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The Replication of Hertz's Cathode Ray Experiments

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I reappraise in detail Hertz's cathode ray experiments. I show that, contrary to Buchwald's (1995) evaluation, the core experiment establishing the electrostatic properties of the rays was successfully replicated by Perrin (probably) and Thomson (certainly). Buchwald's discussion of 'current purification' is shown to be a red herring. My investigation of the origin of Buchwald's misinterpretation of this episode reveals that he was led astray by a focus on what Hertz 'could do'—his experimental *resources*. I argue that one should focus instead on what Hertz wanted to achieve—his experimental *goals*. Focusing on these goals, I find that his explicit and implicit requirements for a successful investigation of the rays' properties are met by Perrin and Thomson. Thus, even by Hertz's standards, they did indeed replicate his experiment. © 2001 Elsevier Science Ltd. All rights reserved.

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1. Introduction

Adjudicating questions of experimental replication is an extremely difficult and subtle matter. The issues involved can be so intricate, in fact, that a leading expert in Hertz's experimental practice, Jed Buchwald (1995), appears to have misjudged the case of attempts to replicate Hertz's cathode ray experiments. An examination, in considerable detail, of Buchwald's analysis of the experiments of Hertz and his successors, illuminates the problems of interpretation that can arise from apparently trivial shifts in analytical perspective. I will argue that the problems in Buchwald's analysis arise from adopting a particular perspective on

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the significance of certain aspects of Hertz's practice—a perspective that directed him quite naturally to misleading remarks in Hertz's experimental report.

Any analysis of attempted experimental replication will have to be concerned with, at a minimum: production of effects; measurement of properties; protection from interference. I will be concerned here especially with the first and last of these items, and I will attempt to clarify what, in Hertz's understanding, plays their respective roles in his cathode ray experiments. Buchwald investigates these questions as well, and so our intentions, at least, overlap. What separates us, I believe, is the *stance* we adopt toward these questions. Buchwald's attention is focused on Hertz's experimental resources—he is thus concerned to find uses for the various pieces of apparatus comprising Hertz's devices. In contrast, my attention is directed toward the ways in which Hertz and his successors achieve their various experimental goals. Clearly there is considerable overlap in these stances. Yet they give rise to quite different conclusions. I will conclude that focusing on the role of a particular piece of apparatus can have the unfortunate consequence of blurring the line between effect and interference, and this can obscure the important question of replication.

Buchwald revisits three seminal experimental reports on the nature of cathode rays, and, in particular, the experiments of Hertz, Perrin and Thomson to measure their charge. In his reconstruction of Hertz's failure to observe the electrostatic properties of cathode rays and the subsequent efforts of Perrin and Thomson to replicate these experiments (1995, pp. 162–163), Buchwald is careful to point out that the pressure in the cathode ray tube could not have been a constraint operating on Hertz's conception of his own practice. Even if we grant that Thomson's success is made possible by his improved vacuum, we still lose insight into *Hertz* by focusing solely on vacuum technology. As Buchwald observes, Hertz's concern with his vacuum's integrity extended only to its suitability as a space for the production of cathode rays. Certainly Hertz (1898) does not complain of his troubles in producing a vacuum deep enough to allow a proper investigation of the properties of the rays.[1] Moreover, Buchwald suggests that Hertz is inclined against viewing the shallowness of his vacuum as an interfering factor. For example, Hertz supposes that a deeper vacuum would serve to impede electrostatic deflection rather than heighten it.[2]

Considerations of this nature are not, by themselves, sufficient to provide a framework for assessing Hertz's probable response to, say, Thomson's suggestion that a deeper vacuum is required for observing the electrostatic effects of the rays. Buchwald attempts, therefore, to produce positive evidence that Hertz would be resistant to Thomson's replication efforts. Such evidence is not available in the case of electrostatic deflection where Buchwald concedes (1995,

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[1] See Hertz (1898, pp. 240–241), where he does complain of some difficulties in *keeping* a vacuum, but never of producing one sufficiently deep for his purposes.

[2] This is Buchwald's example (1995, pp. 162–163).

p. 163) that Hertz would have acknowledged the persuasiveness of Thomson's experiment.[3] Buchwald therefore focuses on the case of charge catching.

Buchwald's analysis of this episode in the history of science, in particular his analysis of Hertz's practice, is open to certain objections. These objections, I shall argue, undermine his claims about experimental replication. I wish to re-examine the issue of the replication of Hertz's experiments investigating the properties of the cathode rays. I will be concerned primarily with what Hertz did and what he claimed about what he did. Heeding the above warning about analytical perspectives, I will focus my attention on Hertz's experimental goals. The advantage of beginning with the question of what plays the role of effect production on the one hand and interference protection on the other will, I hope, become clear. Additionally it will become apparent that Thomson (but perhaps not Perrin) did indeed replicate Hertz's experiment to measure the electrostatic properties of the cathode rays. I will ground my investigation as a response to Buchwald's analysis, an analysis which resulted in a somewhat different conclusion; Buchwald claims that neither Thomson nor Perrin replicated Hertz's experiment.[4]

2. Overview of Hertz's Experiments

In June of 1882, Hertz is once again taking joy in his budding experimental career. The 'fun' he had taken out of his work on hardness, fully written up in May, had been overshadowed by some unsuccessful evaporation experiments (Jones and Schott, 1898, p. xix). But now those experiments had been written up and Hertz was free to choose a new venture. He decided to forego, for a time, exact measurement and instead concentrate on experiment. This would allow him to discover something new and, as he says, of 'great theoretical interest' (*ibid.*, p. xxii). Hertz had already attracted attention as a meticulous researcher who could answer questions requiring exacting observations and detailed control over his apparatus. What he had yet to do was produce qualitatively new knowledge. He thought he could find this knowledge in an investigation of the nature of cathode rays on which 'little has been done' (*ibid.*, p. xxii). Thus he had

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[3] It might be supposed in fact that Hertz would welcome Thomson's results in this case. Buchwald argues that Hertz's goal is to show that cathode rays are *sui generis*, and susceptibility to electrostatic deflection in the absence of charge transport is precisely analogous to susceptibility to magnetic influence coupled with the rays' inability to exert magnetic effects. The conclusion in the electrostatic case would presumably be similar to his discussion of the magnetic case: 'Without attempting any explanation for the present, we may say that the magnet acts on the medium, and that in the magnetized medium the cathode rays are not propagated in the same way as in the unmagnetized medium' (Hertz, 1898, p. 246).

