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How Was Nicholson's Proto-Element Theory Able to Yield Explanatory as well as Predictive Success?

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6.1 Introduction

Let me begin by saying how grateful I am to Peter Vickers and Tim Lyons for having invited me to two of their conferences.[1] I can honestly say that I have seldom participated in such interesting interdisciplinary settings and from which I learned so much. The following article is based on a lecture that I gave at one of these meetings on the work of the English mathematical physicist John Nicholson. This particular episode in the history of physics has received little attention in the scientific realism debate even though Nicholson's theory had considerable explanatory and predictive success. What makes the case all the more remarkable is that by most standards almost everything that Nicholson proposed was overturned.

My study is most closely connected with the research interests of Peter Vickers who has published on the subject of inconsistent theories.[2] To quote from the brief directive that was given to speakers by the conference organizers: "The following is among the key historical questions to be asked: To what extent did theoretical constituents that are now rejected lead to significant predictive or explanatory successes?" Many authors, some of whom are represented in this

[1] I presented papers at the History of Chemistry and Scientific Realism meeting held in Indianapolis between December 6 and 7, 2014, as well as the meeting Contemporary Scientific Realism and the Challenge from the History of Science that also took place in Indianapolis.
[2] P. Vickers, *Understanding Inconsistent Science*, Oxford, Oxford University Press, 2013.

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volume, appear to be somewhat puzzled by the fact that some inconsistent theories were able to attract a good deal of success. However, in the approach that I will be proposing, such features are seen to arise in a far more natural manner.

I will consider the work of Peter Vickers on quantum theory and quantum mechanics as an example of the current research that has focused on the scientific realism debate and the challenge from the history of science. In his recent book Vickers considers two well-trodden examples from the history of quantum theory. The first one consists of Bohr's calculation of the spectrum of He^{+}, a feat that brought admiration from no less a person than Albert Einstein when it was first published. Several authors have claimed that Bohr's theory was in several respects inconsistent to the point of containing internal contradictions.[3] And yet Bohr succeeded not only in calculating exactly the energy of the hydrogen atom but in also assigning the electronic configurations of many atoms in the periodic table. Even more dramatically, Bohr gave a highly accurate calculation for the energy of the helium +1 ion. The second episode began when Fowler criticized Bohr's initially published theory because it predicted an energy of precisely four times that of the hydrogen atom, or 4 Rydbergs, when in fact experiments revealed the energy of He^{+} to be 4.00163 Rydbergs. In response, Bohr pointed out that he would take account of the reduced mass of the electron in order to carry out a more accurate treatment of the problem.[4] On doing so he obtained an energy of 4.00160 Rydbergs, which is what led Einstein to say, "This is an enormous achievement. The theory of Bohr must then be right."[5]

Vickers recounts a similar story concerning Arnold Sommerfeld.[6] It emerges that precisely the same formula is featured in Sommerfeld's semi-classical atomic theory as it is in the fully quantum mechanical theory that came later. This equivalence occurs in spite of the fact that the existence of electron spin and the wave nature of electrons, which play a prominent role in the fully quantum mechanical theory, were completely unknown when Sommerfeld published his account. Many commentators wonder how Sommerfeld could have arrived at the correct formula without knowing about these aspects of the more mature quantum mechanics, as opposed to the old quantum theory within which he worked. For example, while Sommerfeld assumed definite

[3] T. Bartelborth, "Is Bohr's Model of the Atom Inconsistent?" in P Weingartner and G. Schurz, eds., *Philosophy of the Natural Sciences, Proceedings of the 13th International Wittgenstein Symposium : H*, 1989, pp. 220–223.

[4] Bohr's revised calculation essentially consisted of considering the reduced mass of the hydrogen atom rather than assuming that the nucleus was infinitely heavier than the electron.

[5] http://galileo.phys.virginia.edu/classes/252/Bohr_to_Waves/Bohr_to_Waves.html#Mysterious%20Spectral%20Lines.

[6] Vickers's more recent views on the Sommerfeld question appear in P. Vickers, Disarming the Ultimate Historical Challenge to Scientific Realism, *British Journal for the Philosophy of Science*, 2018.

trajectories or orbits for electrons, the quantum mechanical account denies the existence of orbits and prefers to speak of orbitals in which definite particle trajectories are abandoned.

The response from somebody wishing to maintain a form of realism might be to claim that something about Sommerfeld's theory may have been correct, and it is that part of the theory which accounts for the fact that the two mathematical formulas in question were identical. Those wanting to defend such a position might claim that the core ideas were correct and were responsible for Sommerfeld success, in spite of his lack of knowledge of electron spin, the wave nature of electrons, and any other such later developments. Such a preservative realist would presumably claim that *some* aspect of Sommerfeld's theory was latching on to "the truth." There has been much discussion of this issue in the philosophy of science literature and especially in connection with the realism and anti-realism debate. Realists have tended to appeal to the slogan of divide and conquer (*divide et impera*) that was first proposed, as far as I am aware, by my old friend from graduate school in London, Stathis Psillos. Such an approach, although perhaps not precisely under the same banner, has also been promoted by Philip Kitcher and Larry Laudan.[7] Meanwhile Carrier, Chang, Chakravartty, and Lyons among others have expressed strong reservations about this strategy.[8] It should be noted that these debates have usually been waged over what were once very successful theories and scientific entities such as caloric, phlogiston, or the ether.

As I will endeavor to show, it becomes more difficult to argue for some form of "preservative realism" in cases like the one I am about to present, concerning the mathematical physicist John Nicholson who was a contemporary of Niels Bohr at the time of the old quantum theory. In fact I shall be arguing that it is rather difficult to find *anything* about Nicholson's view that has been preserved, except perhaps for the notion of the quantization of angular momentum, which ironically did not play a prominent role in any of the success that Nicholson's theories achieved at the time when they were first presented. What the defenders of preservative realism recommend in

[7] P. Kitcher, *The advancement of science*, Science without legend, objectivity without illusions, Oxford, Oxford University Press, 1993; L. Laudan, "A Confutation of Convergent Realism, *Philosophy of Science*, 48, 19–48, (1981).

[8] M. Carrier, "Experimental Success and the Revelation of Reality: The Miracle Argument or Scientific Realism." In Knowledge and the World: Challenges beyond the Science Wars, ed. Martin Carrier, Johannes Roggenhofer, Günter Küppers, and Philippe Blanchard, 137–61. Berlin: Springer (2004); A. Chakravartty, *A Metaphysics for Scientific Realism: Knowing the Unobservable*. Cambridge: Cambridge University Press (2007); H. Chang, "Preservative Realism and Its Discontents: Revisiting Caloric." *Philosophy of Science* 70, (5), 902–12 (2003); T.D. Lyons, "Scientific Realism and the Pessimistic Meta-modus Tollens." In *Recent Themes in the Philosophy of Science: Scientific Realism and Commonsense*, ed. Steve Clarke and Timothy D. Lyons, 63–90. Dordrecht: Kluwer, (2002).

the case of theories that featured the caloric or phlogiston, is that some parts of the theories tracked the truth while others did not. However, this strategy cannot be deployed in the case of Nicholson. Another strategy, that has been recommended by John Worrall, has been the notion of structural realism. Briefly put, this is the view that although the entities postulated by successive theories might change as history unfolds, the underlying mathematical structure is seen to persist and to display continuity. I believe that this form of strategy too will also fail in the case that I will be examining, all of which leads me to say that my historical case has a good deal to contribute to the general debate addressed in the present volume. I will now give a review of the scientific work in atomic physics of John Nicholson before turning back to the wider questions as to what one should make of the question of realism and the development of scientific theories.

6.2 Introduction to John Nicholson and His Early Work

John Nicholson was born in Darlington in County Durham in 1881. He attended Middlesbrough High School and then the University of Manchester, where he studied mathematics and physical sciences. He continued his education at Trinity College, Cambridge, where he took the mathematical tripos exams in 1904.[9] Nicholson won a number of prizes at Cambridge including the Isaac Newton Scholar Prize for 1906 and was a Smith Prizeman in 1907, as well as an Adams Prizeman in 1913 and again in 1917. His first position was as lecturer at the Cavendish Laboratory in Cambridge, followed by a similar position at Queen's University in Belfast. In 1912 Nicholson was appointed professor of mathematics at King's College, London. In 1921 he was named fellow and director of studies at Balliol College, Oxford, before retiring in 1930 due a recurring problem with alcoholism. He died in Oxford in 1955.

