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CORRESPONDENCE

To the Editors of 'The Observatory'

*Eponyms, Hubble's Law, and the Three Princes of Parallax**

Just who should get credit for what we generally call Hubble's Law has been disputed since 1929. On balance, and perhaps in disagreement with some other recent discussions, I vote for Hubble.

The interface between observational astronomy and General Relativity seems to have had more than its fair share of disputed huzzas. Best known is the case of the gravitational deflection of starlight, first reported for the 1919 solar eclipse from two British expeditions, headed by Arthur Eddington and by Andrew Crommelin, who got the best data, but got left off the triumphant paper¹, and still hardly ever gets any credit. Neither the 1919 weather nor the telescopes, unused to such rapid temperature changes, were entirely satisfactory,

*Of course neither parallax nor the velocity-distance relation were in any sense "fortunate discoveries by accident". Each was the product of multiple searches, hard work, and improved equipment. But for each there were arguably three claimants for the title of discoverer, one of whom (deservedly, I think) gets most of the credit.

but the results were apparently much closer to the general relativistic prediction of $1''.74$ at the solar limb than to the Newtonian value of half that. Both technical and popular press proclaimed the confirmation. The muttering that Eddington had wrongly weighted the results from the three telescopes became scientifically irrelevant after a few more eclipses, but not, of course, historically or sociologically irrelevant. Stanley anyhow says² it wasn't true. The best solar deflection numbers now come from radio interferometry, and you don't even need to wait for a solar eclipse.

Somewhat similarly, Adams reported³ a value of $+19 \text{ km s}^{-1}$ for the "relativity displacement of the spectral lines" in the companion of Sirius in 1925, just in time for Russell, Dugan & Stewart to write⁴ that it was exactly what everyone was expecting from General Relativity plus the mass and radius of Sirius as then understood. Eddington in particular, who had asked that the measurement be attempted, was pleased as punch. When the best-buy radius of Sirius B crept downward from $0.03 R_{\odot}$ to somewhat less than $0.01 R_{\odot}$, however, there were suggestions that Adams had massaged his data to get what was wanted. Daniel Popper, who measured the first robust gravitational redshift (for 40 Eri B in 1954) denied this⁵ and said the problem was just scattered light from Sirius A.

Attacks on the nomenclature for relativistic results also come along from time to time. "Chandrasekhar did not discover the Chandrasekhar Limit" goes back at least to Shklovsky⁶ in 1978 who credited Frenkel⁷. I got entangled by quoting Shklovsky uncritically in a *Sky & Telescope* book review⁸. Scarcely had the *S&T* issue hit news-stands and mail-boxes when there came a largish envelope from Chandra (my *doktor grossvater*, the teacher of my teacher, Guido Münch). It contained reprints with marginal comments, making clear Chandra's disagreement. I sat down with Frenkel and a German-speaking friend, and concluded that what Frenkel's paper had calculated would have been of interest at the time, but was not the Chandrasekhar Limit. She read the short words and I read the long ones, which seemed to work well. The Chandrasekhar Limit issue has been raised yet again recently in the pages of *Physics Today*, among other places, with the most recent rebuttal that of Wali⁹.

Now in the December issue of *The Observatory*, we find Nussbaumer & Bieri saying that Hubble was not the first to recognize the linear velocity–distance (or redshift–magnitude) relationship that we now call Hubble's Law and so should not have it named for him¹⁰. They attribute the motivation for their discussion to an on-line *Nature* item, for which they do not give enough bibliographical information for me, at least, to find it. They also cite van den Bergh¹¹ and Block¹² with no indication of the papers' contents or where they are scheduled to appear. In fact both (and van den Bergh¹³) say that more credit should be given to Lemaitre. They are not the first to do so. Kragh¹⁴, for instance, says (on p. 58) "It could as well have been named Lemaitre's Law". Shapley¹⁵ was clearly voting for "Shapley's Law"¹⁶ and de Sitter¹⁷ for "Slipher's Constant" and "de Sitter's Law" (or perhaps "my grandfather's law").

