# THE OPTIMISTIC META-INDUCTION AND ONTOLOGICAL CONTINUITY: THE CASE OF THE ELECTRON

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> *The first observer to leave any record of what are now known as the Cathode Rays [subsequently renamed 'electrons'] seems to have been Plücker.* (J. J. Thomson, May 1897, p. 104)

**Abstract** The pessimistic meta-induction attempts to make a case for the lack of ontological continuity with theory change; in contrast, its rival the optimistic meta-induction makes a case for considerable ontological continuity. The optimistic meta-induction is argued for in the case of the origin, and continuity, of our talk of electrons (even though the term "electron" was not initially used). The case is made by setting the history of identifying reference to electrons in the context of a generalised version of Russell's theory of descriptions, Ramsey's theory of theoretical terms and a development of these ideas by David Lewis.

**Keywords** Pessimistic Meta-Induction, Electron, Scientific Realism, Ramsey Sentence, Russell's Theory of Descriptions, Ontological Continuity and Theory Change.

1. *The Pessimistic Meta-Induction versus The Optimistic Meta-Induction*
2. *Russellian Descriptions, Ramsey Sentences and Lewis on Fixing Denotation*
3. *Julius Plücker's Experiments with Geissler Tubes*
4. *Hittorf, Goldstein, Cathode Rays and Canal Rays*
5. *Identification and Rival Theories and Models of Cathode Rays*
6. *Thomson and the Identification of Cathode Rays Outside the Cathode Ray Tube*
7. *The Term "Electron" andIts Multiple Introductions in Physics*
8. *Continuity in Ontology from Classical to Quantum Electrons*
9. *Conclusion*

When philosophers wish to cite an example of a theoretical entity whose existence has been established by science, more often than not they cite the electron. Scientists and most philosophers of science tend to be realist with respect to electrons; that is, they think that electrons exist in a strong sense independently of any minds or any theories or languages minds might use to talk about electrons. Millikan even thought that he could see electrons, telling us in his 1924 Nobel Prize lecture:



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> He who has seen the [oil-drop] experiment, and hundreds of investigators have observed it, has literally seen the electron.… But the electron itself, which man has measured … is neither an uncertainty nor an hypothesis. It is a new experimental fact that this generation in which we live has for the first time seen, but which anyone who wills may henceforth see. (Millikan, 1965, pp. 58–59; emphasis in the original)

Realists might demur from such a strong perceptual claim, saying that even if Millikan had an electron directly in front of him, he did not have an electron as an item in his field of vision since they are too small to be seen by our eyes. At best he saw some characteristic movements of oil drops in an electric field and inferred that electrons were present as they hopped on or off the oil-drops causing them to move up or down. But at least Millikan's position connotes a strong realism about electrons, as strong as that we suppose about the ordinary objects we see.

Such a robust realism has been questioned in a number of different ways; the focus here is on just one, the Pessimistic Meta-Induction (PMI). The argument comes in a number of different forms. In Sect. 1 the version first given currency by Putnam is discussed. The argument raises the possibility that Rutherford, Bohr, later Quantum theorists, etc. were not talking about the very same entity, the electron, owing to the very different theories they held about electrons. It will be argued here that PMI should not be accepted; in its place OMI, the Optimistic Meta-Induction, will be advocated. In the case of the electron this means that despite radically changing theories, scientists did talk about the very same thing, electrons, even though they did not use the term "electron" for a considerable time. How is this to be shown? Section 2 sets out a version of Russell's theory of descriptions generalised to apply not only to individuals but to kinds like the electron. It is shown that Russell's theory is a special case of the Ramsey Sentence as developed by David Lewis. This background semantic theory is employed in subsequent sections to show how the same entity can be identified despite changes in theory of the entity so identified. It will emerge later that such descriptions carry the burden of fixing a denotation (despite the quite different names used to denote the kind of entity so identified) in contrast to the full Ramsey Sentence which shows how reference can fail.

Section 3 considers Plücker's 1857 discovery of an unusual phenomenon and the postulation of a "something" as cause of it. Section 4 considers the growth in our knowledge of the "something" and some of the names that were introduced to refer to it, "cathode rays" being the name most widely adopted. Section 5 considers how different theories about the very nature of the "something" were proposed, from wave to particle, while there was continuing reference to the same "something". This requires an account of how the Ramsey-Lewis procedure can be used to identify the same "something" without invoking the full theoretical context in which a term occurs. Section 6 considers Thomson's two important papers of 1897, his summary of relevant experimental knowledge of the time and his unusual theory about the "something". It is in this context that the difference between the use of Russellian descriptions and the full Ramsey sentence becomes important; the former enables ontological continuity to be established while the latter shows how much of what Thomson wanted to say about his "corpuscles" could not apply to any entity. Section 7 makes some brief comments on how the term "electron" was introduced into science in multiple ways; but what is important here is the dependence of each introduction on already well-established definite descriptions. The term "electron" might be multiply ambiguous but this carries no threat of radical incommensurability in science.

Section 8 makes a link to the historical story told in Bain and Norton (2001) which begins with the classical theory of the electron and continues it up to various current quantum theories of the electron. The semantic issues raised by the Lewis version of the Ramsey Sentence come to play an important role in forging this link.

When historians of science come to talk about some episode in the history of science, such as the development of our views on electrons, they often speak of the *concept* of an electron (either with or without relativisation to the individual who entertains the concept, a period of time, etc.). However, the concept of a concept is a notoriously difficult one in philosophy, and given our unclear understanding, it might not be able to discharge the burden that is placed on it, especially in its use to analyse what is going on in science. One difficulty arises in talk of conceptual change. If a concept undergoes change (whatever this might mean), just how much change can it undergo and remain the same concept, and just how much change leads to a different concept? Historians of science get caught up in such matters concerning conceptual change. However, it would be much better to avoid them, and they are avoided here. A different approach would be to eschew concepts in favour of talk of sets of beliefs held by a person at a time. Then one can trace how the sets of beliefs differ from person to person, or from time to time. Here the flourishing theories of belief revision might be of better service than talk of concepts. Turning to a different matter, there should also be a focus on what the concepts are about rather than the concept itself. It remains notoriously difficult to say what the extension of a concept is and whether or not the extension changes with change in the concept. A way of avoiding this problem is suggested in Sect. 2 of the paper, and is then adopted throughout to show that despite radical changes in our "concept" of the electron it is the same thing that the different concepts are about. The traditional theory of concepts cannot really deal with this problem, but the development of the Ramsey sentence by Lewis outlined in the paper can accomplish this. Though we cannot eliminate all mention of concepts, problems that they generate for continuity of ontology can be by-passed.

## 1. THE PESSIMISTIC META-INDUCTION VERSUS THE OPTIMISTIC META-INDUCTION

Putnam expresses one form of the Pessimistic Meta-Induction, PMI, in the context of discussing Kuhnian and Feyerabendian incommensurability, illustrating it with the (alleged) incommensurability of the early Bohr-Rutherford theory of the electron compared with the later Bohr's theory of the electron, and even our current theory of the electron. Is it the same item, the electron, which is being referred to in all of these theories, i.e., is there referential invariance with theory change? Or are there at least three different items, the-Bohr-Rutherford-electron, the mature-Bohr-electron and our-current-electron, i.e., is there referential variance with theory change? Putnam puts the issue in the following general terms:

> What if *all* the theoretical entities postulated by one generation (molecules, genes, etc., as well as electrons) invariably 'don't exist' from the standpoint of later science?'…. One reason this is a serious worry is that eventually the following meta-induction becomes overwhelmingly compelling: *just as no term used in science of more than fifty*

> (or whatever) *years ago referred, so it will turn out that no term used now* (except maybe observation terms, if there are such) *refers*. (Putnam, 1978, pp. 24–25; emphasis in original)

Whether Putnam draws our attention to the meta-induction as a cautionary story to be avoided or a consequence to be welcomed need not detain us; but he does subsequently emphasise that it would be a desideratum of any theory of reference that the argument to such massive reference failure be blocked. Whatever its provenance, PMI has come to have a philosophical life of its own.[1] To set out the induction more clearly, consider the scientific theories θ (relevant to some domain) that have been seriously proposed[2] by scientists over a given period of time. The induction has the following premise:

> For any scientific theory θ seriously proposed at any time t in the past, and whose distinctive theoretical terms were alleged at t to denote some entity, there is some reasonably short span of time N (e.g., 50 years) such that by the end of t + N the theoretical terms of θ were discovered not to denote. (Semantically descending we can say the items in θ's ontology do not exist.)

From this premise we can make the inductive prediction:

> The terms of the theories we currently hold at t = now, will at t + N be shown not to have had a denotation.

We can also make the following inductive generalisation:

> For all theories proposed at any future time t, by later time t + N their theoretical terms will be shown to lack denotation.

From either conclusion we can inductively infer the following pessimistic conclusion: the theoretical terms of our current scientific theories do not denote. Semantically descending we can say that the items postulated in the ontologies of our various theories do not exist. Such a pessimistic conclusion is to be distinguished from a more general kind of philosophical scepticism. Though the conclusion of PMI is close to that of a philosophically based scepticism concerning whether our theories are ever about anything, the considerations invoked based in the history of science are not the usual sort found in premises for arguments about philosophical scepticism. So PMI has a special historical-scientific character.

The conclusions come into direct conflict with a standard conception of an ontological, or metaphysical, realism about scientific entities such as that proposed by Devitt (1997,

1 There is another version of PMI to be found in Laudan (1981) that differs from the Putnam version in that it puts emphasis on the empirical success of theories not mentioned in Putnam's version of the argument. But the conclusion is much the same in that from empirical success of a theory one cannot reliably infer to any referential success. Laudan's argument has been criticised in Lewis (2001) on the grounds that in arguing from the history of science it commits a sampling fallacy. Neither Laudan's version of the PMI argument nor its alleged shortcomings will be discussed in this paper.

2 The qualification "seriously proposed" is to prevent the inclusion in the inductive base of frivolously proposed theories that lack minimal epistemic credentials. The induction is intended to be over those earlier theories that (a) featured in the active history of some science, (b) engaged the attention of a number of working experimentalists and theoreticians who made a contribution to their science over some historical period; (c) finally were seriously proposed in that the theories meet minimal epistemic criteria which, if they did not lead to acceptance, at least led scientists to work on them either to show they were false (as in the case of theories of N-rays) or to show that they had some positive evidence in their favour. This qualification is intended to meet an objection, raised by Laudan to an earlier version of this paper, concerning lack of mention of epistemic warrant for the theories in the inductive base.

Sects. 2.3 and 2.4). Ontological scientific realism is to be understood as an overarching empirical hypothesis which says that *most* of the unobservable posits of our current scientific theories exist in a mind-independent manner. The qualification "most", though imprecise, is meant to highlight the point that only foolish realists would claim that whenever our theories postulate unobservables they are *always* right about their existence; realists need to be cautious since our theories are sometimes wrong about what exists. A modicum of fallibilism is an appropriate accompaniment to the formulation of scientific realism.[3] However for the advocate of PMI such qualifications are unavailing since they wish to make the very strong claim that the empirical thesis of realism is refuted; our theories never put us in touch with unobservable existents. Since realism and the conclusion of PMI are contradictory the (fallibilist) realist needs to disarm PMI.

The inductive inference to both conclusions appears to be strong (but as will be seen it fails the *Requirement of Total Evidence*). Assuming this, the challenge to the argument shifts to the truth of the PMI premise. What truth it contains arises from a number of historical cases in which the central theoretical terms of theories have been shown not to denote, for example theories about non-existent items such as celestial spheres, epicycles and deferents, impetus, phlogiston, caloric, electromagnetic aether, and N-rays. A cautiously formulated realism should not be overthrown by these well-established examples of non-existents which have been postulated within science. The qualifier "most", allows the cautious realist to admit these past failures without abandoning ontological realism. The advocate of PMI will reject this defence claiming that realism is still refuted; the number of cases of theories that have postulated non-existent entities simply overwhelms the cautious "most" rendering it ineffectual as a way of saving realism. But can this response be sustained?

Following a point made by Devitt[4] let us suppose that our very early scientific theories concerning some domain (e.g., theories of combustion or light), proposed, say, *c*.1800, were seriously wrong about what unobservables exist (suppose few or none of their theoretical terms successfully denoted). Can we reliably infer that our theories about the same domain are now equally wrong about what exists? Carnap's *Requirement of Total Evidence* bids us take into account all the relevant information before determining the strength of the PMI inference. There is some important missing information concerning methodology that becomes highly relevant. There is an assumption that our current scientific methods for getting us in touch with what exists are no better than they were *c*.1800 for doing this. We ignore the fact that there may well have been methodological improvement and that our methods are now much more reliable for putting us in touch with what exists then they were *c*.1800. So given the relative methodological poverty of some science *c*.1800, the theories proposed in the light of these methodologies were also poverty-stricken concerning the extent to which they put us in touch with unobservables. Let us suppose that by 1900 there was considerable methodological improvement with corresponding improvement of the extent to which scientific theories *c*.1900 were able to put us in touch with unobservables. And

3 For further qualifications along these lines see Devitt (1997, Chap. 2) and Devitt (2005, Sect. 2).

4 Considerations concerning methodological improvement are introduced in Devitt (1997, Sect. 9.4) and at the end of Devitt (2005, Sect. 4.2); this important point is employed here.

now, at the beginning of the twenty-first century our methods are quite reliable and our current science does put us in touch with unobservables (*modulo* the fallibilism of the cautious realist). This exposes a weakness in PMI; it ignores *The Requirement of Total Evidence* by remaining silent about a relevant matter, viz., whether or not there has been methodological improvement in various sciences. Granted this omission, PMI looks less promising.

