatus that must have produced a much smaller one, for Hertz obtained nearly the same value in his reflection experiments. That result, we shall now see, cannot be explained by wave guidance. Something else must have been involved.

5 Reflecting waves in Karlsruhe, Toronto, and Florence

Hertz's air-wire experiments had indicated that the resonator loop sparked more strongly when it was close to a wall (Hertz 1893, 124). If the air wave did propagate, he early reasoned, then such an enhancement might be due to reflection. This led to a new experimental design that he pursued in February and March of 1888. Once again he turned to interference, this time between the air wave direct from the discharger and from one reflected by an appropriate barrier. We do not unfortunately have Hertz's laboratory notes for the reflection experiments and must consequently rely on his published account. That account was carefully constructed to form part of a trilogy printed in the *Annalen der Physik*, and there is clear evidence that Hertz built all three to present a convincing case in ways that do not directly represent the actual sequence of his investigations.

The air-wire paper as well as the reflection paper were first published in the Berlin Academy's *Sitzungsberichte* on November 10, 1887 and February 2, 1888 respectively. New versions of these two papers were published in the *Annalen* on April 15 and May 15. The paper describing his experiments on the inductive action of his spark-generated oscillations was not previously published in the *Sitzungsberichte* but appeared only in the *Annalen* for the issue ending on March 15 and had been constructed after, not before, the other two. The versions of these two were nevertheless modified from their first publication to refer to this third paper. This was good pedagogy on Hertz's part—he had, after all, to persuade his readers that he had found something new—but it does obscure just what was written when. And, by implication, makes it somewhat problematic to rely on the published papers, whether in the *Sitzungsberichte* or the *Annalen*, to tease out Hertz's working sequence. This is the more true given the evidence of his notebook for the air-wire experiments, which indubitably proffer a less linear sequence than his published account.[21]

The reflection experiments were designed to illustrate and to measure the standing wave that would be produced by placing a large conducting sheet of zinc 4 m high by 2 m wide at the far wall from the discharger with the resonator at various locations in between. Hertz had of course previously measured the length of a standing wave: namely, the length of the wave along the wire in his earlier experiments. To measure it, Hertz had oriented the resonator plane to contain the wire and had kept the spark-gap

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[20] Ironically, wave guidance and related processes were of central interest to British Maxwellians like J.J. Thomson and George Fitzgerald and accounts in part for their conviction that artificially-produced electromagnetic radiation would likely forever escape detection (Buchwald 1994, 333–339).

[21] The publication sequence and its implications in connection with the laboratory notebook were uncovered in Doncel (1991), which provides careful details.

fixed in position to face the wire. He had then moved the resonator along to observe the sparking at different locations. Hertz did perform similar observations in his air-wave reflection experiments, but he did not use them to generate measurements. Instead, Hertz *rotated the gap* around the resonator at each of its positions to observe changes in sparking in order to generate useable data.

To understand what Hertz had in mind here, we must pay careful attention to how he thought sparking was governed in his loop. With the resonator plane parallel to the discharger, Hertz had claimed in his air-wire experiments, "the sparks owe their origin mainly to the electric force which always acts in the part of the secondary circuit opposite to the spark-gap" (Hertz 1893, 109–110). This was due, he remarked in his air-wave reflection paper, to the "unfavorable" situation at the gap itself as compared to the one at the unbroken region opposite to it (Hertz 1893, 129). Reasoning that the spark-gap's ends act analogously to the tied ends of a vibrating string, Hertz had earlier suggested that the "component of the electric force" at a given point around the resonator can be represented as a series of sine and cosine terms that are functions of the angular distance from the spark-gap (Hertz 1893, 87–89). He argued that the sine series cannot effect sparking since they are symmetric about the gap, leaving the cosine series, which he limited to its first term.[22] If the forces near the gap are weaker than those opposite to it, as Hertz assumed (because, in analogy with the vibrating string, the gap ends annul the effect of the force), then only those essentially opposite to the gap can effect an oscillation. Hertz's analogy to the vibrating string accordingly led him to think of such forces as similar to tensions. As such they do not sum to effect an overall *emf* around the resonator. They are instead point actions whose difference across an element determines an oscillation in much the same manner as the force differential in a tensed string.

Turn now to the first series of observations that Hertz described in his published paper on reflection. When the center of the loop is 0.8 m from the wall, the sparking was, Hertz found, "much stronger when the spark-gap is turned toward the wall" than when facing away from it. To calibrate the situation, he adjusted the gap so that no sparks appeared when turned away from the wall. Moving further, at 3.0 m, "a continuous stream of sparks" then arises "when the spark gap is turned away from the wall, whereas the sparks disappear when the spark-gap is turned toward the wall." At 5.5 m "the sparks on the side toward the wall are stronger than the sparks on the opposite side." And finally, at 8 m "the sparking is stronger on the side remote from the wall, but the difference is no longer so noticeable" (Hertz 1893, 127).

These observations correspond to locations *I, II, III, IV* in the graph, Fig. 14, that Hertz drew, to which he added *V, VI, VII* with each of the latter placed halfway between consecutive pairs of the initial four. Two of these (*V* and *VI*) were actually the *only* ones that gave Hertz the 9.6 m wavelength that he assigned to the air wave. They accordingly set the standing wave represented by the dark line in his figure. And then *I, II, III, IV* nicely fit quarter-wave locations for such a wavelength, with the initial node lying at point *A* a distance of .68 cm behind the reflecting wall—a displacement Hertz justified on the grounds that the wall was not perfectly conducting and that his reflector covered only part of it (Hertz 1893, 127–29).

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[22] For details see Buchwald (1994, 251–254).