Possibly there is some language chauvinism in this. The French-speaking Blanchard¹⁸ is pro-Lemaitre (a French-speaking Belgian); American Christianson¹⁹ is a Hubble person; German-speaking Duerbeck & Seitter^{20,21} favour Wirtz; and, by way of feeble humour in public talks, I have often spoken of "Hubble's Law, so called because it was discovered by Lundmark" who, if not Danish like my grandmother, was at least Scandinavian. But my more serious opinion is that Hubble's Law was, almost from the beginning, the right choice. That opinion was not weakened when Nussbaumer & Bieri¹⁰ took almost none of the advice I had offered under a not-very-anonymous referee's hat.

Eponyms and the division of credit

Stigler's Law²² of 1980 very properly credits to Robert K. Merton (who probably had it from Merlin*) the idea that a scientific entity essentially never carries the name of the first man[†] to put it forward. It is not, of course, considered good form to eponymize oneself. We are reliably informed by William C. Saslaw that Martin Schwarzschild, when backed into a corner to come up with a synonym for "the criterion for convective instability", reluctantly said "my father's criterion". I then naturally asked Feynman what he called 'the diagrams'. "The diagrams", said he[‡]. Thus we should not be surprised not to find Hubble writing of Hubble's Law or Lemaître writing of Lemaître's Law.

But the phrase "Hubble's Law" appears in print starting in 1933 with Walker²³, himself commemorated in the Robertson–Walker metric. For what it is worth, the preceding paper²⁴ calls a homogeneous universe Friedmann–Lemaître. Humason²⁵ had already used "Hubble's velocity–distance relation" (and is blamed²⁶ by Nussbaumer & Bieri for starting the whole nefarious custom). When Lemaître²⁷ (in the text of a 1933 talk before the US National Academy of Science) wrote "Hubble's ratio", I think the issue should have been regarded as settled.

An earlier case, not involving eponyms, is perhaps instructive. I came to it as this was being written *via* Hirshfeld²⁸, our foremost expert on the discovery of parallax²⁹. He is commenting on an earlier article about Neptune having returned to the orbital position where it was first seen³⁰. The relevant words begin "their example of the measurement of stellar parallax might inadvertently leave readers with the impression that Friedrich Bessel, who measured the parallax of 61 Cygni in 1838, was scooped by both Thomas Henderson and Wilhelm Struve, who had previously measured the parallaxes of Alpha Centauri and Vega respectively." Hirshfeld²⁸ goes on to say, "The issue was analyzed in depth in the pages of *Sky & Telescope* (November and December 1956) by none other than Otto Struve, Wilhelm's great grandson and a frequent contributor to this magazine." Hirschfeld's conclusion was:

"Bessel justly receives credit for the first determination of stellar parallax.

After presenting the case with lawyerly precision, Otto Struve explains 'the most important thing, however, is not which parallax was determined first, but which parallax actually dispelled all doubts of the contemporary astronomers that the long-searched-for effect had finally been found ... I believe it is important to distinguish the result that appeared convincing to the contemporaries of Bessel, Struve, and Henderson, from what we, with more than a century of hindsight, can recognize as the first successful [measurement].' Establishing priority of discovery can be complicated, as we all know. First is not always first."

The case of the triple-alpha reaction, customarily (and I think rightly) credited to Edwin E. Salpeter, though Ernst Öpik considered it earlier, is similar.

One must, of course, not give Hubble too much credit. The *Y* (velocity) axis of 'the diagram' belonged to Slipher, then Humason and Pease, and later many others. But the *X* (distance) axis belonged to Hubble and the 100-inch telescope for a long time. His distances came from Cepheids, and then brightest stars

*Meyer Robert Schkolnick as a teen performed magic tricks at children's birthday parties under the name Robert Merlin. Persuaded that this was tacky, he switched to Robert Merton, and took this as his legal name when he started college at Temple University, in his home town of Philadelphia.

[†]This is not politically incorrect; I merely want to exempt Noether's Theorem and Leavitt's Law, the proposed new name for the Cepheid period–luminosity relationship.

[‡]Avoiding one's own eponym is not just an historical problem. At a meeting last week (2011 September 19–22), a very distinguished and gracious colleague worked around the issue by speaking, in his introductory talk, of tZ and kZ, for thermal Zel'dovich and kinetic Zel'dovich.

and whole galaxies calibrated on them. Perhaps one should say “miscalibrated”, since his scale was too small by a factor of 5–10 (somewhat distance-dependent), and it took us all many decades to sort things out and reduce H from about $500 \text{ km s}^{-1} \text{ Mpc}^{-1}$ to 72.37 , or whatever your favourite number is.