Is the PMI premise true? If one were to take a proper random, statistical sample of theories over, say, a N-year period (e.g., take N = 50 years), it would appear, on a cursory investigation, that the frequency of cases in which there was no loss of denotation at the end of the N-year period would be far greater than those in which there was denotational loss. To illustrate, consider the list of chemical elements developed over the last 200 years or so. Apart from a few classic cases, such as that of the term "phlogiston" at the beginning of modern analytic chemistry (*c*.1800), there have been a very large number of cases in which once a term has been introduced to denote a chemical element it has continued to have successful denotation until our own time. The same can be said of the compounds discovered and named in analytic chemistry; there is hardly a case of denotational failure. Semantically descending, we can say that, within the chemistry of the last 200 years, an element or compound postulated to exist at an earlier time is still an item postulated in our current theories. Much the same can be said of the large number of subatomic particles discovered within physics from the late 1800s; apart from a few well-known examples such as N-rays, there has been much ontological continuity. And a similar point can be made about the kinds of entities postulated in microbiology (bacteria, viruses) and biochemistry. Proceeding differently, it would be possible to vary the length of the period, rather than adopt a fixed N (=50) year period, and take a proper random sample from different lengths of time period, from a few years to a century or more. Sampling over varying time periods hardly alters the verdict just given on the premise of PMI. Such a view is consonant with the idea that alongside our developing sciences there has been improvement in the reliability of the scientific methods used to show that some entity exists.

The premise of PMI is a false generalisation. Converted to a statistical claim the frequency of denotational loss would be low. Combining the failure to observe *The Requirement of Total Evidence* with the suggested counter-evidence from proper sampling across a range of sciences, PMI gives only very weak support to its conclusions, either as an inductive prediction or a generalisation about all theories.

In contrast to PMI, a rival *optimistic meta-induction*, OMI, can be expressed as follows (where the frequency mentioned is to be determined empirically, with the accompanying conjecture that the frequency will be high, or very high):

> For any scientific theory θ seriously proposed at any time t in the past, and whose distinctive theoretical terms were alleged at t to denote some entity, then for any span of time N (e.g., 50 years) the theoretical terms of θ are found, with high frequency, to have the same denotation at t + N as they had at t. (Semantically descending, there is a high frequency of continuity in the items of θ's ontology with any change in theory over time N.)

On the basis of this, one can make a strong inductive prediction concerning the next case of our current theories:

> [W]ith high frequency the terms of the scientific theories we currently hold at t = now, will at t + N be shown to still denote the same entities.

For OMI the strongly supported inductive generalisation would be:

> [W]ith a high frequency, the theoretical terms of theories at any time t, will at a later time at t + N still have their old denotata.

Assuming the historical work does establish the supposed frequencies, the conjecture is that OMI succeeds far more often over periods in the history of science than its rival PMI. This paper considers just one historical case; it will show that OMI is supported in the case of the electron while PMI is not supported, despite the growth in theories about the electron from the early classical theories to the very different Quantum theories.

Both PMI and OMI involve semantic ascent in that they talk of continuity, or lack of it, in the denotation of theoretical terms of some scientific theory. If one is not thoroughly deflationist, so that by means of the schema " 'N' denotes iff N exists", one deflates away all questions about denotation in favour of questions about existence, then there remains a substantial matter about the relationship between any theoretical term "N" used in science and items out there in the world. This is a relation that needs to be set out in terms of a theory of denotation. However both PMI and OMI are silent about what that theory might be. In fact, as Putnam points out while introducing the PMI argument, there is, lurking in the background, a semantic theory that gives PMI much of its punch. This is a strongly descriptivist theory of the meaning of scientific terms advocated by Kuhn and Feyerabend and many others. On their account there is a massive amount of incommensurability between theories that provides grist to the mill of PMI; the contextualist, descriptivism of the theory of meaning they adopt entails a rapid turnover of denotata for the same term occurring in only slightly different theoretical contexts. But there is also a contrasting theory of denotation to be found in Kripke (1980), and developed by others, in which descriptivism is downplayed; this theory exploits a much more direct causal connection between, on the one hand, the way a term is introduced and transmitted and, on the other hand, the item denoted. On a more narrow causal approach OMI tends to be supported rather than PMI. Thus it appears that PMI, and perhaps OMI, are not innocent of background semantic assumptions about how the denotation of theoretical terms is to be determined. The upshot of this is that PMI cannot be taken at its face value; it makes concealed presuppositions about how the denotation of terms is to be fixed that, if rejected, undermine any challenge it makes to scientific realism.

Which theory of denotation ought PMI, and OMI, employ? The conditions under which one is to employ a broad contextualist, descriptive approach, or employ a more narrow causal approach, are unclear; for some they are so unclear that they advocate a "flight from reference"[5] and eschew any attempt to invoke reference (denotation) at all. But it is not clear that in doing so PMI is also abandoned. The approach adopted in this paper is descriptivist in character; but it rejects a wholesale descriptivism since this can lead to serious trouble. In claiming this it is not assumed that a theory of denotation can be used to solve problems about existence. This is a matter left to science to

[5] The strategy of a "flight to reference" to resolve issues about what exists is criticised in Bishop and Stich (1998).

determine. But what is claimed is that a suitably crafted descriptivist theory of denotation goes hand in hand with both experimental discoveries in science and theorising about what has been experimentally discovered. To this we now turn.

## 2. RUSSELLIAN DESCRIPTIONS, RAMSEY SENTENCES AND LEWIS ON FIXING DENOTATION

### *2.1 Russellian descriptions and a principle of acquaintance*

In the following sections it will be shown how Bertrand Russell's theory of definite descriptions can be used to pick out unobservable items involved in experimental and other situations. The task of this section is to set out aspects of Russell's theory and to show that the theory is a special case of Ramsey's account of theories, or more strictly David Lewis' modification of Ramsey's original proposal.

On the classical Russellian theory, a necessary condition for the truth of a proposition, such as [(¶x)Dx]Ax, containing a definite description, (¶x)Dx, is that there is some unique individual denoted by the description; otherwise if no, or more than one, individual is denoted by (¶x)Dx, then the proposition is false. Russell's theory can be generalised to apply not only to individuals but also to kinds. The variable "x" is commonly understood to range over individual objects; but its range can be extended to cover kinds. In this case the description (¶x)Dx denotes the one and only kind K such that it uniquely satisfies the open sentence "D(-)". What a kind may be is something that will be left undefined; all that is assumed is an intuitive grasp of the notion of a kind as illustrated by physical kinds like electrons, chemical kinds like carbon dioxide, biological kinds such as tigers, etc. Finally if a description (¶x)Dx denotes a unique individual or kind, then a name "N" can be introduced for the individual or kind as follows: "N" denotes (¶x)Dx. Such name introduction will be illustrated in the next section for the supposed kind name "cathode ray". In such cases it is the description which carries the burden of fixing a denotation; the name merely serves as a convenient label to attach to the item the description denotes.

Russell put his theory of descriptions to several important uses one of which was epistemological in character. He was (at most times) a realist who thought that we could have knowledge not only of the items with which we are acquainted, but also items with which we are not, or could not be, acquainted.[6] Though his theory of descriptions originated in his 1905 paper "On Denoting" in connection with semantic issues, epistemological issues are not absent. Thus, using one of Russell's examples (Russell, 1956, p. 41), consider the description "the centre of mass of the Solar System at time t". This is a point about which we can say a large number of things in mechanics. But it is not something with which we can be acquainted (say, perceptually), either

[6] In some cases Russell thought that we could know only their extrinsic, structural properties and not their intrinsic properties; this is a matter not discussed here. But see Demopoulos and Friedman (1985) and Demopoulos (2003).

due to our position in the Solar System, or due to the theoretical and unobservable character of such a point. As Russell puts it, we cannot have knowledge of this point by acquaintance, but we can have knowledge of this point by description. Within Russell's epistemology the notion of acquaintance can carry strongly phenomenalistic overtones as when he claims that we are directly acquainted with, for example, the sense experiences to which tomatoes give rise, but we are not acquainted with the tomatoes themselves. However we are rescued from such a strongly empiricist, even Berkeleyan, account of the world; we can come to have knowledge by description about the tomatoes themselves if we cannot get such knowledge by acquaintance. Russell's overall position gives empiricism an important role, but it is not confined to empiricism. He shows how, using his theory of descriptions, we can transcend the items with which we are acquainted, such as experiential items, and adopt a robust realism about external objects of the physical world with which we are (allegedly) not acquainted.

This position is developed in his 1912 *The Problems of Philosophy* when he announces "the fundamental principle in the analysis of propositions": "*Every Proposition which we can understand must be composed wholly of constituents with which we are acquainted*" (Russell, 1959, p. 32, italics in original). For our purposes we need not go into Russell's account of what he means by "understanding a proposition".[7] And we can also set aside Russell's account of acquaintance in which we are only acquainted with sense-data (or universals and possibly ourselves, but not ordinary objects). On a more relaxed position that Russell also adopts in his 1905 paper, we can say that we are acquainted with ordinary objects such as tomatoes. The important step is the manner in which Russell's "fundamental principle" enables us, particularly in the context of science, to move from knowledge of those items with which we are acquainted (suppose these to be ordinary observable objects and happenings in experimental set-ups) to knowledge by description of that with which we are not acquainted (say, electrons, or centres of mass). The important step is made from (a) the items with which we are acquainted and for which we have names and predicates in some language which denote these items, to (b) items with which we are not acquainted but nonetheless we also have names and predicates in the language which denote the items with which we are not acquainted. This step, which takes us well beyond any empiricism embodied in (a) alone, can be made only if we also have at our disposal the resources of logic involving a theory of quantification, variables, logical connectives and the like. Using just these bare, logical resources, and the non-logical terms which refer to, or are about, items with which we are acquainted, we can form descriptions that pick out items with which we are not acquainted.

[7] For more details on this see Demopoulos (2003) who discusses Russell's account of understanding and the constituents of propositions; this is not a matter of significance here. But the use of Russellian descriptions made here is indebted to the story Demopoulos outlines from Russell to Ramsey, Carnap and others. See also Maxwell (1970) who early on recognised the connection between Russell's and Ramsey's theories. In this paper a link is also made to work on theoretical terms by David Lewis, 1983, Chap. 6.

### 2.2 *Russellian descriptions as a special case of the Lewis version of the Ramsey sentence*

In presenting Russell's account of definite descriptions in this way, a link can be made to the Ramsey sentence, and more particularly, to Lewis' development of it. Suppose we have a set of statements of a theory T which when conjoined can be expressed as (1), where the bold "**T**" is some function of theoretical terms, $t_1$, $t_2$, …, $t_n$, and observational terms, $O_1$, $O_2$, …, $O_m$, with the whole being equivalent to theory T:

$$\mathbf{T}(t_1, t_2, \ldots, t_n, O_1, O_2, \ldots, O_m). \tag{1}$$

The n theoretical terms commonly appear as kind names, predicates or functors. But as Lewis argues (Lewis, 1983, p. 80) they can be rendered as names of properties, relations, functions and the like, thereby enabling the use of only first order logic in what follows. The m "observational" expressions refer to, or are about, observables (which will include the items with which we are acquainted).

If all the theoretical terms are replaced by variables then one obtains the following open sentence (or Russell–Frege propositional function) containing only variables, logical expressions and non-theoretical or descriptive expressions $O_1$, $O_2$, …, $O_m$:

$$\mathbf{T}(x_1, x_2, \ldots, x_n, O_1, O_2, \ldots, O_m). \tag{2}$$

Denote the Ramsey sentence of (2) by '$\mathbf{T}^R$'. The Ramsey sentence is obtained by placing an existential quantifier in front of the open sentence for each of the variables and forming a closed sentence:

$$\mathbf{T}^R = (\exists x_1)(\exists x_2)\ldots(\exists x_n)[\mathbf{T}(x_1, x_2, \ldots, x_n, O_1, O_2, \ldots, O_m)]. \tag{3}$$

David Lewis' innovation (Lewis, 1983, especially Sects. IV and V) is to show how the n-tuple of objects which uniquely realise the open sentence (2) can have names introduced for them, one at a time, via the construction of a definite description. Thus for the first member of the n-tuple a name "$t_1$" can be introduced via the generalised definite description on the right hand side:

$$\begin{aligned} t_1 = (\P y_1)[(\exists y_2) \ldots (\exists y_n)(\forall x_1) \ldots (\forall x_n) \\ \{\mathbf{T}(x_1, \ldots, x_n, O_1, \ldots, O_m) \equiv (y_1 = x_1) \& \ldots \& (y_n = x_n)\}. \end{aligned} \tag{4}$$

As set out (4) expresses only the first of n − 1 other descriptions that enable name-introductions for each of "$t_2$", "$t_3$", … "$t_n$". The other n−1 descriptions are easily constructed and can be taken to be part of (4).

Clearly Lewis' procedure differs from that of Ramsey. Moreover it generalises Russellian descriptions in several ways. One way is that it shows how to introduce theoretical terms not just one at a time, but in pairs for two theoretical terms, in triples for three theoretical terms and so on for families of n theoretical terms of some theory T. As such they take into account not only the logical connections between theoretical terms and observational terms, but also the logical connections between the theoretical terms themselves.

For the sake of convenience and clarity, let us prescind from the generality of the above, and consider T to have just one name of a theoretical item, and thus one variable in its open sentence. Also conjoin all the observational terms $O_1, O_2, \ldots, O_m$ and abbreviate the conjunction by "O". Then we have respectively:

$$\mathbf{T}(t,O). \tag{1*}$$

$$\mathbf{T}(x, O). \tag{2*}$$

$$\mathbf{T}^R = (\exists x)\mathbf{T}(x, O). \tag{3*}$$

Lewis' modification of Ramsey's approach is, rather than place an existential operator in front of ($2^*$) to get a closed existential sentence ($3^*$), to introduce a definite description operator so that a generalised description is produced:

$$(\P x)\mathbf{T}(x, O). \tag{4*}$$

The last expression says that there is a unique item x such that x satisfies the open sentence "$\mathbf{T}(-, O)$". Also it is possible using the description in ($4^*$) to introduce some name "t" for the item described; this is simply a special case of the more general expressions of (4).

From this it is clear that Lewis' approach yields a Russellian definite description as a special case. For Russell the expressions $O_1, O_2, \ldots, O_m$ that comprise "O" denote items with which we are directly acquainted. In the context of science we can take these items to be observables. However Lewis imposes no epistemological condition on what can count as the O-terms, $O_1, O_2, \ldots, O_m$. These can be observational, but they could also be *old* or *original* terms whose meaning we already have grasped. Importantly they are O-terms in the sense that the meaning they have is obtained *outside* the context of theory T and not within it; in contrast the T-terms get their meaning within the context of theory T. This liberality in our understanding of O-terms is consistent with Russell's idea of acquaintance, broadly construed, and will be adopted in subsequent sections.