[4] Buchwald is speaking of replication *to Hertz's satisfaction*. He does not deny that Thomson's experiment correctly shows charge on (the electrons comprising) the cathode rays. So the issue between us is not about what is 'really' going on in these experiments as much as it is about Hertz's probable reaction to them.

little at stake theoretically and could instead concentrate on uncovering those novel features that were surely hidden in 'one or other of the hundred remarkable phenomena' (*ibid*., p. xxii) associated with the cathode rays. Indeed at the beginning he was satisfied with 'rushing about without any fixed plan, finding out what is already known about it, repeating experiments and setting up others as they occur to' him (*ibid*., p. xxii). His flurry of activity culminated in an intention to investigate two of the relatively few positive claims that had been advanced about the nature of cathode rays: (1) the cathode discharge is discontinuous (as advocated by Wiedemann, Goldstein and de la Rue); (2) the cathode rays indicate the path of the current (a claim by then already *de facto* abandoned by the physics community at large, but not disproved either). Investigating these claims produced remarkable fruit:

Hertz's experimental report is in three stages; in each, Hertz is investigating a separate property of cathode rays: (1) the continuity properties of the discharge itself; (2) the electromagnetic properties of the cathode rays; (3) the electrostatic properties of the cathode rays.

(1) Hertz opens his report with a review of a long-standing debate in the field of cathode ray research. The issue is whether the cathode discharge is essentially discrete or continuous. Hertz reports on his own experiments on this question and concludes that the discharge is continuous.

(2) Of more immediate interest to us as we consider the issue of replication are Hertz's investigations of the electromagnetic properties of the cathode rays. Here Hertz breaks the investigation into two questions: 'Do the cathode rays exert electromagnetic influences?' and 'Are the cathode rays susceptible to electromagnetic effects?'. For Hertz these are two quite distinct questions and require two independent answers. The first of these questions is answered by Hertz in a clever pair of experiments. The subsidiary experiment[5] shows that, if the cathode rays exert any electromagnetic influence in the regions outside the tube, they must do so only insofar as they are behaving like currents. To show this, Hertz constructs a cylindrically symmetric cathode/anode pair so that the magnetic flux of the current leaving the cathode will precisely balance that of the current entering the anode (Fig. 1). In this set-up, no magnetic field from the current in the tube can extend outside of the tube. We can conclude then that any magnetic influence arising from the tube must be the result of an action peculiar to cathode rays. Conversely, if no magnetic effect is observed at the surface of the tube then cathode rays exert electromagnetic effects only in their role as currents. No effect is observed. Thus any electromagnetic influence exerted by the cathode rays arises from their current-like properties. This result does not establish that cathode rays exert no electromagnetic effects at all. Instead, it justifies Hertz's plan to use magnetic equipotential lines to build up a map of the actual path of the tube current flowing in a most peculiar device: a square cathode ray 'tube' (Fig. 2). Hertz examines the current lines from several different cathode/anode configurations and decides that these maps 'show

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[5] For a clear and more thorough discussion, see Buchwald (1995, pp. 153–155).

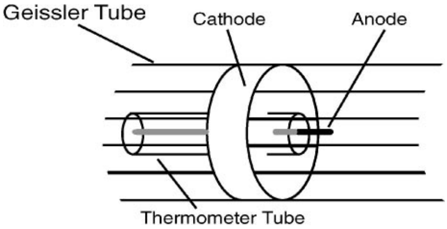

Fig. 1. *Hertz's device with cylindrical anode for showing that the magnetic properties of cathode rays are purely current-like.* (*After Buchwald* (*1995*).)

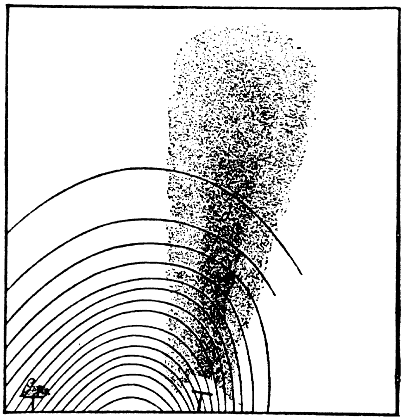

Fig. 2. *Hertz's experiment to map the current* (*Hertz, 1898, p. 25*). *Curves*: *lines of magnetic equipotential. Cloud*: *cathode rays.*

without any doubt that the direction of the cathode rays does not coincide with the direction of the current. In some places the current lines are almost perpendicular to the direction of the cathode rays. Some parts of the gas-space are lit up brilliantly by the cathode light, although the current in them is vanishingly small' (Hertz, 1898, p. 245). These latter investigations demonstrate that the cathode rays are indeed separate from the current flow from cathode to anode and hence, according to the preceding experiment, they exert no electromagnetic effects.

Now, however, Hertz has to explain the well-known but newly-puzzling fact that magnets can exert an influence on cathode rays. One might suppose that this separation of action and reaction is inconsistent with Newtonian mechanics. No new experiments are performed, but a sequence of arguments, resting on the considerable speed with which the rays respond to magnetic influences, ends with the conclusion that the magnet does not really act on the rays themselves. Rather, the magnet acts on the medium through which the rays are propagated and thereby alters the path of the rays through the medium. There is thus no contradiction between these two seemingly irreconcilable phenomena.

Commenting on this conclusion, Hertz cites the views of Wiedemann and Goldstein.[6] On their understanding, 'the discharge consists of an ether disturbance, of itself invisible, and only converted into light by imparting its energy to the gas-particles' (1898, p. 246). Hertz is in general agreement with this view, but suggests that 'discharge' should be replaced with 'cathode ray'. To justify his view that what Wiedemann and Goldstein attribute to the discharge is the result of cathode rays, Hertz refers to the following experiment: the cylindrical tube from the first experiment is evacuated enough to allow a phosphorescence at the far end, and a drop of mercury is introduced into this same end of the tube. Hertz assures us (*ibid.*, p. 247) that, since current is naturally restricted to the 'immediate neighbourhood of the electrodes, which are quite near to one another' no current is present at this end of the tube. When the rays fall on the glass of the tube, they cause it to phosphoresce. Hertz then heats the end of the tube enough to vaporise the mercury. When the rays enter the cloud of mercury vapour, a new light appeared with the 'spectrum of mercury' (*ibid.*, p. 247) and as the mercury continued to glow ever more strongly, the phosphorescence of the glass gradually ceased. By using a magnet to direct the path of the ray alternately either into or away from the cloud of mercury, Hertz could control the appearance of the rays. Diverting the rays away from the cloud removed the mercury glow and restored the phosphorescence of the glass. When the tube was entirely filled with mercury vapour, the cathode rays appeared in their normal form.

This experiment allows Hertz to conclude that the character of the phosphorescence differs from that of cathode rays only because in solids the rays are absorbed by an infinitely thin layer of material, whereas in gas a finite thickness is required. In other words, he can claim that the light associated with the cathode rays is produced by interaction between the rays and ponderable matter. Then, appealing to Goldstein's own research, Hertz asserts (*ibid.*, p. 248) that, because of similarities between the light of the rays and the glowing light accompanying the discharge (the so-called stria), the cathode light should be considered a degenerate form of such stria. Therefore, rather than supposing that these stria are produced by current, we should assume that they are produced in the same way as the cathode light—by absorption of cathode rays. To make this claim stick, it is essential for Hertz to have shown convincingly that the cathode rays are completely distinct from the current. He believes he has done so and thus, here, for the first time, we see Hertz confidently manipulating *pure*[7] cathode rays to probe their unique character.