Every polemic like this has to find a previously obscure hero. I shall pick Arthur Erich Haas (1888–1941), the Austrian–American Notre Dame physicist who wrote³¹ of “Hubble’s factor” in 1938 but the same year issued a conference press release (see ref. 14, p. 96) that spoke of “Canon Lemaitre, ... Einstein, and Richard C. Tolman, ... as the great leaders in science’s most abstruse investigations”. The conference was attended by Lemaitre and Shapley, but not by Hubble. Haas perhaps had a right to be sensitive about eponyms. According to a couple of anonymous web sites, he “prediscovered” Bohr orbits and, as is customary in such cases, was laughed off the stage with a remark that it was, after all, the first day of Carnival that year.

For what it is worth, neither Walker’s²³ nor Haas’s³¹ terminology made the cut for either Kragh¹⁴ or Nussbaumer & Bieri²⁶. Where did I find them? The *Oxford English Dictionary* of course, although the *OED* does not offer an opinion on the Chandrasekhar Limit.

The division of credit, part 2

So we return again to the legitimacy of the universal adoption of “Hubble’s Law” for the redshift–magnitude, velocity–distance, *etc.*, relationships. The underlying cause is that he was taken seriously, while Wirtz³², Lundmark³³, Silberstein³⁴, Lemaitre³⁵, Robertson³⁶, and all between 1924 and 1928, were not. I’ve long thought that there were two reasons for this³⁷. First, it was the choice of Cepheids as distance indicators, so that his numbers, though wildly wrong, were self-consistent; and second, it was as Sandage³⁸ said “the manner of the man”, tall, broad-shouldered, formal and serious-looking, dressed and accented like a fellow of an Oxbridge college.

Earlier commentators have wondered about who had access to what, and when. Nussbaumer & Bieri²⁶ and others have noted that several of Lemaitre’s key papers appeared in the less-than-widely-distributed *Annales de la Société Scientifique de Bruxelles* and wondered why. I think that anyone who has looked into what WWI did to Louvain and the Belgian scientific community in general cannot be surprised by Lemaitre’s desire to ‘support the home team’. That the English translation³⁹ of his 1927 paper left out key passages and portions of “equation (24)” is not in doubt^{11,12}, and, perhaps, as Block, van den Bergh and Nussbaumer & Bieri say, it was deliberate and malicious. Apparently blame cannot be reliably assigned. There is no evidence that Lemaitre complained, unlike Hubble, who “went nonlinear” according to Sandage¹⁶ when Shapley and de Sitter (separately!) tried to take credit for some of his work. [See note added in proof on p. 40.]

In contrast, I think who could read which languages is a *gerookte bokking* or *Ablenkungsobjekt*. My citing³⁷ of Lemaitre³⁵ as “a homogeneous universe of constant mass and croissants made of rayon ...” was supposed to be funny. I’ve slogged through Wirtz³² and the paper in which Frenkel⁷ did not discover the Chandrasekhar Limit, and even the papers in Russian in early issues of the *Bulletin of the Astronomical Institute of Czechoslovakia* to see which telescopes were used, though I can claim to speak nothing but good American and a little English.

There is no particular reason to suppose that, if Hubble’s and Lemaitre’s positions had been reversed, the latter would have done much to try to enhance

the reputation of the former. That Hubble⁴⁰ did not cite Lemaître in 1929 or later was noted by Nussbaumer & Bieri and many others. On the other hand, Lemaître's 1949 "Cosmological applications of relativity"⁴¹ has 11 citations, one each to de Sitter and to Freier *et al.* on heavy nuclei in cosmic rays, and nine to his own papers. In contrast, the next paper in that issue of *Reviews of Modern Physics* (which was an Einstein 70th-birthday festschrift) is by Gamow, who cites a dozen colleagues who are neither himself nor his students.

