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Cosmic Abundances: Past, Present, and Future

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Abstract. To achieve a full grasp of cosmic abundances – that is, what the universe is made of and how it got to be that way – we need considerable knowledge in each of six areas. These are (a) a complete inventory of elements and isotopes, (b) the nuclear properties of each, (c) the observed abundances of stable, decaying, and extinct nuclides as a function of time and place, (d) reaction chains and networks, (e) sites for each, and (f) galactic chemical evolution. Each of these topics is traced through part of its history to our current understanding and on to some possibilities for the future.

1. Introduction

The organizers had originally planned two introductory talks for the workshop, one providing an historical perspective (to be given by me) and one addressing the current situation and future prospects (which it was hoped *A.G.W. Cameron would give). When Al decided not to participate, the two talks were glued together, thereby saving 15 minutes or so extra for poster viewing and coffee, but also mixing historical and current ideas in the sections that follow. My credentials for covering the topics of origins and abundances of the elements consist largely of having reviewed the subject before (Trimble 1975, 1991). The first of these publications is now probably old enough to count as part of the history of the subject.

The sections that follow address what I see as the main subject areas we need to understand. Section 2 covers the problem of achieving a complete inventory of elements and isotopes (generically, nuclides), both stable ones and unstable ones along reaction paths to stable nuclei, and the internal structure of atoms. A long standing problem in this area was figuring out whether any nuclide with 5 or 8 particles was stable. Section 3 is concerned with the properties of the elements and isotopes, their masses, spin and parity values, cross sections and lifetimes against decays, captures, reactions, spallation, and so forth. An ancient problem in this area is the correct low-energy cross section for $^{12}C(\alpha, \gamma)^{16}O$, which determines the ratio of carbon to oxygen that comes out of helium burning and, therefore, the subsequent course of nucleosynthesis. Section 4 summarizes some of what we know about the abundances of the nuclides in the solar system and

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in other places and times and how the anomalies (meaning differences from the solar system average) are correlated with each other and with stellar population types. A long-standing problem was forcing solar and meteoritic abundance to agree for elements, especially iron, that ought not to be enhanced or missing either place. Section 5 includes two closely related topics. First is the identification of the reaction chains and networks that are primarily responsible for the production of each of the nuclides. *Hans Bethe, *Edwin Salpeter, and *Fred Hoyle will appear in the following pages for important contributions in this area. Second is the problem of finding suitable sites for each of the reactions. This requires understanding stellar structure and evolution (including mass loss), the physics of the early universe, cosmic ray spallation, and probably other astrophysical entities. Just where most of the r-process (rapid capture of neutrons by iron peak seeds) occurs is an old question in this territory.

Finally, in Section 6, we look at efforts to calculate galactic chemical evolution. The goal here is to guess or deduce the initial conditions in a typical galaxy, transform gas into stars at some variable rate and with some variable mix of stellar masses (the initial mass function), deal with inflow and outflow of gas from the region being evolved, all as a function of gas composition and other parameters, and to make the end product come out looking like the Milky Way or some other galaxy. This chemical evolution must also be coupled with dynamical evolution of galaxies and with whatever pre-galactic nucleosynthesis occurred. Because we have no real theory of many of the processes, especially star formation, all such models of chemical evolution suffer from the Curse of the Adjustable Parameter.

2.   The Inventory of Nuclides and Atomic Structure

The science textbooks of an earlier generation invariably began by telling you what the ancient Greeks had thought about the subject. One might, therefore, imagine a periodic table compiled by Aristotle (-384 to -322, people lived backwards in those days) as looking something like Figure 1.

In fairness, though, one should also mention the atomists, Leucippus, Democritus (fl. -430), and Epicurus (author of the Elements of Dining?), who believed that complex entities were the result of many very small, identical atoms interacting. Their views were put into poetic form by Lucretius (-96 to -55), an extract from whose De Rerum Natura still hangs in the seminar room of what was once Fred Hoyle's Institute of Theoretical Astronomy. The ancients recognized copper, carbon, gold, iron, lead, mercury, silver, sulphur, and tin as interesting, distinguishable substances, though not as elements in the modern sense.