Nicholson proposed a planetary model of the atom in 1911 that had certain features in common with those of Jean Perrin, Hantaro Nagaoka, and Ernest Rutherford,[10] in that he placed the nucleus at the center of the atom. However, it

[9] The mathematical tripos was a distinctive written examination of undergraduate students of the University of Cambridge, consisting of a series of examination papers taken over a period of eight days in Nicholson's time. The examinations survive to this day although they have been reformed in various ways. A. Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics*. Chicago: The University of Chicago Press, 2003.

[10] E.R. Scerri, *The Periodic Table, Its Story and Its Significance*, New York, Oxford University Press, 2020.

Figure 6.1. John Nicholson

must be emphasized that Nicholson arrived at this conclusion independently of Rutherford and the other physicists just mentioned.

In fact, Nicholson's model had more in common with that of Thomson, which regarded the electrons as being embedded in the positive charge that filled the whole of the volume of the atom. Thomson's later models envisaged electrons as circulating in rings, but still within the main body of the atom. More specifically, the way in which Nicholson's model resembled those of Thomson lies in the mathematical analysis and the concern for the mechanical stability of the system.

Where Nicholson's model differed from all previous ones, regardless of whether planetary or not, was in his emphasis on astronomical data. He postulated a series of proto-atoms, as he called them, that would combine to form the familiar terrestrial elements. Nicholson believed that the proto-atoms, and the corresponding proto-elements, existed only in the stellar regions and not on the earth. This way of thinking was part of a British tradition that included William Crookes and Norman Lockyer, each of whom believed in the evolution of the terrestrial elements from matter present in the solar corona and the astronomical nebulae. Like Crookes

element	symbol	nuclear charge	atomic weight
coronium	Cn	2e	0.51282
hydrogen*	H	3e	1.008
nebulium	Nu	4e	1.6281
Protofluorine	Pf	5e	2.3615

Figure 6.2. Nicholson's proto-elements.
Note: hydrogen* does not represent terrestrial hydrogen but hydrogen the proto-element.

and Lockyer, Nicholson was an early proponent of the study of spectra for gaining a deeper understanding of the physics of stars as well as the nature of terrestrial elements.

The particular details of Nicholson's proto-atoms were entirely original and are represented in the form of a table (Figure 6.2). The first feature to notice is a conspicuous absence of any one-electron atom.[11] This is because Nicholson believed that such a system would be unstable according to his electromagnetic analysis.[12]

For Nicholson, the identity of any particular atom was governed by the number of positive charges in the nucleus, regardless of the number of orbiting electrons present in the atom. Nicholson may thus be said to have anticipated the notion of atomic number that was later elaborated by van den Broek and Moseley. Nicholson argued that a one-electron system could not be stable since he believed this would produce a resultant acceleration towards the nucleus. By contrast, Nicholson assumed that two or more electrons adopted equidistant positions along a ring so that the vector sum of the central accelerations of the orbiting electrons would be zero. The smallest atom therefore had to have at least two electrons in a single ring around a doubly positive nucleus.[13]

By using his proto-atoms, Nicholson set himself the onerous task of calculating the atomic weights of all the elements and of explaining the unidentified spectral lines in some astronomical objects such as the Orion nebula and the solar corona. It is one of the distinctive features of Nicholson's work that

[11] Nicholson rejected a one-electron atom because he believed that at least one more electron was needed to balance the central acceleration of a lone electron. See p. 163 of McCormmach, "The Atomic Theory of John William Nicholson," *Archives for History of Exact Sciences,* 3, 160–184, 1966, for a fuller account.

[12] Nicholson's list of proto-elements was extended to include two further members in 1914 when he added proto-hydrogen with a single electron and archonium with six orbiting electrons.

[13] By the year 1914, he had accepted the possibility of a one-electron atom. See H. Kragh, "Resisting the Bohr Atom," *Perspectives in Physics*, 13, 4–35, (2011).

his interests ranged across physics, chemistry, and astrophysics and that he placed great emphasis on astrophysical data above all other data forms.

6.3 Accounting for Atomic Weights of the Elements

Of the four proto-atoms that Nicholson originally considered, he believed that coronium did not occur terrestrially.[14] He therefore set out to accommodate the atomic weights of all the elements in terms of the three remaining proto-atoms, namely his special kind of hydrogen, nebulium, and proto-fluorine. It is important to consider the relative weights that Nicholson attributed to the proto-atoms and to delve a little further into Nicholson's theory.

Although Rutherford's planetary model had recently been proposed, Nicholson's work was much more indebted to the earlier Thomson model. As is well known, Thomson regarded the atom as consisting of a diffuse positive charge in which electrons were embedded as "plums in a pudding."[15] In a later development the electrons were seen as circulating in concentric rings but still within the main body of the positive charge.

According to Thomson, the orbital radius of any electron had to be less than the size of the atom as a whole. However, Nicholson rejected this notion for reasons that were quite independent of the arguments that were being published by Rutherford at about the same time. There is a sense in which Nicholson's atom can be said to have been intermediate between that of Thomson and the later one due to Rutherford. Nicholson retained much of the mathematical apparatus that Thomson had used to argue for the mechanical stability of the atom but required that the positive nucleus should shrink down to a size much smaller than the radius of the electrons. As a consequence, Nicholson could no longer use estimates of the size of the atom to fix the radius of the electron orbits. On the other hand, Nicholson could use his atom to give what seems to have been an excellent accommodation of the atomic weights of all the elements and some astronomical spectral lines as will be discussed later.

[14] Some of the proto-elements postulated by Nicolson had been invoked earlier by other authors. For example, Mendeleev had predicted the existence of an element called coronium and Emmerson had featured proto-fluorine in one of his periodic tables. B.K. Emmerson, "Helix Chemica, A Study of the Periodic Elements," *American Chemical Journal*, 45, 160–210, (1911).

[15] It turns out that the name "plum pudding" was never used by Thomson nor any of his contemporaries. A.A. Martinez, *Science Secrets*, Pittsburgh, PA, University of Pittsburgh Press, 2011. Because of the currency of the term I will continue to refer to it as such.

In order to see precisely how Nicholson envisaged his atom we consider his expression for the mass of an atom, which he published between 1910 and 1911 in a series of articles[16] on a theory of electrons in metals.[17]

$$m = \frac{2}{3}\left(\frac{e^2}{rc^2}\right)$$

In this expression m is the mass of an atom, e the charge on the nucleus, r the radius of the electron's orbit, and c the velocity of light. This expression can be simplified to read

$$m \propto e^2/r \tag{i}$$

given the constancy of the velocity of light. Nicholson also assumed the positive charge, ne, for any particular nucleus with n electrons would be.

$$ne \propto V$$

Next by substituting e = ne into (i) he obtained

$$m \propto n^2 e^2/r \tag{ii}$$

He also assumed that the positive charge would be uniformly distributed throughout a sphere of volume V so that

$$ne \propto V$$

$$\text{or } ne \propto r^3 \qquad (\text{since } V \propto r^3),$$

$$\text{and so} \qquad r \propto n^{1/3}$$

Substituting into (i) the nuclear mass would take the form

$$m \propto \frac{n^2}{n^{1/3}}$$

or

$$m \propto n^{5/3}$$

[16] Nicholson's theory of metals appears in, J.W. Nicholson, "On the Number of Electrons Concerned in Metallic Conduction," *Philosophical Magazine*, series 6, 22, 245–266, (1911).

[17] Interestingly, Niels Bohr's academic career also began in earnest with his development of a theory of electrons in metals.

Coronium	Cn	=	0.51282
Hydrogen	H	=	1.008
Neptunium	Nu	=	1.6281
Proto-fluorine	Pf	=	2.3615

Figure 6.3. Relative weights of Nicholson's proto-atoms.