### 2.3. *Two useful modifications: imperfect satisfaction and ambiguous name introduction*

A common understanding of the open sentence ($2^*$) is that the item which satisfies it must be a perfect satisfier; if not then ($4^*$) cannot be used to denote any item. But this condition can be relaxed in various ways in scientific contexts. To illustrate, consider women A, B, C, D, E and F who have met similar deaths in similar circumstances over a period of time. Then we can form the open sentence " – killer of A&B&C&D&E&F".

The investigators might suppose that there is a unique satisfier of the open sentence and even introduce a name "JR" ("Jack the Ripper") for the killer. On Russell's theory JR is not an object with which the investigators are acquainted, but they are acquainted with A, …, F. However suppose that it is shown that woman F died of causes that could not have been a killing; or that one person killed F while another killed all of the other five. Then has no name "JR" been properly introduced, even if there is a unique killer of the remaining five women? This would be so only if perfect satisfaction is required of the definite description. But we could admit the idea of less than perfect satisfaction and allow a name to be successfully introduced for the item which is the best imperfect satisfier, that is, that item which is a member of some set of sufficiently adequate, imperfect satisfiers and which is the best of this set. In the case described above the name "JR" would then be successfully introduced even though what it denotes is not a perfect satisfier of the open sentence but the best imperfect satisfier.

Such cases readily arise in science. Suppose as in ($4^*$) term "t" is introduced via the description (¶x)**T**(x, O). But then the laws of T are subsequently altered because they are inaccurate in various ways and T is modified to $T^*$. For example, the mathematical relations between expressions can be changed in various ways; or a new parameter that was previously unnoticed is added to a law to make it more accurate; or a new law is added to T which makes a difference in certain conditions in which T is applied; and so on. Does this mean that in the earlier theory T, "t" denotes nothing since nothing perfectly satisfies the open sentence **T**(x, O)? To assume so is to go down the path to PMI; but this can be avoided. Perhaps there is some item K which is the best imperfect satisfier of **T**(x, O) in the sense that there is a non-empty set of minimally good satisfiers of **T**(x, O) and that K is the best of these. (K in fact might be the only member of the set. Moreover, if two items K and $K^*$ are tied for first place as best imperfect satisfiers then, as discussed next, any introduced term "t" can ambiguously denote both.) It might turn out that K is a perfect satisfier of the modification $\mathbf{T}^*$(x, O); or K may still be an imperfect satisfier of $\mathbf{T}^*$(x, O), but K satisfies this better than **T**(x, O). The second case opens the possibility that each theory of an historical sequence of theories, T, $T^*$, $T^{**}$, etc. can be about the very same item K, where a later theory is a correction of the immediately preceding theory. In such cases ontological continuity under conditions of imperfect satisfaction seems more plausible than failure of denotation throughout the sequence except for the last member when ontological flowering of the real finally takes place (or no such flowering takes place if one accepts PMI). In the past we simply got some things wrong about correctly identified entities. The theory of denotation can be usefully modified to account for such plausible cases of ontological continuity. A term can be introduced by means of an associated open sentence **T**(x, O) (and prior to discovered modifications that give rise to $T^*$); and its denotation is either the perfect satisfier, or if there is none then it is the best imperfect satisfier, of **T**(x, O). What is important here is that it is the world and its constituent objects, properties, events and the like, which are either the perfect satisfiers of our theories, or their best imperfect satisfiers (under the intuitive constraints envisaged in the modification of T to $T^*$); or our theories have no satisfiers. Examples of imperfect satisfaction will be encountered in subsequent sections.[8]

8 For more on imperfect satisfaction see Lewis (1983, Sect. VII) and Lewis (1999, p. 59) where the example used above of term introduction for "Jack the Ripper" is also discussed.

Another modification of the classical theory of descriptions involves the introduction of names which have ambiguous denotation. On the classical theory a description such as “the wife of Ludwig Wittgenstein” does not denote and so cannot be used to successfully introduce a name. In contrast the description “the wife of Frank Ramsey” does uniquely denote and a name can be successfully introduced. But what of the description “the wife of Bertrand Russell”? It does not pick out a unique individual; there were four wives. But does it follow that if some name, say “Xena” is introduced via the description that it fails to denote? The modification proposed is that “Xena” is not non-denoting (as the classical theory would have it) but that it ambiguously denotes each of four distinct women.

There are many cases of term introduction in science in which it is later discovered that the term ambiguously denotes. Such is the case for isotopes in chemistry. Suppose the term “hydrogen” is introduced into science (in whatever way). Once it is discovered that there are several isotopes of hydrogen does it follow that the term “hydrogen” fails to denote? If this is so, then it can be inferred, via the disquotational schema “ ‘hydrogen’ denotes iff hydrogen exists” that hydrogen does not exist. A more plausible alternative would be to claim that there is some denotational contact made with the world when we use the term “hydrogen”, but perhaps at a higher level of a genus. If the term “hydrogen” denotes a higher-level genus of element then denotational refinement occurs when the different kinds of hydrogen are distinguished but still using the same name. If there is a need to have different names for these different kinds, the isotopes, then names can be introduced such as the symbols “$^{1}$H”, “$^{2}$H” and “$^{3}$H”. Unlike other elements, in the case of hydrogen there is a need to have handy proper names for each isotope; so the names “protium”, “deuterium” and “tritium” were introduced via denotational refinement. (There are in fact several more isotopes of hydrogen that do not have specific names.)

In subsequent sections a case of denotational refinement will be mentioned, but such refinement plays no role in the case of the terms “electron” or “cathode ray”. It would appear that when these terms were introduced a fundamental kind of thing was named, so there has been no need for denotational refinement. But the world might, one day, tell us that denotational refinement is in order and that there are, currently unknown to us, different kinds of electron with different internal structures and thus different properties. To conclude that we had not been talking about anything when we used the term “electron” is to be misled by too limited a view of how denotation is fixed. Rather, we had made some appropriate denotational contact with the world; but in the light of the discovery of electrons with different internal structures, our term “electron” was, unbeknownst to us, ambiguous (or had no determinate denotation) and that denotational refinement would be in order.[9] We now turn to an application of the above semantic theory to an episode in physics.

[9] See Lewis (1999, p. 59) who also advocates the idea of what he calls “indeterminacy of reference” following earlier work of Hartry Field (1973) on this. For a further scientific example, a similar story about Lorentz’s use of the term “ion”, as Theo Arabatzis points out to me (private correspondence), underwent referential refinement. Initially it would have ambiguously referred to the ions of electrolysis but later underwent refinement when, after Zeeman’s discovery, he realised that there were important differences, so important that *c*.1899 he started to call them “electrons”; see Arabatzis (2006, pp. 84–85).

## 3. JULIUS PLÜCKER'S EXPERIMENTS WITH GEISSLER TUBES

Experiments on the passage of an electric current between electrodes in a closed tube had begun in the first decade of the 1700s and for the next 120 years a well-recorded sequences of phenomena were noted as the pressure of the gas inside was reduced. In the late 1830s Faraday pushed the limits of the then available electricity sources and vacuum pumps reaching a minimum pressure of approximately 1 mm Mercury. At about this pressure for tubes filled with air, a violet glow appears at the cathode which pushes the pink glow already in the tube towards the anode; the violet and pink glows do not touch and are separated by a dark space. This Faraday investigated and is now known as the "Faraday dark space". These limits of the then available experimental apparatus were transcended when the Rühmkorff induction coil was devised in the early 1850s to produce high-voltage electric currents, and when Geissler invented in 1855 a quite new kind of mercury pump that would enable experimenters to reach entirely new levels of low pressure in tubes. In 1857 he also invented a new kind of discharge tube called by Plücker the "Geissler tube"; these were of various shapes with electrodes fused into them and filled with various kinds of gases. The coil, pump and tubes were a technological breakthrough in the investigation of electric discharge through gases and quickly became standard equipment in physics laboratories. Any experimenter using them could reliably produce the effects already observed and then proceed to make novel investigations. Julius Plücker was the first to do just this.

Some of the phenomena to be observed at the newly available low pressures are as follows. The pink glow on the side towards the anode, breaks up into a series of striations with the concave surfaces facing the anode. At even lower pressures the violet glow around the cathode breaks into two parts with a new dark space emerging between them, now known as the "Crookes dark space" (owing to later investigations by William Crookes). At lower pressures the Crookes dark space grows in size pushing the Faraday dark space and the striations towards to anode until the Crookes dark space fills the tube and there is no luminosity. At about 0.01 mm Mercury (about 1/100,000th of an atmosphere) a completely new phenomenon appears: a greenish-yellow glow bathes the walls of the tube. This usually appears on parts of the tube away from the cathode; but in the case of Plücker's original experiments these were close to the cathode owing to the peculiarity of the tube he used.[10] It was this new phenomenon that excited the interested of many experimentalists, Plücker being the first to record them in papers of 1857–1859 of which Plücker (1858) is an English translation of one paper.

This new phenomenon is a reliably reproducible effect that many could bring about in their laboratories. The production of this, and other phenomena Plücker observed,

[10] Dahl makes the following interesting comment on Plücker's experiment: "Apparently the fluorescence, 'the beautiful green light, whose appearance is so enigmatic' was not uniformly spread over the wall of the tube, as is usually the case in a modern discharge tube when the Crooke's dark space attains a maximum. Instead, probably due to some quirk in tube construction in Plücker's experiments, it was concentrated in patches near the cathode. But for this fortuitous quirk, Plücker would not have discovered that the position and shape of the fluorescent patches are altered by a magnetic field" (Dahl, 1997, p. 54).

can be set out in the form of an experimental law E based on the disposition, under conditions C, of the experimental set up to produce the range of effects $P_i$ (labelled "P" for Plücker who observed or experimentally produced the effects).

> (E) There is a repeatable experimental set-up concerning the apparatus (viz., Plücker's Geissler tube) which, under conditions C of its electrical source and sufficiently low pressure beyond the emergence of the Crookes dark space, has a law-like disposition to produce phenomenon $P_i$.

## *3.1. Plücker's observations*

What are the effects that Plücker observed in conditions C? The first is simply the new phenomenon itself which he refers to as a "beautiful green light, whose appearance is so enigmatical" (Plücker, 1858, Sect. 35, p. 130):

> ($P_1$) There is, in conditions C, a coloured glow (in Plücker's experiment a greenish light)[11] on the glass of the tube (usually at the end opposite the cathode).

Plücker was aware of Davy's experiment in the 1820s which showed that the shape of an electric arc (produced by separating two electrodes initially in contact) could be affected by a magnet. Since he believed there was something akin to a stream of electric current in the tube, then it should also be similarly deflected. So he conducted a number of experiments by placing different kinds of tubes with different gases in different orientations with respect to a magnet. Plücker gives lengthy qualitative descriptions of how a magnetic field affects the light in the tube before the Crookes dark space appears. More important for our purpose is what happens to the "enigmatical beautiful green light" which appears on the inside contours of the glass after the Crookes dark space fills the tube. It can be moved back and forth as the polarity of the surrounding magnet is changed. This is Plücker's central experimentally manipulated effect:

> ($P_2$) In C, the patches of coloured glow can be moved by a magnetic field.

When there was an electric discharge through a Geissler tube, Plücker believed that what he called "rays of magnetic light" radiated from every point of the surface of the cathode. So he coated all of a cathode except a point-like extremity with glass and noted the single stream of light that emanated from the point. Owing to its concentration at a point the light can become visible (ibid., Sect. 30, p. 131). On the basis of this he drew an analogy with what would happen to iron filings placed near a point-like magnetic field; the iron filings would all move into a single line along the line of magnetic force emanating from the point. Similarly for the "magnetic light"; all the rays of light passing through a point would be aligned. But as he makes clear, this is analogy only between the iron filings and the "rays of magnetic light" and not an account of

[11] The colour of the glow depends on the chemical nature of the glass, in this case the yellowish-green colour being due to soda glass; lead glass gives blue, etc. This is something others, such as Crookes, discovered later; the particularities of colour play no role in the story being told here.

the nature of the "magnetic light" itself (ibid., Sects. 47–50, pp. 408–409).[12] This we may sum up as follows:

> ($P_3$) A point-like cathode produced a visible beam and a less diffuse more concentrated glow effect.

Also the following manipulation effect can be observed as a special case of $P_2$:

> ($P_4$) With a point-like cathode, the ray of light, and the patch of coloured glow it causes on the tube, can be deflected by magnet.

Plücker also reports that the coloured glow inside the tube does not depend on the position of the anode; and all the effects he observed were independent of the metal used as electrodes (usually platinum but often coated with other metals):

> ($P_5$) The glow is independent of the anode position.
> ($P_6$) The glow is independent of the metal used as cathode and anode.

What we now know to occur in Geissler tubes is well described by Weinberg:

> We know now that these rays are streams of electrons. They are projected from the cathode by electrical repulsion, coast through the nearly empty space within the tube, strike the glass, depositing energy in its atoms which is then readmitted as visible light, and finally are drawn to the anode, via which they return to the source of electricity. But this was far from obvious to nineteenth century physicists. (Weinberg, 1990, pp. 22–23)

The stream of negative electrons from a finely pointed cathode produce a ray of light; this is so because, even at low pressures of the contained gas, the concentrated stream of electrons still manages to hit the gas molecules thereby emitting light. The stream of negative electrons also repel one another as they pass down the tube; hence the "rays" from the fine point of the cathode are splayed out to a small extent yielding patches of coloured glow inside the glass of the tube. Later experimenters were able to place a screen inside the tube that would detect the electrons that grazed it as they passed down the tube, thereby showing more clearly the beam and its deflection by a magnetic field. However for the remainder of this paper we will eschew the whiggism of considering the experimentation and theorising from our current point of view and remain in the context of the late nineteenth-century physics when these matters were not obvious.

### *3.2. Constructing an identifying description denoting the cause of what Plücker observed*

What philosophical issues arise out of this episode in the history of science? One of the main claims of this paper is that Plücker's investigations provided sufficient information to yield identifying conditions for an entity of some kind which caused

[12] As Plücker says: "The rays proceeding from this point collect in one single line of light, which coincides with the magnetic curve passing through the end of the negative electrode, and which luminosity render such magnetic curve visible. Thus every ray which is bent in this magnetic curve, forming a portion of the arc of light, behaves exactly as if it consisted of little magnetic elements placed with their attracting poles in contact.… By the above illustrations I have merely sought to make the nature of the phenomenon intelligible, without in the least attempting to describe the nature of the magnetic light itself" (Plücker, 1858, Sects. 49–50, p. 409). By speaking of "as if", Plücker is not proposing a theory of what the "rays of light" are, viz., a thread of magnetic elements or anything like that.

what Plücker observed, even though no intrinsic property of the kind was known of the entity but only its extrinsic properties in specified experimental set-ups.[13] Moreover this identification, and re-identification, can occur in the absence of any name introduced to refer to the kind, as was the case for Plücker. Consider the kind presupposition first.