During the middle ages, arbitrary transmutability of substances was regarded as reasonable and possible. The search for the philosopher's stone or an Al-iksir (elixir, from the Arabic) that would facilitate the processes was pursued by Roger Bacon, Albertus Magnus, and Paracelsus, among those whose names have come down to us. Additional substances that came to be regarded as discrete and interesting included arsenic (Magnus, about 1250), zinc, (in India, about 1250), antimony (before 1600), phosphorus (1669), cobalt and platinum
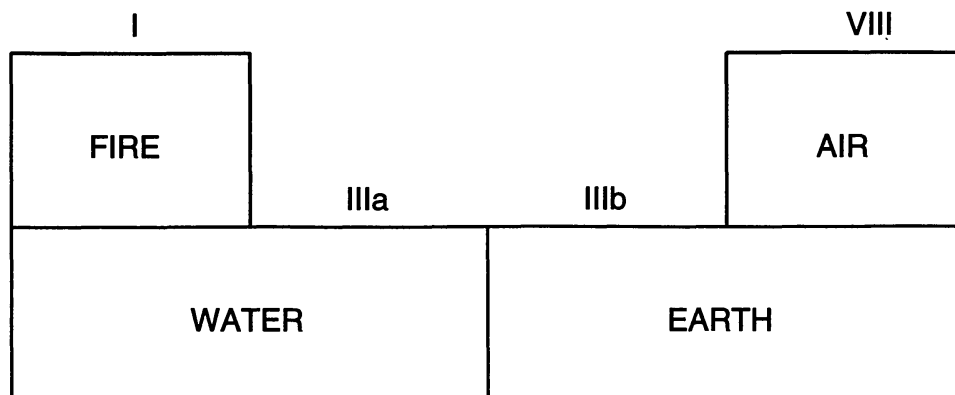
Figure 1.   Early Periodic Table

(1735), nickel (1751), bismuth as distinct from tin and lead (1753), and magnesium (1755).

Then came phlogiston and an era of confusion ushered in by Becker and Stahl in Germany in the 17th and 18th centuries. The idea was that metals were compound substances, when heated, released phlogiston, leaving behind a calx or ash. The phlogiston and ash were then the pure substances or elements, as first defined by Robert Boyle (1627-1691) to mean something that could not be decomposed into anything simpler. The year he said this, 1669, was the era of Charles II in England, Louis XIV in France, and between Shakespeare and Bach in culture. Pierre Gassendi (1592-1655) revived the idea of atomism in the same time frame.

"The rise of modern chemistry" begins in 1774 with Joseph Priestly's recognition of "respirable air" as a discrete substance. Antoine Lavoisier (1743-1794) gave it the name oxygen, and Cavendish demonstrated in 1783 that water was composed of "inflammable air" and oxygen. Lavoisier is the real hero here. His *Elements of Chemistry* (published in French in 1789 and in English in 1790) established the notion of "elements" as Boyle had defined them, with the inventory therefore subject to change. The title of his book was presumably a live pun at the time, at least in English.

The next vital step was taken by Dmitri Ivanovich Mendeleev (1834-1907; he died the year that *Hans Bethe and *Dorrit Hoffleit were born, so that we still can just barely make contact with his epoch). He was not quite the first to attempt to put the elements in some sensible order or pattern, but his 1869 grouping by chemical properties, perpendicular to increasing atomic weight, was essentially our modern periodic table. He looked at the gaps in 1871 and predicted the existence of ekaboron (scandium, found in 1875), ekasilicon (germanium, found in 1876), and eka-aluminum (gallium, found in 1875).

Fifteen naturally-occurring elements were added to the table in the next fifty years: Ho (1878 for a salt; the pure metal not until 1911), Sm and Tm (1879), Gd (1880), Nd and Pr (distinguished in 1885), Dy (1886, but the pure metal only in

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1950), Ar (1894), Kr and Xe (1898), these three particularly important because they added a whole new column to the periodic table, suggesting briefly that it might expand forever, Ac (1899), Eu (1901), Er (1905), Lu (distinguished from Yb in 1907), Hf (separated from Zr in 1922 and the first to be identified from its X-ray spectrum), and Re (distinguished from Pt, the last, in 1925). A plot of the number of known elements as a function of time looks like slightly ragged stairs (Masterton & Slowinski 1973), with sharp rises when new theoretical or experimental tools (like Humphrey Davy's photovoltaic cell) entered the arena, and plateaus in between.