Gas	Formula	Calculated atomic weight	Observed atomic weight
helium	Nu + Pf	3.99	3.99
neon	6(Pf+H)	20.21	20.21
argon	$5He_2$	39.88	39.88
krypton	$5(Nu_4(Pf+H)_3)$	83.0	82.9
xenon	$5(He_4(Pf+H)_3)$	130.29	130.2

Figure 6.4. Nicholson's calculations and observed weights of the noble gases.
Note: Slightly modified table based on a report of Nicholson's presentation.
Source: *Nature*, 87, 2189-501-501, 1911.

At this point Nicholson assigned the mass of 1.008 to his proto-atom of hydrogen,[18] which allowed him to estimate the relative masses of the other proto-atoms as shown in Figure 6.3.

From here Nicholson combined different numbers of these three particles (omitting Cn) to try to obtain the weights of the known terrestrial elements (Figure 6.4). For example, terrestrial helium could be expressed as,

$$He = Nu + Pf = 3.9896,$$

a value that compares very well with the weight of helium that was known at the time, namely 3.99.[19]

[18] This step seems a little odd given Nicholson's statements to the effect that hydrogen the proto-atom is not necessarily the same as terrestrial hydrogen. In using a mass of 1.008 he surely seems to be equating the two.

[19] The error amounts to approximately 0.3 of one percent. Moreover, Nicholson takes account of the much smaller weight of electrons in his atoms. After making a correction for this effect he revises the weight of helium to 3.9881 (or, to three significant figures, 3.99) in apparent perfect agreement with the experimental value. Such was the staggering early success of Nicholson's calculations. See J.W. Nicholson, "A Structural Theory of the Chemical Elements," *Philosophical Magazine* series 6, 22, 864–889, 871–872, (1911).

H	H	1.008	1.008
He	Nu+Pf	3.99	3.99
Li	3Nu+2H	6.90	6.94
Be	3Pf+2H	9.097	9.10
B	2He+3H	11.00	11.00
C	2He+4H	12.00	12.00
N	2He+6H	14.02	14.01
O	3He+4H	15.996	16.00
F	3He+7H	19.020	19.00
Ne	6(Pf+H)	20.21	20.21
Na	4He+7H	23.008	23.01

Figure 6.5. Nicholson's composite atoms for the first 11 elements in the periodic table.

Nicholson's calculations of atomic weights were not confined to just the first few elements as shown in the Figure 6.3. He was able to extend his accommodation to all the elements up to and including the heaviest known at the time, namely uranium, and to a very high degree of accuracy. For example, Figure 6.4 shows his calculations as well as the observed atomic weights for the noble gases. Meanwhile Figure 6.5 shows the calculated and observed weights for the first eleven elements in the periodic table.[20]

Nicholson's contemporaries initially reacted to his work in a positive but cautious manner. For example, one commentator wrote,

> The coincidence between the calculated and observed values is great, but the general attitude of those present seemed to be one of judicial pause pending the fuller presentation of the paper, stress being laid on the fact that any true scheme must ultimately give a satisfactory account of the spectra.[21]

Nicholson promptly rose to this challenge and provided precisely such an account of the spectra of some astronomical bodies in his next publication.

This contribution involved the hypothetical proto-element nebulium, which Nicholson took to have four electrons orbiting on a single ring around a central positive nucleus with four positive charges. Like his other proto-elements,

[21] Anonymous, *Nature*, 501, (October 12, 1911).

[20] Figure 6.5 did not appear in Nicholson's own papers but in a 1911 article in *Nature* magazine as part of a report on the annual conference of the British Association for the Advancement of Science meeting at which Nicholson had presented some of his findings.

Nicholson did not believe that this element existed on the earth but only in the nebulae that had long ago been discovered by astronomers, such as the one in the constellation of Orion. Following a series of intricate mathematical arguments building on Thomson's model of the atom, Nicholson found that he could account for many of the lines in the nebular spectrum that had not been explained by others who had only invoked lines associated with terrestrial hydrogen or helium.

Nicholson's feat could well be regarded as a numerological trick, given that it is always possible to explain a set of known data points given enough doctoring of any theory. In fact when it was first publicly proposed at a meeting of the British Society for the Advancement of Science the reaction was indeed one of further caution. A report that appeared in the magazine *Nature* stated that,

> Dr. J.W. Nicholson contributed a paper on the atomic structure of elements, with theoretical determinations of their atomic weights, in which an attempt was made to build up all the elementary atoms out of four prolytes containing respectively 2, 3, 4 and 5 electrons in a volume distribution of positive electricity. Representing the prolytes by the symbols Cn (coronium), H (hydrogen), Nu (nebulium), Pf (protofluorine), the accompanying table indicates the deductions of the author with regard to the composition of several elements, allowance being made for the masses of both positive and negative electrons.[22]

Scientists usually demand that a good theory should also make successful predictions so as to avoid any suspicion that a theory may have been deliberately rigged in order to agree with the experimental data.[23] Surprisingly enough, Nicholson's theory was also able to make some genuine predictions. In addi-tion to providing a quantitative accommodation of many spectral lines that had not previously been identified, Nicholson predicted several experimental facts that were confirmed soon afterward.

Nicholson assumed that each spectral frequency could be identified with the frequency of vibration of an electron in any particular ring of electrons. Furthermore, he believed that these vibrations took place in a direction that was perpendicular to the direction of circulation of the electrons around the nucleus (Figure 6.6). The model that was eventually developed by Bohr in 1913 differed fundamentally, in that spectral frequencies are regarded as resulting

22 Anonymous, *Nature*, 501 (October 12, 1911).

23 There is nevertheless a long- standing discussion in the philosophy of science regarding the relative worth of temporal predictions as opposed to accommodations, or retro-dictions as they are sometimes termed. See, S.G. Brush, *Making 20th Century Science*, New York, Oxford University Press, 2015.

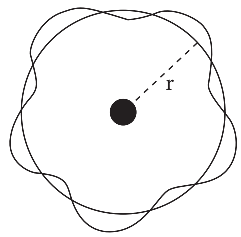

Figure 6.6. Nicholson's atomic model.

Note: Figure created by the present author; no diagram was published by Nicholson. As the electron orbits the nucleus Nicholson supposes that it oscillates in a direction at right angles to the direction or circulation.

from differences between the energies or frequencies of two different levels in the atom. Bohr's spectral frequencies do not correspond directly to any actual orbital frequency that an electron possesses. And it was this new understanding of the relationship between spectra and energy levels that provided Bohr with one of the main ingredients of his own theory. On the face of things, Nicholson was therefore simply wrong, since he based his whole theory on what we now know to be incorrect physics.

But such a view is a typical example of Whiggism and remains at the level of "right" and "wrong" that I will be aiming to move beyond. Parts of Nicholson's theory seem to have succeeded very well, given that many scientists were impressed by his explanation of the nebular spectrum and his successful prediction of new lines before they had been observed. In addition, Nicholson also proposed the notion of quantization of angular momentum, which Niels Bohr very soon embraced, and to much effect.

It is not easy to dismiss Nicholson's accommodation of so many spectral lines and especially his predictions of some unknown lines. It would not be the first time that progress had been gained on the basis of what later seemed like an insecure foundation. Perhaps just enough of Nicholson's overall view was sufficiently correct to allow him to do some useful science. After all, it would be unreasonable to expect there to be uniform progress in every single aspect of a theory. Typically some parts may be regarded as being progressive while others may be degenerating, to use the Lakatosian terminology.

And if we take an even wider perspective and consider the *longue durée* in the history of science, surely all scientific progress has been gained on the basis of what later turned out to be incorrect foundations when seen in the light of later scientific views. What really matters is that science, in the form of the scientific community, should progress as a whole. Attributing credit to a particular scientist may be important in deciding who prizes should be awarded to, but does not matter in the broader question of how the scientific community gains a better understanding of the world.