On the basis of the Causal Principle, we can say that the observed phenomena $P_1$ to $P_6$ are caused by an otherwise unknown "something" or "somewhat" of a particular kind; call this kind "K" (whatever it be). What is clear is that instances x of K arise in each experimental set-up causally bringing about $P_1$ to $P_6$. This involves a kind presupposition of the following sort:

> (K) There is a kind K (unique or not), instances x of which arise in the conditions of E, and which are casually efficacious in bringing about a range of phenomena such as $P_1$ to $P_6$.

We can leave open what kind does the causing. The "somethings" were variously regarded as flow of electricity (whatever that may be), or beams of light (though talk of light can be misleading) or rays, though the connotations of these terms are not, at this stage, important. We can also leave open what ontological category K belongs to, e.g., a kind of substantial object such as particles, corpuscles or whatever; or events or processes such as electromagnetic waves in an aether; or any other ontological category one might like to consider. Such a bare causal assumption is unproblematic; clearly there must be something giving rise to the effects, and not nothing, unless the world is much more indeterministic than we think. Fairly direct evidence that there is a cause at work bringing about the phenomena is shown when the current from the Rühmkorff coil is turned off and on.

But there need not be just one kind of thing that does the causing. It could be that there is, say, a single genus each species of which is, or can be, causally involved. Here the kind presupposition is not dropped but moves to a higher taxonomic level. In the next section an example of a term introduction, that of "canal ray", will be given in which the supposition that there is a unique kind named is dropped in favour of a genus without claiming that there are no canal rays. Here the notion of ambiguous name introduction and indeterminacy of denotation introduced in Sect. 2.3 comes into its own. More extremely, presupposition (K) might be challenged in cases where the causes turn out to be quite heterogeneous and there are several kinds giving rise to some effect. As an example consider the atmosphere which we identify by the casual role it plays in giving us certain feelings on a windy day, sustains our lives when we breath it, sustains fires, and the like. Though it was initially thought to be a single kind of substance, we now know it to be a heterogeneous collection of different kinds. But in discovering this we did not conclude that there is no atmosphere. Here the idea of denotational refinement of Sect. 2.3 once more plays an important role concerning the

[13] It is important to note that, in one sense, we have always been in casual contact with electrons, as when we are struck by lightning, or when earlier investigators experimented on electricity. But none of this was used as away of providing information about identifying a "something" and manipulating it for various purposes. It is Plücker's manipulation-based identification that puts us into contact with electrons in ways that are other than bare casual contact.

terms we use to talk about the atmosphere and talk about the heterogeneous kinds that make it up.

In what follows it will be assumed that (K) can be understood in a sufficiently broad way to include cases where there is no one kind involved and not be undermined by the involvement of multiple kinds. In the case of Plücker's discoveries we now take him to have been performing experiments on streams of electrons. So far as we know, there is just one kind of electron. So in this case supposition (K) has been a correct one to make. But even if it turns out that there are different kinds of electrons, or they have a heterogeneous character and (K) has to be modified in some way, it would not follow that there were no electrons; rather they are quite different from what we took them to be.

Suppose that we are given (E), (K) and Plücker's observable and experimental effects $P_1$ to $P_6$ (in what follows abbreviate the conjunction of the six $P_i$ effects to "P" for Plücker). Then we can reconstruct the manner in which Plücker and others were able to employ a definite description to identify a kind involved in the repeatable experimental set-up governed by (E), proceeding as follows.

Form an open sentence of the following sort (where "–" is a blank for a variable ranging over kinds):

(1) – is a kind and instances x of the kind – in condition (E) are such that x cause effects P.

There are three possibilities to consider concerning the satisfaction of the open sentence. First, there is no kind of thing which satisfies the open sentence (i.e., nothing is true of the open sentence). This alternative can be set aside; it would arise when either no kind of thing at all satisfies the open sentence, or if any particular things do satisfy it, they are quite different from one another and do not even form heterogeneous kinds at all, as in the case of the atmosphere. Second, there is exactly one and only one kind of thing that realises it. In what follows we will assume that this is the case. Third, two or more kinds realise the open sentence. In such a case of heterogeneity, we cannot say that nothing satisfies the open sentence (the first alternative); rather there is nothing akin to a single natural *kind* that satisfies it. Later we will return an example where two or more kinds of thing realise such an open sentence.

If we put an existential operator in front of the open sentence then we get a particular instance of a Ramsey Sentence. Thus where "k" ranges over kinds we have:

(2) There exists a (unique) kind k such that instances x of k in conditions (E) cause effects P.

However such a Ramsey sentence does not say that there is one and only one kind that satisfies the open sentence. But if we adopt the stance of David Lewis's modification of the Ramsey sentence suggested in Sect. 2, then we can form a definite description which picks out a kind (where "(¶ –)" is the definite description operator):

(3) (¶ k)[for instances xs of k in experimental set-up (E), the xs cause effects P].

This is a generalised version of a Russellian definite description, but in this case it is not a unique individual object that is being picked out but a unique individual kind.

Given this description, we are now in a position to introduce a name "K" for the kind specified in the definite description above:

(4) Let "K" denote (¶ k)[for instances xs of k in experimental set-up (E), the xs cause effects P].

Plücker in fact introduced no such proper name – this was something done by his followers. However he did use the phrase "rays of magnetic light" to capture the fact that the light emanating from a point-like cathode could be manipulated by a magnet. The above reconstruction shows that there was available to Plücker and his contemporaries an identifying description that does pick out a kind. What the above makes clear is the main burden of denotation fixing falls on the identifying description and not the name which is introduced on the basis of the description.

One important feature of the definite description in (3) and (4) is that identifying reference is made through purely extrinsic, or relational, properties involving the experimental set-up in which instances of K arise. Nothing is indicated in the description about what intrinsic (non-relational) properties instances of K possess. Nor is anything indicated about its nature or what ontological category it might belong to (substance, property, event, etc.). Later experimental work came to tell us about some of the intrinsic properties of kind K; but knowledge of any of these is not necessary for identifying reference to occur, as (3) shows.[14]

It is the world which determines whether or not there is some (unique) kind which satisfies open sentence (1), and thus whether the description formed in (3), or the name "K" introduced in (4), picks out a kind. The item which satisfies the open sentence may be a perfect satisfier of the open sentence. Or it might be an imperfect satisfier which is the best of a sufficiently satisfactory set of satisfiers. Where there are two or more satisfiers, either perfect, or imperfect best, is a matter to be considered further in the light of the idea of ambiguous denotation. In the above example perfect satisfaction seems more likely than imperfect best satisfaction since the extrinsic properties specified in the denotation fixer are broad enough to ensure that something is captured by them – assuming the world is, in this case, generous to us and does contain a unique kind that fits the description, and is not so ungenerous as to contain only a quite heterogeneous jumble of objects which fit the description.

What is the status of the name-introducing claim? Since it is an introduction of a name "K" for a kind, we know a priori that this is true (at least for those who introduce term "K" and their immediate community to whom the use of "K" is transmitted). But it is not a truth of meaning; nor does it specify the intension expressed by "K", or give us the concept of a K. Moreover it is a quite contingent matter that samples of the kind K ever get involved in Plücker's experimental set-ups with its Geissler tubes and the like. In some possible worlds the course of physics might have been quite different

[14] Some might see in this support for versions of structuralism as advocated in Russell, (1927, pp. 226–227 and 249–250), a position criticised in Newman (1928), and reviewed more recently in Demopoulos and Friedman (1985). The view is also found in Maxwell (1970, especially p. 188) where it is argued that from the bare Ramsey Sentence something like structuralism follows. Though it will not be argued here, from the fact that only extrinsic properties are used in denotation fixing, nothing follows about structuralism. In fact it will be argued that much later, especially with the work of J. J. Thomson and others, we did come to know something of the intrinsic features of what has been extrinsically identified, viz., cathode rays or electrons.

and there never had been discharge tubes, or the inhabitants of that world never went in for physics; yet there are exist Ks in that world. A quite different matter to consider is whether, if electrons do behave the way they do in Geissler tubes, then (contrary to Hume) it is necessary that they bring about effects P; that is, it is not metaphysically possible that there be Ks (electrons) in discharge tubes in the experimental set-up (E) but they not produce these effects. This question does not have to be answered here. The main concern is the Kripkean point that the name-introduction claim is known a priori but it is not a truth of meaning; but this does not decide whether it is a metaphysically necessary truth.

Of the effects $P_1$ to $P_6$ that comprise P the most central ones are $P_1$ and $P_2$, and their corollaries $P_3$ and $P_4$; these involve experimental manipulation of the coloured glow. In the above we have supposed that there is one kind of thing that is being manipulated.[15] But effect $P_2$ is more than a bare causal claim; it tells us that kind K has the disposition to be affected by magnetic fields (but in the absence of any knowledge of how the disposition works and any causal mechanism at work). It is their susceptibility to such manipulation that underlies realism about what is being manipulated. Also the manipulation condition underlies much subsequent quantitative experimental work (sometimes combined with theory) about what is being manipulated.

$P_1$ and $P_2$ also need to be taken in conjunction. Suppose there is a "something", a k kind, instances of which bring about effect $P_1$, and they also have the underlying disposition to be affected by a magnetic field as in $P_2$. These two features are central in identifying the "something". In contrast, suppose there is a possible world containing k and y kinds such that instances of the y kind in C also cause a glow effect but they are not deflected by a magnetic field; we can conclude that the y kind is not the same as the k kind. Again there is a possible world containing k and z kinds such that instances of the z kind in C are deflected by a magnetic field but they do not cause the glow effect; then the z kind is not the same as the k kind (or the y kind). To the best of our knowledge these possible worlds with their y and z kinds are not our actual world; our actual world contains just the k kinds and they both cause the coloured glow effect and are disposed to manipulation in magnetic fields.

### *3.3. An important restriction on what is to be admitted into identifying descriptions*

Identifying description (3) of the previous section would provide any reader of Plücker's paper with a recipe for constructing similar experimental conditions, observing the same phenomena – and so being in the presence of the same unobservable "something", the kind K, that Plücker encountered. However not all the claims that Plücker makes in his paper are included in the denotation fixer. What have been excluded are

[15] Hacking's remark "if you can spray them, then they are real" (Hacking, 1983, p. 22) underlies a realism about what we use when we manipulate (in this case the magnet and its field). But there is also what is manipulated (in this case the "something" that causes ($P_1$), etc.), and this too must be as real, though not much may be known about it. Causal relations can only hold between the equally real "what is used to manipulate" and "what is thereby manipulated".

speculative explanatory hypotheses about what is going on in the tube. Consider the following sputtering effect that Plücker noted (Plücker, 1858, Sects. 3 and 51). During the electric discharge he noted that small particles of the platinum metal of the cathode were torn off and deposited on the glass of the tube in the vicinity of the electrode. Tests were made to prove that the deposit was platinum metal from the cathode. He also noted that the glass was not always so blackened; but when it was there was no tendency for the free cathode particles to move towards the anode. He speculates: "It is clearly most natural to imagine that the magnetic light to be formed by the incandescence of these platinum particles as they are torn from the negative electrode" (ibid., Sect. 51, p. 410).

This we may set out in the following two-part condition S (for sputtering), of which (i) is the report of observations, and (ii) is a causal explanatory claim:

> (S) (i) in (E) bits of incandescent platinum are sputtered off from the cathode and deposited on the glass; and (ii) the occurrence of (i) is the cause of the " 'rays of magnetic light".

Clause (ii), unlike (i), introduces a speculative causal explanatory hypothesis. Should this be included in any denotation fixer? It would be by those who understand Ramsification to apply to whole theories. In this case one could take all of Plücker's observational and explanatory claims in conjunction. Then one could consider the following denotation fixer: the unique kind of thing k such that instances of k satisfy both (P&S). If so, then unlike denotation fixer (3), nothing satisfies both (P&S). The reason is that S is false; the occurrence of (i) is not the cause (ii). As will be discussed in Sect. 4.2, Goldstein showed that the sputtering effect is not due to anything given off from the cathode; rather it is due to a flow of the positively charges ions of the residual gas impacting on the cathode surface which gouge out small bits of the metallic electrode. To include false claim S along with P in any denotation fixer would ensure that no denotation is fixed. So in order to fix a denotation, such speculative causal explanatory hypotheses should be omitted from any definite description; it should employ only terms referring to observational properties, or experimentally determined properties. This raises the matter of what should be included in any denotation fixer, and what excluded.

Should one use in denotation fixing descriptions only terms which denote observables? To do so would be too restrictive. One needs to include terms which denote experimentally determined properties, but also allow for theoretical terms as well (e.g., "charge", "mass", etc.). Laws can also feature in generalised denotation fixing descriptions; such laws need not be restricted only to relations between observables, e.g., the law about the constancy of the ratio of mass to charge (m/e) for electrons. Importantly background theories are often used to experimentally determine such law-like relations between properties, which in turn are used in fixing denotations. (An example of this is given in Sect. 6.1, Thesis (B) on the background theory employed to determine the m/e ratio which is then subsequently used to identify electrons.) If these experimentally determined relations are employed in any denotation fixer, they will bring in their wake an involvement with theory. So the common theory/observation distinction is not the right one to invoke when restricting what terms can go into denotation fixing descriptions.