(3) Hertz seems to find the preceding experiments quite convincing as a demonstration that the rays do not have any properly electromagnetic character. Still, he is troubled because, as he admits in his conclusion, these experiments are consistent with viewing the cathode rays as 'electrified material particles' (*ibid.*, p. 253). For example, the field they produce might be undetectably small, even

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[6] Buchwald has been very helpful in his efforts to clarify for me the relation between the views of Hertz and those of Wiedemann and Goldstein.

[7] My use of this term will be justified below.

by his sensitive device. Therefore a direct demonstration that the rays are electrostatically inert is required. In this third stage, Hertz proceeds as before. He first investigates the electrostatic effects exerted by the cathode rays and then he probes their response to electrostatic effects. The first effort is an attempt to detect charge transport by the rays and the second to deflect the rays by means of an electrostatic field. Neither attempt is successful.

The second experiment is uncontroversial. Hertz uses charged plates sometimes inside and sometimes outside of the tube. These plates produce a strong electrostatic field transverse to the path of the rays. His field produces no discernible deflection in the beam and he concludes that the rays are not susceptible to electrostatic influences.[8] Thomson replicates this experiment with a lower pressure tube and the beam is deflected. Buchwald seems content with the conclusion that Hertz was just mistaken in his assumptions about the effect of gas pressure in this experiment, and he gives the impression (1995, pp. 162–165) that Hertz would readily accept Thomson's result.[9]

On the other hand, Buchwald is not at all content with Thomson's replication of Hertz's charge-catching experiment, and so we come now to what is, for Buchwald, the crucial moment of this entire episode: Hertz's attempt to detect charge transport by the cathode rays. At first sight, the experimental arrangement is quite straightforward (Fig. 3). A circular anode with a hole in its centre allows cathode rays to enter the tube. The outside of the tube is surrounded by a metal box which is connected to an electrometer. If any charge flows past the anode, it will give rise to an electric field in the interior of the tube. This will induce charge on the surface of the metal box which will in turn register on the electrometer. To prevent electrostatic interference from the cathode, the entire device is surrounded by another metal box in metallic connection with the anode. As is well known, Hertz allowed cathode rays to enter the tube and awaited the deflection of the electrometer. This deflection does not occur and Hertz concludes that the cathode rays do not carry charge. To our late twentieth-century eyes, this is the perfect image of a failed experiment. Hertz's device should have registered an effect but it did not. There is nothing to worry about of course. Perrin and Thomson will shortly appear to replicate the experiment and get the correct result.

3. Replication Denied

In their basic operation, Perrin's and Thomson's devices are similar to Hertz's. Perrin's device is almost indistinguishable from Hertz's. Here the

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[8] Hertz concludes that if the cathode rays are still regarded as charged particles, then their speed would have to be so great as to be 'scarcely [...] probable' (p. 253).

[9] I am not claiming that Thomson did replicate Hertz's experiment. Rather I claim that, in accordance with Buchwald's reading, we can see that replication is not an issue *for Hertz* in the deflection experiment. Buchwald is right to point out that there is no fixed theoretical interpretation connecting the deflection experiments of Hertz and Thomson. But he is also correct in his judgement that the interpretation is not at issue in evaluating the results of the deflection experiments.

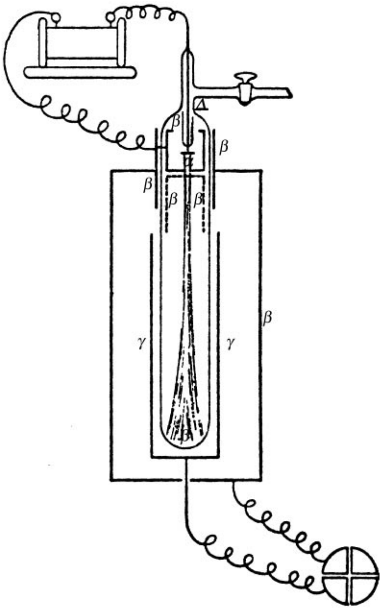

Fig. 3. *Hertz's device for measuring the charge on cathode rays (Hertz, 1898, p. 25). Parts labelled β are in good metallic connection with the 'negative' pole of the induction coil.*

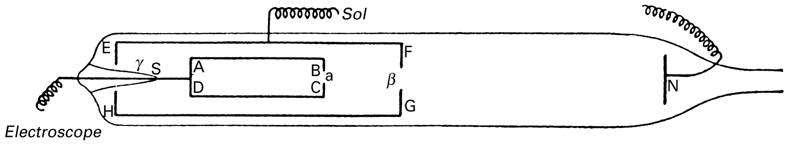

Fig. 4. *Perrin's device for measuring the charge on cathode rays (Perrin, 1895, p. 581).*

external box is replaced by one entirely within the tube and the charge catcher is insulated from this surrounding box (Fig. 4). When cathode rays flow into the charge catcher, it takes on a negative charge. Because of the great distance between the cathode and the protected space, Perrin can check something Hertz could not. He can use a magnetic field to deflect the beam away from the charge catcher. When he does this, no new charge accumulates on the detector. Thus Perrin can claim that the detector 'is charged negatively when the cathode rays enter it and only when they enter it' (1895, p. 581).

Thomson's device is superficially quite distinct from either Hertz's or Perrin's (Fig. 5). He has his charge catcher entirely removed from the scene of the action. Cathode rays will flow through the anode and strike the tube far from his detector. Thomson's technique is an inversion of Perrin's. He directs cathode rays into rather than away from the charge catcher. But the purpose is the same.

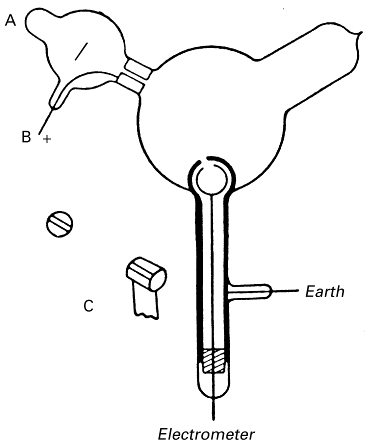

Fig. 5. *Thomson's device for measuring the charge on cathode rays* (*Thomson, 1897, p. 584*).

Both he and Perrin can watch the results of the device in operation independently of cathode ray penetration into their detectors. Thus they are able to distinguish background effects of the device operation from the effects of cathode rays alone.

