

# The origins and abundances of the chemical elements before 1957: from Prout's hypothesis to Pasadena

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**Abstract.** The 1957 papers by Burbidge, Burbidge, Fowler, and Hoyle and by Cameron are generally regarded as the foundations upon which our modern understanding of nucleosynthesis has been erected. They were, however, also the capstones of an extended period of investigation of the composition of the cosmos (earth, sun, and beyond) and of the processes that might have given rise to that composition, dating back at least as far as 1885, that is longer before 1957 than “now” is after 1957.

## 1 Introduction

“What is the world made of?” is one of those ancient questions frequently answered by some sort of creation myth, in which First Turtle dredges mud from beneath the sea to form the land, or First Woman cries over the loss of a child until the land is surrounded by water. Some Greek philosophers distinguished four terrestrial elements – earth, water, air, and fire – and a fifth quintessence, which made up the celestial objects (the moon marking the boundary between air and less earthly things). These were, historians tell us, at least as much principles as actual substances. The alchemists began a transition to the idea of discrete substances, some of which could be transmuted into others by heating, evaporation, condensation, dissolving in acids, burning, and so forth, though somehow gold resisted. The concept of phlogiston, incidentally, is not so old as you might have supposed, and it reigned for only a century or so (Stahl 1702 to about 1800).

And then came Lavoisier and the “rise of modern chemistry”, in which elements became firmly the simplest substances, which could be combined in various ways, but not further decomposed, so that metals were simpler than their calxes (oxides) and sulfur and phosphorous more fundamental than their acids. Lavoisier was aware of 37 elements many of which correspond essentially to modern ones (though he reserved judgment on the possibility that some might be further decomposed) at the time of his guillotining in 1794 (Scerri 2007).

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Inevitably the questions of abundance determinations and formation processes become entwined at various stages, but I will follow them separately here, in that order, although promising ideas of origins are actually contemporaneous with the first quantitative abundance tables even for the earth. The ideas on processes will further subdivided into “cosmic” and “stellar” at the point where transmutation gets tangled with the sources of solar and stellar energy.

## 2 How much is there of what?

Matching the full ranges of elemental and isotopic abundances in and between the stars and galaxies and their changes with time is the ultimate test of our understanding of nucleosynthesis and has, of course, not been achieved. At the other end of time, the ancients obviously knew that there was more iron around than gold, at least in their neighborhoods. But let us plunge into the middle of things with Kleiber (1885), the oldest attempt I have found at a systematic accounting of abundances in meteorites and the earth. He recognized the existence of the iron peak<sup>1</sup> and remarked that light elements (meaning oxygen and silicon) were generally commoner than heavy ones (meaning silver and gold). Next came Vernon (1890), who plotted, in the form of a periodic table as then understood, those elements that had been detected in the sun. There seemed to be a strong correlation with chemical properties: almost all the electrochemically most positive elements (Li, Na, K, to Cs) were there, but fewer and fewer as the elements became more electronegative, with none of F, Cl, Br, or I, or even of O, S, or Si.

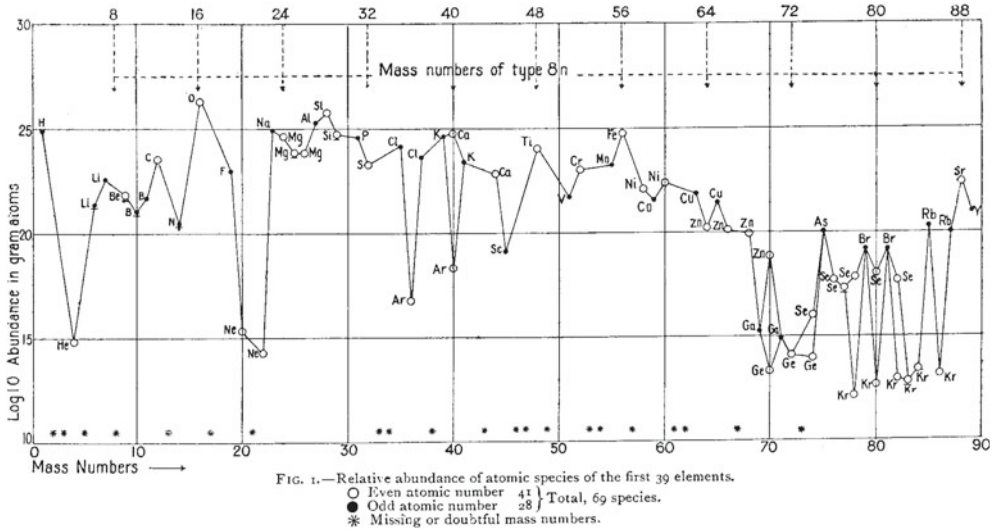
Vernon took this pattern as confirmation of his picture of cosmic origin, in which only one form of primordial atom existed at the beginning, and more complex elements were built up during a process of cooling. The sun, he felt, provided support for this view, with the metals being more stable than the non-metals, confirming chemical evidence then available, like the break-up of Br<sub>2</sub> and I<sub>2</sub> molecules into single atoms when they are heated, and the temperature dependence of specific heats of C, B, and Si. In contrast, he said, molecules of Na, K, Zn, Cd, and Hg consist of single atoms at all temperatures, showing their greater stability. Today one looks at the outer electron shells and notes that there are suitable, low-lying excited states of one or two outer electrons to absorb visible photons at stellar temperatures in the atoms of Na, Ca, and so forth, while the resonance lines of O, C, and N fall in the ultraviolet.

Vernon’s “abundance table” was purely qualitative. But in the same time frame, Frank Wigglesworth Clarke (1892) of the US Geological Survey, later President of the American Chemical Society, attempted a quantitative inventory of the parts of earth available to him for study – crust, ocean, and atmosphere. The most abundant elements were oxygen (nearly half by number of atoms) and silicon (nearly half by mass), with Al, Fe, Ca, Mg, Na, and K each 1–10% by number, and everything else much rarer. He noted that almost nothing could be said about the interior composition, which we now know considerably increases the proportion of iron and nickel and, probably, sulfur. Clarke also suggested that large amounts of O and Si could be hiding in the sun in the form of non-volatile silica (not that the sun was cooler then, but that not much was known about laboratory behavior at 6000 K). Like Vernon, Clarke perceived evidence for a gradual build-up of elements from a primordial substance, which went slowly until oxygen was reached and then produced most abundantly the elements that form stable oxides, again a correlation with chemical properties.

The next 30 years saw the discoveries of radioactivity, electrons and nuclei, and much more, including the key idea (credited with the usual simplification of physicists

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<sup>1</sup> That is, that Fe and its immediate neighbors in the periodic table are more abundant than the elements on either side, e.g. Sc and Ga.