6.4 Accommodating the Spectra of Four Nebula, Including Orion Nebula

In this section the manner in which Nicholson was able to assign many unknown lines in the spectrum of the Orion nebula will be examined. First I present a figure containing the spectral lines that had been accounted for in terms of terrestrial hydrogen and helium (Figure 6.7). The dotted lines signify the spectral lines that had not yet been assigned, or identified, in any way. This situation therefore provided Nicholson with another opportunity to test his theory of proto-atoms and proto-elements.

As in many other features of Nicholson's work his approach was rather simple. He began by assuming that ratios of spectral frequencies correspond to ratios of mechanical frequencies among the postulated electron motion.[24] In mathematical terms he assumed

Nebular line	Vacuum tubes	Nebular line	Vacuum tubes
3726.4	...	4101.91	4101.89 Hδ
3729.0	...	4340.62	4340.63 Hγ
3835.8	3835.6 Hη	4363.37	...
3868.88	...	4471.71	helium
3889.14	3889.15 Hζ	4685.73	...
3965.1	3964.9 helium	4740.0	...
3967.65	...	4861.54	4861.50 Hβ
3970.23	3970.25 Hε	4959.05	...
4026.7	...	5006.89	...
4068.8	...		

Figure 6.7. Spectrum of Orion nebula showing many unassigned lines.

24 The detailed calculations can be found in Nicholson's articles, J.W. Nicholson, "The Spectrum of Nebulium," *Monthly Notices of the Royal Astronomical Society, (London)*, 72, 49–64, (1911); "A Structural Theory of the Chemical Elements," *Philosophical Magazine*, 6, (22), 864–89, (1911); "The Constitution of the Solar Corona I, Protofluorine," *Monthly Notices of the Royal Astronomical Society (London),* 72, 139–50, (1911); "The Constitution of the Ring Nebula in Lyra," *Monthly Notices of the*

Nebular line	Identification	Nebular line	Identification
3726.4	Nu+	4101.91	Hδ
3729.0	...	4340.62	Hγ
3835.8	Hη, Nu−, Nu++	4363.37	Nu
3868.88	Nu−	4471.71	helium
3889.14	Hζ	4685.73	...
3965.1	helium	4740.0	Nu−
3967.65	Nu++	4861.54	Hβ
3970.23	Hε	4959.05	Nu−−
4026.7	Helium?, Nu+	5006.89	Nu
4068.8	Nu−−	...	...

Figure 6.8. Nicholson's accommodation of 9 of the 11 unidentified lines in Figure 6.7.

$$\frac{\nu_{\text{spectral line 1}}}{\nu_{\text{spectral line 2}}} = \frac{f_{\text{rotation A}}}{f_{\text{rotation B}}}$$

where the f values emerged from his calculations, while one f the ν values was obtained empirically from the spectra in question and the other one was predicted. In his 1911 article "The Spectrum of Nebulium," Nicholson also predicted the existence of a new spectral line for the nebulae in question:

> Now the case of k = −2 for the neutral atom has been seen to lead to another line which will probably be very weak. Its wavelength should be 5006.9 × .86939 = 4352.9. It does not appear in Wright's table.[25]

Remarkably enough this prediction was also soon confirmed. In a short note in the same journal in the following year, 1912, Nicholson was able to report that the spectral line had been found at a wavelength of 4353.3 Angstroms

Royal Astronomical Society (London), 72, 176–77, (1912); "The Constitution of the Solar Corona II, Protofluorine," *Monthly Notices of the Royal Astronomical Society, (London),* 72, 1677–692, (1912); "The Constitution of the Solar Corona III," *Monthly Notices of the Royal Astronomical Society (London),* 72, 729–39, (1912).

[25] J.W. Nicholson, "The Spectrum of Nebulium," *Monthly Notices of the Royal Astronomical Society*, 72 (1), 49–64 (1911). https://doi.org/10.1093/mnras/72.1.49.

with an error of just 0.009% or roughly 1 in 11,110 by comparison with his prediction.

> At the meeting of the Society of 1912 March the writer announced the discovery of the new nebular line at λ4353 which had been predicted in his paper on "The Spectrum of Nebulium." A plate of the spectrum of the Orion nebula, on which the line was found, and which had been taken with a long exposure at the Lick Observatory in 1908 by Dr. W.H. Wright, was also exhibited. In the meantime the line has been recorded again by Dr. Max Wolf, of Heidelberg, who has, in a letter, given an account of its discovery, and this brief note gives a record of some of the details of the observation.
>
> The plate on which the line is shown was exposed at Heidelberg between 1912 January 20 and February 28, with an exposure of 40h 48m. The most northern star of the Trapezium is in the center of the photographed region, and the new line is visible fairly strongly, especially in the spectrum of the star and on both sides.
>
> The wave-length in the Orion nebula, obtained by plotting from an iron curve, is 4353.9, which is of course, too large, as all the lines in this nebula are shifted to greater wave-lengths, on account of the motion of the nebula. But the correction is not so large as a tenth-metre.
>
> The wave-length of the line on the Lick plate, as measured at the Cambridge Observatory by Mr. Stratton, is 4353.3, the value calculated in the paper being 4352.9.[26]

Nicholson experienced a similar triumph with the prediction of a new spectral line, which he believed was due to proto-fluorine and which he estimated to have a wavelength of 6374.8 Angstroms. A new spectral line was soon discovered in the solar corona with a wavelength of 6374.6.

Considered together, these successes by Nicholson are indeed rather remarkable. Just to recap, he accounted for 9 of 11 previously unidentified lines in the spectrum of the Orion Nebulae; 14 of the unidentified spectral lines in the solar corona. In addition, and perhaps more impressively, Nicholson predicted two completely unknown lines, one in each of these spectra, both of which were promptly discovered and found to have almost exactly the wavelengths he predicted:

Nebulium prediction: 4352.9A	observation: 4353.3A	error: 1in 11,111
Solar corona prediction: 6374.8A	observation: 6374.6A	error: 1 in 31,745

[26] J.W., Nicholson, "On the New Nebular Line at λ4353," *Monthly Notices of the Royal Astronomical Society (London),* 72, 693, (1912).

6.5 Nicholson's Calculations on the Spectrum of the Solar Corona

Nicholson next turned his attention to the spectrum of the solar corona that had been much studied and that showed numerous lines that had not yet been accounted for (Figure 6.9). In this study Nicholson was even more successful than he had been with the spectrum of the Orion nebula,

1900	1901	1905	Mean	Intensity
...		5535.8	5535.8	2
...	5304	5303.1	5303.5	20
...	...	5117.7	5117.7	2
5073	...	...	5073	1
4779	...	...	4779	1
4725	...	...	4725	...
4722	...	...	4722	...
4586.3	...	...	4586.3	4
4566.5	4565	...	4566	6
4400	...	...	4400	1
4358.8	...	...	4359	4
4311.3	...	...	4311	2
4230.6	4230.9	4231.1	4231.0	5
4130	...	...	4130	...
...	...	4087.4	4087	...
3987.2	3987	3987.1	3987.1	3
...	3891.2	...	3891	...
3800.8	3801.1	3800.8	3800.9	3
3642.9	...	3642.0	3642.5	2
...	3505	...	3505	...
3461.3	...	...	3461	1
...	3454.4	...	3454	9
...	3387.9	...	3387.9	12
...	3361	...	3361	...

Figure 6.9. Observed lines in the solar corona spectrum at various dates.

Coronal line	Suggested origin	Intensity
5535.8	Pf, −2e	2
5073	Pf, −3e (?)	1
4586	Pf, +2e	4
4566	Pf, −e	6
4359	Pf, −2e	4
4311	Pf, +2e	2
4231	Pf, +e	5
4087	Pf, +3e (?)	...
3987.1	neutral atom, +3e (?)	3
3800.9	Pf, +e	3
3642.5	Pf, −e	2
3454	Pf, neutral atom	9
3387.9	Pf, neutral atom	12
3361(?)	Pf, −3e (?)	...

Figure 6.10. Nicholson's accommodation of 14 of the lines from Figure 6.9 using proto-fluorine and ionized forms of this proto-atom.

because he succeeded in accounting quantitatively for as many as 16 unexplained lines.