As Buchwald points out, however, there is an unusual feature of Hertz's device that was neither noticed nor commented on even by those who considered Hertz's experiments immediately after they were performed. On the tube-side of the anode, Hertz connected a screen of wire gauze. According to Buchwald, this gauze purifies the cathode rays of residual tube current and allows Hertz to avoid accidentally measuring the charge transport of the current rather than of the cathode rays. Whatever *we* may think about the nature of cathode rays and about Perrin's and Thomson's success or failure in measuring their charge, surely, argues Buchwald (1995, p. 160), *Hertz* would not have been convinced by their experiments. Neither Perrin nor Thomson bothered to purify their rays. Thus they would have been unable to persuade Hertz that the charge they measured belonged to the rays rather than to the current. Buchwald points to a suggestive remark from Hertz: '[T]he cathode rays are to be regarded as pure after they have passed through the opening in the metal cylinder and the wire-gauze beyond it' (1898, p. 250). He concludes from this remark that Hertz was centrally concerned with ray purification and that (speaking of the region past Hertz's wire gauze) '*only* here, he had produced a region within which the rays could be manipulated in their purest form' (1995, p. 157).

In addition to failing to purify their rays, Perrin and Thomson each complicate matters further by locating their charge catchers inside their tubes. Buchwald claims (1995, p. 160) that this is likely to yield a measurable effect if any current does flow into the tube. This is a further obstacle to Hertz's approbation of the results of Perrin's and Thomson's efforts. For, once again, we find both

Perrin and Thomson measuring not the charge of the cathode rays but rather the charge of the current.

There are then two critical defects common to the devices of Perrin and Thomson. These devices fail to purify the cathode rays and, by including charge catchers within the tube, have a tendency to overstate any effects properly associated with cathode rays. Both of these defects, Buchwald argues (1995, p. 160), 'Hertz would certainly have noticed had he lived'. Let us consider them in reverse order.

My initial reading of Buchwald's objection to the location of the charge catcher was simply that, for Hertz, charge catchers must be outside of tubes before we can ascribe any validity to results obtained through their use. However, there is no basis in Hertz's report for such a claim. What does Hertz say about this issue? Regarding explicit evaluation of the value of locating charge catchers either inside or outside the tube, Hertz has no comment. The only insight into Hertz's thinking about this matter is to be derived from the claims he makes about his expectations concerning the magnitude of the effect. In fact, the only remark he makes that has any bearing at all on this question is in the form of an analogy. Hertz comments (1898, p. 250), before his charge-catching experiment, about the character of the result to be expected should the cathode rays carry charge: 'When even a small amount of electricity was brought inside this mantle, it attracted by induction electricity of the opposite sign from the electrometer, so that a deflection was produced. The electricity could, e.g., be introduced by replacing the tube AB inside the protected space and the mantle γ by a metal rod which had about the same size as the cathode rays'. The metal rod is thus at the same potential as the cathode. Under the assumption that the cathode rays are bits of material charged to the potential of the cathode, the two cases are (neglecting the much greater density of the metal rod) just the same. There is, on Hertz's view, no current flowing into the detector along with the cathode rays, and in the case of the metal rod, the current flows only in the tube (now removed from the protective case).

This analogy makes it seem that Hertz ought to have included his charge catcher within the tube. There was no glass tube surrounding the metal rod when Hertz measured the effect that *it* produced. So, if the rod really is analogous to the cathode rays, then the proper location for the charge catcher is inside rather than outside. Since Hertz instead applies his charge catcher to the outside of the tube, the electric field at the surface of his detector (which is supposed to be inducing charge accumulation on this surface) will be reduced by a factor equal to the dielectric constant of Hertz's glass. Hertz was no doubt well aware of the dielectric properties of his glass, for he had, about a year earlier reported to Berlin's Physical Society concerning his extensive work on residual charges in liquid insulators—work which drew heavily on previous investigations of the dielectric properties of crystals. Were Hertz in fact concerned with the location of the charge catcher, I would think he should have put it inside—according to his own analogy.

However, there is another way to view Buchwald's objection. Perhaps Hertz would not be concerned with mere location in space but rather with what we might call the electro-topological configuration of the device. That is, Hertz might claim that either Perrin's or Thomson's device was faulty because of the location of the charge catcher with respect to the anode/cathode arrangement. Consider Perrin's device again (Fig. 4). Perhaps it was a mistake for Perrin to locate his charge catcher inside his anode. If so, Hertz committed the same mistake. Recall that the protective cage surrounding Hertz's tube is in good metallic connection with the anode. In other words, Hertz's charge catcher is also inside the anode. In fact, except for the glass of the tube, Perrin's device is electro-topologically identical to Hertz's. So, any stray currents or extraneous charges would be no more likely to register on Perrin's detector than on Hertz's. Indeed, such rogue charges would be more likely to register on Hertz's device at the termination of a run of the experiment for, if there were residual currents attempting to find alternate paths to the anode, they could end up stuck to the glass of Hertz's tube. No similar possibility exists for Perrin's device. As Hertz would be aware, no field lines cross the 'dead' zone occupied by the electrically isolated charge catcher. What about Thomson's device (Fig. 5)? It should be obvious that there is really no room for objection on this point.[10] His charge-catching device is electrically isolated from the remainder of his apparatus. Any charge that enters the catcher after the experiment has begun will register on the device independently of the actual state of the anode/cathode pair.

Since, as I have argued, the location of Hertz's charge-catching device was no better than and, in some respects, not as good as the location of either Perrin's or Thomson's devices, I conclude that Hertz is probably unconcerned about the location of the charge catcher. On the other hand, if he is concerned then his own understanding of the issue would compel him to recognise the adequacy of the locations chosen by Perrin and Thomson.

4. Ray Contamination?

What about cathode ray purification? At no point does Hertz express a concern that current might play a contaminating role in his charge measurements. There is, of course, a great deal of evidence that Hertz was concerned with cathode ray purity. But, as we will see, these are two distinct issues. In contrast, my view, that Hertz was unconcerned with contamination (i.e. that he thought the *production* of pure rays was a trivial matter), does have positive textual support: (1) In the experiment performed by Hertz to divorce cathode rays conceptually from current paths, he explicitly acknowledges the absence of tube

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[10] Buchwald is in agreement with me with respect to this aspect of Thomson's device. In his comments on an earlier version of this paper, Buchwald (1998) suggests that the latter reading of his objection—that the key is electro-topological configuration, not location in space—is closer to the mark and finds that Thomson's device would not be subject to this objection.

current in certain regions that are filled with cathode rays. (2) Recall Hertz's comments on Goldstein's view that the light emitted in the tube is a subsidiary effect produced by the interaction of cathode rays with molecules of the gas. Recall also his description of the tube: 'there was a brilliant phosphorescence at the end opposite the cathode. After what we have already said, there can be no doubt that the current-paths are restricted to the immediate neighbourhood of the electrodes, which are quite near to one another, and that only the cathode rays traverse the length of the tube' (Hertz, 1898, p. 247). (3) Finally, the only remark Hertz makes about the shielding of his tube refers to his desire to secure a space free from *electrostatic* interference. In contrast to Buchwald's insistence on the necessity of securing a current-free space for the manipulation of cathode rays, Hertz seems to regard purification as a trivial matter.