**Fig. 1.** Abundances of the nuclides by mass from Aston (1924). The black circles at the bottom are masses (2, 3, 5, 8, 13, etc.) for which he has no data. The peaks at mass 8, 16, 24, 32, etc. are actually the nuclear binding energy effects he was looking for. Correct, much larger, solar abundances for elements at 4 (He), 12 (C), 20 (Ne), 40 (Ar) would have made this much clearer.

to Henry Moseley, who died at Gallipoli) that there was a unique, correct ordering of the elements by integers, not perfectly correlated with the ordering by atomic weights. We now recognize those integers as the numbers of protons in atomic nuclei.

This brings us to William D. Harkins (1917, 1921, Allison and Harkins 1924, Harkins and Wilson 1915), of the chemistry department of the University of Chicago. Samuel K. Allison (Chicago nuclear physicist) was Harkins’s student and, in turn, the teacher of James Cronin (Chicago particle physicist), who supported in Allison’s honor a lecture given at the April, 2007 meeting of the American Physical Society by Geoffrey R. Burbidge, who had known Allison in his own early postdoctoral days at Chicago. Readers who had British or American childhoods may be reminded of the house that Jack built.

Harkins and his colleagues used meteoritic and earth-crust data to assemble abundance values, plotted graphically as a function of the extent to which an element or isotope nucleus deviated from being explainable as an assembly of helium nuclei. They thought that He nuclei (alpha particles) retained their identity inside of more complex nuclei and attempted to liberate some from mercury vapor at high temperatures. (They failed.) Harkins wrote in 1917 of the hydrogen-helium structure of more complex atoms, and he used the word neutron in 1921 to mean a (proton-electron) entity inside a nucleus<sup>2</sup>. His abundance compilations exclude helium itself, neon, and argon, though they ought to be quite abundant on his H-He hypotheses (and, of course, are, though not in meteorites).

The graph of abundances presented by Aston (1924) is particularly interesting for several reasons. First, some of the isotope numbers come from his own work on intensity of lines in a mass-spectrogram. Second, having set out in hopes of finding evidence for relative stability of nuclei during the evolution of the atoms, he is discouraged by the very ragged appearance of abundances vs. mass numbers. The raggedness is the

<sup>2</sup> This works as long as you don’t know enough quantum mechanics to worry about confining electrons inside nuclei.

odd-even effect and dominance of alpha-particle nuclei, though with He, Ne, and Ar far too low (he is again using mostly meteoritic data). That is, he is seeing just the stability (or binding energy) effects he set out to look for, and also the rapid drop-off beyond iron, the most tightly bound nucleus. Third, then, his mix has, when fully ionized, a mean molecular weight near 2.5, just what Eddington (1926) had in mind. The most abundant elements are again O and Si, with Fe down a factor of 10 or so and more or less tied with Mg and Ca. One reads fairly often that Eddington's sun was "mostly iron, like the earth", both parts of the analogy being false. Only the compilation by Noddack and Noddack (1930) included element 43, which they called masurium.

The number of elements recognized in the sun gradually increased with time, and Russell et al. (1926) have 49, but the noble gases (apart from He) are still missing, as are the entire halogen series and P, S, As, and Se from the period where the more metallic elements have been identified. Again we recognize this as reflecting atomic, not nuclear, physics.

Viktor Goldschmidt's (1937) table of cosmic abundances is sometimes described as the first, which, as we have seen, is clearly not true. It was, however, the first to incorporate solar data (from Russell 1929), so that Si/H by number is  $4 \times 10^{-5}$ , roughly right. He/H = 0.004 is still too small, however, the modern number being close to 0.1. No numbers are given for Ne, Ar, or Kr, but odd-even effects, the importance of alpha nuclei, and fall-off beyond the iron peak are all clear. Goldschmidt is not impossibly lost in the past. His last graduate student, Brian H. Mason, the discoverer of Antarctic meteorites from the moon, who fled with Goldschmidt from Norway to Sweden in 1939, died on 3 December 2009, at the age of 92.

Another important compilation of abundances, finally including the noble gases, came from Brown (1949), but it was the tables of Suess and Urey (1956) which guided B<sup>2</sup>FH (1957) and Cameron (1957). These are, at least on log-log paper, nearly indistinguishable from more recent results. Solar system abundances of this sort were frequently called "cosmic", and the extent to which this is, or is not, appropriate turned out to be important.

Payne (1925) put two very important conclusions into the mix. First was that cool (K) giants must have atmospheres dominated by hydrogen and helium, or, given the large excitation energies needed to get them out of their ground states to levels where they could absorb visible light, they would never be seen at all. This was correct, but it was only very slowly accepted. Russell in particular insisted that she account for the hydrogen lines in cool stars by "anomalous excitation", and you have to dig fairly far into her thesis publication to see the real conclusion. A large percentage of hydrogen is, of course, essential if H fusion to He is to be the main source of stellar energy (Sect. 5). Eddington (1926) wanted a large mean molecular weight so that gas and radiation pressure would be equal in typical stars. (In fact gas pressure is much the larger except in very hot stars.) Thus he was prepared to accept at most 7% hydrogen, just enough to fuse for  $10^{10}$  years, if the whole star burned. The H abundance crept slowly upward (Eddington 1932) only when others analyzed the sun some years later (Unsöld 1928, 1938, McCreia 1931 on the solar chromosphere, Strömgren 1932). Most unfairly, Russell (1929) is often given credit for demonstrating lots of hydrogen (50% in his mind by this time). Wildt's (1939) discovery that H<sup>-</sup> is the primary source of continuum opacity in cool stars like the sun was essential to finally getting reliable ratios of heavy elements to hydrogen.

Payne's (1925) second conclusion was that all stars had essentially the same composition, both proportion of heavy elements and ratios among them, spectral differences being due to differences in photospheric temperature. In contrast to the dominance of hydrogen, "all stars the same" was accepted very quickly (it appears already in Russell et al. 1926) and had great staying power, the last advocate, Anne

B. Underhill<sup>3</sup>, dying early in the present century. It is indeed true for a large majority of stars in the disk of the Milky Way, excluding carbon stars, a few halo stars that are just passing through, and so forth.

But if the second Payne conclusion were precisely true, it would have required that the heavy elements all have been synthesized before the formation of the stars we now see, and under fairly uniform conditions. On the other hand, variation in total abundance of heavies and their ratios (especially if correlated with stellar age) would imply on-going production and probably processes in stars not too different from the ones we now see. There were actually decades in the 20th century when on-going star formation was also doubted (Trimble 2009).