Figure 6.10 shows the observed frequencies of the lines, along with Nicholson's assignments in terms of the atom of proto-fluorine and various ionized forms of the same atom.

6.6 Nicholson and Planck's Constant

The manner in which Nicholson arrived at the all-important Planck constant was by calculating the ratio of the energy of a particle to its frequency and finding that this ratio was equal to a multiple of Planck's constant. Nicholson concluded that this constant therefore had an atomic significance and indicated that angular momentum could only change in discrete amounts when electrons leave or return from an atom. The relevance of this finding lies in the fact that up to this point the quantum had only been associated with energy and not with angular momentum. Nicholson was the first person to make this association, in what would soon become an integral aspect of Bohr's theory of the hydrogen atom. In the case of the proto-fluorine atom, Nicholson calculated the ratio of potential energy to frequency to be approximately

$$\text{Potential energy/frequency} = 154.94 \times 10^{-27} \text{ erg seconds} = 25 \text{ h}$$

In arriving at his result Nicholson had used the measured values of e and m, the charge and mass of the electron. However, his method still did not provide a means of estimating the radius of the electron, and he was forced to eliminate this quantity from his equations, a problem that he would overcome a little later.

Nicholson proceeded to calculate the ratio of potential energy to frequency in proto-fluorine ions with one or two fewer electrons and found 22 h and 18 h, respectively. He noted that the three values for Pf, Pf^{+1}, and Pf^{+2} were members of a harmonic sequence,

$$25, 22, 18, 13, 7, 0.$$

By dividing each value by the number of electrons in the atom he found the Planck units of angular momentum per electron to be,

$$5, 5.5, 6, 6.5, \text{and } 7.$$

Nicholson thus arrived at the general formula for the angular momentum of a ring of n electrons as

$$\tfrac{1}{2}(15 - n)\ n$$

This formula then allowed him to fix the values of the atomic radius in each case, and since angular momentum did not change gradually, he took this to mean that atomic radius would also be quantized.

Several authors have traced the manner in which Bohr picked up this hint of quantizing angular momentum.[27] This feature was not present in Bohr's initial atomic model, and he only incorporated it over a series of steps following a close study of Nicholson's papers. Bohr also spent a good deal of time trying to establish the connection between his own and Nicholson's atomic theory.

[27] J. Heilbron, T.S. Kuhn, "The Genesis of Bohr's Atom," *Historical Studies in the Physical Sciences*, 1, 211–90, (1969).

6.7 Reactions to the Work of Nicholson

As the historian of physics John Heilbron stated in a recent plenary lecture to the American Physical Society, the success of Nicholson's work on nebulium had been "spectacular." Heilbron also commented on how it had served as a motivation for Bohr's work. But looking at the literature in physics and the history of physics one finds a remarkable range of views expressed concerning Nicholson's work. The following is a brief survey of these varied reactions.

Initially the commentators tended to praise Nicholson. For example, after a meeting held in Australia in 1914, W. M. Hicks remarked,

> Nicholson's calculated frequencies and the observed lines were "so close and so numerous as to leave little doubt of the general correctness of the theory . . . Nicholson's theory stands alone as a first satisfactory theory of one type of spectra."[28]

In a paper published at the end of 1913, William Wilson observed that Nicholson had

> used the quantum hypothesis with extraordinary success in his valuable investigations of the sun's corona.[29]

Physics historian Abraham Pais saw the relationship between Bohr and Nicholson sometime later in the following terms,

> Bohr was not impressed by Nicholson when he met him in Cambridge in 1911 and much later said that most of Nicholson's work was not very good. Be that as it may, Bohr had taken note of his ideas on angular momentum, at a crucial moment for him . . . He also quoted him in his own paper on hydrogen. It is quite probable that Nicholson's work influenced him at that time.[30]

Returning to Heilbron:

> The success of Nicholson's atom bothered Bohr. Both models assumed a nucleus, and both obeyed the quantum; yet Nicholson's radiated—and with unprecedented accuracy—while Bohr's was, so to speak, spectroscopically mute.

[28] McCormmach, "The Atomic Theory of John William Nicholson," *Archives for History of Exact Sciences,* 3, 160–184, (1966), p. 183.

[29] Ibid, p. 184.

[30] A. Pais, *Niels Bohr's Times, In Physics, Philosophy, and Polity*, Oxford, Oxford University Press, 1991, p. 145.

> By Christmas 1912, Bohr had worked out a compromise: his atoms related to the ground state, when all the allowed energy had been radiated away; Nicholson's dealt with earlier stages in the binding. . . . Just how a Nicholson atom reached its ground state Bohr never bothered to specify. He aimed merely to establish the compatibility of the two models. The compromise with Nicholson was to leave an important legacy to the definitive form of the theory.[31]
>
> Later in the same paper Bohr proposed other formulations of his quantum rule, including, with full acknowledgement of Nicholson's priority, the quantization of the angular momentum.[32]

Another historian-philosopher of physics, Max Jammer, writes

> It should also be pointed out that Nicholson's anticipations of some of Bohr's conclusions were based, as Rosenfeld has pointed out, on *the most questionable and often even fallacious reasoning*.[33] (emphasis added)

Now for one last commentator, Leon Rosenfeld, who reveals some further aspects of how Nicholson has been regarded. In his introduction to a book by Niels Bohr to celebrate the 50th anniversary of the 1913 theory of the hydrogen atom, Rosenfeld writes:

> The ratio of the frequencies of the two first modes happens to coincide with that of two lines of the nebular spectra: this is enough for Nicholson to see in this system a model of the neutral "nebulium" atom; and as luck would have it, the frequency of the third mode, which he could then compute, also coincided with that of another nebular line, which—to make things more dramatic—was not known when he made the prediction in his first paper, but was actually found somewhat later. . . .
>
> From the mathematical point of view Nicholson's discussion of the stability conditions for the ring configurations and of their modes of oscillation is an able and painstaking piece of work; but the way in which he tries to apply the model . . . must strike one as unfortunate accidents.[34]
>
> In the third paper, however, published in 1912, occurs the first mention of Planck's constant in connection with the angular momentum of the rotating

[31] J. Heilbron, "Lectures in the History of Atomic Physics, 1900–1922." In *History of Twentieth Century Physics*, C. Weiner, ed., 40–108, Academic Press, New York, 1977, p. 69.

[32] Ibid., p. 70.

[33] M. Jammer, *The Conceptual Development of Quantum Mechanics*, New York, McGraw-Hill, 1966. p., 73.

[34] L. Rosenfeld, in preface to N. Bohr, *On the Constitution of Atoms and Molecules*, New York, W.E. Benjamin, 1963, p. xii.

> electrons: again here there is no question of any physical argument, but just a further display of numerology. . . .
>
> Bohr did not learn of Nicholson's investigations, as we shall see, before the end of 1912, when he had already given his own ideas of atomic structure their fully developed form.[35]
>
> By contrast [with Nicholson] the thoroughness of Bohr's single-handed attack on the problem and the depth of his conception will appear still more impressive.[36]

There is clearly no "fence sitting" here to give Nicholson any benefit of the doubt. Rosenfeld does not even believe that Nicholson's apparent early success was due to a cancellation of errors. But perhaps some aspects of Rosenfeld's life serve to explain some of his reaction. Rosenfeld was without doubt one of Bohr's leading supporters and also acted as a leading spokesperson for Bohr's Copenhagen interpretation of quantum mechanics for the last 30 or so years of Bohr's life. Rosenfeld is also known to have been an especially vitriolic and harsh critic in spite of his apparently shy and retiring personality. His fellow Belgian and one-time collaborator, the physical chemist Ilya Prigogine, described him as a "paper tiger."[37] It is hardly surprising therefore that Rosenfeld championed Bohr against any claims from anyone, such as Nicholson, who he regarded as imposters, or anyone who might try to steal some of the thunder from Bohr.