False alarms of identification of elements 43 (Masurium) and 61 (Illinium) preceeded their actual creation as artificial, radioactive elements by Emilio Segre and his colleagues in 1937 and 1941-45, respectively. And so, after WWII, onward to Seaborgium, if the powers that be allow it to keep that name while Glenn is still alive. I have some personal interest in the issue, since *Seaborg and my father, *Lyne Starling Trimble, were 2/3's of the graduating class in chemistry from UCLA in 1935. Seaborg is now thought of primarily as a physicist. My father remained a chemist, but always said that the physicists had stolen an enormous mount of territory, and that the whole topic of the internal structure of atoms and their constituent parts should have been part of chemistry, leaving the physicists with classical mechanics, electromagnetism, and so forth.

Curiously, E.O. Lawrence (1935) raised the question of chemistry vs. physics in the same time frame. His last sentence in the proceedings of a 1935 workshop (sponsored by Sigma Xi of Michigan) was, "Shall we call it nuclear physics, or shall we call it nuclear chemistry?"

Our modern notion of atoms goes back only to John Dalton (1761-1844), who, in 1803 (the year of the Louisiana Purchase), suggested, that one could define an element as a set of identical atoms, and compounds would then consist of the sum of a few atoms and have definite atomic weights. Joseph Proust (1754-1826) provided the closely-related idea that elements would combine in fixed proportions by weight to make compounds. The idea of fixed (often equal) volumes for gases combining belongs, of course, to Amadeo Avogadro, who was born the year of the American revolution and put forward his best-known idea in 1811.

A suggestion that an oxygen atom might be rather like sixteen hydrogens glued closely together came from William Prout (1816) and strikes me as exceedingly important and deserving of having made his name better known. We reach the threshold of atoms ceasing to be "not dividable" in 1887 when Svant Arrhenius showed that a Faraday of electricity could deposit at most one gram-atomic-weight of a substance, and so must contain an Avogadro's number of unit charges. G.J. Stoney (1826-1911) had already provided the name "electron" for this unit charge in the year Garfield was assassinated (1881).

These unit charges became real particles in 1897, when J.J. Thomson (1856-1940) showed that cathode rays could be bent by both electric and magnetic fields. His image of atomic structure has been dubbed the plum pudding model, with electrons, like raisins, studded through a diffuse blob of positive charge. Ernest Rutherford (1871-1937) entered the picture in 1909 with the demonstration that alpha rays were the same as helium nuclei. He placed the positive charge of atoms in a dense central knot through his experiments on scattering of

positively charged particles by thin gold foils in 1911. Two years later, Philipp Lenard (1862-1947) wanted to put electrons and pluses in compact pairs with empty spaces in between, a picture that even then must have been somewhat at variance with the implications of Rutherford scattering.

Rutherford and Frederick Soddy (1877-1956) recognized the existence of isotopes and coined the name in 1913. For Soddy, at least, chemical identity was an essential part of the definition. And we bid farewell to Rutherford in 1920, when he proposed the name proton, as part of a model of the nucleus in which oxygen, for instance, would have 16 positive charges and 8 negative charge units (electrons) in its nucleus, with 8 more negative charges at a distance.

Meanwhile, in 1913, Henry Moseley (born the same year as my maternal grandmother, 1887, and idiotically sent to die at Gallipoli) had provided an absolutely vital idea. He said that atomic number was simply the number of discrete positive charge units in a nucleus. This required that the periodic table must be finite, not at all clear when the whole new column of noble gases had just been added and the rare earths were continuing to proliferate.