More promising is a distinction between those items which do not feature in explanations and those which do. Physicists often draw a distinction between the phenomenological and the fundamental; it is the phenomenological which stands in need of explanation but does not itself explain, while the fundamental does the explaining of the phenomenological.[16] Alternatively the distinction can be drawn in terms of observables and experimentally determined properties and relations versus theoretical models which purport to explain what we observe or experimentally determine. Though the distinction is not sharp, it is some such distinction which underpins the distinction between those terms which go into denotation fixing descriptions and those which do not. A related distinction is sometimes made between *detection* properties versus *auxiliary* properties. The terms in a denotation fixing description are about the properties used to detect some item (such as the properties used to pick out cathode rays); but other non-detection properties do not perform this function but may have other auxiliary functions such as specifying how the item picked out by detection properties behaves in other respects, or how it explains what we can detect.[17]

The significance of a distinction drawn along the above lines is that it precludes forming a denotation fixing description which incorporates the total context in which a term occurs, as do most accounts of the use of the Ramsey Sentence. In such cases it is much more probable that no denotation is fixed than if a more restricted context is drawn upon. In what follows, the context will be restricted to those which are not explanatory and pertain to what we can observe or experimentally determine. So restricted, the denotation fixing description is less likely to pick out nothing, or to carry excess explanatory baggage which takes us in the direction of incommensurability.[18]

## 4. HITTORF, GOLDSTEIN, CATHODE RAYS AND CANAL RAYS

### *4.1. Hittorf's Maltese Cross*

For our purposes, it was Johann Hittorf who, following Plücker's experimental recipe, made the next important advance in our knowledge of what goes on in Geissler tubes. He placed a solid body, for example one in the shape of a Maltese Cross, between a point-like cathode and the glow on the walls of the tube. He noted that a shadow of the body, a cross, was cast on the tube's walls. He inferred from this that the glow on the tube is formed by a "something" coming from the point-cathode, travelling in straight lines in a cone with its apex at the point-like cathode (assuming of course that there is no magnetic field present), and impacting on the walls of the tube causing the coloured glow. The paths of these "somethings" can be blocked by a body causing its shadow to appear on the walls of the tube which otherwise exhibit Plücker's enigmatic green glow. We can sum this up in a further clause (H), for Hittorf:

[16] Such a distinction is drawn in Cartwright (1983, pp. 1–2), though here a greater emphasis is put on what does and does not explain.

[17] The distinction between "detection properties" and "auxiliary properties" is made in Chakravartty (1998, p. 394). The distinction is put to much the same purposes as those required in this paper, viz., to enable the fixing of denotations independently of other matters such as theoretical explanations.

[18] Restricted versions of the Ramsey Sentence have been proposed by others; the version in Papineau (1996) is the one most congenial to the purposes of this paper.

> (H) The "somethings", xs of kind K, travel in straight lines from a cathode to the walls of the tube causing its glow; and if the cathode is point-like, and xs are intercepted by an opaque body, then a shadow is cast on the walls of the tube.

What Hittorf shows is that the kind of "something" that Plücker identified, viz. (¶ k)Pk, has a further set of properties – call these "the H-properties". That is, we can now claim: [(¶ k)Pk]Hk.

From our point of view, the other important thing that Hittorf did was to introduce a name of the "somethings" of kind K. We can reconstruct his name introduction in the following way:

Glimmstrahlen (glow rays) = (¶ k)Pk.

This is not a Kripke-style baptismal reference to a kind via pointing to some perceptually available samples in one's vicinity and then introducing a kind name for anything that is of the same kind as the samples. This is not possible since the kind, whatever it is, is not directly observably accessible to us. All that is accessible are the properties we can observe in some experimental set-up. By means of our acquaintance with these properties a generalised Russellian description can then be formed that takes us to the kind with which we are not acquainted.

Note also that this name might convey some connotations that are unwarranted by the descriptive denotation fixer on the right-hand side. That they are "glow" rays is perhaps a connotation due to their causal effect when impacting the glass of the tube. What connotations the term "ray" contains might vary according to one's theory of what rays are; but this need not be obtrusive in this term introduction. Both particle and aether theorists talked of "rays" of light, the term "ray" being neutral between the two theories. However some physicists might wish to build more into the concept of a ray than this, in which case the connotations of "glow rays" goes beyond what the description (¶k)Pk contains.

### *4.2 Goldstein's two discoveries and name introductions*

The next salient advance was made by Eugen Goldstein in 1876. He discovered the following, which we can lump together to get clause (G) for Goldstein. He showed that shadows were cast by an opaque object not only when the cathode was point-like, but also when the cathode formed an extended surface. In the latter case a shadow of an object was cast on the tube only when the object was close to the cathode; if the object was too far from the extended cathode then the shadow edges would become diffuse and not sharply defined, or no shadow would be formed at all. Goldstein also showed that whatever came from the cathode was not emitted in all directions but was largely at right angles to the surface, and travelled in straight lines (in the absence of a magnetic field). In this respect what is emitted from the cathode behaves differently from the light that is emitted from an incandescent lamp. He also definitively established a result that Plücker proposed – whatever is emitted from the cathode is the same regardless of its metal composition. These were important additional discoveries[19] that we can incorporate into clause (G) as follows:

> (G) The "somethings" of kind K are emitted at right angles to the surface of the cathode and travel in straight lines and are not emitted in a spectrum of angles (as in the case of light); and they are independent of the material of the cathode.

The new knowledge we have of kind K is: [(¶ k)Pk]Gk; and combining the knowledge obtained by Hittorf and Goldstein we have: [(¶ k)Pk](Gk&Hk).

In 1876 Goldstein introduced another name to refer to the "somethings" in the following way:

Kathodenstrahlen (cathode rays) = (¶ k)Pk.

This new name has connotations of its own some of which arise from the denotation fixer. Thus "cathode" tells us something about the extrinsic, causal origin of the "somethings", unlike Hittorf's that tells us only about their effect. But "ray" is still vague and may convey connotations that go beyond the denotation fixer according to what theory one adopts of the nature of the rays. What is known of cathode rays remains largely its extrinsic properties in experimental set-up E, and hardly anything intrinsic.

The term "cathode rays" caught on while that of "glow rays" did not. Whichever name we adopt, why do we want a name to refer to kinds of thing? As is evident, to have continued to use any of the above descriptions, such as (¶x)Px, would have been rather cumbersome. In addition, to keep talking about *the "somethings" we know not what* that arise in the experimental set-up is hardly helpful. Our conversation and communication is vastly improved if we can have names for individuals and kinds. And this is true of the community of scientists. Hence their need to coin a name to refer to the kind of thing they were all intent on investigating. Other names were also used in the nineteenth century. For example, John Peter Gassiot (a collaborator with Faraday in the late 1850s whose flagging interest since the late 1830s in discharge tubes had been rekindled by Plücker's work), had coined the term "negative rays" (Shiers, 1974, p. 93). But the name for the kind, and some of its connotations both relevant and irrelevant, is not as important as the description that fixes the denotation for the name.

Goldstein is also responsible for the discovery of Kanalstrahlen, i.e., canal rays (also known as "positive rays"). These play a small role in the story being told here since they concern the sputtering effect that Plücker had observed and which Goldstein investigated. One issue confronting experimenters was the problem of the suspected, but missing, positive counterflow in the direction from anode to cathode. This proved difficult to detect until Goldstein in 1886 devised a way. He bored one or more holes in a solid plate-like cathode through which the positive flow might pass through the plate and then beyond (on the opposite side from the anode), where they can be detected. The holes in the plate were to function like a duct or channel (in German "Kanal") for the flow rather than blocking the flow. In Goldstein's apparatus, at low enough

[19] For an account of this work see Dahl (1997, p. 57).

pressures the characteristic cathode ray effects arose; but on the back side there were long columns of yellow light: "The yellow light consists of regular rays which travel in straight lines. From every opening in the cathode there arises a straight, bright, slightly divergent yellow beam of rays" (cited in Dahl, 1997, p. 81; Magie, 1935, p. 577). Unlike cathode rays, the light is readily visible; and its colour varies with the gas used in the tube. One important difference was that they were unaffected by a magnetic field just strong enough to deflect cathode rays.

These features are sufficient to provide a description of a repeatable experimental set up, $E^*$, and a set of claims ($G^*$) (for Goldstein) which can then be used to form a description (¶x)$G^*$x that supposedly denotes a unique kind of item. Goldstein tentatively introduces a name for the kind on the basis of the following extrinsic description: "Until a suitable name has been found, these rays, which we now cannot distinguish any longer by their colour, which changes from gas to gas, may be known as 'canal rays' ['Kanalstrahlen']" (loc. cit.). Put more formally in terms of the theory of name introduction by means of descriptions we have: "canal rays" denotes (¶x)$G^*$x. The name was adopted for a considerable time; but its connotations are based on a highly contingent feature of the "canal" rays, viz., the channelled holes in the cathode through which they pass before detection.

Goldstein's discovery remained in limbo until later experimenters, from Wilhelm Wien in 1898 onwards, were able to deflect them in strong magnetic fields. It turned out that different samples of canal rays could be deflected through different degrees. We now know canal rays to be a quite broad genus, positive ions, of which there are many species, each having a different angle of deflection in the same field.[20] We now know that the smallest is a proton which has the greatest degree of deflection, or as Stark calls them "hydrogen canal rays"[21] when hydrogen was the gas in the tube. When helium is the gas in the tube then "helium canal rays" are produced; and so on. Here it would be misleading to conclude that there was no denotation for the name "canal rays" since a large number of different kinds fit the description; rather it turns out that the term Goldstein introduced either picks out a broad genus, or it names ambiguously a number of different kinds. Finally Goldstein's discovery of canal rays showed that Plücker's causal explanatory hypothesis about sputtering was quite wrong.

## 5. IDENTIFICATION AND RIVAL THEORIES AND MODELS OF CATHODE RAYS

Given the identification of cathode rays and the advancing knowledge of their properties, such as that contained in [(¶ k)Pk](Gk&Hk), which was obtained by observation or experiment, it is now possible to advance theoretical and/or explanatory models of

[20] See Dahl (1997, pp. 80–81 and pp. 265–266) on Goldstein's discovery of canal rays. Extracts from Goldstein's 1886 paper appear in Magie (1935), pp. 576–577.

[21] See the 1919 Nobel Prize lecture of Johannes Stark (1967, p. 430) who did experimental work on canal rays in the early 1900s and discovered the Doppler effect for canal rays; he also worked on hydrogen canal rays, helium canal rays, etc., including molecule canal rays depending on the molecular gas in the tube.

the behaviour of cathode rays. These models require the prior identification of cathode rays and do not contribute to their identification. Goldstein was an advocate of models in which cathodes rays were some kind of electromagnetic wave propagation in an all-pervasive aether. Others advocated models in which they were some kind of particle. It seems that the first person to claim this was Cromwell Varley in 1871 who said that they are " composed of attenuated particles of matter projected from the negative pole by electricity in all directions, but that the magnet controls their course" (cited in Dahl, 1997, p. 62). Nothing is said about the particles concerning their size or mass; but this is the beginnings of a simple model in which cathode rays are streams of small particles.

A similar view was developed by William Crookes who investigated the dark space now named after him, making a not implausible hypothesis about what was happening in it. He suggested that the molecules of the remaining gas come into contact with the cathode and acquire from it an electric charge. They are immediately repelled from the surface of the cathode at right angles because of the mutual repulsion of like particles. This would produce a stream of moving molecules of the gas at the very low pressures of the gas. Crookes used this hypothesis to explain the growing Crookes' dark space that appears in the tube. He also suggested that one might be able to measure the mean free path of the molecules of the gas at such a low pressure in the dark space, and speculated further on how this theory might explain the brightness that emerged at the end of the Crookes dark space away from the cathode:

> The extra velocity with which the molecules rebounded from the excited negative pole keeps back the more slowly moving molecules which are advancing towards that pole. The conflict occurs at the boundary of the dark space where the luminous margin bears witness to the energy of the collisions. (Cited in Whittaker, 1951, p. 352)

According to Crookes, the dark space is dark because there are no collisions occurring in it; and the bright space at one end of the tube is bright because that is where the collisions take place. But some of the charged molecules do get through and cause the characteristic yellowish-green glow on the glass of the tube; the lower the pressure the more get through until it is at a maximum when the Crookes' dark space fills the tube.[22]

Crookes' theory entails the idea that the charged molecules can exert a pressure, a consequence he developed both theoretically and experimentally. The experimental demonstration consisted of the radiometer fly developed largely by Crookes' laboratory technician Gimingham (see Dahl, 1997, p. 72). It comprises a horizontal cross suspended on a steel point so that it could rotate freely. Attached to each arm of the cross was a small thin mica disc. If one side of the disc was blackened and the other left shiny then when it was exposed to bright light the cross rotated suggesting that there was pressure due to the light exerted on the mica discs. Crookes got Gimingham to adapt this for one of his "Crookes' tubes" (his version of Geissler tube) in an experiment to show that his radiometer would rotate. When placed on some glass rods aligned like railway lines the fly easily roll along them. When the charges were

[22] For an account of Crookes' ideas see Whittaker (1951, p. 352).

reversed on the anode and cathode the fly could be made to roll back the other way (see Dahl, 1997, pp. 73–74).

Crookes' "torrent of charged molecules" theory is a good example of how an erroneous theoretical model can give rise to a correct experimental prediction such as the rotating radiometer. And as reported by Whittaker (1951, p. 353), Eduard Riecke showed in 1881 that when one investigates the equations of motion for such charged molecules of a given mass, a good account of the deviation of the cathode rays by a magnetic field could be deduced.

Various clouds of doubt hung over Crookes' theory, only one of which will be mentioned here.[23] Goldstein, the aether theorist, considered the issue of the mean free path of the molecules at the low vacuums Crookes was using. He determined by calculation that, for the best low vacuums that could be obtained, the mean free path of the gas molecules was about 0.6 cm according to Crookes' own model of his electrified molecules. In contrast experiment showed that the cathode rays travelled at least 90 cm, that is more than 150 times the calculated mean free path. So Crookes' "molecular torrent" model was in trouble (Dahl, 1997, p. 77; also Weinberg, 1990, p. 24).

It was Thomson who much later developed an argument against Crookes' theory that cathode rays are moving charged molecules, but only after the electron or corpuscle theory had been proposed. He argued in his 1903 book, *The Conduction of Electricity Through Gases*[24] that the momentum of the much smaller impacting charged particles would have been insufficient to cause the observed rotation of the fly. According to Thomson it is the heating of the vanes of the radiometer fly caused by the impacting particles; this generates a temperature difference which in turn produces the rotation. Such a mechanical effect supposedly due to the transfer of momentum of Crookes' molecular torrent could not have been caused by the very small corpuscles that Thomson envisaged in 1897.