The views of Rosenfeld can be contrasted this with those of Kragh, the contemporary historian and a Dane like Bohr,

> No wonder Bohr, when he came across Nicholson's atomic theory found it to be interesting as well as disturbingly similar to his own ideas. Nicholson's atom was a rival to Bohr's and Nicholson was the chief critic of Bohr's ideas of the quantum atom.[38]

But let us say, for the sake of argument, that Rosenfeld is right and that Nicholson's work was completely worthless. Even if this were true, I contend that Nicholson's publications contributed to Bohr's developing his own atomic theory for the simple reason that Nicholson served as his foil. In some places Bohr is quite dismissive of Nicholson's work, such as when writing to his Swedish colleague Carl Oseen, where he describes Nicholson's work as "pretty crazy" while adding,[39]

35 Ibid., p. xiii.
36 Ibid., p. xiii–xiv.
37 Prigogine was born in Russia but emigrated to Belgium.
38 H. Kragh, *Niels Bohr and the Quantum Atom: The Bohr Model of Atomic Structure 1913–1925*, Oxford, Oxford University Press, 2012, p. 27.
39 This comment by Bohr refers to Nicholson's earlier theory of electrons in metals, although one gathers the impression that Bohr continued to hold this crucial view about Nicholson. I am grateful for a reviewer for drawing my attention to this qualification.

> I have also had discussion with Nicholson: He was extremely kind but with him I shall hardly be able to agree about very much.[40]

In other places Bohr shows Nicholson considerably more respect, such as when writing to Rutherford while he was on the point of submitting his famous trilogy paper that was published in 1913.

> It seems therefore to me to be a reasonable hypothesis, to assume that the state of the systems considered in my calculations is to be identified with that of atoms in their permanent (natural) state . . . According to the hypothesis in question the states of the system considered by Nicholson are, on th contrary, of a less stable character; they are states passed during the formation of the atoms, and are states in which the energy corresponding to the lines in the spectrum char-acteristic for the element in question is radiated out. From this point of view systems of a state as that considered by Nicholson are only present in sensible amount in places in which atoms are continually broken up and formed again; i.e. in places such as excited vacuum tubes or stellar nebulae.[41]

In another passage from a letter to his brother Harald, Niels Bohr writes

> Nicholson's theory is not incompatible with my own. In fact my calculations would be valid for the final chemical state of the atoms, whereas Nicholson would deal with the atoms sending out radiation, when the electrons are in the process of losing energy before they have occupied their final positions. The radiation would thus proceed by pulses (which much speaks well for) and Nicholson would be considering the atoms while their energy content is still too large that they emit light in the visible spectrum. Later light is emitted in the ultraviolet, until at last the energy which can be radiated away is lost.[42]

After Bohr had published his three-part article, Nicholson continued to press him in a number of further publications. If we must speak in terms of winners and losers, Bohr would be regarded as a winner and Nicholson as a loser. But as in all walks of life, it is not just about winning, but more about partaking. There would be no athletic races for spectators to watch if the losers were not even to

[40] Bohr to Oseen, December 1, 1911, cited in N. Bohr, L. Rosenfeld, E. Rüdinger, & F. Aaserud, "Collected Works." In *Early Work (1905–1911)*, Vol. 1, J.R. Nielsen, ed., North-Holland Publishing, Amsterdam, 1972, pp. 426–431.

[41] N. Bohr to Rutherford, 1913, cited in L. Rosenfeld, preface to N. Bohr, *On the Constitution of Atoms and Molecules*, New York, W.E. Benjamin, 1963, p. xxxvii.

[42] Bohr to Harald, in N. Bohr, L. Rosenfeld, E. Rüdinger, & F. Aaserud, *Early Work (1905–1911)*. In both instances where I have written "my," Bohr had actually written "his"—which must surely be typos.

participate in the race. The very terms winner and loser are necessarily codependent in the context of any scientific debate, as were the roles of Bohr and Nicholson.

Now this picture that I have painted would seem to raise at least one obvious objection. If all competing theories are allowed to bloom because there is no such thing as a right or wrong theory, how would scientists ever know which theories to utilize and which ones to ignore? Indeed this is the kind of criticism that was levelled against Feyerabend[43] when he claimed that "anything goes" and was promptly criticized by numerous authors.[44] I think the answer to this question can be found in evolutionary biology. Nature has the means of finding the best way forward. Just as any physical trait with an evolutionary advantage eventually takes precedence, so the most productive theory will eventually be adopted by more and more scientists in a gradual, or trial and error fashion. The theories that lead to the most progress will be those that garner the largest amount of experimental support and which provide the most satisfactory explanations of the facts. This entire process will not be rendered any the weaker even if one acknowledges an anti-personality and anti–"right or wrong view" of the growth of science.

More generally, I believe that the two aspects can coexist quite happily. Scientists can, and regularly do, argue issues out to establish the superiority of their own views, as well as their claims to priority. But the march of progress, to use an old-fashioned term, does not care one iota about these human squabbles. And it is the overall arc of progress that really matters, not whose egos are bruised or who obtains the greater number of prizes and accolades.

6.8 How Was Any of the Success Possible Given the Limitations of Nicholson's Theory?

Having examined the apparent successes of Nicholson's theory we must still ask how any of this was even possible given what we now know of his ideas. Here is a brief list of what seems to be problematical in Nicholson's scheme: First of all, the proto-elements like nebulium that he postulated do not exist. Second, he identified mechanical frequencies of electrons with spectral frequencies and assumed that these oscillations took place at right angles to the direction of electron circulation. Third, Nicholson's electrons were all in one single ring, unlike the subsequent Bohr model in which they are distributed across different rings or shells.

[43] P. Feyerabend, *Against Method*, London, Verso, 1975.
[44] J. Preston, Paul Feyerabend entry in Edward N. Zalta, ed., *The Stanford Encyclopedia of Philosophy*, Winter 2016 Edition. https://plato.stanford.edu/archives/win2016/entries/feyerabend/.

So in the light of modern knowledge, Nicholson seemed to be making several false assumptions. According to the traditional way that a realist might regard such matters, Nicholson's ideas were not even approximately correct since they would be regarded as downright false.[45] And yet he achieved remarkable success, at least according to most of the commentators whose views were quoted here. Are cases such as Nicholson exceptional or can other examples be found in the history of science? If one accepts that all, or most, theories are eventually refuted, one has to concede that the progress of science implies that "wrong theories" regularly lead to progress!

Having gone into some details about Nicholson's work in early 20th century atomic physics as well as astrophysics, I would now like to return to the general philosophical question and try to make some sense of the apparent success his view seemed to enjoy, at least initially.

I am proposing an evolutionary theory of the development of science in a literal biological sense.[46] While appealing to some of the ideas that have emerged from evolutionary epistemology as supported by Donald T. Campbell and many others, my account also involves a new departure as I will be explaining later. The general idea that is common to most approaches in evolutionary epistemology is that since evolution drives all biological development, it also drives the way in which we human agents think and how we develop theories and experimentation. It is a central tenet of evolutionary epistemology that all the knowledge of the natural world that we possess is essentially determined by evolutionary biology. Consequently, it seems plausible to further suggest that scientific knowledge is neither right nor wrong at any epoch in history but instead better or worse suited to the environment that science finds itself working in, namely the way in which scientific theories are suited to or correspond with the natural world as revealed by experimentation.

Needless to say, I do not dispute such scientific facts as the view that the earth is round rather than flat. Of course I accept that there are truths with a small "t" such as it is true that the earth is round or that grass is green. My concern is more with the notion that there may be a scientific truth with a capital "T." It is also im-portant to distinguish theories from facts or observational statements. Whereas it is true that grass is simply green, this is a far cry from the question of whether Bohr's theory or the theory of evolution is true or correct. Theories, especially those in the physical sciences, consist of mathematical relationships. They do not just assert whether the earth circles the sun or vice versa. Similarly, Newton's theory is based on his famous three mathematical laws, rather than making a

[45] As I have already indicated I reject such an over-simplification, especially when it comes to assumptions and theories.
[46] E.R. Scerri, *A Tale of Seven Scientists and A New Philosophy of Science*, New York, Oxford University Press, 2016.

simple assertion on whether or not the moon is made of blue cheese or anything quite so specific.