The years 1931-33 were enormously fruitful for atomic and molecular physics. *Harold Urey (1893-1981) separated deuterium from normal hydrogen in 1931. It is arguably the most important isotope from a nucleosynthetic point of view (because it is the essential bridge from protons to nuclei with neutrons), though its chemical distinctiveness caused Soddy to deny that it was an isotope to his dying day. The same year, John Cockcroft (b. 1897) and Ernest Walton (1903-1995) produced the first laboratory nuclear reaction triggered by artificially accelerated particles. They bombarded ^{7}Li with protons and found themselves with a bunch of alpha particles (helium nuclei). The first Van de Graaff accelerator, the first cyclotron (built by Lawrence), and the first linear accelerator also belong to 1931.

James Chadwick (b. 1891) was, meanwhile, carrying on a slightly older form of nuclear alchemy, using alpha particles from naturally radioactive substances as his projectiles. When, in 1932, he turned his beam on ^{9}Be , something came out that was both non-ionizing (uncharged) and capable of knocking protons out of paraffin (massive). Called the neutron, the new particle provided the key to a correct understanding of nuclei as compact assemblages of protons and neutrons (Werner Heisenberg 1933).

The portion of the cosmic abundances program described in this section is complete. We know about all possible stable nuclides and their (Z, A) values and the unstable ones near enough to them to get involved in nucleosynthesis. One of the last outstanding issues in this field was the possible existence of an island of relative stability for elements around $Z = 112$, the next magic number or closed shell for protons after $Pb(Z = 82)$. The evidence was meteoritic xenon that seemed, on the basis of the preponderance of the heaviest isotopes, to have come from fission of such a superheavy nuclide. It was a false alarm (Anders and Zinner 1991). There was an even briefer false alarm in connection with giant halos in mica, which the discoverer attributed to decay of superheavy radioactive nuclides in situ. I won a bottle of red wine from Al Cameron by betting instantly against the discovery on first hearing (based mostly on the location of the discoverer at a fundamentalist college; and indeed he was primarily interested in establishing that mesozoic rocks were very young).

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Most of the historical material here has been taken from CRC (1949, 1987), McKie (1951), Asimov (1966), and a long-out-of-print history of chemistry by J.H. Moore, the 1922 edition of which lived on the bookshelves at home for many years.

3. Nuclear Properties and Cross Sections

Fredrick Aston (1920, 1927) pioneered the use of the mass spectrograph to measure the first atomic/nuclear masses accurate enough to reveal, for instances, the four hydrogens add up to more than one helium, and that many elements in their common forms do not have integral atomic weight (later largely explained as the effect of mixtures of isotopes). Very shortly before, Rutherford had triggered the first man-made nuclear reaction by firing alpha particles from a natural source of radioactivity at ^{14}N. Hydrogen was liberated and oxygen remained in a reaction, ^{14}N$(\alpha, p)^{17}$O confirmed by P.M.S. Blackett in 1925.

For most of us, this whole topic is associated inextricably with the names of *William A. Fowler and his associates Charles and *Tommie Lauritsen at Kellogg Lab (California Institute of Technology). Fowler (1984, 1992) has told the story himself, and it is far above our poor power to add or subtract, except that he does not seem to have included in either place the description of his very first cross section measurement. Attempting to learn something about energy dependence by repeating the same experiment "with copper shield and without copper shield", he was told by the elder Lauritsen that, if you aren't using a shield, it doesn't matter what it is made of.

The bible of reaction rates of astrophysical importance has been through almost as many editions as there are politically correct modern translations of the King James testaments (Fowler et al. 1967, 1975; Harris et al. 1983; Caughlan & Fowler 1988). Since the last of these, several groups have been attempting to maintain up-to-date listings of the most important rates in electronic form. These include F.K. Thielemann and M. Wiescher in Basle, S.E. Woosley and R. Hoffman at University of California, Santa Cruz, and C. Rolfs and M. Arnoud as part of a collaboration funded by the European Union (Thielemann 1995).

Through the various editions, rates of some of the critical reaction, including ^{15}N$(p, \gamma)^{16}$O have changed by factors of two or three, and others, like the triple alpha and $p + p$ by 20-30%. We still, of course, await an actual measurement of this last cross section at stellar energies, the rates in use being calculated from the lifetime of the neutron.

Three topics of historical interest in this area are the stability of $A = 5$ and 8, the existence of an essential excited state in ^{12}C that makes helium burning go smoothly, and the correct low-energy cross section for ^{12}C$(\alpha, \gamma)^{16}$O. None of the stories can be told in an entirely linear, straightforward way.