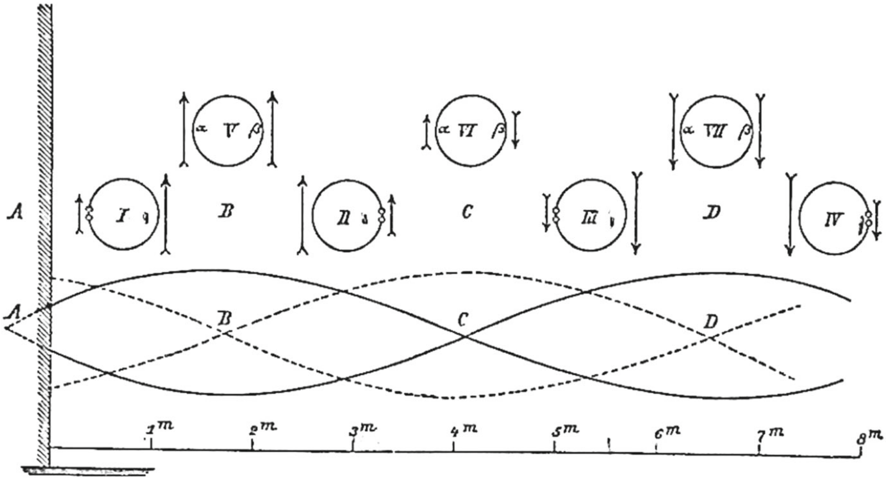

Fig. 14 Hertz's graph for his standing air wave (Hertz 1893, 128)

It's worthwhile essaying a reproduction of Hertz's alternating-gap procedure to see just how accurately one might be able to place points like his first four. Our use of a neon bulb presumably made such observations simpler than it would have been for Hertz, who had to work in a completely dark room with weak sparks. To make a conducting surface for reflection, we used aluminum foil to cover a rectangular area about 3 m high and 2 m wide on the wall at one end of the Toronto college foyer. The discharger was placed 7–12 m away. Hertz had set his discharger vertically for the reflection experiments, but our apparatus did not easily rotate to the vertical, and so we set the resonator plane in the horizontal to contain (like his) the discharger and elevated it to 1.5 m above the ground. We moved the resonator to a given position, set its spark-gap to face the reflecting wall, observed the sparking, then rotated it directly away from the wall and again observed the sparking.

Even with our neon bulb, we found it quite difficult precisely to map alternations in the sparking by observing the intensity change on rotation in measurements where sparks occur at both orientations of the gap but with different apparent strengths. Table 6 provides three different observation series made in Toronto. In it we again follow Hertz in marking a change from stronger to weaker effect when the resonator gap moved from closer to the conducting wall to farther from the wall (in our case, bulb illumination) with a '+', a '–' for weaker to stronger, and a '0' for no visible change. Reversals are certainly visible here, but it would be hard to argue that they form a consistent pattern, much less to attempt deriving any actual measurements from them. At Florence we repeated these measurements with essentially the same variable results as at Toronto. It's simply not possible to find a stable value for a wavelength just by estimating comparative sparking strengths on rotating the resonator at sequential positions.

This is no doubt why Hertz in the end produced his value for the wavelength from measurements in which the spark vanished altogether at one or more orientations of the resonator rather than by such estimations. We'll turn in a moment to those

Table 6 Bulb illumination changes on resonator rotation

Distance	0.5	0.8	1	1.5	2	2.5	3	3.5	4	4.5	5	5.5	6	6.5	7	7.5	8	8.5	9
Hertz		+					–					+					–		
9 m 13/6/2018	–		–	0	+	+	+	+	0	0	0	0							
12 m 12/10/2018	–		–	–	0	+	+	–	–	–	+	+	+	+	0	+	+	0	
12 m 12/10/2018	–		–	0	+	+	+	0	0	0	–	+	+	+	+	+	+	0	0

Table 7 The waveguide modes compatible for Hertz's reflection experiment with an effective ceiling height of 5.6 m and a room width of 8.5 m

M	N	λ_{eff}.	E_z
0	1	5.93	0.2899
0	2	7.44	0
0	3	36.62	– 0.2899
1	0	6.47	0
1	1	6.99	0.02671
1	2	9.96	0

observations, but let's first note that it is simply not possible for Hertz's discharger to have *produced* the wavelength of 9.6 m that his measurements here yielded. Given the dimensions and configuration of Hertz's discharger, close to 5.6 m is what would be measured in a reflection experiment carried out in the open air, as both calculation and direct observation prove (Smith 2016; Simpson 2018). Hertz's experiments were of course carried out in a space within which guidance did apparently have a measurable effect as a result of intermodulation between the air and wire waves. In his reflection experiment any guidance effects cannot arise out of intermodulation because the only difference between the direct and reflected waves is a reversal of phase. Guidance does however generate multiple modes, and we can again examine their particular effects. Since Hertz turned his discharger into a vertical position for the reflection experiments, we consider the effective field to be E_z rather than E_y. Inserting the appropriate values, we obtain the following table of modes (Table 7)

Three modes are now present, corresponding to effective wavelengths of approximately 5.93 m, 36.62 m, and 6.99 m. Note the difference from Hertz's previous air-wire experiments, due to the change in discharger orientation from horizontal to vertical. The modes are not equally efficacious. The 36.62 m mode would require about 18.3 m to produce two nodes or anti-nodes and so falls outside the 8 m range of Hertz's experiment. The 6.99 m mode is extremely weak and so could not have much of a detectable effect. That leaves the 5.93 m mode—and this is very close to the 5.6 m wavelength that Hertz had measured for the wire wave.[23] In other words,

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[23] This is in contrast to the *two* air modes that would have arisen had the discharger been oriented horizontally—but even in such a case intermodulation would not have affected Hertz's reflection measurements because the effective wavelength due to it would have been 18.43 m.

Table 8 Toronto reflection experiments to locate successive sparking minima with gap facing away from the wall and with gap facing the wall. Note how the locations of the sparking minima depend strongly on the orientation of the gap

Resonator center 1 for weakest light	Resonator center 2 for weakest light	Resonator center 1 for weakest light	Resonator center 2 for weakest light	Resonator center 2–Resonator center 1
1.37 m away	3.82 m away			2.45 m
1.18 m away	4.13 m away			3.05 m
		3.4 m facing	5.98 m facing	2.58 m
		3.31 m facing	6.31 m facing	3 m

guidance *implies* that Hertz should have obtained about the same air wavelength in his interference experiment as the one that he had found for the wire wave. Yet he did not.

We however were able to produce a 5–6 m wavelength in our own experiments by abandoning any effort to observe comparative strengths by resonator rotation. We set the resonator's gap either to face the reflecting wall or to face away from it, keeping the gap's orientation *fixed*. In each such series we moved the resonator along until the bulb dimmed out nearly completely or glowed steadily and very brightly. We did not have to consider whether such observations corresponded to a node or to an anti-node but only whether the *same type of clearly visible effect* (no light or very bright and steady) occurred at our measured locations. Intervals between loci of zero light or between loci of bright light had to be spaced at half-wavelengths if we were indeed measuring a standing wave. We obtained the following results (Table 8), in which we sought minima with the gap facing both toward and opposite to the wall. The results for both orientations yield a mean value for the air wave's length of about 5.54 m with a standard deviation of .3 m. This is certainly close to the 5.6 m of Hertz's wire wave, as indeed it should be even including the effect of guidance in the confines of his experimental space.[24]

Hertz himself described a measurement procedure that, like ours, also involved fixing the orientation of the gap, one that he linked to the dotted line in his graph (Fig. 14). That line, he remarked, represents the "stationary wave of magnetic force, which, according to theory, accompanies the [9.6 m] electric wave and is displaced a quarter wave-length relatively to it" (Hertz 1893, 130)[25] Keeping the plane of the resonator vertical, Hertz described what would be measured at locations *B,C,D* on

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[24] Unlike Hertz's, both in Toronto and Florence our spaces were nearly devoid of disturbing metal barriers and were much smaller in length and width than his.