So widely had the “all stars are the same” conclusion been accepted that the first folks who pointed out it wasn’t really true struggled for publication. Chamberlain and Aller (1951), for instance, in their first draft suggested a factor of 100 heavy element deficiency in a couple of subdwarfs. The referee required this to be weakened to a factor 10. It is also not true that Baade’s (1944) populations I and II sprang to life with age and composition differences in place, and he himself denied the possibility until 1947-48 (Osterbrock 1998). Chamberlain and Aller (1951) were not entirely alone. Iwanowska (1950, 1953) reported composition differences in RR Lyrae stars observed when she visited Yerkes and worked briefly with Struve in 1948. Roman (1952, 1955) repeatedly found that high velocity stars had weak lines. Schwarzschild and Schwarzschild (1950) and Schwarzschild, Spitzer, and Wildt (1951) also joined the group concluding that weak lines (and true low metallicity) were characteristic of old, high-velocity, Population II stars.

An age-metallicity relation of course opened the possibility for synthesis of heavy elements from on-going processes, though not quite to the extent advocated by Rutherford (1929), who, when he had shown that the oldest earth rocks date to billions of years ago, while Jeans required the sun to be  $10^{12}$  years old, concluded that the sun was making uranium now and that the planets had spun off from it relatively recently. Von Weizsäcker (1938) also got somewhat tangled up with uranium, but it was not his primary concern.

Non-solar metal abundance ratios and numbers for stars in other galaxies belong largely to the post-1957 era (Pagel 1997).

### 3 Early thoughts on the assembly process

The official starting point in this marathon is John Dalton’s atomic theory of 1801, which, though doubted by a few for something like a century thereafter, was never lost to mainstream scientists (see Scerri 2007 or any standard history of chemistry). Dalton’s own table of atomic and molecular weights had only one integer, hydrogen = 1, while carbon was 4.3, oxygen 5.5, water 6.5, and so forth, but the next round of experiments produced a number of integer or near-integer values.

These prompted what is now called Prout’s hypothesis, though his actual papers on the topic are anonymous (Prout 1815, 1916). The idea is that atoms of all the other elements could be regarded as being made up of some suitable number of hydrogens. Though this comes rather close to the current view of both nuclear structure and nucleosynthetic processes, it went out of fashion quite soon when numbers like Cl = 35.5 were firmly established (and isotopes were a century away). In Prout’s own words, “If the views we have ventured to advance be correct, we may almost consider the protyle of the ancients to be realized in hydrogen; an opinion by-the-bye not altogether new”.

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<sup>3</sup> Who denied the absence of hydrogen from the atmospheres of Wolf-Rayet stars.

“Protyle” is a contraction of proto-hyle or first stuff, the hyle part appearing later as Gamow et al.’s ylem. It is not certain who might have anticipated Prout’s idea. Scerri makes a case for Humphrey Davy, who, as you know, detested gravy, because he lived with the odium of having discovered sodium.

Those who turned Prout’s hypothesis into a scenario for producing the elements, when the universe was young, from a primordial substance that gradually agglomerated into the other elements include Vernon (1890) and Clarke (1892), whom we have already met, and Crookes (1888) of the tube. The 1888 version of Crookes’s ideas appears as a presidential address to the Chemical Society (in London), meaning that he was allowed to say whatever he wanted<sup>4</sup>. He is worth quoting. “If we may hazard any conjectures as to the source of energy embodied in a chemical atom, we may, I think, premise that the heat radiations propagated outwards through the ether from the ponderable matter of the universe, by some process of nature not yet known to me, are transformed at the confines of the universe into the primary – the essential – motions of chemical atoms, which the instant they are formed gravitate inwards and thus restore to the universe the energy which otherwise would be lost to it through radiant heat. If this conjecture be well founded, Sir William Thomson’s startling prediction of the final decrepitude of the universe through the dissipation of its energy falls to the ground”<sup>5</sup>.

These are cosmic processes, occurring in the long-ago universe, or at least Galaxy, as a whole, though Crookes also proposed stellar interiors as a possible site for assembling protyle into the elements at an 1879 meeting of the British Association for the Advancement of Science.

Independent of site, you may well be surprised (I was) that any progress at all could be made in the absence of neutrons to put into nuclei and the notion of barrier penetration to help bring charged particles together. Indeed without them, a very high temperature is needed to bring even hydrogen and helium into equilibrium (Tolman 1922). Tolman incidentally started life as a chemist, though we now probably more often associate him with relativity and cosmology, thanks to his classic 1934 text (Tolman 1934).

Let us therefore turn to George Gamow, whose career spanned these fundamental discoveries and contributed to one of them.

## 4 The Gamow pedigree and cosmological nucleosynthesis

Because we associate George Gamow so strongly with the idea of Nucleosynthesis from a neutron gas in the early universe and because his first paper (Gamow 1935) on this was not published until he was past 30, it is natural to ask, what was Gamow doing before he understood the universe? It is part of the folklore that he moved from his hometown of Odessa to Petrograd/St. Petersburg/Leningrad to work with A.A. Friedman, who died almost immediately thereafter (not, we suppose, causal). Instead

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<sup>4</sup> Admittedly he was the editor of another journal in which many of his papers appear, *Chemical News*, allowing him to put forward new elements with names like Decipium, Philippium, and Mosandrum, to live among the rare earths, which were just then being differentiated by a number of chemists (Crookes, 1886, 1889).

<sup>5</sup> Crookes’s universe was our Galaxy; Thomson is later Lord Kelvin. Worrying about what became of light and heat not caught by planets also occupied Simon Newcomb (1906) in the same time frame. Crookes notes further along that, although he is speaking of the Milky Way, M31 probably behaves in a similar fashion. And a similar sort of restorative process, in which photons travel around the universe and come back as cosmic ray particles was advocated by Jenő and Madelaine Barnothy in the 1960s (Barnothy 1963, Barnothy and Forro (1946).

he worked partly with D. Iwanenko (whom you could have met at relativity meetings into the 1970's), with a first published paper (Gamow and Iwanenko 1926) on five-dimensional Kaluza-Klein theory as a possible way of unifying electromagnetism with general relativity. This was a bandwagon topic at the time, with other papers by Klein (1926), Fock (1926, another relativist), and Mandel (1926), who wrote from Petersburg, while Gamow and Iwanenko later in the year said Leningrad.

Gamow's classic barrier penetration paper (1928) formed part of his Ph.D. dissertation, and before leaving for fellowships in Copenhagen and Cambridge, he handed on the torch (via tunneling of course) to Fritz Houtermans (Gamow and Houtermans 1928). This brings us into contact with nucleosynthesis in stars and the stellar energy problem (next section), because Atkinson and Houtermans (1929, Atkinson 1931) pioneered the idea of heavier elements acting as catalysts to bring four protons and two electrons together to make an alpha particle. Indeed Gamow (1938) himself was not entirely faithful to the cosmological nucleosynthesis point of view.