The very short lifetime of ^{8}Be was clear in a series of pre-war German experiments (Kirchner et al. 1937; Fink 1939), with the analysis corrected by *John A. Wheeler (1941) to reveal the first excited state (of some importance in helium burning). But a seemingly much more accurate experiment (Allison et al. 1939) found ^{8}Be to be bound relative to two alpha particles by about 0.3 MeV. Everyone was then very busy with war work for the next six years, and the unboundedness of ^{8}Be had to be re-established both theoretically (Fermi &

Terkevich 1949) and experimentally (Hemmendinger 1948, and work at Kellogg by *Tollestrup, Lauritsen, and Fowler) before everyone was fully persuaded.

The excited state of ^{12}C that is essential for helium burning to work at stellar temperatures has a similarly spotted history. It is *There* in the ^{14}N$(d, \alpha)^{12}$C results of Holloway & Moore (1939) at 7.62 MeV above ground (as the second excited state and with the right spin and parity to give a large cross section for ^{8}Be + ^{4}He), and equally *Not There* in seemingly better data for the same reaction by Malm & Buechner (1951). It had correspondingly moved into and out of standard tables before Hoyle came to Kellogg in 1953. Hoyle's theoretical conclusion that the state must nevertheless exist or we wouldn't be here to argue about it was, therefore, drawn not exactly in the absence of data, but in the face of the data. That he persuaded *Ward Whaling to put together a group (of which Fowler was not a member) to repeat the experiment yet again and find the critical level (Dunbar et al. 1953) is in some ways, therefore, the more impressive. Many of the references in this paragraph and the previous one have been traced from privately supplied manuscripts by Brown (1984), for which I am most grateful.

^{12}C$(\alpha, \gamma)^{16}$O is sneaky enough, even without human frailty, to cast doubts on Einstein's good opinion of God. It goes through a resonance that is 7.15 MeV above the ground state of ^{16}O – but slightly *below* the threshold for ^{12}C and ^{4}He approaching each other at zero relative velocity. Properties of the level and the resulting reaction rate must, therefore, be determined by examining related nuclides and reactions. B^{2}FH suspected that one critical component was not known better than to a factor 100. This had narrowed to only(?) a factor 40 by the time of the conference reported in Trimble (1975). If the rate is low, helium burning makes lots of carbon. If the rate is high, most of the carbon burns through to oxygen. Buchmann et al. (1993) and Zhao et al. (1993) recently matched nature in sneakiness by looking at the alpha decay of ^{16}N. They found a rate at the upper end of the previously allowed range for one of the two branches leading to ^{16}O consistent with the implications of the large amount of oxygen $(1–3M_\odot)$ expelled by supernova 1987A.

4. Abundances of the Elements and Isotopes

It is customary to look first at the solar system (on the grounds that "normal" means "a lot like me") and then at the differences to be found in astronomical objects of other ages and at other locations.

4.1. Abundances Here and Now

A pedant might insist (in fact a pedant at the workshop did insist) that this should say "here and 4.5 Gyr ago", but you know what I mean. *Kuchowitz (1967) has provided a very complete annotated bibliography of the history of nucleosynthesis and can be consulted for many details missing here.