[25] Maxwell's theory drew a sharp distinction between electric and magnetic fields, linking the two through the Ampère and Faraday laws. Helmholtz's, whether in the Maxwell limit of infinite (electric) polarizability or not, does not work with fields but with forces—and throughout his early papers Hertz did not use the term 'field'. In Helmholtz's system both electric and magnetic forces arise from a potential function, electric by time variation, magnetic by space variation. In an electrically and magnetically polarizable substance both the electric and magnetic forces will as a result obey a wave equation, with the two forces differing in phase

his graph if its spark-gap were *kept fixed* at "the highest point" and not placed to face toward or away from the wall: "…near the wall there is vigorous sparking which rapidly diminishes, disappears at B, increases again up to C, then again decreases to a marked minimum at D, after which it continually increases as we approach the primary oscillation" (Hertz 1893, 131). Did Hertz actually carry out such measurements? His wording suggests that he did: e.g., that the dotted-line in his graph represents the magnetic force "can be illustrated experimentally as follows," namely by performing the type of measurements just described. As Hertz understood it, placing the spark-gap at the top (or bottom) of the resonator meant that the electric forces at these two locations have almost no effect since the electric force is there orthogonal to the gap. In which case the estimation of the *emf* is most directly evaluated from the changing magnetic flux through the loop. Nevertheless, Hertz did not use these measurements—with the gap fixed at the "highest point" of the resonator—to generate numbers.

We can model the situation by taking direct account of the current distribution around his resonator. It had not been necessary to do so in modeling Hertz's previous measurements because in them he never altered the orientation of the spark-gap *around* the resonator; it remained fixed in both the vertical and horizontal experiments with either the plane of the resonator rotating or the wire flipping to one or the other side of it. Now however the essence of the measurement required rotating the gap, and that implicates the distribution of current. To model the situation it's reasonable to treat Hertz's resonator as a circular loop receiving antenna. Using the Lorentz reciprocity theorem produces the following expression for the *emf* induced across the terminals of the resonator (i.e. the spark-gap) for unit incident intensity, in which I is the current at a point of the resonator loop, ϕ_0 represents the angle of the gap, zero when it points away from the reflecting wall and toward the discharger, r_o is the loop radius, x is the distance between the loop center and the wall, λ is the wavelength, and ϕ is the angular position of an arbitrary point on the loop[26]:

$$\text{emf} = -i2r_0 \int_0^{2\pi} \mathrm{d}\phi I(\phi - \phi_0)\cos\phi \sin[2\pi(x + r_0\cos\phi)/\lambda]$$

How, next, to specify the distribution of current around the loop? Hertz had himself essayed a speculation, in which he argued that the force tangent to the loop and due to the discharger could be represented as the sum of a sine and cosine series centered on the spark-gap. Noting that in such a case the sine terms, being symmetric, could not produce a net effect, Hertz limited the result to the cosine terms, which, he asserted, have a net effect provided that their actions are greatest in regions opposite to the gap.[27]

Footnote 25 continued
by 90°. Only in the limit of infinite polarizability however do Helmholtz's equations become fully equivalent to Maxwell's. For details see Buchwald (1994, 375–388) and especially Darrigol (2000, 412–419).

[26] The reciprocity theorem allows the interchange of transmitting and receiving antennae in calculating field results. If, namely, a volume distribution $\mathbf{J}_1$ of current produces a field $\mathbf{E}_1$, while a current distribution $\mathbf{J}_2$ produces a field $\mathbf{E}_2$, then the integral of the inner product of $\mathbf{E}_1$ and $\mathbf{J}_2$ over the volume equals that of the inner product of $\mathbf{E}_2$ and $\mathbf{J}_1$.

[27] For details see Buchwald (1994, 251–254).

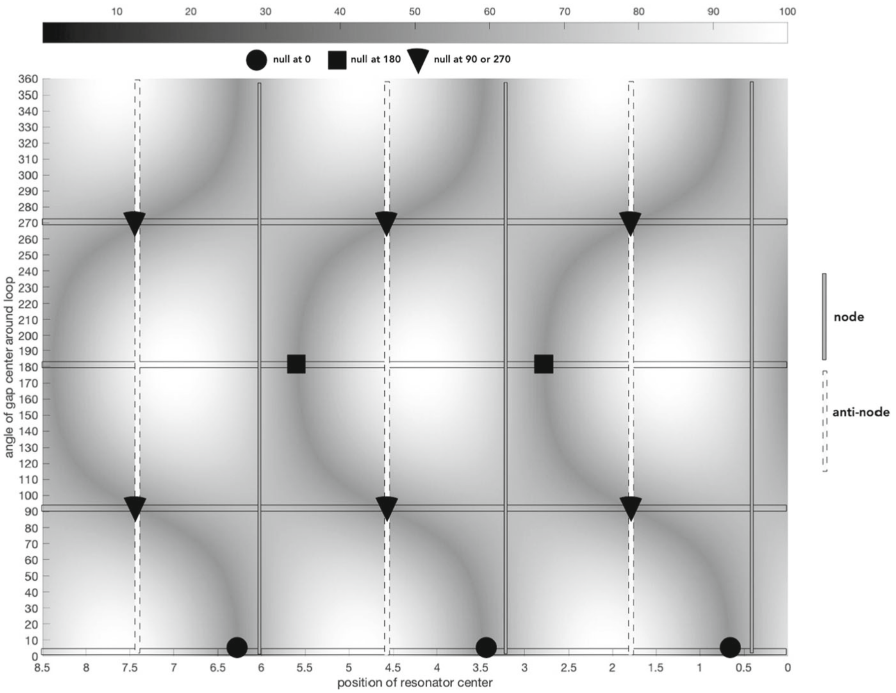

Fig. 15 Model of the emf around Hertz's loop in his reflection experiments, here for a 5.6 m wave

We can represent the current distribution as a Fourier series centered on the spark-gap in such a fashion that the current effectively vanishes at gap termini. Since the gap is very small in Hertz's experiments, we may use a series of sine functions. Limiting the expansion to the first such term, we model the current in a first approximation as $\sin^2(\phi - \phi_0)$.[28] The *emf* around the loop can then be represented by the following surface graph, in which the horizontal axis represents the position of the loop's center while the vertical axis represent the angle of its spark-gap (Fig. 15). The square icons represent loci (S180 hereafter) where the *emf* vanishes when the spark gap faces *toward* the reflecting wall (i.e. at 180°), while the circle icons (S0 hereafter) represent loci where the *emf* vanishes when the gap faces *away from* the wall (i.e. at 0°).

