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Discussion

Prediction and the 'periodic law': a rejoinder to Barnes

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1. Introduction

My (2002) develops a general account of theory-confirmation that reconciles the following two judgments (both of which I regard as, on reflection, undeniable):

1. Suppose a theory T predicts some piece of evidence e without any need for special ad hoc assumptions, but rival theory T' subsequently accommodates e precisely by invoking such special ad hoc assumptions, then e still continues to provide a reason to prefer T. For example, the result of the two-slit experiment continued to support the wave theory even after corpuscularists had suggested ways to account for the fringes within their system (by invoking a complicated diffracting force whose expression involved a large number of parameters that could be adjusted to the known phenomena).
2. Scientists often use evidence in the construction of theories in ways that are entirely scientifically acceptable, and generally—and seemingly justifiably—think of evidence used in this way as supplying very strong support for the specific theory thus arrived at. So, for example, theoretical considerations may well leave the value of some parameter entirely free—a scientist will then use evidence e to fix the value of that parameter and will regard e as strong support for the specific theory (the one with the parameter value fixed) constructed in this way.

The reconciliation involves a distinction between what might be called 'intra'-paradigm (or intra-research programme) support and support *for the paradigm or research programme itself*. If, for example, e is used to fix the value of an initially free parameter λ and thus turn more general theory $T(\lambda)$ into more specific theory $T(\lambda_0)$ then, since e deductively entails $T(\lambda_0)$ *given* $T(\lambda)$, there is of course a clear sense in which e supports—indeed

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maximally supports—$T(\lambda_0)$. Evidence e *establishes* $T(\lambda_0)$, given that the more general theory $T(\lambda)$ has already been accepted. But e gives no *extra* reason to accept the more general $T(\lambda)$ itself (though of course *other* evidence may do so—see below).

By contrast, and despite the fact that the Duhem thesis always holds and so what entails evidential statements like e is always a more specific theory or theoretical framework involving specific assumptions and auxiliaries, there are other cases in which support 'spreads' from such a specific theoretical system to the underlying general theory itself. Such cases, I argued, fall into two categories: (i) cases where general theory T plus 'natural' auxiliaries entail e and (ii) cases where evidence e_1 is involved in constructing specific theory T' out of general theory T but where T' then turns out to make an *independently* testable (and observationally verified) prediction e_2. An example of (i) is the prediction of the (of course long known) phenomenon of stations and retrogressions by Copernican heliocentric theory. It is not true that stations and retrogressions are direct consequences of the heliocentric assumption (here, as always, the Duhem thesis holds), but they *appear to be* direct consequences precisely because the necessary further assumptions are so natural within the heliocentric approach. An example of (ii) is Leverrier's (ad hoc) use of the Uranian data to produce a specific version of Newtonian theory that gets those data correct, but at the same time predicts the further testable consequence of a hitherto unobserved planet. The account, surely correctly, says that the observations of Neptune did, but the Uranian data did not, give support to the underlying Newtonian theory.[1]

One immediate and pleasing consequence of this general account is that no part is played in empirical support by considerations of when a piece of evidence e was discovered to hold. *So long as e was not used in constructing some specific theory T* then, should e be entailed by T, there is no question of any reduction of support if e was known in advance of T's articulation (and conversely, of course, no question of any increase in support just because e was only discovered as a result of T's entailing it). In our (2001)—written after, but published before, my (2002)—Eric Scerri and I took a careful and detailed look at one of the cases from history of science where successful predictions (in the temporal sense) *have* generally been regarded as playing a very special role: namely the case of Mendeleev and his prediction on the basis of his periodic table of new elements such as gallium. We found, *inter alia*, that in line with the above general account, there is no historical evidence that the prediction of the new elements had any more important a role in supporting Mendeleev's 'periodic law' than did, for example, the successful accommodation within his periodic table of the independently discovered noble gases.

Barnes's paper (this issue) is first and foremost a critical attack on my general account of confirmation—a critical attack to which he appends some remarks suggesting that, because of failings in the general account, the specific account of the Mendeleev episode developed by Scerri and myself is itself awry in certain respects. Here I first explain *some* of the mistakes that Barnes makes concerning my general account of confirmation (Section 2); and then show why his criticisms of our account of Mendeleev also carry not the slightest weight (Section 3).