I anticipate an objection at this juncture. Hertz does in fact refer to the rays that emerge from his wire gauze as *pure*. Does not this indicate that ray purification and contamination are major issues in this experiment? I think not. He defines pure rays quite casually. The idea is that there are, in fact, some regions of the tube where both cathode rays and current are present, whereas in other regions we find rays by themselves. Speaking of rays in the latter regions he says (1898, p. 249): 'to avoid confusion we shall call these pure cathode rays'. The potential confusion would result from his preceding discussion where he suggests that (1898, p. 248), on the basis of Goldstein's researches, the cathode rays can be regarded as merely degenerated forms of the glowing bands surrounding the anode. These stria are intimately bound up with the current, however, so it is best to reserve the name 'pure cathode ray' for those rays that exist apart from the current. Hertz's desire for clarity of reference provides his only motivation for introducing the notion of 'purity'. And now that the issue of 'purity' here has been clarified, it should be obvious that the two examples considered earlier—the rays in the square tube and the rays in the mercury filled tube—qualify as pure cathode rays. Hertz's concern with purity, therefore, is a concern to demonstrate that cathode rays, *when produced*, are indeed distinct from the current, and separate themselves from it *naturally*; it is not a concern to develop powerful tools to safeguard against the contamination of already produced rays.

But how does all of this address the question of the purpose of the wire gauze? Buchwald cites Hertz's claim that the cathode rays are pure after they emerge from the gauze. Does not this suggest that the purpose of the gauze is to filter out the presence of tube current? Buchwald points to the following remark from Hertz: 'If the results we have already obtained have any meaning, the cathode rays are to be regarded as pure after they have passed through the opening in the metal cylinder and the wire-gauze beyond it' (1898, p. 250). The question here is: 'What results are we being directed back to?' For Buchwald's interpretation to be credible, the answer would have to be Hertz's mention, in his preceding paragraph, of some observations involving wire gauze. My answer, however, is that the obvious candidates are the current mapping results from his earlier experiments. Let us compare these responses.

Hertz's only discussion of wire gauze prior to this occurs in a list of observations that are naturally explained by his claim that the path of the current is distinct from that of the cathode rays. There he mentions that he has seen cathode rays emerge from fine wire gauze and that the possibility of this effect follows 'almost as a matter of course when we regard the cathode rays as a disturbance which is quite independent of the actual discharge, and no more connected with it than the light which radiates from the discharge', while it is 'difficult' to explain this otherwise (1898, p. 248). If we recall the care Hertz takes to justify his method of tracing out current paths,[11] I do not believe we can view this brief mention of the wire mesh as a previous result that supports his claim that cathode rays in the protected region of his tube are pure. Viewing his comment this way is even more problematic when we notice that Hertz believes his observations (of cathode rays penetrating wire meshes) are consistent with a belief that 'cathode rays consist of streams of electrified material particles' at the potential of the cathode (1898, p. 253). So while it is 'difficult' to explain the effect, from a charged particle point of view, it *is* possible according to Hertz. Thus he cannot be claiming that his results from before *establish* the purifying effect of the gauze.

The arrangement of Hertz's text may have something to do with the attractiveness of viewing the mesh as a current purifier. It might seem too much of a coincidence that Hertz should make his first mention of the wire gauze immediately before he employs it in his tube. Indeed, the fact that he then refers to the rays as pure after they have passed through the gauze might make Buchwald's interpretation appear quite plausible—and it provides, moreover, the bulk of his evidence. However, Hertz's wording and his preceding experiments make this interpretation unlikely. If, as Buchwald maintains, the power of the mesh to exclude current is convincing enough for Hertz to rely on it, *without even mentioning its purpose*, then why go to such elaborate lengths to convince others that current is distinct from cathode rays? Why not simply report those experiments in which he has seen the rays emerge from fine wire gauze? The problem, as Hertz says himself, is that it is not impossible to explain such an observation under the assumption that cathode rays track the path of the current. The observations alluded to can only be useful to Hertz as supporting his view of the distinction between current and cathode ray because they are explained most naturally by admitting this distinction. They cannot serve as *independent* justifications for his views on ray purity. Hertz is thus not in a position to argue that current cannot flow through the mesh into the tube without appealing to the fact that currents and rays are naturally distinct. But then we do not need the mesh. Once again, it appears unlikely that Hertz would use, for his most potent experimental resource, a device which he has not introduced as such (and which we shall see has an important alternate purpose)

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[11] There Hertz both cites a mathematical proof in Maxwell's treatise and describes a physical argument of his own to justify his current tracing technique (1898, p. 242). This all occurs after the researches with the cylindrically symmetric cathode/anode pair.

and which, moreover, he has good reason to believe can be consistently explained as not doing its job (i.e. as being not opaque to current).

The preceding considerations make it appear reasonable to conclude that the referent of 'previous results' is the current-tracing experiment. In addition, we have seen Hertz cite this experiment in a quite similar context. There he says (1898, p. 247) of his mercury experiment: '[A]fter what we have already said, there can be no doubt that the current-paths are restricted to the immediate neighbourhood of the electrodes'. On my reading, it then follows that the separation of cathode rays from current occurs as a matter of course once the rays are out of the immediate neighbourhood of the cathode/anode system.[12]

Buchwald (1998) claims that the *nature* of the charge measuring experiment is very different from that of either the mercury experiment or of the current-mapping experiment. In these earlier experiments, he says, Hertz was primarily concerned with the visual features of the rays rather than with their electromagnetic properties. Thus, the magnitude of the errors (in locating the current) involved in these earlier experiments might be perfectly acceptable *there*, but unacceptable in the charge measuring experiment. Let me make two points about this issue. First, this conclusion seems at odds with the use Hertz makes of the earlier experiments. He justifies, as we have seen, his claim that the cathode rays entering his detector are pure, by appealing to the evidence of his current-mapping experiment. Once again, this appeal relies precisely on the natural separation between ray and current which he demonstrates in that experiment. Second, while it is true that the *rays* in the current-mapping experiment are examined visually, the *current* is mapped with an elaborately defended measuring technique—a technique, moreover, that Hertz (1898, p. 242) considers to be extremely sensitive. The issue, thus, does not concern two ways of measuring the properties of the rays. Instead, the issue concerns the presence or absence of current in various regions of the tube. Hertz has justified to his own satisfaction that currents are restricted to the immediate neighbourhood of the cathode/anode system, and that they are *naturally* so restricted. He thus has no need to restrict them himself.