A bit more about Lemaître, his life, his times (mostly from McVittie⁴²)

Georges Lemaître first saw light of day in Charleroi, Belgium, in 1894 and went from the Jesuit school in Louvain into the engineering school at the University there in 1911. At the outbreak of World War I (not then so called), he immediately volunteered, serving for the duration and receiving the Belgian *Croix de Guerre*. He returned to the University of Louvain, but to study mathematics and physical science, receiving a degree in 1920, partly for work with de la Vallée Poussin on approximations to functions of many variables. He went on to Malines seminary and was ordained in 1923. This apparent change in direction suggests a considerable effect of the war on him, but I do not know this. A travelling fellowship carried him to Cambridge (UK), Harvard, and MIT, and back to Louvain, where he submitted a thesis in 1927 on the gravitational field in a fluid sphere of uniform, invariant density, according to the theory of relativity (which I have skimmed, thanks to Walter Lewin, who has, or had, a copy). The thesis made clear that the Schwarzschild horizon is only a coordinate singularity, not a real, physical one.

Lemaître spent nearly all the rest of his career at Louvain, teaching mathematical methods and history of physical and mathematical sciences as well as relativity. McVittie⁴² tells us that, when he was Eddington's student in Cambridge in 1930 and had been put to work finding expanding solutions to the Einstein equations, there came a letter from Lemaître to remind Eddington of the 1927 Belgian paper. McVittie reported Eddington's response as an apology plus an immediate letter to *Nature* designed to set the record straight. Eddington's 1930 item⁴³ is in fact a review of Silberstein's *The Size of the Universe*, with a paragraph at the end that says of Lemaître's solution: "It renders obsolete the contest between Einstein's and de Sitter's cosmologies. We can now prove that Einstein's universe is unstable [proof not given there]. The equilibrium having been disturbed, the universe will progress through a continuous series of intermediate states toward de Sitter's universe. By Lemaître's analysis, the history of the progress can be studied; and the intermediate stages (one of which must represent the present state of the universe) can be treated in detail." This seems fair enough. The same issue of *Nature* has the text of a 'tired light' talk given by Zwicky⁴⁴ in which he describes large redshifts as due to "a sort of gravitational analogue of the Compton effect". This was never popular, but in a sense was not entirely ruled out until supernova light-curves demonstrated time dilation in 1998⁴⁵.

Eventually, Lemaître's ideas also became unpopular, and that has perhaps also contributed to his relative non-recognition. From 1933 onward, his universe began with a primordial atom which began to break up, going through some 10^{11} years as nearly an Einstein static solution with a cosmological constant balancing the matter, then taking off in an exponential expansion, in which we now live, with Λ becoming ever more dominant. Cosmic rays in his view were left-over bits of the primordial atom. Even very late, he was not entirely convinced that there are protons among the cosmic rays, and he predicted that

the ratio of hydrogen to heavier nuclei in them would, at least, be very much smaller than it is in stars.*

Lemaître's universe, with primordial atom but continued neglect of any particular high-density, high-temperature processes, is intact in the text⁴⁸ of a talk he gave on stability of clusters of galaxies at a meeting in August of 1961 in Santa Barbara (just before the 1961 IAU General Assembly in Berkeley). He is definitely opposed to the clusters being stabilized by low-luminosity material. Rather, he wrote, they consist of galaxies being exchanged back and forth with the field.

Near the end of the paper, he describes the attitude of the community toward his model as "a strong prejudice against it, due to reverence of an authority whose influence can only be compared to that of Aristotle in older times." Kragh¹⁴ (see p. 58) thinks that he meant Fred Hoyle. My first thought (and that also of Paul Hodge, who was there) was Einstein, but over the days of writing this, I have come to think Hubble more likely. Neither, anyhow, could defend himself by 1961! Einstein had, of course, strongly repudiated his Λ from the 1930s onward; and Hubble (as is remarked upon by everyone who writes on these topics) was not much given to theoretical interpretation.

The verdict of history

Science is a self-correcting process if you wait long enough. This is surely true even for history of science, though 'long enough' may be generations. One stepping stone along the path is citation analysis. Since none of Nussbaumer & Bieri^{10,26}, van den Bergh^{11,13}, or Block¹² did this, I thought I would, picking out Lemaître, Hubble, Eddington, Friedmann, and de Sitter for examination and the time windows 1965–69 (the second flowering of cosmology following discovery of the 3K background) and 1980–84 (the latest period for which a five-year compendium of the *Science Citation Index* on paper is available in our library). You are welcome to add other folks and other time windows.