[1] I hope that this outline of my general account of confirmation will be enough for present purposes, but, for further details, interested readers should consult my paper (Worrall, 2002) and *not* the muddied rendition in Barnes (this issue). I develop the account and defend it against the similarly motivated but rival account of Deborah Mayo in my paper, Worrall (forthcoming, 2006).

2. Barnes on confirmation

2.1. My account of confirmation gives no role to scientists' 'motivations'

One underlying error made by Barnes is to suppose—despite explicit disclaimers both in my (2002) and in earlier work[2]—that my account gives a role to what 'motivated' or 'induced' or 'led' a scientist to produce some theory. He for example raises the issue of whether Copernicus might have been motivated to develop his heliocentric (or, more strictly, heliostatic) theory by the general phenomenon of planetary stations and retrogressions or whether Ptolemaists were 'motivated' to develop their auxiliary assumptions about epicycles by that data (Barnes, this issue); and later talks about what data might have 'induced' Mendeleev to propose his periodic law (ibid.).

My account gives no role to any such psychological factor. Although presented as a version of the 'heuristic approach', it is at root a *logical* theory of confirmation—the important logical relations being between (i) the evidence at issue *e*, (ii) the general theoretical framework involved T and (iii) the specific theory T' developed within that framework (and which (at least in the straightforward case) entails *e*). In the paradigm case, the chief question will be whether some parameter having a fixed value in T' was set at that value by theoretical considerations, or as a 'natural consequence' of such general considerations, or whether instead the value was fixed on the basis of the evidence (and if so whether the evidence needed to fix the value of the parameter was, or included, e itself). Whether or not Copernicus was personally inspired to develop his theory by planetary stations and retrogressions is entirely irrelevant—the only question is whether there was some free parameter in some theory available to him which could be fixed on the basis of stations and retrogressions to produce his heliocentric theory (and patently there was not).[3] To take a case where we have some clear historical evidence: there seems to be no doubt that Einstein was consciously searching for an account of gravitation that, of course amongst other things, would account for the well-known precession of Mercury's perihelion. But this fact about Einstein has no relevance for the issue of whether or not its account of Mercury's perihelion confirms the General Theory of Relativity—the already well known observations of Mercury *do* support GTR, because there is no parameter within that theory that could have been fixed on the basis of them so as to produce a specific theory that entailed those observations. On the other hand, Ptolemaists' 'intentions' and 'motivations' are also irrelevant but this time because there are parameters (reflecting the relative sizes and speeds of the deferent and epicyclic motions)[4] the values of which are not theoretically constrained but can only be fixed on the basis of evidence including that concerning stations and retrogressions.

As we shall see, Barnes's mistake here is consequential when it comes to his criticism of the Scerri/Worrall account of the Mendeleev story.

[2] Such as my paper, Worrall (1985).

[3] This is why Barnes's worry that Copernicus's 'core' theory might have been 'motivated' by the observations of planetary stations and retrogressions is unfounded.

[4] Barnes erroneously assumes that simply postulating epicycles yields an account of these phenomena within Ptolemaic theory.

2.2. My account of 'the' two-slit experiment

Critics of earlier formulations of the heuristic account of confirmation—such as Nickles, Mayo, and Howson (for references see Worrall, 2002)—essentially pointed out that scientists often deduce specific theories 'from the phenomena' (where this of course really means from the phenomena *plus* general background theoretical assumptions) and they will invariably (and entirely reasonably) regard the specific theory thus deduced as very strongly supported by those phenomena. I tried to provide a simple case of a 'deduction from the phenomena' to illustrate the real confirmational lessons at issue here.

The general ('classical') wave theory of light T entails (*modulo* a couple of idealisations) a functional relationship between the theoretical notion of wavelength and measurable slit and fringe distances in the two-slit experiment. It does not however specify any reason why a particular monochromatic light source (a sodium arc is the usual example) should emit light of any particular wavelength. However, using the functional relationship that *is* implied by T, together with the result, e, of the two-slit experiment *performed with light from a sodium arc*, a value of the wavelength of such light can be deduced and hence so can a more specific version T' of the general theory T. T' unlike T *does* specify the value of this theoretical quantity.