The first recorded cut at "how much of what" was an examination of the earth and meteorites by Kleiber (1885). He recognized the existence of the iron peak and remarked that light elements (meaning oxygen and silicon) were generally commoner than heavy ones (meaning silver and gold). Soon after, Clarke (1889) concluded that there were no periodicities in abundance to be
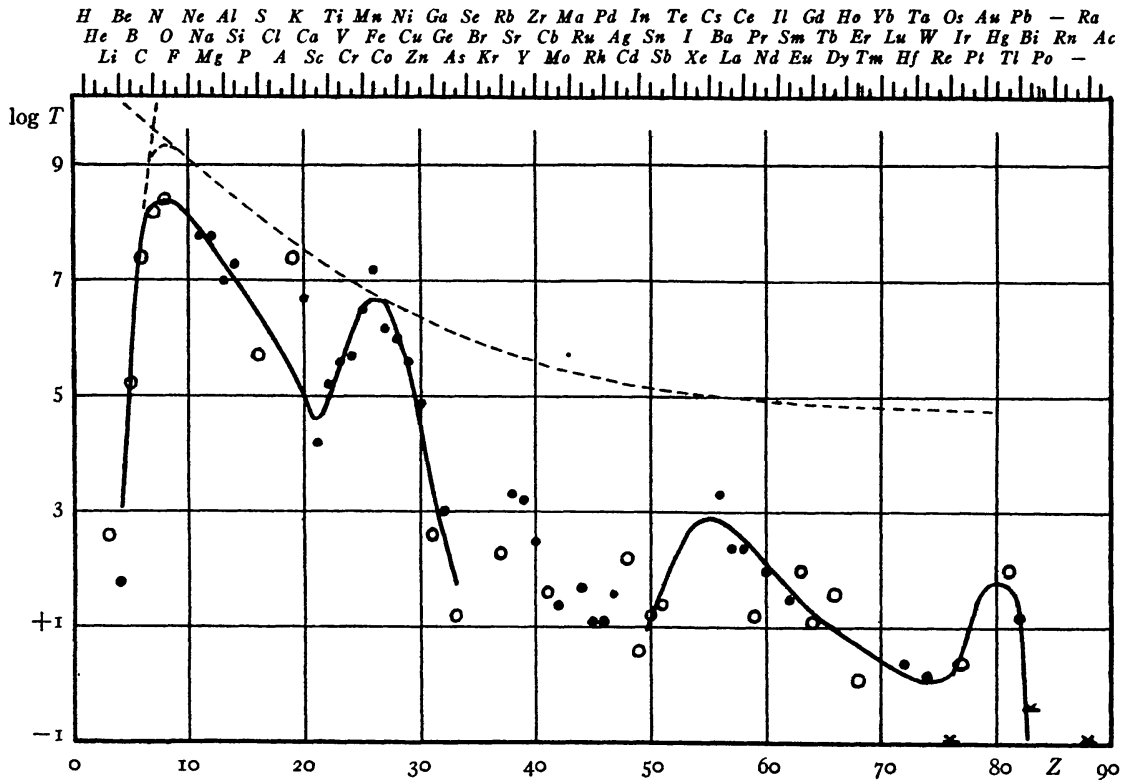
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Figure 2.   Abundances of the elements as they were known to Atkinson (1931), largely from Russell's (1929) analysis of the sun, but with odd Z artificially enhanced by $\log N = 0.6$, thereby obscuring an important nucleosynthetic fact (the odd-even effect). CNO and Fe peaks are present, and some structure associated with the $N = 82$ and 126 closed neutron shells (though the neutron was not yet known).

seen when looking along the rows and columns of the periodic table. Oddo (1914) and Harkins (1917) recognized that you must look for correlations with nuclear properties rather than with chemical ones. They concluded that both even Z and even A were commoner than nearby odd ones.

The standard work on solar abundances was (and for a few elements still is) Russell (1929). His numbers were the ones available to *R. d'E. Atkinson (1931) when he first attempted to account for the solar system composition with nuclear reactions distantly related to the CNO cycle. A sharp-eyed observer of his plot (Fig. 2) can see the iron peak, peaks at neutron number 82 and 126 (but not 50), and the illusory elements Ma and Il. Hydrogen and helium do not appear at all.