Table I shows what I found, after taking some care to filter out a few other people with similar names and dates and to catch citations to these five under all variants of their names. Most complex was Friedmann, who has been cited as A. Friedman, A. A. Friedman, A. A. Friedmann, and A. Freidman. Only his two cosmology papers were cited in those time frames. For the others, I did not attempt to separate cosmological papers from their others. Eddington was the superstar, but his most-cited items are the 1924 and 1926 books, *Mathematical Theory of Relativity* and *The Internal Constitution of the Stars*.

Everybody got more citations in the later period, mostly because the numbers of papers published and the numbers of citations contained in the average paper have grown monotonically for decades. Clearly Hubble trumps Lemaître, which is perhaps what Nussbaumer & Bieri¹⁰ are complaining about. I was surprised at how prominent de Sitter appears in those days before anti-de-Sitter space. More recent samples will undoubtedly reveal more of everything and Hubble

*The short biographies (see, e.g., ref. 46) all credit Lemaître for part of the discovery that cosmic rays are positively charged and, indeed, mostly protons. This is not quite as odd as it sounds given his other statements about the particles. A series of four papers, ending with Lemaître and Vallarta⁴⁷, addresses careful calculation of the paths of particles through the Earth's magnetic field, giving rise to a latitude-dependence in the flux received at the top of the atmosphere. The analysis requires only that the particles start out well away from Earth, so that a primordial egg or supernovae will do equally well. Manuel Sandoval Vallarta (who preferred to publish under his mother's rather than his father's surname) deserves a paper of his own, but this is not it. His post-war work with Luis Alvarez and others did indeed help to firm up the proton identification.

TABLE I
Citations to papers by five of the leading contributors to cosmology 1922–61

Astronomer	1964–69			1980–84		
	Citations	Papers	Most-cited	Citations	Papers	Most-cited
Lemaître	60	32	<i>Phy.R.</i> , <i>ASSB</i>	6	25	<i>ASSB</i> (1933)
Hubble	168	25	<i>ApJ</i> (1934)	193	48	<i>ApJ</i> (1936)
Eddington	2·6 cols.	22	books	3 cols.	30+	books
Friedmann	32	2	equal	46	2	equal
de Sitter	48	24	<i>BAN</i> , 8, 213	88	43	<i>MN</i> , 77, 155

continuing to dominate as far as cosmology papers are concerned. I knew him only through the now-also-closed eyes of “Uncle Allan” (as he always signed his letters) Sandage, and am inclined to think he deserved it.

Many of the commentators have remarked on the interface between Lemaître’s science and theology. I have only one anecdote to add. It came from Willam A. (Willy) Fowler, describing an incident at a conference he and Lemaître had both attended in the era when “accompanying persons” were common and were called “ladies”. Fowler asked Lemaître when he found time to say his offices during crowded days like those of the conference. His response was that he waited until after breakfast, when the ladies said they were going upstairs to dress and would be back “in just a moment”. That, said Lemaître, gave him plenty of time.

Block¹² and perhaps others have suggested that someone, probably the Europeans, should name their next big telescope for Lemaître, to balance the *Hubble Space Telescope*. Let them, however, be warned. Shortly after the launch of *HST* and the discovery of the spherical aberration in its primary mirror, Jesse Greenstein remarked to all within hearing, “Well, Edwin Hubble finally got the telescope he deserved.”

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Note added in proof:

The *Nature* “mystery item” has just appeared. It is, very appropriately, by Mario Livio of the Space Telescope Science Institute (*Nature*, **479**, 171, 2011 November 10), who has established by documentary evidence that the paragraphs and equations not present in the *MNRAS* translation of Lemaître’s 1927 paper were removed by the Abbé himself. I would add only one word to Livio’s definitive statement, which is that a native speaker of French, writing in English, who uses the word “actual” almost certainly means the equivalent of “actuelle”, that is, “current”. Thus Lemaître was saying that some of the details of his 1927 paper were of “no current interest”, not that they were “of no real interest”.