I argued in my (2002) that, once the logic is made clear, it is obvious that while e (the result of the two-slit experiment with light from a sodium arc) provides very good (in fact conclusive) reason to accept T', *given* that T has already been accepted, it provides absolutely no *further* reason to accept T itself.[5] After all, T would have been compatible with any particular outcome of the experiment—*so long, of course, as that outcome instantiated the general functional relationship between wavelength and measurable quantities* that T does entail (*modulo* idealisations and natural auxiliaries). I was taking it for granted that one of the experimental results that already supports the general version of the wave theory T is the *general form* of the interference pattern produced in the two-slit experiment (for example, the fact that, whatever the actual fringe-distances, the central portion of the observation screen is illuminated and is bordered on either side by dark fringes, symmetrically placed relative to that central fringe). Thus the general two-slit result, let's call it e', recording that, whatever the details, the fringe separations do instantiate this general functional relationship, already supports T. What I was claiming was that the extra details about the particular outcome when using, say, light from a sodium arc (not just, for example, that the two fringes next to the central one are symmetrically placed with respect to it, but that they are each particular distance d from the centre) gives no *extra* support to the general theory over and above the support it has from the general features of any such experiment with any monochromatic light source—although it does support (indeed establish) the specific theory involving the particular value for the wavelength, *relative to* the general wave theory.

[5] Scerri & Worrall (2001) explicitly refers to this evidence from the two-slit experiment using sodium light as providing 'no *extra* reason at all for accepting [the] general wave framework' (p. 425; emphasis added)—implicitly this means no extra reason beyond that supplied by the confirmation of the consequence of the general (free parameter) theory by the confirmation of the general functional relationship between fringes and other measurable distances in any version of the two-slit experiment. (By the way, this argument leading to the identification of particular wavelength values, contrary to Barnes's entirely historically baseless assumption, is *not* one given by Thomas Young.)

Contrary then to Barnes's claims, there is no substantial difference between my views about this particular case and the ones of his own that he reports: he talks about 'the' experimental result being 'dual layered'. It seems clearer to me to talk in terms of two different pieces of experimental information—the general two-slit pattern, e', and the particular pattern, e, obtained with light from a sodium arc. e' supports the general theory T on my view, while e provides no *extra* support for T over and above that already provided by e'; but e does support the particular version of T , T', indeed it establishes it, against the background of an already accepted T.

Again this misunderstanding of my general view about confirmation will turn out to be important when analysing Barnes's criticism of our account of Mendeleev.

2.3. The 'Gosse dodge'

Darwinian theory (quasi-)entails the existence of the fossil record. Do 'scientific' creationists catch up in terms of empirical support by adopting Gosse's wheeze of supposing that God simply decided to paint pretty pictures in the rocks and include bone-*like* structures in desert sands and so on as parts of his creation? The answer is clearly 'no' even though Creationism plus Gosse entails the fossil data. This is exactly the sort of case where using some data in the construction of a specific theory out of some more general framework produces no support for that specific theory—unless of course the specific theory went on to make some further independent, and also testable prediction (again whether or not temporally novel). In the form in which, as I understand it, Gosse and other creationists use the 'dodge', it definitely does *not* lead to any independently testable conclusions. As I point out, the Creationist programme, at least as construed in this way, in a loose sense gives itself an indefinite number of free parameters and then proceeds to fill them in on the basis of observation: God created the world in 4004 BC or whenever; what are the details of his creation?; well, just observe and basically whatever you see is what he created. This heuristic commits Creationists to a position that can never obtain any empirical support on the view of empirical support I endorse.

Gosse's approach in particular, as I understand it, was exactly to 'read off' the details of the creation from the data; hence he would have claimed that we know that God painted this and this pretty picture in the rocks (reinterpreting the 'fossil' data that was already available), and, whatever other 'fossils' happen to be discovered in the future, the same 'explanation' will apply.

Barnes (this issue) imagines a fairytale Creationist who, on the basis of the known 'fossil record' f_1 *somehow or other* produces a generalisation f* that presumably (Barnes is not altogether clear) says that the *overall* details of God's handiwork concerning pretty pictures in rocks of various types and bone-like structures, etc. is thus and so. f*, unsurprisingly, entails f_1, but also—miraculously—turns out to be independently confirmed by subsequently discovered 'fossil' data. Of course this would be miraculous precisely because his general theoretical framework gives such a creationist no reason at all to prefer the particular f* over any other extension of f_1 that would yield contradictory predictions.