From the surface plot we can identify the salient features that Hertz remarked. Move the loop away from the reflective wall on the right and observe the change in the *emf*

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[28] Hertz himself estimated the circumference of his resonator loop at roughly half the radiation wavelength. Antenna theory indicates that for a half-wavelength loop or dipole, the current is distributed in a form similar to a single-lump standing-wave pattern, in which the current amplitude is minimum at the open-circuit points and maximum at a point opposite to the center between them. Our representation, strictly speaking, does not precisely capture the distribution around the gap since the current in that region does not vanish but is closer to the excitation current introduced to the antenna through its terminals. Nevertheless, our purpose here is not to assess the details of the distribution but rather to estimate the overall effect of the receiving antenna on the incident signal intensity. For that purpose it is important to focus on the region of the loop that makes the most significant contribution, in which case inaccuracies around the gap proper will not substantially affect our claim (which is consistent with Hertz's own, and his was based in part on observation), that the region opposite the gap principally governs the loop *emf*.

at various angles. First, we see that the nodes occur successively at positions where the *emf* does not alter substantially across gap positions. As we move from a node, the *emf* begins to differentiate as the gap rotates, until at a certain point it vanishes when the gap is at S0, thereby marking a null point with the gap facing away from the wall (the first black circle from the right). The *emf* then reaches its greatest magnitude altogether as we move further away, which occurs when the gap faces the wall (S180). At this position of the resonator, which marks an anti-node, the *emf* vanishes when the gap is located at either S90 or S270 (the first two black triangles from the right). Between this position and the preceding S0 null point, the *emf* vanishes once in the lower half of the resonator and once at a symmetric point in its upper half. As we move further, we eventually reach a point where the *emf* now disappears when the gap is at S180 (the first black square from the right). Moving a bit past the S180 null point, we eventually come to a place where the *emf* does not change on gap rotation again. The same pattern repeats itself.

Hertz carefully described these basic observational characteristics. In the vicinity of the wall, a point is reached at which the *emf* vanishes at S0. This distance, he asserted, "can be ascertained within a few centimeters." Moving along, sparking reappears at S0 and "on rotating the circle within itself the sparking becomes zero once in the upper and once in the lower half of the circle." Further on, at a point *B*, these two null points move to "the highest and lowest points of the circle." This location, he continued, "can be determined with fair accuracy." Past it, "these zero-points slide over toward the side of the circle facing the wall [S180], approach each other, and again coincide in a single zero-point at a certain distance from the wall that can be sharply determined." The previous location of the point *B*, he noted, "must lie exactly between this and the analogous point first observed [the initial S0]" Moving on, a point *C* is reached at which "the sparks at all points of the circle tend to become of equal value, and do so become at *C*" (Hertz 1893, 131–132).

Hertz considered point *B* to mark the location of the first anti-node past the wall. Our model shows this to be correct, but Hertz had no such plot. To see why he nevertheless had good reasons to place the anti-node at *B,* consider his observational technique, which focused on the generation, splitting, and recombining of null points: the singular S0 null splits in two as the resonator moves away from the wall, with the newly-born duo lying in the upper and lower halves of the resonator. Bit by bit the upper null moves toward the top of the resonator while the lower null moves toward its bottom, eventually reaching the extremes. They then move respectively down and up until recombining at S180. If a standing wave were present, then the move of a null point through 180° would have to coincide with less than a half-wavelength (~0.44 wavelength in our theoretical calculation). Though he doesn't mention the point, Hertz would have seen that the S180 sparking at *B* was quite intense since to find the null extrema at S90,S270 he must have rotated the resonator through the 180deg location—hence *B* observationally marked an anti-node, as in fact it does. For different reasons to which we will shortly turn, Hertz also considered his observed point *C,* at which the *emf* does not alter much on rotation of the gap, to mark the succeeding node. Table 9 lists Hertz's observational data.

Turn now to the measurements from which Hertz found his 9.6 m wavelength. These form part of the second group in Table 9, which are preceded by the first set

Table 9 Hertz's observational results. Set 1 in the table were not used for wavelength computation but just to sustain the existence of a standing wave. **Measurements 2.2, 2.3 and 2.4** were the only ones that he used to compute a 9.6 m wavelength

	Loop center	Null S0	Null S180	S180 ≫ S0	S0 ≫ S180	S180 > S0	S0 > S180	No nulls
1.1	0.8			X				
1.2	3		X					
1.3	5.5					X		
1.4	8						X	
2.1	Near wall					X		
2.2	**1.08**	**X**						
2.3	**2.35**		**X**					
2.4	**4.12**							**X**
2.5	6–7.5							Minima at 90°, 270°

that he used in setting his graph points *I, II, III, IV* (Fig. 14). We construct our plot (Fig. 16) to represent the situation for a 5.6 m wave with the first anti-node occurring at Hertz's value of 1.72 m, which according to his data requires the S180 null to be at the second series value of 2.35 m given the S0 null at 1.08 m. Yet if the anti-node is indeed located at or reasonably close to 1.72 m, then Hertz's S0 and S180 nulls cannot be where he placed them, according to our theoretical calculation. Specifically, the S0 null would have occurred at about .67 m instead of at Hertz's 1.08 m, while the S180 null would have been at about 2.77 m and not at the 2.35 m that he used. The differences in both cases are close to .4 m, with S0 displaced away from the wall and S180 toward it, as we see from Fig. 16. But since the observational errors at S0, S180 occur in opposite directions, the net result leaves the difference between the calculated and the actual position of *B*, whatever the latter may have been, unaffected.

Hertz had obtained the significantly different value of 3 m for the null S180 in his series 1 from the 2.35 m in series 2, but he had dropped the 3 m result. What might have justified discarding it rather than, say, taking a mean between it and 2.35 m? After all, Hertz thought that (at least for the second series measurements) he could place the S0 null "within a few centimeters," and that the S180 null could be "sharply determined" (Hertz 1893, 131–132). Why might the S180 null from the first series have been less "sharply determined," at least when paired with the S0 null from the second series?