Ideas over the next decade or two that seem to be part of the cosmological story are, I think, (1) the impossibility of accounting for all abundance ratios in terms of equilibrium at a single temperature and density (Urey and Bradley 1931, Pokrowski 1931), so that light and heavy elements have to be produced under different circumstances, (2) the need to include the effects of "freezing out" of nuclear processes as matter gradually cools from the synthesis temperature (Farkas and Harteck 1931), (3) the possibility of successive neutron captures followed by beta decays as an alternative to charged particle reactions (Gamow 1935 was preceded in this by Walke 1934, who, however, made his neutrons in stars), (4) the suitability of the early universe as a site for such captures simultaneously with cooling (Gamow 1946), (5) the strong resemblance between a smoothed version of the observed abundances and what would be produced in an expanding universe consisting initially of an "ylem" of neutrons (Alpher et al. 1948, Gamow 1949, Alpher and Herman, 1950), provided that the product of the expansion time scale and the neutron density was about  $1.3 \times 10^{-6} \text{ g s cm}^{-3}$ , (6) the possibility of starting with radiation, protons, neutrons, electrons, and positrons in equilibrium, rather than with pure neutrons, and getting roughly the observed He/H ratio, largely independent of initial temperature, provided only that it is very high (Hayashi 1950, who found He/H = 1/6), and (7) the hopelessness of the whole endeavor in view of the absence of stable nuclides with masses 5 and 8 (Fermi and Turkevitch, 1949). The problem is that by the time the universe is cool enough for deuterons not to be photodisintegrated, the density is too small for three-body interactions to carry the observed 1% of matter across the gap. The seemingly-logical equal mix of matter and anti-matter is also impossible because it all annihilates (Alpher et al. 1953). A comparable number of ideas and papers from the same period seem in retrospect not to have contributed to our understanding of the topic (see Trimble 1975, p. 918).

Focus on nucleosynthetic research then shifted to stars for a couple of decades, driven by multiple successes in modeling stellar structure and evolution with hydrogen fusion as the main energy source. Hoyle and Taylor (1964) revisited the cosmological case, taking He/H = 0.1 as a universal value to be reproduced, and the subject took off again after the discovery of the 3K isotropic, microwave background, relic radiation made it possible to follow a unique, correct trajectory in the temperature-density plane. Peebles (1966a, b) quickly showed that the Big Bang is, indeed, good for making all the elements up to helium, including small amounts of deuterium and He<sup>3</sup> <sup>6</sup>. Wagoner et al. (1967) covered the same ground more thoroughly, finding that

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<sup>6</sup> For many decades, including the period covered in this article, atomic weights were indicated with a small number in the upper right hand corner, C<sup>13</sup> for instance. This convention is used here. The primary reason for it was probably to indicate correct pronunciation!

a small amount of  $\text{Li}^7$  is also produced, and that various non-standard cases could reduce helium production almost to zero or otherwise spoil the good agreement with observations. Forty + years downstream, their conclusions largely stand, and “not messing up big Bang Nucleosynthesis” remains a strong constraint on variant models.

## 5 Synthesis of the elements in stars

This was actually the title of Burbidge et al. (1957, B<sup>2</sup>FH), and in a logical universe, it would have been established first that the basic stellar energy source is hydrogen fusing to helium, and the community would then have moved on to making heavier elements up to iron with charged particle reactions and then beyond with neutron captures. In fact, well before the details of the proton-proton chain were established, Walke (1934) put forward a series of neutron captures and beta decays very much like what is now called the s-process. He noted that  $\text{Se}^{74}$  gets missed; it is now thought of as a p-process nuclide. His neutron source was part of the proton-proton chain  $\text{H}^2(\text{d}, \text{n})\text{He}^3$ .

The principle of conservation of energy and its applications to the sun and stars is of sufficient importance that there is probably enough credit to go around among all who claimed some portions (Julius Robert Mayer, William Waterston, William Thomson = Kelvin, Hermann von Helmholtz, James Prescott Joule, and others, Lindley 2004, Ch. 4). But it is convenient to focus on Kelvin, because he applied the principle to two objects and got the same wrong answer for both. First he considered the cooling of the earth from a hypothetical molten stage (for which his 1862 number was 20–400 million years). Second was the possible maximum lifetime of the sun, living on gravitational contraction (15–100 million years found also in the 1860s). Kelvin never really backed away from these numbers, and late in life said that this was the work of which he was proudest. He also denied the possibility of the transmutation of elements, attributing radioactive heating to energy extracted by atoms from the ether and released in bursts. Radioactivity is, of course, relevant to the temperature history of the earth’s interior, though not (Kelvin was right at this point) to solar energy. Mendeleev (1899) was surely the last proponent of chemical energy.

In the same time frame, however, uniformitarian geologists and evolutionary biologists were asking for something more like  $10^9$  years than at most  $10^8$ . Darwin himself estimated that 300,000,000 years had been necessary to build up a particular English geological formation, The Weald, though later editions of his book left this out. Interchanges among the various disciplines were not always very polite.

Both amateur astronomer William Wilson and (very!) professional physicist Ernest Rutherford (1903) noted that a sun made of radium could have a much longer life than the Kelvin-Helmholtz time scale. This was not meant as a model – spectroscopists already knew that the sun was not made mostly of radium – but as an indicator that the existence of subatomic energy was going to change the rules of the game. Joly (1903) made the same point about the earth.

Then there seems to have been a silence of 15 years or so on the stellar energy / stellar nucleosynthesis problems, most of which predated WW I. Part at least of the

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One now occasionally hears young scientists speaking of “seven-lithium” and has to wonder what they mean! The upper right hand corner was still correct when I was a graduate student and is used in what was, for many years, the standard text in the subject, Donald D. Clayton’s *Principles of Stellar Evolution and Nucleosynthesis*, published by McGraw-Hill in 1968. Putting the isotope number in the upper left hand corner seems to have come in some time in the 1970s.



reason, I think, must have been that it proved possible to make considerable progress on issues of stellar structure, for instance mass-luminosity-radius relations on the main sequence, without knowing the nature of the energy source, provided only that energy transfer was by radiation. R. Emden, K. Schwarzschild, and others contributed, and the topic is discussed by Eddington (1926) in Chapter V.

Harkins and Wilson (1915) perhaps led the revival, but it was a slightly younger generation, after war clouds had begun to settle, that we associate primarily with the idea of “subatomic energy” and transmutation of the elements and their potential for allowing solar and stellar lifetimes of Gigayears, at least for the smaller stars, e.g. Perrin (1919, 1920, 1921), Russell (1919), Eddington (1919, 1920), Arrhenius (1922). Jeans, beginning in 1917, in *Monthly Notices*, then held firm for another 20 years to the need for lifetimes of  $10^{12-14}$  years (on stellar dynamical grounds) and, therefore, a need for complete annihilation of matter as the energy source (e.g. Jeans 1929). The community soon followed the subatomicists, though Russell et al. (1926) were not quite ready to rule out longer lifetimes and annihilation completely.