Barnes believes that my account of confirmation is committed to the view that the general Creationist 'theory' would—in this fairytale scenario—be supported by the empirical success of f*. In fact, however, I entirely agree with him that, in so far as the scenario can be taken seriously at all, no such support would accrue. This shows again just how far Barnes is from understanding my position which, I emphasise again, highlights the logical relationships between the evidence, general theory, and the specific theory developed within the framework specified by the general theory. *Provided* that, as I supposed above, our creationist has not (equally miraculously, I'd say) found some hitherto unexploited idea within his research programme that indicates that f* is indeed the correct generalisation of the known 'fossil' record f_1, then there is of course not the slightest reason why any empirical success of f* should redound to the credit of the basic ideas underlying that programme. This is precisely what my account entails—exactly because of the absence in this case of the appropriate relationships between the general and specific theories. The sorts of case in which what Barnes ill-advisedly calls 'conjunction predictions' *do* provide support can be illustrated using my wave theory example. Having fixed the theoretical parameter for the wavelength of light from a sodium arc using the detailed outcome of the two-slit experiment using such light, the same value of that parameter will of course be plugged into *other* experimentally testable functional relationships, such as that describing the outcome of 'the' (that is the general, wavelength-not-specified) one-slit experiment. Should these precise predictions of the outcome of the one-slit experiment with light from a sodium arc be verified then this will of course provide important support not only for the more precise version of the wave theory with this parameter fixed but for the wave theory in general. However we can see that there are tight logical constraints imposed by the general theory here that are an integral part of the reason why any scientist would regard such a verification of such a prediction as a success for that general theory. In particular the general theory entails of course that monochromatic light of a particular kind has but one wavelength! Hence having, relative to the general theory, fixed the wavelength for light from a sodium arc on the basis of the two-slit experiment, the scientist is of course constrained by that same general framework to use the same value when making the precise prediction about the one-slit diffraction result in sodium light. The fairytale Creationist, by contrast, and as I already pointed out, is operating in a completely 'random' way, unaffected by any theoretical constraints, in postulating f* rather than any other f*' that would differ from it but equally well entail f_1.

2.4. Probabilities and the 'correlation thesis'

Barnes believes that what confirmation is really about are probabilities (or rather increased probabilities). The reason why, for example, planetary stations and retrogressions R give better support to the Copernican theory than the Ptolemaic is essentially that, taking CC and PC to stand for the core Copernican and Ptolemaic theories (unadorned by any auxiliaries involving epicycles and the like), $P(R/CC) > P(R/RC)$.

He considers the possibility (the 'Correlation thesis') that 'Facts about use-novelty are reliably correlated with facts about probabilities...' (Barnes, this issue). But then dismisses this suggestion on the grounds of alleged errors in my account—allegations that I have already dealt with and shown to be based on misunderstandings.

It is indeed tempting to think that 'the probability' of stations and retrogressions is higher on the core Copernican than on the core Ptolemaic view. However I find it strange in view of decades of criticisms of probabilistic approaches to confirmation that Barnes is so ready to talk about 'facts' about probabilities or '*the* values' of probabilities in this connection, and still stranger that he holds that these 'facts' may have *explanatory* value. It is well known that all attempts to develop any objective version of Bayesianism have foundered (at any rate so far) and that the only remotely philosophically defensible approach currently available is that of personalist Bayesianism. On this approach we can talk only of 'the' value of a probability *in the case of a particular agent*. Because of the 'subjectivity of the priors', one agent/scientist might regard P(R/CC) > P(R/RC), while another such agent to the contrary holds that P(R/PC) > P(R/CC). This approach, then, cannot yield the sort of objective judgments about confirmation that I, and I take it Barnes, want to support. The relevant judgments can—loosely—be expressed using probabilities, but far from it being the case that 'it is ultimately the probabilistic facts that are "doing the work" (ibid.), it is the sort of logical connections between evidence, general and specific theories highlighted in my approach that really do the work. Rather than our being able to 'see' directly that the probability of there being stations and retrogressions is higher on the basic heliocentric view, this probability judgement is a (loose) expression of the fact that natural auxiliaries within the heliocentric view lead to stations and retrogressions, while 'unnatural' auxiliaries—put in 'by hand' because they have to be read off the data—are the only ones that will do the job within the geocentric framework.