Crookes played a number of variations on his view that cathode rays were a charged molecular torrent. He sometimes called the rays "charged molecules", and sometimes even "charged matter" or "radiant matter". He also referred to them in an 1879 lecture as a "fourth state of matter":

> In studying this Fourth State of Matter we seem at length to have within our grasp and obedient to our control the little indivisible particles which with good warrant are supposed to constitute the physical basis of the Universe.… We have actually touched the border land where Matter and Force seem to merge into one another, the shadowy realm, between the Known and the Unknown which for me has peculiar temptations. (Dahl, 1997, pp. 71–72)

[23] Whittaker (1951, p. 353) discusses not just the problem mentioned above but also Tait's objection that an expected Doppler effect was not detectable, and Hertz's objection that he had failed to uncover any deflection of cathode rays by an electric field, something he took to support a wave theory of cathode rays. See also Dahl (1997, pp. 76–77) on these problems. The authors cited canvass possible replies to the first objection. And Thomson in his paper of October 1897 showed why Hertz's failure was to be expected, and then produced an electric field deflection at lower pressures, using it as one method of measuring the mass to charge ratio. Another of Hertz's objections to the particle theory was the emission of cathode rays through a thin film of metal at one end of a tube into the outer atmosphere; however through the work of Lenard, as will be seen in Sect. 6.3, Thomson was able to turn this to the advantage of a particle theory.

[24] The relevant section was republished as Thomson and Thomson (1933, pp. 7–8).

With Crookes we now have a proliferation of names for the kind of "something" that all the scientists mentioned took themselves to be investigating. Note however that the kind names carry with them theoretical connotations. If we take these connotations seriously as part of the denotation fixer, for example we take Crookes' idea that they must be a torrent of charged molecules that have picked up their charge from the cathode and are repelled by it, then there are no such things for the names to denote. The different scientists of different theoretical persuasions could not be talking about the same things. This is one of the problematic consequences of the notion of incommensurability raised by Kuhn and others and to which the pessimistic meta-induction gives some credence.

Much more could be said of the conflict between the wave and particle models of cathode rays. But the main point of this section is that no matter how radically different the models of cathode rays might be in the ontololgies they presuppose, the conditions which enable the identification of cathode rays are independent of these theoretical models. In fact the very rival models presuppose that there are such independent identity conditions for them to be rivals of the same "something" – the cathode rays – whatever they be.

## 6. THOMSON AND THE IDENTIFICATION OF CATHODE RAYS OUTSIDE THE CATHODE RAY TUBE

In the story told so far, most of the properties of cathode rays are extrinsic, experimentally determined properties; little is said of their intrinsic properties.[25] But this began to change by the 1890s. Moreover it was commonly thought that cathode rays were not entities parochially confined to the behaviour of cathode ray tubes; many, wave and particle theorist alike, came to believe that they were universal constituents of matter. Thomson indicates an experimental breakthrough on this matter when he said: "So far I have only considered the behaviour of the cathode rays inside the bulb, but Lenard has been able to get these rays outside the tube" (Thomson, May 1897, p. 108). The identity conditions for cathode rays are closely tied to the features of cathode ray tubes. If they are to be identified outside such tubes then how is the identification to be made? The problem of re-identification is not commonly discussed in histories of our encounter with electrons, but it is an urgent philosophical issue for the theory of denotation and identification give here.

### *6.1 Thomson's experimentally based theses concerning cathode rays*

Though it had already been recognised by many that cathode rays were charged (charge being an intrinsic property), a further significant intrinsic property was established by Perrin in 1895 when he showed what many had supposed, viz., the charge is negative.

[25] The intrinsic/extrinsic distinction adopted here is that discussed in Lewis (1999, Chap. 6). Lewis accepts the basic style of definition proposed by others that a thing has an intrinsic property does not entail that a further thing exists; however there are difficulties that Lewis raises that need to be overcome for a more adequate definition The suggested revisions, though important, are not relevant to this paper.

Thomson investigated this further to remove an objection raised by aether theorists, viz., that something negatively charged did arise from the cathode but this something need not be the same as cathode rays. He reports the result of his redesigned experiment in two major 1897 papers (an 30th April address referred to as Thomson, May 1897 and October 1897). The conclusion he draws cautiously in the earlier paper is that "the stream of negatively-electrified particles is an *invariable accompaniment* of the cathode rays" (May 1897, p. 7, italics added). But he says something stronger in his October 1897 paper: "the negative electrification follows the same path as the rays, and that this negative electrification is *indissolubly connected* with the cathode rays" (October 1897, p. 295, italics added). His main hypothesis in both papers is that cathode rays are (the same as) negatively electrified particles (op. cit., p. 294). The two weaker claims of invariable accompaniment or indissoluble connection follow logically from the stronger identity claim; but do not establish the identity.

In what follows, some of the main experimental findings about cathode rays in Thomson's two papers will be listed as theses (A) to (G):

Thesis (A): All cathode rays are negatively charged.

Thomson's papers are within the context of a particle model of cathode rays rather than a wave model: "The electrified particle theory has for purposes of research a great advantage over the ætherial theory, since it is definite and its consequences can be predicted; with the ætherial theory it is impossible to predict what will happen under any given circumstances, as on this theory we are dealing with hitherto unobserved phenomena in the æther, of whose laws we are ignorant" (op. cit., pp. 293–294). In discussing Perrin's experiment, and his own modification of it, Thomson says: "This experiment proves that something charged with negative electricity is shot off from the cathode, travelling at right angles to it, and that this something is deflected by a magnet" (op. cit., p. 294). Thomson's talk of a "something" in this context fits well the analysis given here in terms of definite descriptions in which the "something" is captured by the variable "x".

Further experimentally determined properties of cathode rays follow in theses (B) to (G).

Thesis (B): The m/e ratios for cathode rays converge on values between $0.3 \times 10^{-7}$ and $1.5 \times 10^{-7}$.

Thesis (B) concerns a convergence, using different background theories, of values of the mass-to-charge ratio, m/e, of cathode rays. Already by 1890 Schuster had developed a theory of the motion of a charged particle in a field and had set upper and lower limits to the m/e ratio. A number of different m/e ratios for different charged particles were investigated in the 1890s both in England and Germany. Thomson's two 1897 papers use different background theories for deducing a value of the m/e ratio for cathode rays (all in the range of $0.3 \times 10^{-7}$–$1.5 \times 10^{-7}$; Thomson op. cit., the tables on p. 306 and p. 309).

The first method that Thomson used to determine the m/e ratio supposed a background theory about the charged particles striking a sold body causing its temperature to rise. To establish this quantitative result he assumed background theoretical hypotheses about heat and about the motion of particles in a uniform field (op. cit., pp. 302–307; Thomson, May 1897, p. 109). The second method was quite different; he measured the amount of deflection experienced by cathode rays when they travelled the same length, first in a uniform magnetic field then in a uniform electric field, and compared their ratios (op. cit., pp. 307–310). For this he assumed a simple theory about motion of a charged massive object in a field. In both cases he helped himself to some background theory to establish the experimentally determined m/e ratios. So the determination of the m/e ratios is not theory-free. This in no way detracts from the "phenomenological" character (in the sense of Sect. 3.3) of the experimental determinations. In this context, the theory used is not explanatory; it is used to deduce the quantitative ratios in conjunction with experimental information. In addition the theories Thomson used are idealisations. At best cathode rays are not perfect satisfiers of the theories he applied to them; rather they are imperfect satisfiers.

Theses (C), (D) and (E) introduce three important independence claims. The first is:

Thesis (C): The amount of magnetic deflection of cathode rays (in a constant field) is independent of the gas in the tubes.

What Thomson, building on the work of some others, showed for a number of different gases confined in tubes or jars was that the amount of deflection is always the same, assuming that the magnetic field is the same. In this respect cathode rays behaved quite differently from canal rays. This provides the basis for an inductive inference to the conclusion: for all gases the cathode ray deflection is the same.

Thesis (D): The m/e ratio is independent of the kind of gas used in the cathode tube.

This result is a consequence of Thomson's work on the m/e ratio of cathode rays investigated in different gases in the tubes such as air, hydrogen, carbonic acid, etc. (op. cit., pp. 306–307). This result can be inductively generalised: for all gases the m/e ratio of cathode rays is independent of the gas.

Thesis (E): The m/e ratios are independent of the kind of electrode used.

This thesis (see op. cit., final section) builds on the work of Plücker, Goldstein and others all of whom used electrodes of different metals (aluminium, iron, platinum, tin, lead, copper, etc.), though they noted that the appearance of the discharge varied. By inductive generalisation one can infer thesis (E).

These three independence claims provide some of the evidence for a further inductive inference to the ubiquity of the "somethings" that are cathode rays; that is, cathode rays are a distinct kind of thing that are present in all substances and are not simply due to the peculiarities of cathode tubes.

Thesis (F): (Concerning the smallness of cathode rays): cathode rays are (1) much smaller than any other known chemical element (2) by a factor of about 1/1000.

In the October 1897 paper Thomson notes that "for the carriers of the electricity in the cathode rays m/e is very small compared with its value in electrolysis. The smallness of m/e may be due to the smallness of m of the largeness of e or a combination of these two" (op. cit., p. 310). He then cites some evidence for the smallness of m (in the absence of any direct measurement of it). The first has to do with considerations due to Lenard, on which Thomson puts great reliance in both the April and October 1897 papers. Hittorf had already noticed that cathode rays could penetrate films of metal that were opaque to light.[26] Lenard experimented with a tube that had fitted, at the end where the cathode rays impacted, a thin film of different kinds of metal, such as aluminium. He then detected cathode rays travelling from the outer side of the metal film into the surrounding atmosphere (commonly air). He noted a range of their characteristic effects such as the fluorescence they can give rise to, the fact that they can be deflected by magnetic fields, and so on.

Of interest was the distance the cathode rays travelled, or their mean free path, i.e., the distance they travel before the intensity of the rays falls by a half. For cathode rays it is about half a centimetre while that of a molecule of air is $10^{-5}$ cm. That is, on average cathode rays travel in air thousands of times further than the constituents of air do. From this Thomson concludes: "Thus, from Lenard's experiments on the absorption of the rays outside the tube, it follows on the hypothesis that the cathode rays are charged particles moving with high velocity[27]; that the size of the carriers must be small with the dimensions of ordinary atoms or molecules." (Thomson, May 1897, p. 108). And in the October paper he adds that the m/e ratio of cathode rays is a thousandth that of the smallest known ratio, that of the hydrogen ion in electrolysis (Thomson, October 1897, p. 310). In the quotation note the distinction Thomson makes between an experimental fact (about mean free path of cathode rays outside the tube and their relative size) and the *hypothesis* that is meant to explain this, viz., that cathode rays are charged particles. It is the result of Thesis (F) that goes a considerable way to establish the hypothesis about the ubiquitous nature of cathode rays.

Thesis (G): The distance the rays travel outside the tube is only dependent on the density of the surrounding atmosphere and not the chemical nature of the outside medium (whether air, hydrogen, sulphur dioxide, etc.), nor its physical state.

This is a further independence claim. The path of the cathode rays outside the tube depends only on the density of the medium through which they travel. This also supports Thesis (F) since cathode rays must be much smaller than the atomic elements they pass through if their mean free path is greater by an order of a thousand. Being so comparatively small, they can, so to speak, get through all the gaps that there must be in atmospheres at ordinary pressures without bumping into, or being absorbed by, the atoms of the atmosphere. This adds support to the independence claim since their mean free path does not depend on the chemical nature of the atmosphere but only physical matters such as its pressure and density.

[26] See Whitaker (1951, p. 354, n. 1) for people who investigated this phenomenon, including Lenard. This raised one apparent difficulty for the particle theory of cathode rays since it was hard to think how particles could pass through solid metal, even as thin as aluminium or gold film, if they did pass at all. Thomson held the view that nothing passed through, but the negative charge on the side of the film inside the tube (due to the presence of the cathode rays in the tube) caused a negative charge on the outer side thereby causing further cathode rays to travel outside the tube in a surrounding atmosphere (Thomson, May 1897, p. 108).

[27] Thomson had already made measurements of the velocity of cathode rays which were much lower than those for rays of light, thus casting much doubt on the aether theory of cathode rays.

There are many other background considerations that also could be introduced at this point. For example, in 1896 Zeeman and Lorentz had produced a value for m/e based on the "Zeeman effect" that was in accord with Thomson's values published in the following year, but based on different considerations from those of Thomson. The historian Isobel Falconer (1987, p. 270) suggests that, when these results were published in English in late 1896 they may have given extra impetus to Thomson's investigations into cathode rays, since Thomson's experimental interests before 1896 lay largely elsewhere. Importantly she also argues that the particle hypothesis, to be considered next, was not something that just struck Thomson at the time; he was already well acquainted with such views but may have seen in the Zeeman-Lorentz work support for this hypothesis.

### *6.2 A new description for fixing the denotation of "cathode rays"*

All of (A) to (G) are experimental discoveries about a "something" which has already been identified by the description (¶x)Px, and on the basis of which names have been introduced. If we conjoin all of the discoveries (A) to (G) and replace any "theoretical term" such as "cathode rays" by a variable x to obtain the open sentence indicated by [(A)& … &(G)]x, then we can form a new generalised definite description (¶x)[(A)& … &(G)]x. What this says is that there is some unique kind of thing that is picked out by the description, viz., the something that satisfies [(A)& … &(G)]x (where the satisfaction is either perfect or the best, sufficiently good, imperfect satisfier). Moreover what is picked out by this new description is the same as what is picked out by (¶x)Px. And these are just cathode rays. So we can now claim:

Cathode rays = (¶x)Px = (¶x)[(A) &     & (G)]x.

The first description identifies cathode rays in the parochial setting of cathode rays tubes. Moreover it is couched in terms which refer only to extrinsic relations in the cathode rays. The second description contains quite new elements which arise from discoveries about cathode rays in their parochial setting, and then outside it. It is also couched in terms which refer to some of the intrinsic features of cathode rays such as charge and mass. Moreover it contains identifying features for cathode rays which at the time obtained wide currency. One of these is the distinctive m/e ratio possessed by cathode rays but not by any other "particles" known in the physics of the time. To ensure this there was an urgent need to obtain even better values for the m/e ratio than those of Thomson. Another is the distinctive angle of deflection of cathode rays in magnetic fields. Other particles would have different angles of deflection; this would serve to differentiate one particle from another if there were several in the same field (such as in a Wilson Cloud Chamber).