Suppose that Hertz had combined the S0 from his second series (1.08 m) with the S180 from his first one (3 m). He would then have obtained 2.04 m for the locus of *B*, producing a 32 cm difference from the 1.72 m obtained using the S180 from his second series. If he had instead taken a mean for S180 from his two series, yielding 2.7 m for its position, then *B* would have been placed at 1.88 m, for a difference of 16 cm from his value. Both differences would have conflicted with the value that resulted on measuring the simultaneous nulls at S90, S270. For, according to Hertz, he was able to locate *B* using these latter nulls "with fair accuracy," indeed that his

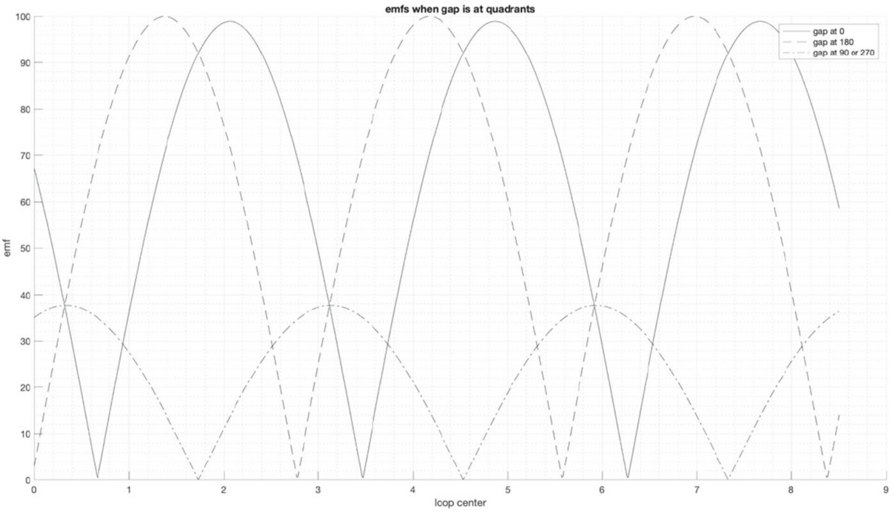

Fig. 16 The *emf* at gap quadrants for resonator center locations

two methods (midway between S0,S180 nulls and simultaneous nulls at S90,S270) themselves agree "within a few centimeters," which is certainly much different from either 16 cm or 32 cm (Hertz 1893, 132).

This of course means that Hertz had reasonable confidence in his S90, S270 measurement, even if he felt it to be somewhat less accurate than finding the S0, S180 midpoint. Why might he have felt secure in such a judgment? To place *B* using the simultaneous nulls Hertz had to find a position at which sparking vanished at two diametrically-opposed orientations of the gap. He might well have been off by some amount for one of them, but not by essentially the same quantity and in the same direction in placing the other. After all, he had to rotate the resonator back and forth, which might certainly alter whatever error arose at one position when turning to the other. In which case Hertz had good reason to ignore the earlier value of 3 m for the S180 null in order to maintain consistency with his dual-null placement of *B*. This was reasonable, since the 3 m measurement for S180 in series 1 was taken at a very different time from the 1.08 m position of the S0 null in series 2. Far better to have taken both measurements during a single, continuous series of observations to avoid inaccuracies that would inevitably arise over too great a time interval.

How though Hertz did make his measurements? His published account is sparse, but there's enough to gauge the problems he faced. The center of Hertz's vertically-placed discharger "was 2.5 meters above the level floor; the observations were also carried out at the same distance above the floor, a gangway for the observer being built up with tables and boards at a suitable height" (Hertz 1893, 126). The resonator loop was quite large, having a diameter of 70 cm, and Hertz had to mount it in a "wooden frame" that allowed rotation about an axis through the center while having the observer hold the device "in his hand" keeping the axis horizontal, placing the center of the loop at particular distances from the reflecting wall, and rotating the loop's spark-gap to the necessary orientations. Such a complex series of maneuvers is certainly not conducive to precision. We saw above that our model (based on requiring the first anti-node to be at 1.72 cm) implies that Hertz would likely have been off by about 40 cm or thereabouts in marking his S0, S180 nulls. That's reasonably consistent with the difference of 65 cm (3.0–2.35 m) between the values for S180 in Hertz's two series if we allow for an additional imprecision due to the measurements having been done at different times. He had to climb up on the "tables and boards" he had cobbled together, stepping from place to place along them holding the large resonator, trying to keep it vertical and rotating it around. Nevertheless, the close match between Hertz's two methods of locating *B* makes it reasonably sure that he had correctly found an anti-node within several centimeters or so of 1.72 m if we assume that the error in placing the S0 null is compensated by the S180 error, while the simultaneous-nulls measurement is comparatively secure.

Locating *B* gave Hertz the first of his two data points—one that he was certain marked the location of an anti-node for reasons we discussed above. How to find the position of the succeeding node? To do so required moving the loop along a few centimeters past the S180 null, rotating the gap around to observe the change in sparking at each location until one was reached at which the spark seemed not to change much at all. That much he could conclude from his graph of the standing wave (Fig. 14): from it he divined that at a node "the direction of the oscillation does not change; and therefore the sparks must either not disappear at all, or else they must disappear an even number of times" (Hertz 1893, 129).[29] This is inherently a *comparative process* because in practice Hertz sought places where the sparking doesn't vanish at all, or at least change much in intensity. He thought he could pinpoint such a location quite well because, he asserted, "in its neighborhood the phenomena first described [visible sparking strength] alter very rapidly" (Hertz 1893, 132). But do they? Given the vagaries of comparative judgments, much depends on just how the measurement was done. For example, if perchance Hertz set the initial gap orientation at 90° or 270° for a given loop position and then rotated around from there, the situation might well be perceived very differently compared to starting at S0 or S180—because, as our plot indicates, at S90 or S270 the width of the region across loop positions within which the *emf* remains quite stable is very broad, as we can see from Fig. 17, which displays our theoretical calculation.

We estimate that the node is located at 3.12 m (Hertz's more or less precise antinode B at 1.72 m + a quarter wavelength 5.6 m/4 = 1.4 m) where the *emf* has a steady value across all gap orientations of magnitude about 43 on a scale of 0 to 100 across all values between loop locations from 2.5 to 3.5 m. From the figure, we see that whereas at gap position S0 and S180 the region of stability across loop positions is quite narrow, the same does not hold at positions of S90 or S270. At those gap locations the change is much less rapid. Given that Hertz held the loop in his hand, and that he would have had

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[29] Presumably this follows from Hertz's graph because the direct and standing wave exchange positions on either side of the node, thereby reversing the direction of the driving force at symmetric positions on either side of the node (*C* on his graph) and so keeping the *emf* across the gap running always in the same direction around the loop. In which case either the *emf* never changes at all whatever the position of the gap, or if it does vanish at some orientation that places the gap on the side of the resonator toward the wall, then it must also vanish at a symmetric position on the side facing away.