3. Barnes on Mendeleev

There are some aspects of Barnes's analysis of our (2001) that I initially found mystifying. He frequently claims that we presented a 'predictivist' account of the Mendeleev episode, and, although he sometimes indicates that he has understood that our general account is predictivist only if predictions are understood to include what we call, for clarificatory purposes, accommodations$_1$, he barely mentions until very late in the piece that one of our central claims is that Mendeleev's table (or rather Mendeleev's 'periodic law' that rather loosely underpins it) was well supported by various accommodations of independently known elements, and that the predictions of new elements although certainly important were far from being the be all and end all as they are often portrayed. He states (this issue) that our view is that 'a particular successful prediction (of say, a new chemical element's existence) supported the periodic law more strongly than a particular accommodation of a known element'; whereas we explicitly claim that the accommodation of the noble gases, like argon, played an equally significant role in the acceptance of Mendeleev's views as any prediction of a new element. (It is true that argon was not known when Mendeleev began to produce his tables, but it became known quite independently of those tables and the underlying 'law'.) Finally, he claims that one of our 'primary theses' is that 'while predictivism does hold true in this case, the degree of difference of support between a successful prediction and an accommodation is substantially less than earlier proponents of predictivism like Maher and Lipton have argued' (ibid.). In fact not only is this not central, we explicitly dismiss Brush's claim to this effect as based on error and we hold it to be true only in an attenuated sense explained below.

These mysteries may result from the fact that Barnes regards the whole analysis we provide as, quite literally, incoherent ('There is thus a serious tension among S & W's various claims [about Mendeleev and confirmation] in their 2001 paper' (ibid.)). No wonder then that he could make no real sense of it. There is in fact no such 'serious tension', as I shall now try to explain.

Perhaps Barnes's basic complaint concerning the Mendeleev material is that the question of how it relates to the general account of confirmation developed in my (2002) and sketched in our (2001) 'is not dealt with in much detail by S & W' (Barnes, this issue, p. XX). In fact the whole of Section 5 of our paper, covering some ten pages, was devoted entirely to this issue. It does, however, leave one aspect perhaps rather less clear-cut than it might have been and so I am glad to have the opportunity to explain this further. However let me begin by reiterating what we say several times in our (2001) and what is indeed the burden of the whole of Section 4 of that paper—that we are not to expect in dealing with confirmation in chemistry the same sort of sharpness that we meet in typical (and roughly equivalent) cases in physics. Mendeleev's table, of course, makes no predictions, whether temporally novel or not. What does make predictions, if anything, is the 'periodic function' embodied in the 'periodic law' that Mendeleev saw as underpinning his (various) tables. And, as we say in our (2001), p. 437:

> It is no surprise that Mendeleev never gave precise mathematical expression to this 'periodic function'. In fact, it would be impossible, we claim, to state at all precisely the content of Mendeleev's 'periodic law'.

This is in sharp contrast to cases from physics—such as that of Fresnel's wave theory of diffraction and the relative confirmational impact of the known straightedge diffraction and the (novel) white spot results[6]—where it *is* possible to state the relevant theory at issue precisely. This fact about Mendeleev's views inevitably means that the judgments of confirmation to be endorsed here are rather more subtle, and less clear-cut, than one might like.

Since no precise articulation of Mendeleev's periodic law can be provided, there is no question of checking how many free parameters there were in its initial formulation that had to be fixed on the basis of evidence about the properties of particular known elements (known in the sense of 'known to Mendeleev when he began his codificational work'). It is clear that Mendeleev had a general commitment to *some sort* of periodicity and then operated by 'pattern spotting' and interpolation and extrapolation. The question of how many of the properties of previously known elements he needed in order to set out any of his particular tables does not, then, have a precise answer. But clearly not all of those properties. That is, this was not a case of an ersatz regularity produced by 'generalising' on all the known instances—otherwise a relatively simple ordering would not have been produced and the judges of the Davy Medal would not have been impressed by the 'marvellous regularity' produced in ordering the known elements in his table/s. So this was clearly in a vague sense (though the vagueness, note, results from the science not the account of confirmation) a case of using *some* of the known properties of the known elements and going on to make some (non-novel) predictions about other known elements. There is thus some discount to be made with respect to the support the 'Law' receives from the *known elements in Mendeleev's original table,* but taken as a whole they certainly do support it—it is just that the extent is not entirely quantifiable. Notice that even in this less than clear-cut situation, facts about Mendeleev's psychological 'motivation' are still irrelevant. I have no doubt that Mendeleev was 'motivated' to codify the properties of *all* elements known to him; but this does not mean that he needed all the elements and their properties to generate the patterns that he articulated in his tables. The fact that Barnes thinks that my view *does* involve recourse to Mendeleev's motivations may well be a large part of the reason for the 'tension' that he sees between our claims about the confirmational power of the known elements and the general view (see Barnes, this issue).