Oddest contribution from this period is probably a series of papers ending with Nicholson (1917). He has in mind evolution beginning with nebulae (Orion and the planetaries being the same sorts of beast) via Wolf-Rayet stars to stars with absorption spectra like the sun. But his nebular spectra are dominated by Nebulium and other elements that, he says, cannot be fit into the periodic table.

It is about now that we make contact with the standard story of “how we know the stars live on nuclear energy”. There are six pieces.

1. Nothing else can provide enough energy, once Ernest Rutherford showed that pitchblends from earth rocks have uranium-lead ages of a Gyr and more. A contemporary argument is at least as persuasive, once one has slogged through the arithmetic. If Delta Cephei has to live on gravitational contraction, its radius, density, and hence pulsation period would be changing much faster than observational limits permitted, even in 1926 (Eddington 1926, Sect. 202). This leads to the requirement that the true energy source must be about 100 times as generous as gravitational potential energy, nicely taking Kelvin’s  $10^8$  years maximum for the sun up to  $10^{10}$  years.
2.  $E = mc^2$ , or, perhaps more insightfully,  $\Delta E = \Delta mc^2$ , which appears in one of several Einstein papers from the “wonderyear” of 1905 and means that if mass vanishes, energy is available (and, of course, conversely).
3. Accurate masses for hydrogen and helium atoms from Aston (1920, 1927). Showing that “delta” in this context is about 0.7% of the total rest mass-energy. In fact Edward W. Morley had reported in 1895 that the atomic weight of oxygen relative to hydrogen was 15.789. He regarded this as a refutation of Prout’s hypothesis, but it also meant that there was 1.3% of  $mc^2$  to be had as energy, if you could figure out how to extract it.
4. Next, if hydrogen fusion is to be important, one needs to know that there is quite a lot of hydrogen in the sun and stars. As noted above, Payne’s (1925) demonstration of this for the K giants took a while to achieve full acceptance.
5. Barrier penetration (Gamow 1928, Gurney and Condon 1928, 1929, often mis-cited as Condon and Gurney) is required as the more technical version of Eddington’s remark: But we do not argue with the critic who urges that the stars are not hot enough for this process; we tell him to go and find a hotter place. That is, classically two protons will have enough kinetic energy to approach within a Fermi (femtometer) of each other only at  $T \approx 10^9$ , while Eddington had already shown that pressure balance requires temperatures  $\approx 10^7$  at the centers of stars.
6. Detailed sequence of nuclear reactions that can occur at stellar temperatures and indeed fuse hydrogen to helium. Credit is divided by the Atlantic ocean between

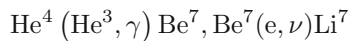
von Weizsäcker (1937, 1938) and Bethe (1939, Bethe and Critchfield 1938), with the two earlier papers in each case addressing what we now call the proton-proton chain and the absolute need for deuterium (Urey 1933) formation before anything else can be done. The later Bethe paper is the CNO cycle. No neutrinos are explicitly written in any of the reactions.

Information did not necessarily diffuse very rapidly just then. Blau (1940), a native of Vienna, but writing in Spanish in Mexico about the sun, thinks of fusion but has evidently seen neither Bethe and Critchfield nor the von Weizsäcker papers. It was the display of a page from her paper at the 2007 50th anniversary symposium at Caltech that prompted G.R. Burbidge to ask, “Virginia, where do you find these things?” and then, lest he should have been misunderstood as having uttered a compliment, “why do you find these things?”

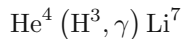
Two relevant additional ideas appeared in the late 1930s that are not quite part of that mainstream, one good and one bad. The good one was the recognition that the secret to red giant structure is a composition discontinuity, enabling the helium core to be very hot and dense while the atmosphere is cool and tenuous (Hoyle and Lyttleton 1939, Opik 1938, 1939). That something else would have to happen once the inert core grew to 10% of the stellar mass (Schoenberg and Chandrasekhar 1942) leads logically to the next section. The bad idea, which led no place useful, was that stars might derive their energy primarily not from subatomic processes but from core collapse right down to neutron densities. So briefly said Gamow (1937), with synthesis of heavy elements en route, but unimportant, and Landau (1938). Gamow got over it almost immediately (Gamow and Teller 1938), and Landau was perhaps merely exercising his lifelong disdain for astrophysics and cosmology, expressed in phrases like “horrific nonsense” and “pathological” (actually the Russian equivalent, I suppose, Hall 2009).

## 6 Synthesis in stars – on beyond helium

It is not quite true that the absence of stable  $N = 5$  and 8 nuclei constitutes an impassible barrier for two-body nuclear fusion. If you happen to have some  $\text{He}^4$  around (easy) plus either  $\text{He}^3$  (possible) or  $\text{H}^3$  (more difficult for systems more than 12 years old), then either



or



is possible (Kolb and Turner 1990, Sect. 4.3). Thus we now recognize that big bang nucleosynthesis provides about one part in  $10^{10}$  by mass of lithium, as seen in the older, unprocessed Galactic stars. But then one is really stuck. Lithium burns easily, but the product is only a scrap of additional helium, from proton capture and disintegration of the product  $\text{Be}^8$ . Thus every atom in your body that is not hydrogen (or helium) has a 3-nuclide fusion in its past.

The first to attempt to follow fusion beyond helium was Sterne (1933), who had in mind a gradually contracting star, with equilibrium composition gradually shifting as the density and temperature increased. Sterne thought that the equilibrium would extend up the periodic table at least to zinc, the heavy end of the iron peak, which is indeed more or less where we now get with silicon burning, and where  $\text{B}^2\text{FH}$  got with their nuclear statistical equilibrium. Some less systematic proposals for the origin or transmutation of elements had come from Stone (1930), Wilson (1931), Steensholt (1932), and Pokrowski (1929). Gamow’s (1935) scenario, though it began with pure neutrons, was not so very different. Then there was a war.

Hoyle (1946), whose pre-steady-state work on stellar nucleosynthesis is more often cited than Sterne's, described specifically the case of a massive star core of helium that had contracted to roughly  $\rho \approx 10^7$  g/cm<sup>3</sup> and  $T \approx 4 \times 10^9$  K. He proposed that nuclear statistical equilibrium would then establish something like the mix of heavies we see and that rotational instabilities would eject the processed matter from the star. In the same time frame, there were several other efforts to make the full range of heavy elements from a single environment. Both Klein (1947) and Beskow and Treffenberg (1947) examined a succession of equilibria during gravitational contraction, à la Sterne. The combination of very high density ( $10^{11}$ – $10^{12}$  g/cm<sup>3</sup>) but low temperature was addressed by van Albada (1946), while Mayer and Teller (1949) looked at fission in a single, large blob of neutrons (a hold-over from bomb work perhaps). Murgai (1952) made deuterium (but apparently nothing else) from an explosion of a proton gas. A Robert Frost fan might say that several roads diverged in a wood, and luckily we took none of them.