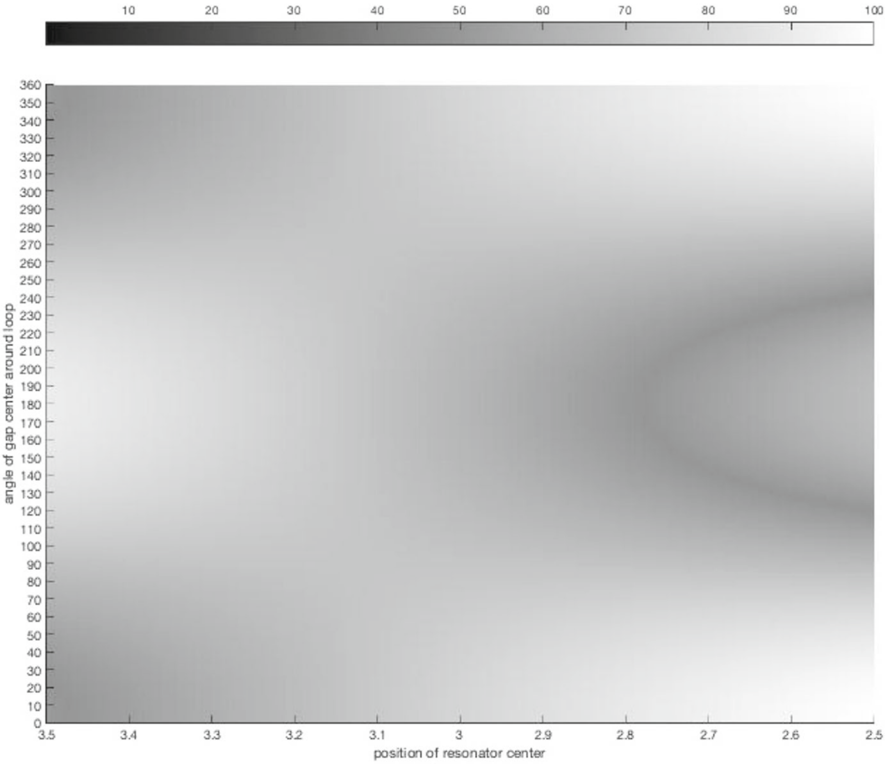

Fig. 17 The *emf* near a node

to rotate it around to assess sparking strength in looking for a node, it's most unlikely, to say the least, that he measured the sparks at a series of orientations for each loop position by changing the gap size until the spark just appeared. In which case he had to estimate the strength visibly, and the eye judges luminosity in proportion to the logarithm of intensity.

For comparison, the *emf* at a node is about 38% of its value at peak in an anti-node. Setting the latter to 100, the nodal spark strength would accordingly amount visibly to about 80% of the sparking maximum, making it difficult to judge small changes in a node's vicinity. At gap positions of S90 and S270 even the absolute sparking strength (i.e. the *emf*) changes hardly at all as the loop displaces up to about 40 cm from the node. Starting from either of those two orientations, the visible intensity changes on rotations of up to 50° or so by at most 30% or thereabouts at any loop position within the 40 cm region to either side of the node. Such a change would not have been readily noticeable given the difficulties inherent to Hertz's setup. He in fact needed an assistant, and the actual spark observations had to be made by either him or his assistant from a distance: "inasmuch as the body of the observer always exercises a slight influence, the observations thus obtained must be controlled by others obtained from greater distances. The sparks too are strong enough to be seen in the dark several meters off; but in a well-lit room practically nothing can be seen, even at close quarters…" (Hertz 1893, 126). From a distance it would certainly be very hard indeed to judge comparative spark strengths.

It would moreover have been tedious, to say the least, to take the gap all the way from top to bottom or the reverse in trying to spy whether the spark changed. Just turning through about a quarter revolution or so would have seemed a reasonably sufficient and expeditious way to work. Unfortunately doing so can be subject to considerable inaccuracy. It consequently seems likely that Hertz or his assistant worked by setting the gap at each resonator location near its top or bottom and then moving it through some amount from there. In which case even an observer placed close to the apparatus might not easily distinguish spark changes across a displacement of as much as 40 cm, much less someone located at a distance. We know from the imprecision in Hertz's singular null observations at S0 and S180 that he or his assistant could not set even those, where sparking actually vanished, to a greater accuracy than about 40 cm. And since the actual observer in the reflection experiments was apparently located at some distance from the spark-gap, the imprecision might well have reached as much as a meter, taking the two factors together and allowing for the effect of distance.

We cannot know with certainty just how Hertz or his assistant worked in locating point *C*, but with an anti-node at 1.72 m the succeeding node would have been near 3.12 m, not at the 4.12 m Hertz decided on (taking a mean between his observations that placed the node at 4.1 m and 4.15 m). The locations of *B* and *C* are *the only two points* that Hertz used to calculate a wavelength, finding thereby that the quarter-wave distance between the node and anti-node was 2.4 m. The implication seems unavoidable: *Hertz's wavelength of 9.6 m resulted from misplacement of the node at C by almost a full meter*.[30] He might have continued further by locating a succeeding anti-node (his *D*), but *D* "could not be accurately determined for the phenomena had here become very feeble; only this much could be asserted, that its distance from the wall was between 6 and 7.5 m" (Hertz 1893, 132).

Hertz's air-wire experiment had yielded a length for the air wave of 9 m, not the newly-found 9.6 m. "The difference," he argued, "is not so great as to prevent us from regarding the new measurement as confirming the earlier one." He decided in the end to take a mean between the two, yielding a wavelength of 9.3 m. By this time Hertz thought it reasonable to assume that the air wave propagated at the speed of light, in which case the period of the discharger's oscillation had to be changed from his previously calculated value of about 36 MHz to the slightly lower 32 MHz. The earlier calculation of the period had, recall, incorrectly doubled the system's capacitance. Again, adjusting for the error changes the discharger's true frequency to about 50 MHz. Had Hertz known about the error, his requirement that the air wave travel at about the speed of light would have failed, since the corrected frequency produces a lightspeed about 1.5 times too large for his 'mean' wavelength of 9.3 m.