I take it that one would never wish to claim that biological evolution is either right or wrong, but rather that any biological developments are either suited to their environment or not. Those developments that are suited to their biological niches result in their being perpetuated in future generations while those that are not simply wither away. So, I claim, it is with the development of science.

In passing, I should note some kinship with the views of the influential anti-realist philosopher Van Fraassen, who supports an evolutionary view of the progress of science when he writes,

> For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which in fact latched onto the actual regularities in nature.[47]

This view that has been criticized in particular by Kitcher,[48] and Stanford,[49] although it would take me too far afield to comment on this exchange here.

My own brand of evolutionary epistemology also holds that the study of scientific development should not be approached by concentrating on individual discoverers nor through individual theories. I claim that science essentially develops as one unified organism, while fully anticipating that this aspect will meet the greatest resistance from critics. Needless to say, many people have realized the societal/collective nature of science, and I cannot claim any originality on that score. For example, there have been many programs such as the Strong Program, Science Studies, and the Sociology of Science, which all take a more holistic approach to studying how science develops. However, these programs generally hold that social factors determine scientific discoveries. My interest lies primarily in the actual science, while at the same time maintaining a radical form of sociology that considers scientific society to be a single organism which is essentially developing in a unified fashion.

I propose that scientific research is conducted by a tacit network of scholars, and researchers, who frequently appear to be at odds with themselves, and frequently are involved in bitter priority disputes, while unknowingly partaking in the same overall process. For example, in the case of John Nicholson, and several others that I have discussed in a recent book, the protagonists were seldom in direct contact with the Bohrs, Paulis and other luminaries in the world of early

47 B. Van Fraassen, *The Scientific Image*, Oxford, Clarendon Press, 1980, p. 40.

48 P. Kitcher, *The Advancement of Science: Science without Legend, Objectivity without Illusions*, New York, Oxford University Press, 1993.

49 K. Stanford, An Antirealist Explanation of the Success of Science, *Philosophy of Science*, 67, 266–84, (2000).

20th century atomic physics. Nevertheless, one sees an entangled, organic development in which ideas compete and collide with each other, even if one famed individual is eventually associated with any particular scientific discovery or episode.

I regard science as very much proceeding via modification by trial and error rather than via "cold rationality." My view is aligned with biological evolution rather than with an enlightenment view of the supremacy of rationality and the powers of pure deduction. I regard scientific development as being guided by evolutionary forces, blind chance, and random mutation. When seen from a distance science appears to develop as one unified organism. Under a magnifying glass there may be little sign of unity, so much so that some philosophers and modern scholars have been driven to declare the dis-unity of science, a view that I believe to be deeply mistaken.[50]

From the perspective that I propose, one can better appreciate how what a realist regards as wrong theories, as in the case of most of John Nicholson's scientific output, can lead to progress. I do not need to explain wrong theories away in my account. Nor do I believe that simultaneous or multiple discoveries deserve to be explained away as being aberrations. Similarly, priority disputes, which are so prevalent in science, can be seen as resulting from the denial of the unity of science and an underlying body-scientific that I allude to.

Developments that might normally be regarded as wrong ideas frequently produce progress, especially when picked up and modified by other scientists. Nicholson's notion of angular momentum quantization, which he developed in the course of what has generally been regarded as an incorrect theory, was picked up by Niels Bohr who used it to transform the understanding of atomic physics. The image of science that I envisage resembles a tapestry of gradually evolving ideas that undergo mutations and collectively yield progress. In some cases the source of the mutation is random, just as mutations are random in the biological world. This view stands in sharp contrast with the traditional notion of purposive and well thought out ideas on the part of scientists.[51]

Consequently, the kinds of inconsistent theories that Peter Vickers and others have examined no longer seem to be in such urgent need of explanation and should not be regarded as being so puzzling. Indeed, such developments should

[50] P. Galison, D. Stump (eds.), *The Disunity of Science*, Palo Alto, Stanford University Press, 1996.

[51] I regard mutations in scientific concepts to occur on the level of individual scientists and not primarily in entire social groups. My evolutionary epistemology thus also differs from Kuhn's in this respect since his unit of evolutionary change is the social group of scientists. See some interesting discussions regarding Kuhn's evolutionary account of scientific progress in B.A. Renzi, "Kuhn's Evolutionary Epistemology and Its Being Undermined by Inadequate Biological Concepts," *Philosophy of Science,* 76, 143–59, . (2009); T.A.C. Raydon, P. Hoyningen-Huene, "Discussion: Kuhn's Evolutionary Analogy in *The Structure of Scientific Revolutions* and 'The Road since Structure,' " *Philosophy of Science*, 77, 468–76, (2010).

be regarded as the rule rather than exceptions. I agree completely with Kuhn that ideas and theories that survive should be regarded as facilitating progress and not as tracking the truth.

As so many evolutionary epistemologists have urged, I regard knowledge as being presumptive, partial, hypothetical, and fallible. If scientific progress is far more organic than usually supposed, we can make better sense of why Nicholson was able to contribute to the growth of science. None of the scientists I have examined really knew what they were doing, in a sense.[52] Their crude ideas developed in an evolutionary trial and error manner instead of in a strictly rational manner, or so I propose.

Nicholson and many people like him contributed very significantly to the development of science. Nicholson was not simply wrong, since he inadvertently helped Bohr to begin to quantize angular momentum. Nicholson is as much part of the history as Bohr is, and minor contributors matter as much as the well-known ones. In anticipation of the following section, I believe that the early Kuhn's focus on revolutions may have served to diminish the importance of such marginal figures.

6.9 Is the Proposed View Compatible with Kuhnian Revolutions?

Is an evolutionary view of the kind I propose compatible with the revolutions for which Kuhn is so well known? First of all, as many authors have claimed, Kuhn's own work on historical episodes sometimes points to continuity rather than revolution, such as in the case of quantum theory and the Copernican Revolution.[53]

Of course, Kuhn also acknowledges the role of normal science that in very broad terms can be seen as upholding the role of minor historical contributors to the growth of science. Nevertheless, as I see it, there was no sharp revolution in the development of quantum theory only an evolution. Paradoxically, such an evolution is easier to appreciate from the wider perspective of science as one unified whole than from the perspective of individual contributors or individual theories. Viewing theory-change as *revolutionary*, on the other hand, may mask the essentially biological-like growth of science that I am defending.

Nevertheless, I am in complete agreement with Kuhn in supposing that science does not drive toward some external truth. In this respect I am on the side of anti-realism. I prefer to regard scientific process as driven from *within*, by

52 Scerri 2016.

53 T.S. Kuhn, *Black-Body Radiation and the Quantum Discontinuity, 1894–1912*, Chicago, Chicago University Press, 1987; J. Heilbron, T.S. Kuhn, "The Genesis of the Bohr Atom," *Historical Studies in the Physical Sciences,* 1, 211–290 (1969).

evolutionary forces which look back to past science.[54] This is not to say that the world does not constrain our theorizing. Kuhn just wants us to see that the scope of our theories is not determined by nature in advance of our inquiring about it. Indeed, I find that I agree with Kuhn on a great number of issues, the main exception being the occurrence of scientific revolutions.

As scholars generally concur, Kuhn's later work was aimed toward developing an *evolutionary* epistemology.[55] As Kuhn developed his epistemology of science, he saw increasingly more similarities between biological evolution and scientific change. Consequently as he developed his epistemology of science it became a more thoroughly evolutionary epistemology of science. Wray makes the very perceptive remark that while Kuhn was one of the key philosophers of science who initiated the *historical turn* in the philosophy of science in the early 1960s, he later came to adopt what he termed a *historical perspective*. According to Wray this historical perspective, or developmental view, is nothing less than an *evo-lutionary perspective* on science. In accord with Wray and others, Kuukkanen (2013, 134) points out that,the later Kuhn felt that his evolutionary image of science did not get the amount of attention that it deserved. In the course of his last interview, Kuhn deplored this situation while saying, 'I would now argue very strongly that the Darwinian metaphor at the end of the book [SSR] is right and should have been taken more seriously than it was.'[56]

Just as evolution lacks a *telos* and is not driven towards a set goal in advance, science is not aiming at a goal set by nature in advance. Kuhn continued to re-gard his evolutionary view as important to the end of his life, or as Wray writes, "Whatever else he changed he did not change this aspect." Kuhn also claims that in the history of astronomy, the earth- centered models held the field back for many years. Similarly, he claimed that, truth-centered models of scientific change were holding back philosophy of science. Somewhat grandiosely, Kuhn notes a similarity between the reception of Darwin's theory, and the reception of his own theory his own view on the evolution of science, in that both meet the greatest resistance on the point of elimination of teleology.