Between Sterne (1933) and Hoyle (1946), Bethe (1939) had actually proposed the fusion of three alpha particles ( $\text{He}^4$ ) to make  $\text{C}^{12}$ , but without being firm about the conditions required or the likely stellar sites. The standard “pre-discovery” of the triple-alpha reaction is that of Opik (1951), cited correctly in the cores of inhomogeneous red giants, but incorrectly at a temperature of  $4$ – $6 \times 10^8$  K, because he was not aware of a resonance in the unbound  $\text{Be}^8$  nucleus, just 95 eV above the energy of two incoming alphas (though it had been pointed out by Wheeler 1941). The resonance increases the cross section for transient  $\text{Be}^8$  formation and so lowers the temperature at which it is possible. Opik also considered successive additional alpha captures to produce  $\text{O}^{16}$ ,  $\text{Ne}^{20}$ , etc. at slightly higher temperatures.

The standard “discovery” of triple-alpha is Salpeter (1952). He had been “lent” by Hans Bethe (at Cornell) to William A. Fowler (at Caltech)<sup>7</sup> to tackle various problems in nuclear astrophysics (Salpeter 2008). Salpeter was aware of the “Wheeler” resonance but not of the work by Opik, though the latter (Opik 1977) was quite scrupulous about sending copies of papers produced at his Armagh Observatory to other institutions on an exchange basis. The Pasadena copy was almost certainly at the offices of Mt. Wilson and Palomar Observatories, in those days a pleasant half-hour walk from Fowler's lab, which, however, very few people took. Salpeter (1952) required a temperature for  $3 \text{He}^4 \rightarrow \text{C}^{12}$  lower than Opik's (where an additional alpha capture and other reactions would have destroyed the carbon as fast as it formed), but high enough that  $\text{C}^{12}(\alpha, \gamma)\text{O}^{16}$  had a cross section larger than that for triple alpha, so that oxygen would dominate the product mix by a large factor. Indeed it is the more abundant of the two in stars and interstellar gas and all, but there is, in fact, enough carbon in the real world for us to be organic, in the original meaning of the word.

This brings us to the “Hoyle resonance” in  $\text{C}^{12}$  (Hoyle 1954). It is the second excited state and has the right spin and parity to increase the cross section for  $\text{Be}^8(\alpha, \gamma)\text{C}^{12}$  by a very large factor. This lowers the temperature needed for triple alpha and so discriminates against oxygen production, for which the Coulomb barrier is, of course, higher. The resonance was present in a pre-war study (Holloway and Moore, 1938) but not there in seemingly better data (Malm and Beuchner 1951). Thus when Fred Hoyle came to Kellogg Radiation Lab at Caltech in 1953 and said that the state must exist or we wouldn't be here to talk about it, he was arguing not exactly in the absence of data but in the face of the data. And he actually persuaded the Kellogg crew to have another look and find the resonance (Dunbar et al. 1953). This permitted

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<sup>7</sup> Yes, virtually everybody called him Willy, but he told me in 1971, in connection with a 60th birthday conference, that he had decided he would like to be known as William A., at least on paper.

us all to continue to exist and left Fowler in Pasadena very much impressed by Fred Hoyle.

In 1954, Hoyle was back home at the Department of Applied Mathematics and Theoretical Physics in Cambridge. Fowler was spending a year there on a Fulbright Fellowship. And the Burbidges were also in Cambridge because GRB had taken a junior position in Martin Ryle's radio astronomy group at the Cavendish Laboratory. The four clearly bonded, because in 1955, when Fowler was back home in Pasadena, Hoyle returned as a visitor, and the Burbidges came on fellowships, he, the theorist, at Mt. Wilson and Palomar Observatory, and she, the observer, at Kellogg Lab. This appears to have been a way of circumventing the rule against woman observers, which changed only in 1965. The four worked on aspects of "synthesis of the elements in stars" through the early months of 1957, writing what eventually became a hundred page article in *Reviews of Modern Physics* (Burbidge, Burbidge, Fowler, & Hoyle 1957). And the rest is not history, but, as it were, current events. The four have told their own versions of the story (Hoyle 1982, Fowler 1992, E.M. Burbidge 1994, G.R. Burbidge 2007), as have Cameron (1999), Salpeter (2002), Bethe (2003), and Opik (1977), the last addressing many of his innovative ideas in astrophysics but not the triple alpha reaction.

The 1957 synthesis papers (B<sup>2</sup>FH, Cameron 1957a,b) carried on from carbon and oxygen with a series of alpha captures up to Ca<sup>44</sup> and Ti<sup>48</sup> and then considered a short-lived pre-supernova phase of nuclear statistical equilibrium to build the iron peak elements. It later became clear that Ne<sup>20</sup> had no ground or low lying excited state with suitable spin and parity to give O<sup>16</sup> ( $\alpha, \gamma$ ) Ne<sup>20</sup> a large cross section. As a result, interactions between pairs of C<sup>12</sup> nuclei occur first, producing Ne<sup>20</sup>, Ne<sup>23</sup>, and so forth, so that carbon burning is followed by neon burning, oxygen burning, and silicon burning, this last via photo-liberation of alphas from some Si<sup>28</sup>'s which are then captured by other Si<sup>28</sup>'s (summarized in Trimble 1991).

Although the focus here has been on fusion of the lighter elements, the most impressive aspect of the 1957 syntheses was probably the division of nuclides above about  $A = 60$  into three groups, the valley of beta stability, the neutron-rich, and neutron-poor classes and the description of the sequences of neutron captures (slowly or rapidly, s- and r-processes)<sup>8</sup> and removal of neutrons (or proton captures, p-process) to produce them all in the right proportions. It is perhaps worth reiterating that a chain of neutron captures with intervening beta decays already appears in the work of Walke (1934) who notes that he cannot make Se<sup>74</sup> that way (it is a p-product).

Cameron (1999) has provided a particularly clear description of how he was able to estimate the necessary neutron capture cross sections adequately, despite limited laboratory data and very limited computing power at Chalk River. Initially at least his synthesis was less widely known than B<sup>2</sup>FH because the extended version was classified as a Chalk River report (Cameron 1957a) and the public version (Cameron 1957b) was very short.

## 7 Summary and forecast

This review had its origins in an invited talk at a symposium organized at the California Institute of Technology, in July 2007, to mark the 50th anniversary of the landmark paper, "Synthesis of the Elements in Stars" by E.M.Burbidge, G.R.

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<sup>8</sup> Cameron said slow and fast.