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[30] Instead of looking for a node with its attendant observational difficulties, Hertz might instead have looked for a second S0 null point, which would presumptively have occurred a half-wavelength further on. This would have given him a different way to measure the wavelength (S01–S02). Why did he not do so? In the absence of his laboratory notes we cannot offer a definitive answer. However, to work in such a manner would not have been convincing to his intended audience because the positions of the gap nulls do not relate in an obvious manner to the characteristics of the standing wave, as we can see from Hertz's discussion in which he carefully described what happens to a null point as the loop moves along. To be convincing he needed the distance between a node and an antinode.

That alone might have suggested that something was amiss. But he didn't know about the error, and so, once again, it seemed that the air wave did propagate more rapidly than the wire wave, based now on a direct measurement rather than an inference from interference with a wire wave.

6 Conclusion

Hertz certainly hoped to find reasonable consistency between his previous result of 9 m for the length of the air wave and his new, direct measurement. When he found a close match, he stopped. In retrospect, the consistency depended in the end on *only three* actual numbers—the locations of the S0 and S180 nulls (backed up by close results for the S90 and S270 ones) and the position of his nodal point *C*. And so one might say that Hertz's two discovery papers claimed results that were based, for the first one, on the disturbing effect of wave guidance, and, for the second, on the combination of a hoped-for consistency with the first result shadowed by unavoidably imprecise observation, given at the very least the jerry-built tables and boards that he or his assistant clambered around in a room festooned with hidden pipes and bordered by metal pillars while trying to move, hold vertical and rotate a quite large device by hand. It's a testimony to Hertz's experimental skill that he was able to obtain any persuasive results at all. And yet the papers in which he published these experiments were the very ones that quickly convinced physicists who might otherwise have been skeptical that electric waves in air do indeed exist.

This is not the place to pursue in detail the sequence of investigations that followed Hertz's discovery among other physicists, but we will conclude with among the first of these, by Frederick Trouton (1863–1922) at Trinity College in Dublin with the advice of George FitzGerald (1851–1901) to whom he was assistant. FitzGerald had, among other accomplishments, been the first to deduce Fresnel's equations for the amount of light reflected and refracted at a surface from a set of equations based directly on Maxwell's scheme. He, like others who worked on Maxwell's field theory, had long been skeptical that a feasible way could be found to detect artificially-produced electromagnetic radiation. Trouton had solid experimental experience, though not in electrodynamics. In collaboration with (or, more accurately, with the advice and supervision of) FitzGerald, in October, 1888 he undertook to look into Hertz's exciting discovery.

Trouton wrote a paper published in *Nature* the following February that detailed their results. He constructed a discharger (which he termed a "vibrator") similar in dimensions to Hertz's, consisting of "thin brass plates, about 40 centimeters square [that] were suspended by silk threads at about 60 centimeters apart, so as to be in the same plane. Each plate carried a stiff wire furnished at the end with a brass knob" (Trouton 1889b, 391). Such a device accordingly oscillated at about the same 50 MHz that Hertz's had—but Trouton, following Hertz, claimed instead "speaking roughly" 30 MHz. Although he provided an expression (in British units) for the period of such a discharger in terms of capacitance and inductance, he clearly had not computed it, preferring apparently to just adopt about Hertz's value as adjusted by Hertz in his reflection paper (to 32 MHz) on the basis of an air wave having a 9.3 m length and traveling at lightspeed.

All of Trouton's actual measurements were taken with the resonator plane containing the discharger, and the spark gap oriented to face it—and so, in terms of our previous convention, with the gap at S180. In explaining what he thought to be the forces involved in such a situation, Trouton revealed that he was not well-up on the fundamentals of Maxwell's field theory. If, he remarked, the resonator plane is parallel to the discharger, and "the knobs of the circle are brought round through 90° so as to be parallel to the" discharger then only "the electric part of the disturbance comes into play." This was not just loose phrasing on Trouton's part, because he insisted on placing the resonator plane to contain the discharger and then orient the gap in the perpendicular to the discharger so that "only the magnetic part of the disturbance had effect." And, indeed, Trouton wrote that his observations were made in such a position, so that "only the magnetic part of the disturbance had effect" (Trouton 1889b, 392). He may perhaps have been thinking along lines similar to those suggested by Hertz's wording in his air-wire paper, though such a distinction made sense only in Helmholtz's theory, and Hertz himself seems to have dropped it by the time of his reflection experiments.

Trouton described three observations undertaken, he asserted, with the gap normal to the discharger. Placing "a large sheet of metal (3 meters square, consisting of sheet zinc)" directly behind the resonator "the sparking increased in brightness and allowed the knobs [of the resonator] to be taken further apart without the sparking ceasing." Holding the resonator fixed—unlike Hertz, who moved it along—Trouton pulled the zinc sheet back 2.5 m. There "the sparking ceased, and could not be obtained again by screwing up the knobs." Moving the sheet to about 5 m, the situation shifted, and there "the sparking was slightly greater than without the sheet." Though Trouton did not provide a specific number for the resultant wavelength, he asserted that the "distance from the position of interference to the sheet is a quarter of the wave-length, being half the distance between these simultaneous positions of opposite effects" (Trouton 1889b, 392). According to this, the length of the air wave had to be about 10 m, providing fair consistency with Hertz's claim.

There does not seem to be any reasonable way in which Trouton could have obtained his three observations with the spark-gap always normal to the discharger whatever the specific circumstances of his experimental space may have been since we have seen above that guidance can have little effect in reflection experiments of this kind. He claimed to have measured at S90 (or, equivalently, S270) in our conventions where he observed reasonably-bright sparking near the reflector, where the center of his resonator would consequently be about 30 cm away. Trouton then moved the reflector 2.5 m further, placing its center at 2.8 m, where he found that the sparking disappeared. Shifting an additional 2.5 m to a distance of 5.3 m or thereabouts, he observed more or less the same large sparking as at the sheet's initial position.

Taking 5.6 m as the length of the air wave, there should be a near *emf* maximum across all loop positions for a gap orientation of S90 (or S270) when the loop's center is about 30 cm from the reflector, as we can see in Fig. 16 above. The same *emf* will reappear when the center is about 3.1 m from the reflector and again at about 5.9 m distance. Trouton could not have perceived a difference in visual spark intensity of less than about 20%, in which case visual *emf* equivalents would have run for him between about 25% and 35% of the absolute maximum. That could place the 5.9 m distance for equivalence anywhere between about 5.4 m and 6.4 m, the low end of which is close to Trouton's 5.3 m.

What now of Trouton's claim that the sparking disappeared at a distance of 2.8 m? The *emf* does indeed disappear in the vicinity of such a loop center—namely at 2.85 m—which is close to his value given visual insensitivity. *However, the disappearance occurs at a gap orientation of S180 (i.e. with the gap facing the discharger) and not at Trouton's claimed S90 or S270* (Fig. 16). At the latter two orientations, the *emf* is close to its value at .3 m and 5.3 m.