My more general view, that the gauze was not a critical piece of current-excluding apparatus, finds further support in Hertz's cavalier attitude toward the permeability of his device. The device leaks charge but Hertz is unconcerned: 'As a matter of fact, then, electricity does penetrate through the wire gauze into the protected portion of the tube until its entrance is prevented by the rise of potential. We shall not here establish the laws which underlie this penetration of electricity; it is enough that it has nothing to do with the cathode rays' (1898, p. 251). Electricity can pass through. But note Hertz's language—*electricity*, not *current* penetrates the protected region. Current is confined naturally to the cathode/anode system.

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[12] Buchwald (1998), again in correspondence, has agreed with my reading of 'previous results'. However, he does not agree that the natural purity of the rays, in the charge-catching experiment, follows from such a reading. This issue will be addressed below.

One might argue that the only source of electricity is either current (terminating on some inconvenient location within the tube), or the cathode rays themselves. Hertz does not himself make this argument. As his remarks above make clear, some further laws, beyond those he has already discussed, are governing this case. Perhaps the charge results from a species of the 'electric wind', associated with highly charged surfaces, blowing from the cathode to anode (cf. Maxwell, 1954, p. 59). What is certain is that, for Hertz, what Buchwald calls 'tube current' terminates on the anode. This is made clear in the two experiments referred to above. In the experiment with the cylindrical cathode, the claim that the current lines form closed-ring magnets relies crucially on this assumption. For this claim to make sense, there must be exactly as much current flowing from the cathode as flows into the anode. There is not the slightest suggestion, there, that the small deflections he does observe have anything to do with 'residual' currents that are not terminating properly.

Even stronger evidence that Hertz believes 'tube current' must terminate on the anode is found in the current mapping experiment itself. The technique Hertz uses in this experiment is justified at great length. He is using novel tools under a novel interpretation to do what has not been done before—map the actual path of the current. The legitimacy of every step of his procedure is carefully argued for, and these steps are meticulously carried out. However, at the end, he rejects his own measurements (or, as one might say, he massages his measurement data in light of his theoretical preconceptions). His diagrams clearly show that magnetic equipotential lines terminate not only on the anode but also on the brass enclosure of the tube. Rather than attempting to explain these results, Hertz simply rejects them. Any measurement of current that does not terminate on the anode is incorrect. He says: 'In order to obtain the actual current-lines from the equipotential lines here drawn, we must imagine the end-points of the latter joined to the electrodes, and the lines themselves compressed somewhat more towards those places in which they are closest. It is clear that the actual current-lines could never cut the sides of the vessel, as the lines in our drawing do' (Hertz, 1898, pp. 244–245). There is no suggestion here of residual currents. Rather, Hertz's technique is used to show how the current lines, which begin on the cathode and end on the anode (as, for Hertz, they must), distribute themselves in the intervening space.

One might argue that these two devices are of a very different nature from the one Hertz uses in the charge-catching experiment. Perhaps, because of the different arrangement of cathode and anode, the latter device can allow current that does not terminate on the anode.[13] This is unlikely for two reasons. First, as we saw, Hertz must rely on the similarities between this tube and those in the current-mapping experiment or he cannot justify his claim that purity *here* follows from purity *there*. Second, the tube in the charge-catching experiment is

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[13] Buchwald (1998) has made this suggestion in correspondence. There he connects up the difference in the devices to the difference in the experiments to argue that the earlier experiments do not demonstrate the 'natural purity' I advocate.

the only one in which the path from cathode to anode is direct—thus it is 'easier' for the current to get to the anode in this case. In the square tube, the cathode is sometimes on the side adjacent to that of the anode. But since current flow must begin perpendicularly to the cathode surface (because all electric fields at conducting surfaces are perpendicular to the surface), it must then turn about to arrive at the anode. Likewise, in the mercury experiment with the cylindrical cathode, as the current leaves the cathode, it must begin by flowing away from the anode and down the tube before it turns back around. In the charge-catching experiment, there is no need for the current to even get to the tube side of the anode. A convenient path exists directly to the anode from the cathode. One might imagine the current moving past the anode and then terminating on its backside. However, such a possibility is even more likely in the square tube case, and Hertz never considers it. For Hertz, currents go as straight as they can toward the anode, and rays that exist away from this locus are pure without the need for experimental intervention. For him, current does not spring out willy-nilly from the cathode surface only to have the bulk of it 'sucked away' into the anode as Buchwald (1995, p. 162) suggests. Rather, '[r]oughly speaking, the distribution of current in its flow from pole to pole is similar to what it would be in a solid or liquid conductor' (Hertz, 1898, p. 245). However seriously *we* take this remark, it appears clear that *Hertz* takes it seriously enough to rule out anything like 'residual' currents—despite his own observations which would tend to undermine it. Current is what begins at the cathode and flows as directly as possible to the anode.

Except for providing illumination about how Hertz conceives of current, however, this issue is ultimately of secondary importance. For whatever the source of the electricity, it is clear that Hertz is unconcerned by it. Some electricity does get into the tube (through the mesh, of course, since that is where the holes are) but his experiment is unaffected. This electricity, Hertz (1898, p. 251) says, has 'nothing to do with the cathode rays'. The electricity appears as soon as the device begins to operate. Then, as the potential within the tube increases, any further electricity is excluded. But cathode rays continue to enter the tube long after the electricity has reached its peak. They then cannot be responsible for the electricity (Hertz, 1898, p. 251). By the same token, if Hertz's purpose with the wire gauze were to exclude interfering current, he need not have bothered. The rise in potential alone accomplishes this in a very short time. Since Hertz (1898, p. 251) recognises this, it seems unlikely that he could fault an experimental replication of his measurements simply because the gauze is missing. Stray electricity, whether from current or some other source, plays such a minor role that Hertz cannot be bothered to explain how it originates: why would he demand greater diligence from others?

But what about the wire gauze? Is it not of some crucial importance? Can we possibly understand what Hertz is doing if we do not explain his dependence on this critical piece of apparatus? It is interesting to note that a similar wire mesh is investigated in Maxwell's *Treatise*. He there devotes eight pages (Maxwell, 1954, articles 203–206) to explaining and justifying the use of such wire screens[14] as electrostatic shields. These screens are not as effective as full metallic surfaces but are still quite effective for those situations where it is important to be able to see the apparatus enclosed in the protective shield (Maxwell, 1954, pp. 310–318). It is certain that Hertz was familiar with Maxwell's work since he cites (1898, p. 242) a theorem from the 1873 edition. While Hertz does not need to *see* into the tube, he does need to *get the cathode rays in*. Maxwell is concerned about cases where one might wish to observe some apparatus in the absence of electrostatic effects. Here, Hertz wants to bathe his detector in cathode rays in the absence of disturbing electrostatic effects. He has already noted (1898, p. 247) that cathode rays are absorbed by infinitely thin layers of solid matter, so it is imperative that he provide entry routes for his rays.[15] I believe, therefore, that there is nothing mysterious about Hertz's wire gauze. It is simply a device, already in common use by 1873, for providing approximate electrostatic shielding.[16] Hertz has already explained the outer case surrounding the whole device. It is there to prevent the electrostatic interference which arises from, among other things, the cathode (the only influence he names). He says (1898, p. 250) that the metal cage protects the part of the tube lying 'beyond the wire-gauze from any electrostatic forces which might be produced from without, *e.g.* from the cathode'. It would be strange if Hertz had recognised the potential for electrostatic interference by the cathode on the charge catcher and had done nothing to protect against the most immediate influence of the cathode—the direct-line influence through the hole in the anode. But of course he did. The mesh is nothing more than the final wall (with holes poked into it to allow ray entry) of a Faraday cage surrounding Hertz's detector.