Burbidge, W.A. Fowler, and F. Hoyle<sup>9</sup>. The first two authors were participants, GRB speaking in the first session, partly on historical issues concerning the lead-up to that paper, and EMB after the conference dinner. Salpeter also spoke, primarily about his own pre-1957 involvement in nuclear astrophysics. Fowler, Hoyle, and Cameron were no longer alive. Cameron had called his process “nucleogenesis”, and convergence on “nucleosynthesis” was gradual, with Fowler insisting on it in the 1970s on the grounds that “nucleogenesis” should be reserved for the process that produced protons and neutrons, which we now generally call baryogenesis.

Is my discussion complete? Of course not! The key 1957 papers are number 978 and 979 on a chronological list of relevant publications in nuclear astrophysics (Kuchowicz 1967). Admittedly the first is a Chinese report of the 1054 supernovae (now the Crab Nebula), but the second is the discussion by Kleiber (1885) of the chemical composition of celestial bodies. Nor is the Kuchowicz compilation complete, having missed, for instance, Blau (1940) on the sun and Iwanowska (1950, 1953) on low metallicity in RR Lyrae stars, though a couple of later Iwanowska papers (in Polish) are there. Brush (1996) has written a book-length history of nucleosynthesis, which I have not seen. It might be interesting to do a concordance of his pre-1957 references with the 953 in Kuchowicz and the much smaller number here. Brush is a serious historian of science, rather than an aging astronomer looking back in his or her own past and is certain to have picked out different items as most important.

Shortly after the July, 2007 meeting, Joe Tenn (newsletter editor for the Historical Astronomy Division of the American Astronomical Society) sent me a list due to Cameron of key events between 1928 and 1957. It has a few items I have not picked out above (e.g. Paul Merrill’s 1952 discovery of technetium in a few red giants, implying “current” nuclear reactions to stars, and papers from 1939 by J.R. Oppenheimer and his colleagues on neutron stars and gravitational collapse). And none of us emphasized that 1957 was also the year of the Vatican Conference on stellar populations (O’Connell 1958) from which the participants emerged with at least modest agreement about age-metallicity correlations in the Milky Way, opening the door to attempts at calculation of galactic evolution, meaning luminosity, star formation rates, and residual gas, as well as composition changes. Cameron began thinking about this in 1962, but concluded that the topic was not ripe for exploitation, at least in collaboration with his graduate students (W.D. Arnett, C.J. Hansen, and J.W. Truran), who wanted to finish their degrees in finite time (Cameron 1999). So it was, naturally, a graduate student (Tinsley 1968) who took the first major steps to show that temporal changes in galaxies were both calculable and important.

What about post-1957? Kuchowicz’s list continues to 1964, by which time it had reached 2101 publications on nuclear astrophysics. A decade later (Trimble 1975), it was still just about possible for one person to describe the entire topic of the origin and abundances of the chemical elements in 100 pages, without deeply offending experts, though that article was the outcome of a two-week NATO summer school at which very many experts had spoken. Another decade or two downstream (Trimble 1991, 1996) about all one could do was draw attention to iceberg tips of significant unsolved problems, on a background that had really changed rather little from the 1957 syntheses, apart from the firm adoption of big bang nucleosynthesis as part of the story, and the incorporation of cosmic ray spallation contributions to Li, Be and B. There are now, of course, whole conferences and review articles devoted to nucleosynthesis,

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<sup>9</sup> The decision was made not to publish proceedings from this meeting, but many of the presentations can, at least temporarily, be found at [www.na2007.caltech.edu/](http://www.na2007.caltech.edu/). The proceedings of a European event marking the same 50th anniversary have been published as C. Charbonnel & J.-P. Zahn (Eds.) 50 years after B<sup>2</sup>FH, 2009, ISBN 978-2-7598-0365-1.

galactic chemical evolution, or, more often just relatively narrow aspects of them. The titles “Nucleosynthesis in asymptotic giant branch stars”, “Abundance variations within globular clusters”, “The discovery and analysis of very metal poor stars in the Galaxy”, and “Neutron capture elements in the early galaxy” are examples from recent issues of *Annual Reviews of Astronomy and Astrophysics*, each running to 40 or so pages.

From the vantage point of “now”, it is clear that the topic of nucleosynthesis and chemical evolution is connected with almost the full range of modern astrophysics. The amounts of ordinary hydrogen, deuterium, helium, and lithium in unprocessed material are a test of our understanding of the big bang. The rate at which heavy elements accumulated and their presence in successive generations of stars will matter for the number of habitable planets expected in the Milky Way. In between, one needs to do something about the contributions from Population III stars (theoretical entities with only calculated yields so far) and reactions associated with the first generation of active galaxies, whose emission line gas is already at least as metal rich as our sun. Binaries kick out different mixes of products from single stars, and, at some level, magnetic fields, cosmic rays, stellar winds, and perhaps even the great redspot on Jupiter matter.

Does this portend a future of hopeless confusion? No, but it does, I think, imply a future in which, if nucleosynthesis is your primary interest and you were to read traditional journals, the fraction of papers about which you could say “Oh, I don’t need to know this; on to the next!” would shrink monotonically, leaving the “must know” territory to expand exponentially. But, of course, no one today lays claim to such large territories or attempts to read all the relevant papers. Similar trends are present in all sciences, and science is not going to come to an end as a result. But it does mean, I suppose, that the pleasure of rootling around among books and journals and coming across things that you hadn’t even realized you ought to know will be increasingly reserved for us historians.

One example of such a fortunate discovery by accident: In leafing through issues of *Physical Review* for a biographical memoir of Kenneth I. Greisen (the G of the GZK effect), I came upon a reference to Bowen and Wise (1938) on abundances in nebulae (especially the Ring in Lyra). They reported  $\text{He}/\text{H} = 0.1$ ,  $\text{Ne}/\text{H} = 0.001$ , and  $\text{Ar}/\text{H} = 10^{-4}$ , significantly better than anything that had gone before. Their CNO values are too high for the current interstellar medium, but I have not chased down the paper to see how much of this might be due to emphasizing planetary nebulae, which we now know to be enriched by their parent stars.

And, if you have read at least portions of this article, you are already some ways along to joining us, whatever your age. Enjoy!

*Acknowledgements.* I am grateful to S.E. Woosley and his organizing committee for the invitation to speak on nucleosynthesis before 1957 at the fiftieth anniversary conference at Caltech, to Christian Caron of Springer for suggesting I submit something for an early issue of their new journal, and to Alison Lara for keyboarding the result.

## Appendix A: Chemistry, spectroscopy, and the periodic table

These appendices have been added upon the advice of the referee, who pointed out that the paper was really only self-contained for a reader with more or less the same very miscellaneous data base as the author.