[54] Kuhn prefers the phrase "pushed from behind." I believe that "driven from within" better conveys the way that scientific knowledge is being generated in an outward fashion rather than being "pulled" toward the truth.

[55] B. Wray, *Kuhn's Evolutionary Social Epistemology*, Cambridge, UK: Cambridge University Press, 2011, 84.

[56] K. Jouni- Matti, "Revolution as Evolution: The Concept of Evolution in Kuhn's Philosophy." In *Kuhn's The Structure of Scientific Revolutions Revisited*, Theodore Arabatzis, ed., 134– 152, Routledge, New York, 2013, p. 134. See T.S. Kuhn, J. Conant, & J. Haugeland, *The Road Since Structure: Philosophical Essays, 1970- 1993* (with an autobiographical interview), 2000, Chicago: University of Chicago Press. Kuhn's first thoughts on epistemology based on evolutionary lines appear at the end of *The Structure of Scientific Revolutions*, 1962, Chicago: University of Chicago Press, 169–172.

Whereas Kuhn did not initially have an explanation for the success of science, he later proposed that *scientific specialization* was the missing factor. Kuhn argued that just as biological evolution leads to an increasing variety of species, so the evolution of science leads to an increasing variety of scientific sub-disciplines and specializations.[57] For Kuhn science is a complex social activity, and the unit of explanation is the group, not the individual scientists. This resonates with my own view that I alluded to earlier that the growth of science is not successfully tracked by considering individual scientists or individual theories. Kuhn asks us to judge changes in theory from the perspective of the research community rather than the individual scientists involved. Both early converts to a theory and holdouts aid the community in making the rational choice between competing theories. This, I suggest, is how the work of Nicholson should be viewed, namely as a step toward the new theory in the context of Bohr and others.[58]

However, there is another respect in which I would want to qualify this view to also reaffirm the importance of change on the level of individual scientists rather than change predominantly within social groups. Kuhn's evolutionary account of the growth of science appears to be somewhat half-hearted. He does not say very much on the mechanism of evolutionary change in scientific theories except for his talk of specialization. Kuhn believes that the development of new sub-disciplines such as the emergence of biochemistry from chemistry, for example, is analogous to the evolution of new species which become incapable of mating with members of the species from which they have evolved. Similarly Kuhn holds that the members of the new scientific sub-discipline, which has branched off from an older one, are unable to communicate with members of the mother discipline. Kuhn even makes a virtue of this reconceptualized incommensurability in suggesting that such isolation allows for a greater development within a particular sub-discipline.

While authors like Renzi have criticized Kuhn's evolutionary analogy, Raydon and Hoyningen-Huene defend him, in suggesting that Kuhn did not intend his analogy to be taken literally. Be that as it may, I *do* intend the evolutionary analogy to be literal. Since the evolution of scientific theories is part of an underlying biological evolution of the human species I do not regard this suggestion to be too far-fetched, even though these evolutionary processes may be occurring at levels that are far removed from each other.

[57] Out of physics and chemistry there emerges physical chemistry. Biology and chemistry give rise to biochemistry. Biochemistry in turn gives rise to physical-biochemistry.

[58] This project has shown me that contrary to what I believed for about 25 years, there is much merit in Kuhn's work. I had been too stuck on the cartoon Kuhn (the best-seller Kuhn) who is supposed to deny progress and who is often taken to be at the root of all evil, such as science wars and the sociological turn—but only because I arrived at the idea of an evolutionary epistemology through my own work in asking how a "wrong" theory can be so successful in so many cases.

In the case of the evolution of organisms, modern biology has taught us that the underlying mechanism is one of random mutation on the level of errors that occur in the copying of DNA sequences. The more accurate biological analogy for the development of scientific theories would seem to be for the "mutation" of ideas to occur in the minds of individual scientists such as Bohr or Nicholson. The unit involved in the evolution of scientific theories would therefore be individual scientists and not the social group that scientists belong to. Here then is where I depart from Kuhn's evolutionary epistemology. In my account the evolution of scientific theories takes place in a literally biological sense and is mainly situated at the level of individuals rather than social groups.

6.10 Can Kuhn Have it Both Ways?

Can revolution coexist with evolution in science as Kuhn seems to believe? In the later Kuhn, revolutions are no longer paradigm changes. They are now taxonomic or lexical changes, thus raising the question of whether the revolutions he gave as examples no longer count as revolutions.[59]

> The early Kuhn's wholesale and psychologically drastic revolution becomes a gradual and piecemeal communitarian evolution in the later Kuhn, something that may show simultaneous continuity and discontinuity between prerevolutionary and revolutionary stages.[60]

Brad Wray has claimed that revolutions are essential to Kuhn because they are incompatible with the view that scientific knowledge is cumulative and that scientists are constantly marching ever closer to the truth. I must say that I disagree with this view. Scientists may not be moving toward a fixed truth, but the development may still be gradual and not revolutionary. After all, biological evolution is not teleological but is generally thought to be gradual (pace Stephen Jay Gould et al.). To me the main insight from Kuhn is his evolutionary epistemology not his notion of discontinuous theory-change.

For example, why consider the quantum revolution to have ended in 1912 as Kuhn does? Surely an equally important revolution due to Bohr *began* around this time of 1912. Or was the true revolution the coming of quantum mechanics à la Heisenberg and Schrödinger in 1925–26 or maybe QED sometime later in the

[59] A quick reply to this question is that Kuhn gives very few examples of what he considers to be scientific revolutions in the sense of lexically driven revolutions.

[60] J.M. Kuukkanen, "Revolution as Evolution." In, *Kuhn's The Structure of Scientific Revolutions Revisited*, eds., V. Kindi and T. Abratzis, Routledge, London 2012.

late 1940s or QCD in the 1970s? Or could it be that there are so many revolutions that the very concept ceases to be helpful?

6.11 Conclusions

Nicholson is somewhat neglected in the history of science, but I don't believe it is because he was simply wrong in many of his basic assumptions.[61] In spite of holding some assumptions concerning the structure of the atom that were subsequently abandoned, Nicholson was still able to make a number of highly successful accommodations of known data and predictions of completely unknown data. Furthermore, Nicholson's idea of the quantization of angular momentum was key to Bohr's subsequent progress in the development of atomic physics. Nicholson was part of the organic manner in which science evolves or, in this case, the way that atomic physics evolved. He was an important "missing link" between the old classical physics and the new quantum theory and the way that it was applied to the atom.

Needless to say, if Nicholson had not been the first to propose the quantization of angular momentum somebody else would probably have done so. I am not trying to rehabilitate Nicholson's role, but merely wishing to highlight the crucial and catalytic role that is often played by the "little people" in science. Moreover, it is quite conceivable that the history of atomic physics might have taken a different path, perhaps one not involving the quantization of angular momentum. The fact remains that it did, and that Nicholson played an undeniable role in what actually took place. My main point is to try to highlight the organic and evolutionary way in which science develops and that it is only in retrospect that priority is attributed to certain contributors. Given our limitations in attempting to reconstruct such an organic and interconnected growth process, it is hardly surprising that we tend to simplify the story by latching on to the leading players in any particular scientific episode. As the reader will have noticed, I am not unduly puzzled by what may appear to be inconsistent theories and, in the case I have examined, even some bizarre theories can lead to scientific progress. If one accepts the notion that scientific work is at root a collective exercise even if not literally so, then inconsistencies can be thought of as temporary road-blocks which delay progress but which get ironed out in the *longue durée*.

61 I am speaking mainly of popular science and science textbooks. Professional historians of science know better of course.