I am not suggesting that Hertz was uninterested in pure cathode rays. Indeed his work crucially depends on such pure rays. However, he had no reason to go out of his way to try to *produce* these pure rays. His investigations have shown him that cathode rays away from the electrodes just *are* pure. As we can see clearly from the experiment with mercury vapour, Hertz has already manipulated and investigated rays in their purest form, and he did so without a wire mesh. Nor do I suppose that the wire mesh is unimportant, but only that it should not be viewed as part of an experimental programme to fully eradicate the contaminating influence of tube current. There are, in fact, only three tubes

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[14] Maxwell's mesh is merely parallel strips of wire rather than a square-grid array of wires.

[15] As Hertz (1898, p. 250) remarks: '[A]t low densities [the cathode rays] cause the glass at B to shine with a brilliant green phosphorescence, upon which the shadow of the wire-gauze is plainly marked'. It should be clear from this that Hertz could not have protected his detector entirely from electrostatic effects (by e.g. surrounding the *entire* tube with a solid enclosure) *and* introduced rays into the tube.

[16] I am not aware of how common this technique was by the time Thomson was doing his own replication. However, Maxwell refers to it as an often used technique and by December of 1891, Thomson has already brought out a new edition of Maxwell's *Treatise* and is certainly familiar with the technique. I find it very implausible that Thomson would simply have *overlooked* the gauze, but as we will see below, he could safely *ignore* it.

mentioned in Hertz's report. In all three Hertz has found pure cathode rays in the absence of current, and in only one does the wire gauze (or any suitable analogue for purifying charge) appear. The inescapable conclusion seems to be that the only purpose for the gauze is to shield the tube, as much as is practicable, from static field interference. According to Hertz, in the charge-catching experiment the detector operates through the action of electrostatic induction. Charge within the device induces a surface charge on the detector which registers on the electrometer. Thus it is crucial to protect against disrupting fields *not* produced by charges entering the device. Without the gauze, the interior of the detector would be open to influence from the cathode which is just beyond the hole in the anode. Thus the gauze really is an essential piece of apparatus for Hertz. It is just that its purpose is to shield the tube from *electrostatic fields*—not *current*.

To summarise the results of the preceding two sections: (1) Buchwald suggests that the charge catchers belong *outside* the tube while the only evidence we have for Hertz's view on this matter suggests that the charge catcher belongs *inside* the tube. Thus either Hertz is simply confused about his own analogy or the location of the charge catcher is, for him, irrelevant. (2) Buchwald suggests that only with the metal gauze in place could Hertz find an experimental space filled with rays and free of tube current. Again Hertz's text is at odds with this conclusion. We see Hertz manipulating pure rays (in the square tube the current is 'infinitesimal' and in the mercury case there are 'only' rays) in the absence of the gauze. Moreover, while Hertz is *explicitly* worried about electrostatic interference,[17] he never mentions the possibility that current might insinuate itself into the detector. The only plausible use for the gauze is as an *electrostatic* barrier.

5. Replication Affirmed

We are now in a position, finally, to address efforts to replicate the charge-catching experiment. I will not attempt to argue that Perrin's experiment would fully satisfy Hertz. There are two reasons for this. First, it is common to view Thomson's experiment as the decisive rejoinder to Hertz. It is consequently sufficient to answer the question of replication in the affirmative in his case alone. Second, the proper attitude to take toward Perrin's experiment is not as clear as Thomson and Buchwald make it seem. The second half of Perrin's paper is an investigation of the effects of interchanging the role of cathode and anode in his previous experiment. Here he finds induced electricity of opposite sign while the tube current supposedly responsible for his earlier effect is now flowing *away* from the charge catcher (Perrin, 1895, pp. 581–583). This suggests the possibility that Perrin's results might prove much more robust than is com-

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[17] Electrostatic interference is apparently weighing heavily on Hertz's mind. His battery has failed, and so he 'had to make use of the discharges of a small induction coil. On account of their irregularity and suddenness these are very ill adapted for electrostatic measurements' (Hertz, 1898, p. 249).

monly assumed.[18] Let me merely point out here, however, that the only obvious defect in Perrin's device is the large distance from cathode to anode. As I argued above, in other respects Perrin's device is nearly indistinguishable from Hertz's. While one might argue that this large separation makes Perrin's device unreliable, I do not know that Hertz would.[19] Hertz was concerned all along to convince others that current and cathode ray are distinct. Thus to show to *his* satisfaction that a region is free of current might be considerably easier than Buchwald makes out. Rather than pursuing this issue, however, I will turn to Thomson's experiment.

As I pointed out above, Thomson's device appears quite different from Hertz's. Thomson's charge catcher emerges off to the side preventing cathode rays from flowing directly into it (Fig. 5). Moreover, his Faraday cage is nearly flush against his detector, thus inadequate to protect it from outside influence. As we will see, however, the device is indeed adequate as a replication of Hertz's experimental apparatus. First note that the charge catcher is at the far end of the tube. Thus, whatever view one takes on the real purpose of Hertz's gauze, there is still no tube current at the location of the charge catcher, *even by Hertz's* (1898, p. 247) *own standards*.[20]

But what about the lack of *electrostatic* protection in Thomson's experiment? Assuming that I am correct in my assessment of the role of the wire gauze, would not Hertz claim that Thomson's efforts fail because he did not adequately protect the charge catcher from electrostatic pollution? Here the geometry of Thomson's tube takes on crucial significance. The charge catcher is off to the side and no cathode rays can enter it unless they are directed there by a magnetic field.[21] For this reason, Thomson can ignore electrostatic effects; he has transformed them into background effects. When Thomson's device begins to produce cathode rays, he does, as a matter of fact, get oscillations in his electrometer (Thomson, 1897, p. 585). These oscillations are, presumably, caused

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[18] I should point out that Buchwald as well believes that something significant may be going on in Perrin's experiment (1998). I take it, though, that he would view Perrin's experiment as a *new* one in this case and, thus, irrelevant to his own views on Hertz and replication.

[19] Buchwald would perhaps argue that the great distance from cathode to anode magnifies the region in which current lines can be found, so that now the 'immediate neighbourhood' of the cathode/anode pair would be greatly expanded. Yet, in the current tracing experiments, the distance from cathode to anode is sometimes this large, while there is *no* current *beyond* the anode. Thus this 'flaw' in Perrin's device may be of no consequence.