Picture a periodic table. The extreme right column consists of noble gases, whose atoms want very little to do with anybody. Thus they did not easily condense when

the solar system was forming and are greatly under-represented on earth. The closed electron shells mean that it is also quite difficult to get them out of the ground state, so that only ultraviolet photons or very high temperatures are likely to do the job, and they are, therefore, not easily seen in the spectra of cool stars like the sun.

Moving into the next few periods to the left, we find atoms with “holes” in their outer shells. They tend also to be difficult to excite, so that the spectral lines connecting to the ground state of C, N, and O also lie in the UV. They happily form compounds, some very tightly bound (including CO, the second most abundant molecule, after H<sub>2</sub> in the interstellar medium), but these are gaseous. Thus the ices of C, N, O, and H are said to be volatile, and they, too, did not condense at high enough temperatures to be fairly represented on earth, though they are abundant in the outer giant planets, which scooped up gases gravitationally.

In contrast on the far left of the periodic table live elements with one or two electrons outside a closed shell. These can be excited by photons with visible energies, so that lines of Na, Ca, Mg, and K (and to a lesser extent Li) are strong in the solar spectrum. They also form stable compounds with silicon and oxygen that are condensed into solids (along with Fe, Ni, and a few other common metals) at temperatures well above 1000 °C. These elements are called refractory, and are well represented in the terrestrial planets, which condensed close to the sun, where it was (and is) hot. At least some fraction of the volatiles we have were apparently brought in as comets and asteroids that originally condensed out beyond Mars and were kicked inward by the giant planets migrating through the proto-planetary disk from the locations where they originally formed. This gave rise to the phase in solar system evolution called the Late Heavy Bombardment, which may have been essential for formation of life on earth.

H and He are also, of course, difficult to excite, which is why Payne was able to conclude from their visibility in stellar spectra that they must be very abundant, and anyone who didn't want to accept this had to mumble “anomalous excitation” so that their  $n = 2$  levels would be populated and could absorb visible light.

## Appendix B: Stellar evolution and nuclear reactions, the view from 2010

It is arguably easier to follow the sections of the article that describe how we got here if you already know where we are going! A star is, by definition, a gaseous entity that transforms nuclear energy into radiation by (mostly at least) fusion reactions. And the life of a star is a competition between gravity trying to enforce contraction and pressure fighting back – thermal pressure supported by the nuclear reactions until either gravity wins (black holes) or degenerate pressure takes over (white dwarfs for electron degeneracy; neutron stars for neutron degeneracy). The end point and nearly everything that happens along the way depends primarily on the mass of the star (with small corrections for composition, rotation, and magnetic fields, at least, and larger corrections for the presence of a close companion). Burning is habitually used as a synonym for fusion, whether of hydrogen or some heavier element. Because the energy supply available from each reaction is proportional to the mass of the star, but the usage rate (luminosity) scales as  $M^3$  or steeper, massive stars live millions of years, ones like the sun billions of years, and the dinkiest longer than the present age of the universe.

Deuterium ignites first, but because there is so little of it, deuterium burning doesn't even really slow down the collapse of a gas cloud toward stellar conditions. This comes from regular hydrogen, which can fuse either by direct interactions of protons (at relatively low temperature) or using C, N, and O nuclei as catalysts (at

somewhat higher temperature, perhaps  $2 \times 10^7$  K). Stars living on hydrogen fusion in their cores are called main sequence, because they occupy a fairly tight and densely populated locus on a plot of stellar brightness (“magnitude”) vs. surface temperature (color or spectral type), variously called a Hertzsprung-Russell or color magnitude diagram. The sun, at an age of about 4.55 Gyr, is about half way through its main sequence life. Exhaustion of hydrogen fuel at the core sets the core to contracting (gravity in the ascendant for the moment) and the outer layers into expansion (no, I’m not going to try to tell you why; it won’t be on the exam). Hydrogen fusion continues in a thin shell around the inert, exhausted core, and the star appears in the sky as a red giant, for perhaps 10% of its main sequence lifetime (and everything else goes more quickly).

All masses in excess of about 0.5 solar masses contract so that the central temperature reaches  $10^8$  K before the gas becomes degenerate, and helium fusion sets in. Because  $\text{Be}^8$  is unbound by only 92 keV, two helium nuclei can remain close to each other for long enough (about  $10^{-16}$  seconds) for a third one to approach within barrier penetration distance, and the three fuse to yield a  $\text{C}^{12}$ .  $\text{C}^{12}$  can capture a fourth helium at only slightly higher temperature, and the ratio of C/O that results is therefore a sensitive function of stellar mass (bigger = hotter, remember). At the temperature required by Opik (1951), no carbon would have been produced in the final mix.

The carbon + oxygen cores of stars of  $0.5 - 8 M_{\odot}$  (somewhat dependent on initial composition) become degenerate before getting hot enough for further fusion reactions and end their lives as white dwarfs as our sun will in another 5 Gyr or so). More massive (and, remember, very short-lived) stars, reach carbon ignition temperatures and beyond. From here on, the reaction networks become quite complex, and there is no one dominant product (in the way that hydrogen fusion makes helium, and helium fusion yields carbon and oxygen). The details depend not just on coulomb barriers but also on availability of appropriate spin and parity states of the product nuclei. Thus an important product of carbon burning is  $\text{Ne}^{20}$  (but not  $\text{Mg}^{24}$ );  $\text{Ne}^{23}$  is another (though it beta-decays to  $\text{Na}^{23}$  before long). Neon burning yields more oxygen and some  $\text{Mg}^{24}$ . Oxygen burning makes quite a lot of  $\text{Si}^{28}$  (but less  $\text{S}^{32}$ ). Silicon burning sets in at a central temperature near  $3 \times 10^9$  K, by which time energetic photons are knocking alpha particles (helium nuclei) off some of the  $\text{Si}^{28}$ ’s, which then get captured by other  $\text{Si}^{28}$ ’s, building up to  $\text{Ni}^{56}$  (which beta decays to  $\text{Fe}^{56}$ ). Because there are also stray neutrons and protons around, a full range of nuclei from  $\text{C}^{12}$  to  $\text{Fe}^{56}$  is eventually assembled in various cores of the stars. For lots more detail, please see D.J. Hansen, S.D. Kawaler, and V. Trimble, 2004, *Stellar Interiors*, second edition, from Springer, which is, not surprisingly, my favorite text of stellar structure and evolution.

Elements beyond the iron peak (gallium to at least plutonium and perhaps beyond) are built by addition (and sometimes subtraction) of neutrons during the supernova explosions that end the lives of very massive stars (r-process, p-process) and during the phase when intermediate mass stars are burning both hydrogen and helium in thin shells around an inert core of carbon and oxygen (s-process). In the end, only lithium, beryllium, and boron get left out, and they come (mostly) from spallation when cosmic ray protons hit CNO nuclei in the interstellar medium.

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