### *6.3 Thomson's startling hypothesis*

Theses (A) to (G) are experimentally determined claims about cathode rays that have been included in a new denotation fixer. But Thomson makes many other claims that have not been included because they pertain to his speculative theoretical model of cathode rays, or are employed to explain some of the experimental discoveries (A) to (G) (and others). To these hypotheses we now turn, arguing that they should have no place in any denotation fixing description because they are not about anything in the world. To include them in any denotation fixer would be to render the description denotationless.

Following on from his discussion of Lenard's result concerning the smallness of cathode rays compared with any other known particle, Thomson gives us what he calls his "startling hypothesis":

> The assumption of a state of matter more finely subdivided than the atom of an element is a somewhat startling one; but a hypothesis that would involve somewhat similar consequences – viz., that the so-called elements are compounds of some primordial element – has been put forward from time to time by various chemists. (Thomson, May 1897, p. 108)

In this context Thomson mentions a contemporary astronomer Lockyer, but also Prout who had much earlier in the nineteenth century proposed a similar hypothesis, except that Prout was mistaken in thinking that the primordial element was hydrogen. Thomson is right to call his claim a *hypothesis*, in one sense of that word. The hypothesis is intended to imply, and thus explain, claims (A) to (G); but that does not preclude the hypothesis being false. The following are seven different claims $H_1$ to $H_7$ that can be found as constituents of Thomson's "startling hypothesis"; some are consistent with modern physical theory while others are not.

Hypothesis $H_1$: (1) There is a primordial element of matter, much smaller in mass than that of any known atomic element, and (2) it is a constituent of all matter.

This is unexceptional, but hardly uniquely identifying. However associated with it is a further claim that is clearly false and which Thomson came to reject only well after his 1897 papers:

Hypothesis $H_2$: The primordial element is the *only* element out of which all matter is constituted.

Thomson then develops his explanatory hypothesis: "Let us trace the consequence of supposing that the atoms of the elements are aggregations of very small particles, all similar to one another; we shall call them corpuscles, so that the atoms of the ordinary elements are made up of corpuscles and holes, the holes being predominant." (loc. cit.) Two points can be highlighted, the first being a further hypothesis:

Hypothesis $H_3$: There is a predominance of holes in matter.

Thomson cites no direct experimental evidence for this, though he does use it to explain why such corpuscles have a greater mean free path than any of the atoms they comprise.

The second point concerns Thomson's introduction of a kind name "corpuscle". But it is unclear what description is to be used to fix its putative denotation. If it is claims (A)–(G) then it simply denotes cathode rays. But if the term is introduced in the context of Thomson's speculative hypothesis about the nature of cathode rays then, as will be argued shortly, it has no denotation. The hypotheses at the core of the corpuscle theory are not satisfied by anything in the world. There is an ambiguity about the term "corpuscle" that can be resolved in different ways with different consequences as to whether or not it has a denotation.

Further aspects of Thomson's broad "startling hypothesis" emerge when he continues:

> Let us suppose that at the cathode some of the molecules of the gas get split up into these corpuscles, and that these, charged with negative electricity, and moving with high velocity form the cathode rays. (Thomson, May 1897, pp. 108–109)

Two further hypotheses can be identified here. The first concerns how the cathode rays arise at the cathode and the second his core identity claim:

Hypothesis $H_4$: The molecules of the residual gas get torn apart at the cathode releasing some of the corpuscles to form cathode rays.

Hypothesis $H_5$: Cathode rays are nothing but streams of corpuscles.

Thomson's October 1897 paper expands on the startling hypothesis of the earlier May paper making clear two further speculative hypotheses concerning how the primordial corpuscles come together to form atoms. When talking of Prout's earlier anticipation of a kindred hypothesis that all matter is constituted out of hydrogen atoms, he rejects this saying that it is untenable but we can "substitute for hydrogen some unknown primordial substance X" adding that "these primordial atoms … we shall for brevity call corpuscles" (Thomson, October 1897, p. 311). But what Thomson goes on to say about the primordial corpuscles definitely shows that there are no such things.

In the second 1897 paper he reiterates $H_1$ and the false $H_2$ when he says that "we have in the cathode rays matter in a new state … in which all matter – that is, matter derived from different sources such as hydrogen, oxygen, etc. – is of one and the same kind; this matter being the substance from which all the chemical elements are made up" (Thomson, October 1897, p. 312). Thomson then develops a speculative theory about how aggregations of such primordial corpuscles would hang together in a stable configuration to form atoms. This is something that reaches back to his work in the early 1880s on how centres of repellent forces might arrange themselves in stable patterns.[28] It is part of a speculative theory, not based in experiment, concerning the vortex atom as a singularity in a uniform aether suggested earlier by William Thomson and Maxwell. The theory originates in work by Helmholz on perfect fluids in which indestructible vortices emerge that obey certain laws of rotational and translational motion.[29] The theory has an application in hydrodynamics, but its more speculative use was as a theory of how the primordial atoms that constitute all matter in the universe emerge as vortices in a universal plenum such as the aether. One suggestion Thomson

[28] Thomson (1883) is an essay on how vortex rings can form stable combinations and that "the properties of bodies may be explained by supposing matter to be collections of vortex lines in a perfect fluid that fills the universe" (op. cit., p. 1). Aspects of the theory of vortex rings last for quite some time in Thomson's thinking about his corpuscles; he devotes a whole chapter to how aspects of the vortex model might work in his informal Yale lectures of 1903; see also Thomson (1911), Chap. V.

[29] For aspects of the vortex theory see Silliman (1963) and Kragh (2001). The rudiments of the vortex atom theory are set out in Maxwell's 1875 *Encyclopaedia Britannica* article on the *Atom* reprinted in Garber et al. (1986, pp. 176–215), especially pp. 197–213.

makes is that there is a law of force of the kind envisaged by Boscovich in which at small distances the force is repulsive but at greater distances is attractive – but this involves considerable mathematical complexity owing to the number of interactions involved. As an alternative he suggests a model based on experiments concerning how different numbers of floating magnets arrange themselves in patterns of equilibrium (op. cit., pp. 313–314). This leads to Thomson's further two fundamental hypothesis about his corpuscles, one about how the primordial atoms arrange themselves and a possible second hypothesis about what these atoms are:

Hypothesis $H_6$: As the only constituents of nature, the corpuscles are primordial atoms (not the same as chemical elements) which arrange themselves in a law-governed way to constitute all matter including chemical elements.

Hypothesis $H_7$: The primordial atoms are really vortices in the aether and obey aether-vortex laws.

These seven hypotheses are the core of Thomson's speculative model of his corpuscles. If we conjoin these hypotheses and create an open sentence by deleting the theoretical term "corpuscle", viz. $(H_1)\& \ldots \&(H_7)]x$, and place a definite description operator in front, then we can form a definite description $(¶x)[(H_1)\& \ldots \&(H_7)]x$. The definite description can then be used to fix a denotation for Thomson's theoretical term "corpuscle". This corresponds to the quite general use of the Ramsey Sentence, or the Lewis–Ramsey denotation fixer, which employs all the elements of a theory rather than some restricted subset of claims associated with the theory.

Does anything perfectly satisfy the open sentence, or even play the role of being the best but imperfect satisfier? Since there is no such thing as the aether, then $H_7$ is false; and so the description denotes nothing. However it is possible to reject $H_7$ while adopting $H_6$. This would occur if one were to adopt the view that cathode rays are really material particles but still held the view of $H_6$ that such charged material particles constituted all elements and still have to come together in some way to form chemical elements. Such is one way of taking Thomson's talk of corpuscles by dropping the view that there is an aether. However it is still the case that $H_2$ and $H_6$ (with or without $H_7$), and following in their train a false $H_5$, ensure that nothing either perfectly or imperfectly satisfies the open sentence. So, the definite description denotes nothing and the term "corpuscle" fails to denote.

Not all uses of the term "corpuscle" have their reference fixed in this way. As was indicated the term could just as well have its reference fixed by the quite different denotation fixer, $(¶x)[(A)\& \ldots \&(G)]x$. In this case the term is ambiguous depending on whether its denotation is to be fixed by a theory which has several false constituent hypotheses which are part of an explanatory model, or it is to be fixed by well-established experimental claims. This locates an important ambiguity at the heart of Thomson's theory concerning whether it is about anything at all, and suggests how the ambiguity can be resolved. This is a matter often obscured by talk of concepts, such as the Thomson corpuscular concept. Ontologists wish to know: "Is the concept instantiated or not?" No clear answer is forthcoming from within the theory of concepts. But answers are forthcoming in terms of the theory of generalised descriptions used to fix denotations.

## 7. THE TERM "ELECTRON" AND ITS MULTIPLE INTRODUCTIONS IN PHYSICS

One of the main claims in the above is that, as far as the unobservable items of science are concerned, definite descriptions are fundamental in initially picking them out as denotata while names follow in their wake picking up as their denotata what the descriptions denote. If this is the case then it is unproblematic that the very same name can be used many times over to denote quite different unobservables. In the case of individuals the proper name "John Smith" is unproblematically ambiguous in denoting many different people. Similarly for names for scientific kinds, observable and unobservable. This is so of the term "electron". The Ancient Greek term ηλεκτρον was used to denote amber. Perhaps a Kripkean story can be told of how the name was introduced in the presence of some samples and then passed on through the Ancient Greek community.[30] The Greeks knew of the attractive powers of amber, and it was for this reason that the classically trained Elizabethan scientist William Gilbert first coined the cognate term "electric" to refer to the attractive power of amber rather than the substance amber.

George Stoney is credited with introducing the term "electron" into modern physics. From the 1870s Stoney proposed the idea that there existed a smallest unit, or atom, of electric charge involved in electrolytic processes, and in 1881 he gave an estimate of the magnitude of the charge. It was only in an 1891 address that he referred to this smallest unit using the name "electron" to denote what is picked out by the description "the smallest quantity of electricity (in electrolytic processes)" (see Dahl, 1997, pp. 205/403, n. 10–34). That the Greeks used the word ηλεκτρον to denote one kind of thing and Stoney, followed by others, used the same-sounding word "electron" to refer to another should occasion no problem; it is an ambiguity that can be removed by relativisation to languages. Stoney also believed that the electrons were permanently attached to atoms, and their oscillation gave rise to "electromagnetic stresses in the surrounding ether" (cited in Arabatzis, 2001, p. 181). But this is an additional extra belief about his electrons that purports to explain something and is not part of the description that Stoney used to introduce the term "electron".

However physicists subsequently co-opted Stoney's term "electron" to refer to two quite different kinds of thing.[31] The physicist Larmor also used the term "electron" to refer to – what? Here we need to return to the vortex ring theory that Thomson used (see Sect. 6.3). In the 1860s William Thomson proposed that atoms were vortices of motion, these being permanent, indestructible, ring-like structures capable of internal motion or vibration; they are in effect singularities in a primitive substance, a continuous and perfectly elastic fluid, the aether. In this theory neither mass nor matter nor

[30] See Kripke (1981), Lecture III, for an account of how names get introduced in some baptismal ceremony for proper names and for kinds.

[31] The story sketched draws on the work of Arabatzis (2001), Falconer (1987, 2001) and Kragh (2001) but within the context of a descriptivist account of the fixing of denotation. It will be evident that the descriptions used to introduce the term "electron" are often loaded with theory and that these cannot be readily replaced by other descriptions that lack the theory loading yet still refer to the same item (if they do refer at all).

Newtonian gravitation are primitives, but have to be "accounted for" within the vortex theory in some way. This was a view also explored by Maxwell[32] and many of his followers. As mentioned it was also a theory upon which J. J. Thomson worked in the early 1880s and, as Kragh says, "provided him with a framework of thinking" (Kragh, 2001, p. 198) that lasted even beyond the first decade of the twentieth century.

Joseph Larmor also worked within this framework trying to resolve some of the difficulties it faced. In the final section of an 1894 paper Larmor, at a suggestion of Fitzgerald, introduced the term "electron" and sometimes even spoke of "free electrons" The new denotation for the term "electron" can be reconstructed as follows: the unique kind k such that all instances of k are structural features of the aether which have a vacuous core around which is distributed a radial twist in the aetherial medium, have a permanent radial vibration which can not diminish, have a vibration and fixed amplitude and phase, have the same electric charge and the same mass, and are the universal constituents of all matter.[33] Larmor's overall view, expressed in 1895, is that "material systems are built up solely out of singular points in the ether which we have called electrons and that atoms are simply very stable collocations of revolving electrons" (cited in Falconer, 2001, p. 83).

On the point of what is or is not primitive in Larmor's ontology, his position is a little clearer in his 1900 book *Aether and Matter*:

> It is not superfluous to repeat here that the object of a gyrostatic model of the rotational ether is not to represent its actual structure, but to help us to realise that the scheme of mathematical relations which defines its activity is a legitimate conception. Matter may be and likely is a structure in the aether, but certainly aether is not a structure made of matter. This introduction of a supersensual aetherial medium, which is not the same as matter, may of course be described as leaving reality behind us; and so in fact may every result of thought be described which is more than a record of comparison of sensations. (Larmor, 1900, p. vi. Also cited, in part, in Harman, 1982, p. 102)

In the final sentence Larmor gives way to phenomenalist or empiricist or instrumentalist considerations in which "reality" is left behind; the first sentence has a slightly different emphasis in its talk of models, schemes of mathematical relations and a failure to represent. But for our purposes, the interest lies in the more realist middle sentence in which the order of ontological dependence is of matter on aether, and not of aether on matter. Even if this is not to be taken too strongly as a realist claim, it has methodological implications in that the direction of methodological analysis is from aether to matter and not conversely. This "methodological realism" is underlined when Larmor goes on to say:

[32] See Maxwell's 1875 *Encyclopaedia Britannica* article on the *Atom* reprinted in Garber et al. (1986), especially pp. 197–213.

[33] These characteristics are best set out in Arabatzis (2001, p. 183). The attribution of mass is not a primitive feature of electrons, understood as singularities in the aether, but as something for which an explanatory or reductive account needs to be given. This is not a matter that need concern us here. Issues of reduction, and especially realism about theories, would be less urgent if, as Achinstein (1991) Part II suggests, we take Maxwell and his followers to be offering theories as analogical models which downplay, in varying degrees, matters about what the world is really like, though clearly many followers of Maxwell and aether theorists took their theories realistically.