[20] I will make just one more remark about current purification. Despite my view that purification was not at all the point of Hertz's gauze, I should point out that, even granting Buchwald's interpretation of the gauze, Thomson would fare at least as well as Hertz. Look carefully at Thomson's diagram. One thing to notice is *his* anode. It is a large metallic plug with a channel bored through it. The much greater surface area of this channel would be much more effective at 'sucking away' current than would a single layer of wire mesh—if such 'sucking away' were indeed going on. Thus, it seems to me that, on whatever interpretation we give to Hertz's gauze, Thomson's device would have been entirely satisfactory in terms of isolating and examining cathode rays in their purest form.

[21] Naturally, given my earlier description of Perrin's device, I think his experiment has also transformed any residual electrostatic effects into a stable background. Again, I will not pursue Perrin's experiment here.

by electrostatic interference from the cathode (including his induction coil voltage source). Because the order of magnitude of these oscillations is negligible with respect to the measured effect produced by the cathode rays, they amount to a stable background for Thomson. In contrast to Hertz, for whom the path of the rays is fixed, Thomson does not need to shield his device from low-level external fields. His off-axis charge catcher affords him a unique opportunity to transform them into stable background effects. Repeatably and at will, Thomson can direct the cathode rays into the charge catcher. Once he does so, the effect on the electrometer grows by several orders of magnitude. Thus external fields are clearly incapable of explaining the measured charge—the cathode rays must be carrying it. While Hertz relies on a Faraday cage to suppress electrostatic interference, Thomson can use his power over the cathode ray's access to his charge catcher to transform this interference into background.[22]

Buchwald (1995, p. 164) illustrates his evaluation of Thomson's attempt to replicate Hertz's experiment with a Radderian table. The idea behind these tables is to graphically compare the elements that are deemed necessary for a successful experiment, according to the background beliefs of the scientists conducting the experiments (cf. Radder, 1995).

I reproduce the columns for Hertz and Thomson below.

Hertz		Thomson	
Result: Rays do not carry charge		Result: Rays carry charge	
1.	Anode close to cathode	1.	Anode close to cathode
2.	Current trapped past anode	2.	*Irrelevant*
3.	*Irrelevant*	3.	Charge catcher not on cathode anode axis

In light of the preceding problems with Buchwald's evaluation, it might be helpful to see my corrections to this table. Categories 2 and 3 from Buchwald's table can now be seen to be instances of the same category—the category of elimination of unwanted electrostatic interference. Hertz' hides his detector from electrostatic fields while Thomson removes his detector to a region where these fields provide no more than a background effect.

Hertz		Thomson	
Result: Rays do not carry charge		Result: Rays carry charge	
1.	Anode close to cathode	1.	Anode close to cathode
2.	Faraday cage around entire apparatus	2.	Charge catcher not on cathode anode axis

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[22] Recall that Thomson's detector is itself very nearly surrounded by its own Faraday cage. Even so, as I mentioned before, the protection is not absolute because of the proximity of detector to cage wall. However, Thomson need not go to Hertz's elaborate lengths to correct the electrostatic permeability of his detector. The off-axis nature of Thomson's device affords him sufficient control over the effect.

This is then a graphic representation of the theoretical context shared by Hertz and Thomson. The anode close to the cathode assures us that they had similar ideas about how to isolate cathode rays from the current. Each uses a device to eliminate the effects of electrostatic interference that, I believe, the other would have understood and recognised as appropriate to the task.

6. Conclusion

Buchwald's focus on novel experimental *resources* provides us with an illustration of the subtle problems associated with judging matters of experimental replication. When we approach Hertz's practice from this perspective we find that certain of his resources (most notably the wire gauze) were either not noticed or simply ignored by subsequent experimenters. It is natural to conclude from this, as Buchwald does, that these subsequent experimenters have missed issues central to their predecessor's understanding of his practice and thus have not successfully replicated his experiments. Consider Buchwald's following remarks:

> In retrospect we think that Hertz was strikingly limited by the gas pressures he could achieve. Because I knew this, I came to Hertz's papers with the notion that gas pressure was unlikely to have been something to which he paid much attention. This constraint directed my attention away from what Hertz could not do much about (gas pressure) and toward what he could do (ray purification) (1995, p. 169).

By focusing on what Hertz 'could do', Buchwald has little trouble finding something. He decides on ray purification and forces his understanding of the text to fit that decision. Once his choice is made it is not possible for Buchwald to appreciate Hertz's discovery that rays purify themselves. Ray purification is a novel effect produced by application of Hertz's experimental resources.

On the other hand, adopting the perspective of experimental *goals* forces us to cast a broader net. We focus our attention first on the requirements of the experiment itself. What is necessary for the production of the effect? For measuring the effect? For eliminating unwanted background influences? Only when we have answered these questions can we move on to the question of *relevant* replication. Rather than asking if Thomson utilised Hertz's mesh, we ask if Hertz's answers to the above experimental questions are suitably represented in Thomson's experiment.

Buchwald finds that the first 'pure' rays in Hertz's experiments are produced after the introduction of the wire mesh. He concludes from this that the purpose of the mesh is ray purification. This perspective on the matter, I suggest, stems from Buchwald's focus on novel resources as the driving force behind his investigation of replication. By beginning from the issue of experimental goals, however, we are led to spend more time focused on the nature of Hertz's *effects*.

When we find that his earlier rays also meet his definition of purity, we begin to understand that the gauze plays no role in the production of pure rays. The purpose of the gauze is then an open question.

Rather than attempting to answer this question directly, we turn instead to Hertz's technique for eliminating background influences. We find that Hertz is deeply troubled by electrostatic interference. To eliminate this interference, Hertz surrounds the entire charge-catching apparatus with a metal box—or almost the entire apparatus. A large (on electrostatic scales) hole is left through which electrostatic influences could propagate from the cathode to the charge catcher. However, covering this hole is Hertz's gauze. The purpose of the gauze is then clear. When we find that the gauze serves as a barrier to electrostatic influence, it is no longer necessary to look for the corresponding device in Thomson's experiment. Instead, the issue became: 'How does Thomson deal with electrostatic interference?'. In Thomson's device, distance and control over the path of the rays are the keys. Rather than excluding electrostatic fields, Thomson mitigates their influence. On the surface, the techniques of the two men are quite dissimilar. However, I believe that they are *relevantly* similar. Neither technique has to do with the production of the effect but rather with background interference. Neither technique introduces a new influence which needs to be accommodated. Rather both techniques serve the same passive protective function.

Did Thomson replicate Hertz's experiment? I believe he did. Thomson mimicked Hertz's charge catcher with a relevantly similar device. Although he used a different technique to eliminate unwanted electrostatic effects, I see no essential differences this introduces into the theoretical context he shares with Hertz. At issue is whether the 'same' rays produce an effect on the 'same' device. They do. Hertz was wrong.

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