For Trouton to have seen the spark vanish at 2.8 m he must have inadvertently turned the gap to face the discharger. According to his way of thinking, such an orientation will respond to both the "electric" and to the "magnetic" force, while he had explicitly decided to set the gap to obviate the "electric" action. If Trouton had now unintentionally turned the gap to face the discharger, thereby picking up both forces, he might not have thought it significant *in situations where the spark seemed to disappear* because both forces should vanish. Indeed, he might not even have noticed it. We cannot of course say just what might have taken place since we do not have Trouton's notes, but the only way that he could have seen the sparking vanish near 2.8 m would be if the gap had been turned to face the discharger (S180).

Trouton had to construct apparatus that mimicked as closely as possible Hertz's design, about which he had only Hertz's published paper. In doing so he undoubtedly expected to obtain about the same results as Hertz, and so it's hardly surprising that he just took over Hertz's (incorrect) value for the discharger's frequency. It would also not have surprised him—quite the contrary—to find a wavelength about the same as Hertz's, even though such a value conflicted with the wire wavelength that Hertz had measured given Maxwell's electrodynamics. Trouton mentioned nothing about that and may not even have been aware of the problem, though FitzGerald certainly would have been had he been paying close attention. In the end Trouton's principal interest (as was FitzGerald's) involved analogies between the new Hertzian waves and those of light. His next paper in *Nature*, published the following August (1889), for example investigated the "periodic reflection of electric radiation from plates of different thicknesses, analogous to Newton's rings, with the view of further identifying these radiations with 'light'" (Trouton 1889a, 398).

The earliest investigations of what came to be called "Hertzian waves" were clearly troubled by the difficulties inherent both in observation and in understanding. Within a fairly short time experiments were undertaken on reflection, in particular by Edouard Sarasin and Auguste de la Rive in Geneva, that were more carefully designed to produce precise position values than Hertz's or apparently Trouton's (Sarasin and de la Rive 1893). They moreover probed the behavior of the resonator by altering its radius, finding that it responded even when not tuned to the discharger. That discovery, which had not been anticipated, generated a good deal of discussion. In retrospect, since the discharger produced a highly damped wave, its Fourier spectrum was sufficiently broad that even a resonator not tuned to its frequency could respond. But the question of how and why such a purely mathematical-decomposition could have detectable physical effects was hardly obvious at the time.

Our account does not indicate in any way that Hertz fabricated results or indeed that he did not produce evidence for the existence of electromagnetic radiation. Quite the contrary: he labored hard and effectively to design and work an entirely novel system of apparatus to tease out an extremely subtle effect. Both his wire-air and reflection experiments certainly succeeded in doing so. The first was affected by something that Hertz did not expect, and certainly could not have calculated even if he had, namely the very structure and content of his experimental space. As a result he found that air waves do exist, albeit that they travel much more rapidly than in wires. Wave guidance due to the nature of the space surrounding his apparatus abolishes the difference: there was nothing deficient in Hertz's measurements here; weakly-developed theory, not experiment, led up a blind alley in respect to the speed of the air wave. Neither did Hertz's reflection measurements obliterate the difference, albeit for a different reason: here difficulties of measurement combined with the expectation raised by his earlier experiments to reproduce the same result. None of this is surprising. Hertz aimed artificially to generate and then to explore something that even the British followers of Maxwell thought it likely impossible to detect. Hertz evolved a way to do so. His first results provided convincing evidence that the effect did indeed exist, even if in a form inconsistent with Maxwell's scheme—but not perhaps with Helmholtz's, for whom, after all, Hertz had been an assistant.

Hertz's work had a rapid impact, leading over the next decade to the creation of radio technology in the hands of John Ambrose Fleming and Guglielmo Marconi (Hong 2001; Yeang 2013). Over time his devices were mutated in ways that enabled investigators to achieve levels of precision that had escaped both him and Trouton. The sought-after object—an electromagnetic air-wave—bit by bit infused various apparatus as both the structure of devices built to incorporate the effect and conceptions of its properties evolved in physical and mathematical ways.

The same type of development inevitably occurs whenever a theoretical scheme that entails the existence of a novel effect becomes entrenched in the scientific community. Consider for example the development of the wave theory of light following its elaboration and exploration by Augustin Jean Fresnel between 1816 and the mid 1820s. Even though Fresnel won a prize on the subject of diffraction set by the Paris Academy in 1818, the examiners, who included partisans of an optics based on the separate existence of light rays such as the Marquis de Laplace and his protégé Siméon Denis Poisson, refused to accept waves as physical structures. They did accede to the mathematics of diffraction, but interpreted it according to their preferred physical scheme. Over the next decade or so, novel devices were produced on the basis of wave concepts, devices that simply had no clear purchase in an alternative system based on rays and particles of light. Such partisans of the older scheme as David Brewster did not give up. They tried, with some success, to adapt the mathematics of the new system to their way of thinking. But they never were able to generate novel apparatus and effects on the basis of their schemes, which accordingly did not attract new proponents (Buchwald 1989). After all, as science gradually became more professionalized in the first half of the 19th century, success in generating novel effects and attendant apparatus became ever more a signal desideratum.

That one might be able only to spawn an explanation of what proponents of an alternative could create de novo became an increasing sign of inadequacy. Explanation took a back-seat to productive generation, which has ever since become a critically-influential characteristic of persuasive science. The existence of gravitational radiation for example remained an open question until the creation and deployment of wave-detecting apparatus in the first decades of the 21st century. Detecting something that had the sign of a wave was of intrinsic theoretical significance, and the Laser Interferometer Gravitational-Wave Observatory (LIGO) went further by providing information about a natural process, black-hole radiation, that was previously unavailable. The descendants of Hertz's apparatus similarly provide evidence for such things as the microwave background radiation, and consequently for underlying physical phenomena. This, we submit, is the core, the essential character, of a persuasive theoretical system: the detection of natural processes by means of apparatus designed on the basis of a phenomenon entailed by the system—electromagnetic radiation in Hertzian progeny, and, now, gravitational radiation in LIGO-like interferometers. Kip Thorne, who with Barry Barish and Rai Weiss was awarded the Nobel Prize in 2017 for LIGO, expressed this particularly well: "Whenever a new method has been devised for observing the universe, surprises have come; and gravitational waves are so radically different from other ways of observing that huge surprises are almost guaranteed" (Thorne 2020, 46).

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Compliance with ethical standards

Conflict of interest There are no conflicts of interest on the parts of any of the authors.

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