> It is incumbent upon us to recognise an aetherial substratum to matter, in so far as this proves conducive to simplicity and logical consistency in our scheme of physical relations, and helpful towards the discovery of hitherto unnoticed ones; but it would be a breach of scientific method to complicate its properties by any hypothesis, as distinct from logical development, beyond what is required for this purpose. (Larmor, 1900, pp. vii–viii)

The above fleshes out the definite description used to pick out the denotation of the term "electron" as introduced by Larmor. Such a Ramsey–Lewis denotation fixer contains a number of other theoretical terms. So fixing the denotation of "electron" can only take place in the context of fixing the denotation of other terms (such as "aether" and "radial twist", providing they have not been introduced into physical theory independently of the context of Larmor's theory). What is immediately evident is that the Larmor use of the term "electron" (to denote a structural feature of the aether) cannot not have the same denotation as Stoney's use of the term (to denote a unit of electrical charge, though Larmor's electrons do have the Stoney unit of electric charge); their denotation fixers are quite different and could not pick out the same kind of thing. So there are multiple introductions of the same term to refer to things even in quite different ontological categories. But does the Larmor term "electron" have a denotation? The verdict of the world is that there is no such thing which fits the reconstructed description given above; so there is no denotation for the Larmor term "electron". As it transpired, Larmor was developing his electron theory based in aetherial vortices just when the originator of the vortex atom, William Thomson, had doubts about it saying "'I am afraid it is not possible to explain all the properties of matter by Vortex-atom Theory alone".[34]

Not only did Fitzgerald make a suggestion to Larmor that he use the term "electron" but he made a similar suggestion to Thomson about what he could call his corpuscles. Thomson's May 1987 paper in *The Electrician* is a printing of an address given on 30 April 1897 of which Fitzgerald was aware. Fitzgerald comments on Thomson's address in a paper entitled "Dissociation of Atoms"; this appears in the same issue of *The Electrician*, and surprisingly is placed directly *before* Thomson's paper.[35] Of the several issues Fitzgerald raises about the address the main one for our purposes concerns Thomson's startling hypotheses about his corpuscles being the ultimate constituents of chemical elements. Fitzgerald "expresses the hope that Professor J. J. Thomson is quite right in his by no means impossible hypothesis". But despite this he raises some critical points about the corpuscle hypothesis, and then makes a suggestion about the existence of free electrons:

> [W]e are dealing with free electrons in these cathode rays. This is somewhat like Prof. J. J. Thomson's hypothesis, except that it does not assume the electron to be a constituent part of an atom, nor that we are dissociating atoms, nor consequently that we are on the track of the alchemists. There seems every reason to suppose that electrons can be transferred from atom to atom without at all destroying or indeed sensibly changing the characteristic properties of the atom: that in fact there is a considerable analogy between a charged sphere and an atom with an electron charge. If this be so, the question of course

[34] Cited in Silliman (1963, p. 472). Falconer (2001) also lists a number of similarities and differences between Lorenz's electrons, Larmor's electrons and Thomson's corpuscles that fleshes out much more of the story than can be done here.

[35] There is no immediate reply by Thomson to Fitzgerald's prefacing paper, though there is editorial comment (see Gooday, 2001 p. 111). However Smith (2001, p. 38) argues that Fitzgerald's comments influenced his subsequent experimentation and the topics covered in his later paper of October 1987.

> arises, how far can an electron jump in going from atom to atom? Why not the length of a cathode, say, or at least from molecule to molecule along it, or anyway in nearly straight lines along it? (Fitzgerald, 1897, p. 104)

The critical and correct, but at this time still speculative, point is the claim that atoms can retain their identity despite abandoning the idea of a Thomson corpuscle. That is, what Fitzgerald calls an electron can be free of the atom with which it has been associated and move around in various ways in cathodes, cathode tubes, and elsewhere. Such an assumption of free electrons, he says, should not lead us down the path of the alchemists who sought the one thing that could be transmuted into anything else, in which case the loss of "electrons" is taken to wrongly entail the transmutation of substances. That cathode rays are free electrons is a modification that can be made to Thomson's speculative hypotheses about his corpuscles.

Nothing is said in Fitzgerald's commentary about Larmor's account of electrons as singularities in the aether; that is well in the background and nowhere comes to the fore. Nor does Thomson mention in his May 1987 paper his more speculative Hypothesis 6 about how his corpuscles are to configure themselves to form atoms; this is a matter only raised in his later paper of October 1987. Importantly, in the second paper he does not explicitly say anything about $H_7$, the view that cathodes rays are really structural features of the aether (a view that Larmor, and Fitzgerald partly shared but not Thomson). It is open to the reader of Fitzgerald's paper to co-opt his term "free electrons", but not Larmor's aether theory of them, and then use the term "electron" rather than Thomson's term "corpuscle" to denote cathode rays (which are, on Thomson's "hypothesis" "charged particles moving with high velocities" (Thomson, May 1897, p. 108).

The more startling character of Thomson's hypothesis that Fitzgerald queries (because it might be taken to entail the dissociation of atoms when they lose their charged particle) is "that the atoms of all the elements are aggregations of very small particles, all similar to one another; we shall call such particles corpuscles" (loc. cit.). But then we could introduce any name on the basis of Thomson's denotation fixing description. He chose "corpuscles". Fitzgerald proposed that they be called "electrons" and that is the name that caught on in the physics community. From this point onwards, there is a sociological and historical story to be told that is well-recounted in Falconer (2001) that need not be repeated here. As Falconer expresses the complexity of what went on:

> Fitzgerald rejected the importance of corpuscles for atomic structure and shifted the context of Thomson's results to Larmor's electron theory. He ensured that the term "electron" was associated with Thomson's experimental work several years before there was full assent to Thomson's theory. That "electrons" were originally proposed as an alternative interpretation of the cathode ray results to "corpuscles" was forgotten. (Falconer, 2001, p. 86)

As is well known, Thomson resisted the use of the term "electron" to refer to the same item as his term "corpuscle", a resistance that went on to about 1915, well beyond his 1906 Nobel Prize lecture in which he did not use the term "electron" even though the lecture was entitled "Carriers of Negative Electricity".[36] What this paper adds to the

[36] Thomson's Nobel Prize lecture is reprinted as Thomson 1967. See Dahl (1997, p. 188) who emphasises Thomson's concern about distinguishing the real, material, negatively charged electron from the positive electron which, in his view, remained hypothetical. A fuller account of Thomson's resistance can be found in Falconer (2001) and Kragh (2001).

story generally told is a semantic background of denotation fixing via generalised definite descriptions. Given the different descriptions that can be culled from Thomson's papers of 1897, the more important issue is not what name to use for what the correct generalised description picks out. Rather the main issue concerns the correct description to employ and what aspect of theories of cathode rays are to be omitted from such descriptions, but nevertheless play an important role in the theoretical models of cathode rays, albeit models which are in many respects, false of what they purport to model.

## 8. CONTINUITY IN ONTOLOGY FROM CLASSICAL TO QUANTUM ELECTRONS

The identification of the electron is a story of ontological continuity with theory change, though not a story of name continuity. It is also a story of changing criteria of identification from Plücker's initial identification to that of Thomson's, with the same thing being identified throughout. The "something" so identified survived its several changing theoretical models, such as various kinds of aetherial wave disturbance or various kinds of particle (molecule, new subatomic particle, etc.). From the beginning of the twentieth century the electron was taken to be a charged particle obeying the classical laws of physics. A change occurred when Einstein introduced the Special Theory of Relativity; the electron now obeyed relativist dynamics. However with the various Bohr theories of the electron quite new discrete, non-classical properties were introduced, so that the electron obeyed new quantum laws. J. J. Thomson's son, G. P. Thomson, was even awarded a Nobel Prize for showing that the electron is wave-like, his experimentally determined wavelength closely agreeing with an equation derived by de Broglie. Pauli also made the quite non-classical proposal that the electron obeyed an exclusion principle: no two electrons can have the same energy state in an atom. The electron was also shown to have spin. Finally the electron has its place within both Heisenberg's matrix quantum mechanics and the Schrödinger's wave equation and more recent Quantum theories (such as QED).

Are there two (or more) electrons here, at least the classical-electron and then the quantum-electron? Or is there just the same electron we have encountered under different names, different conditions of identification and different theories or models? Bain and Norton (2001) answer "yes" to the last question – as does this paper. This final section shows how the considerations raised by Bain and Norton fit the story told here.

The first continuity they locate (Bain and Norton, 2001, pp. 453–455) is that of historically stable, intrinsic properties of electrons, stable in the sense that properties of electrons that are discovered at one point in the historical development of experiment, theory and theoretical models are kept on in later historical phases (some properties might not be kept on). Historically early properties (intrinsic or extrinsic) include: charge; Millikan's determination of the non-fractional character of the charge; the m/e ratio (though better values of this were obtained over time); the degree of deflection in a given magnetic field; and so on. Later properties include spin, the Pauli exclusion property, etc. These add to the core of growing knowledge of the properties of electrons that any theory of the electron ought to preserve. If the appeal to stable properties is to assist in the identification of electrons, then Plücker's initial denotation fixer, (¶ k)Pk, did its job of work for forty years until it was replaced by a set of identifications that enabled electrons to be located outside the context of cathode ray tubes. And this new set of identifications can have further members added to it without any change in the "something" picked out by the different identifying descriptions.

A second continuity they locate is "structure", a common feature that is preserved through changes in theory; this "is simply the smallest part of the latest theory that is able to explain the success of the earlier theories" (Bain and Norton, 2001, p. 456). There is no guarantee that there will always be such a structure, but when there is, and it is combined with historically stable properties, as is the case with the electron, then new identifying conditions can emerge. However as will be argued, even if electrons perfectly satisfy the historically stable properties, they do not perfectly satisfy these structures; at best they imperfectly satisfy them in the sense set out in Sect. 2.3. To flesh out the role that structure is to play, a theory is needed of imperfect satisfaction by an entity that is the best of a set of minimally satisfactory satisfiers.

According to Bain and Norton the structure that fills the bill is the Hamiltonian or Hamiltonian for the electron in its corr esponding theory. There are a number of different Hamiltonians (a function expressing the energy of a system in terms of momentum and position (potential energy)) according as it is embedded in one or another theory. Thus, following Bain and Norton (2001, p. 456) the Hamiltonian for the electron is

$$H = (\mathbf{p} - e\mathbf{A})^2/2m + e\varphi$$

(where **p** is the momentum, e is the charge, m is the mass of the electron and **A** and φ are the vector and scalar electromagnetic potentials). Embedding this into classical dynamics is enough for the theory that Thomson needed for his account of the deflection of his corpuscles in an electric field, or Millikan needed for his oil-drop experiment. Embedding the Hamiltonian in relativity theory produces a new equation with the additional factors above those provided by classical theory:

$$H = [(\mathbf{p} - e\mathbf{A}/c)^2c^2 + m^2c^4]^{1/2} + e\varphi$$

This introduces no new property but it does describe the behaviour of electrons by taking into account relativistic effects through additional factors.

Using these equations one can form a generalised, definite description (call this "the Hamiltonian description"), which says roughly: the unique kind of thing k such that k satisfies Hamiltonian equation H (there being a different definite description for each version of the Hamiltonian). Does the electron perfectly satisfy the above Hamiltonian descriptions, or does it only imperfectly satisfy them (in the sense of satisfaction of Sect. 2.3)? The electron cannot perfectly satisfy both; in fact it perfectly satisfies neither. But it is the best imperfect satisfier of both; but it satisfies less well the first Hamiltonian description embedded in classical theory, while it satisfies better the second Hamiltonian description embedded in relativistic theory (because the latter describes the behaviour of electrons better than the former). The difference lies in the added factors and the different functional relation between the expressions of the latter.

There are further versions of the Hamiltonian. A third form takes into account the novel property of the spin of the electron; in this case a newly discovered, but subsequently stable, property of electrons is accommodated within a new theory and yields a new Hamiltonian. A fourth version is the Hamiltonian due to Dirac; and a fifth builds on this by yielding additional terms to form a quantum field-theory account of the electron within quantum electro-dynamics (QED). And so on. Again, each of these Hamiltonians forms a Hamiltonian description. Does the electron perfectly satisfy all of these Hamiltonian descriptions? It does not, but the electron remains the best but imperfect satisfier of each of these Hamiltonian descriptions, with increasing degree of satisfaction.

The upshot is that for most of the twentieth century a case can be made for saying that (1) electrons perfectly satisfy the historically stable properties listed above; and (2) electrons satisfy imperfectly, but increasingly more accurately, a succession of descriptions built out of various Hamiltonians. Both (1) and (2) can provide identifying criteria for electrons, but (2) only within the context of the theory of identifying descriptions which allows for imperfect best satisfaction. These later descriptions build on earlier identifying descriptions which are qualitative and not quantitative in that they do not employ formulae such as the various Hamiltonians. But the succession of identifying descriptions, from the first one used by Plücker and his contemporaries to those which are based on the Hamiltonian, still manage to pick out the same entity, the electron, despite dramatic change in theory and rivalry in theory.

## 9. CONCLUSION

The story above argues for ontological continuity of the electron from its initial identification in the absence of any theory of the electron and via only its extrinsic properties, to later identifications through new criteria which begin to involve intrinsic properties and a succession of quite different theories, some rivalling one another. The story is told using a generalised version of Russell's theory of descriptions which is shown to be a special case of theory of the Ramsey Sentence as developed by David Lewis. To apply this version of the theory of descriptions it is necessary to draw a distinction between (a) features of theories and models of the item to be modelled, the electron, that are proposed to explain (b) the non-theoretical, experimentally determined properties or observable effects of the electron. Only the latter play a role in initially picking out and identifying the electron; if the former are included, then no story of ontological continuity can be told. This need not always be the case for entities postulated in physics and elsewhere. Sometimes the descriptions used do contain large amounts of a theory of the entity to be picked out. Two such examples are Schwarzschild's postulation of black holes (though he did not name them as such) as a development of the General Theory of Relativity shortly after it was published, and the 1930 postulation of the neutrino in the context of a theory designed to save the energy conservation principle. In such cases the existence or non-existence of such entities stands or falls with the theory used in their identification. In such cases the full generalised version of Russellian descriptions as developed by Lewis comes into play. But this is not the case for electrons; they were identified in a purely experimental context in ways which were not theory dependent. This is also the case for many other entities discovered in science but not mentioned here.

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