It was the fact that this judgment about the elements originally known to Mendeleev is not as clear-cut as one might like, that led us to concentrate on a quite different case of

[6] See my paper, Worrall (1989).

accommodation in our attempt to criticise the usual versions of the predictivist story. This is the altogether clearer case of the accommodation of the noble gases, such as argon. Argon was discovered only after Mendeleev began to produce his tables, but far from being predicted by those tables (or rather quasi-predicted by the underlying law), it was discovered quite independently and indeed was initially regarded as certainly a difficulty for, and even perhaps a possible refutation of, that law. It is surprising that Barnes never so much as mentions this—truly central—part of our analysis.

Argon, along with helium, was eventually 'accommodated' within Mendeleev's table—by dint of adding a new group within the table. We argue that, historically speaking, the accommodation of the noble gases seems to have been regarded as providing equally impressive confirmation for Mendeleev's 'law' as any prediction of a new element. What would one have expected on the general account of confirmation we endorse? One might, at first, think that the addition of new group is exactly the sort of ad hoc, intuitively untestable, manoeuvre that our account downgrades and hence that our account is in tension with the history. However, as we argued, this first impression is misleading:

> At first sight, the accommodation of argon and helium by inventing a new group looks exactly like the sort of ad hoc accommodation$_2$, that we insisted ought to carry less evidential weight. Surely inventing a new group for these elements is exactly a case of 'writing a phenomenon into' a pre-accepted theory without any independent testability?… [A]ppearances are deceptive. It is not simply a question of inventing a new section of the table to fit the noble gases into, it must then also be checked that the periodicities previously noted in terms of valencies, 'analogous' properties and the like among the already accommodated elements are preserved. The atomic weights of the four newly discovered noble gases have to be such that each one would fit into a particular space in each successive period of the table. That is, each of these atomic weights had to be intermediate between two other elements in each period. In addition, this insertion of the four new elements had to result in all of them lying vertically below one another in the newly created group. These are stringent (and ultimately empirically based) constraints: it is perfectly conceivable that there was no way of placing the noble gases into the table that simultaneously satisfied all those constraints. In effect, creating a new group for the noble gases leads to a new series of predictions (in the atemporal sense) about already known analogies between elements. (Scerri & Worrall, 2001, pp. 445–446)

The history then in respect of the positive impact of this successful accommodation is perfectly in line with our general account. As for the negative side, this account would lead one to expect that particular assumptions made entirely ad hoc to fit elements into the Table that were not independently testable would not be accorded any empirical support despite achieving that fit. And we show that again the historical record seems to be in accord with this philosophical prediction:

> For example, Rayleigh and Ramsey … suggested that the gas they investigated [which was eventually accepted as being pure argon] might consist not of a single element but of a 93.3% to 6.7% mixture of elements with atomic weights 37 and 82. Needless to say the 93.3% to 6.7% split was exactly designed to give atomic weights that might fit into the table—a classic case of parameter-adjustment … This

> suggestion was never taken up—precisely [we suggest] because the *only* evidence in its favour was accommodated$_2$ evidence. (Ibid., p. 447)

Again it is surprising that Barnes never even mentions this part of our analysis.

Once the problems that Barnes sees in Worrall's (2002) account of these are eliminated (see Sect. 2 above), there is no hint of any 'tension' between that account and Scerri and Worrall's (2001) account of the Mendeleev story—again, once this account is properly understood. The issue is at intuitive root that of independent testability. Our account would suggest that successful independent tests of Mendeleev's 'law', whether involving known or hitherto unknown elements, would be taken as supporting that scheme; while specific theoretical claims made in the attempt to save the scheme without being independently testable would carry no weight. Our historical analysis shows that, far from being in any tension, either internally or with the history, this account of confirmation is both entirely coherent and in what is, bearing in mind the vagueness involved in this case, remarkable agreement with the historical record.

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