The best available numbers for elemental abundances evolved gradually from Goldschmidt (1937) through *Brown (1949), Suess & *Urey (1956), and Cameron (1968, 1973) to *Anders & *Grevasse (1989). Small modifications continue to appear, but, on a log scale, the most recent plots are indistinguishable from Fig. 3 (taken from Trimble 1975). In fact, apart from beryllium, which has wandered from $\log N = 1$ to $\log N = -1$ (on the scale where $\log N(\mathrm{H}) = 12$), none of the elements Goldschmidt was brave enough to evaluate has changed by more than a factor 3-4, at least in the meteorite data.
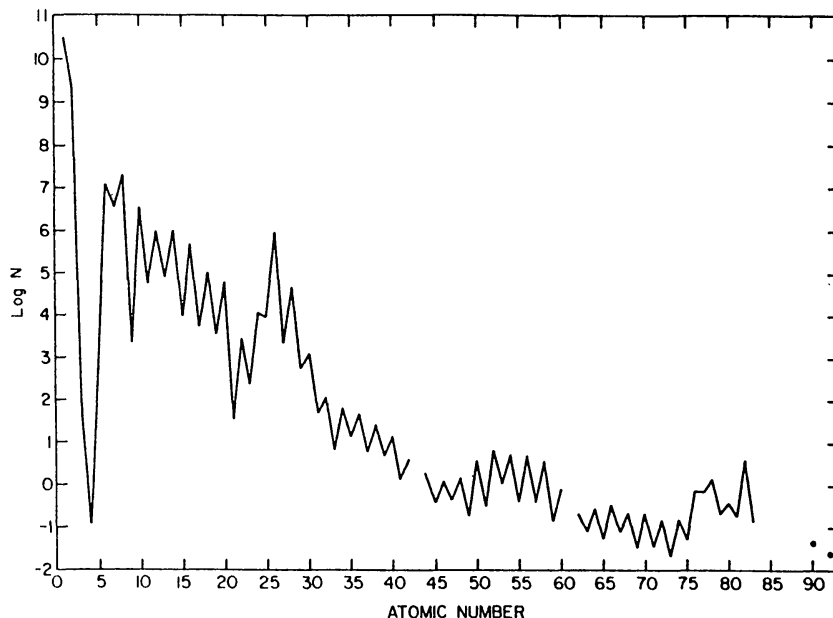
Figure 3.   Abundances of the elements from Trimble (1975), normalized to $\log N(\text{Si}) = 6$. Of the elements tabulated by Goldschmidt (1937), none except Be changed by more than a factor of 3-4. Changes in the last 20 years have been even smaller.

A long-standing problem was how to merge the meteoritic scale (based on silicon $= 10^6$) and the solar one (based on hydrogen $= 10^{12}$). Iron was particularly troublesome, with the solar photospheric abundance derived by Russell and his successors more than an order of magnitude below the meteoritic (and solar coronal) value. The meteoriticists were right, the photospheric determinations having been bedeviled by inaccurate transition probabilities.

Conspicuous features in any modern compilation of elemental abundances include the enormous preponderance of H and He (*Payne 1925; Russell 1929), the scarcity of Li, Be, and B (recognized by Russell and Goldschmidt), the peaks at CNO and Fe, the odd-even effect (which Atkinson, 1931, obscured by arbitrarily enhancing odd Z abundances by $\log z = 0.6$), and moderate enhancements around $Z = 55$ and $76 - 82$.

Greater insight comes from examining abundances of individual nuclides. Some decisions are required. Do you add up all the nuclides at a given value of A, for instance ^{180}Hf, ^{180}Ta, and ^{180}W (the latter two of which are quite rare) and of ^{186}Re and ^{186}Os (with comparable abundances)? Or, if not, how do you separate them? Brown (1949) tried showing only odd-A nuclides (for which duplication is less common), because he thought the plot looked smoother. This turned out to be a poor choice; much informative structure has been obscured, and great peaks appear at, e.g., $A = 25$ and 57, without telling us much about Mg or Fe. *Suess and *Urey (1956) tried all sorts of combinations of adding, separating, and being guided by theoretical considerations.

The Suess and Urey (1956) compilation reveals a number of interesting features. First, the dominant magic numbers (closed nucleon shells) are determined

by different physics for inner and outer shells. Thus, of the two sequences, {2, 8, 20, 40, 112} and {2, 6, 14, 28, 50, 82, 126}, we see high abundances associated with $Z = 2$, 8, and 20 (He, O, Ca) and with 28 (Ni), 50 (Sn, the element with the largest number of stable isotopes), 82 (Pb), and for neutron shells also $N = 126$. Second, they pointed out that, among light elements, the isotopes with the lowest stable n/p ratios were commonest, while high n/p ratios predominate among heavier elements. Third, they recognized that some nuclides might not be synthesized as themselves, the natural pathway to ^{56}Fe from ^{28}Si, for instance, going through the unstable ^{56}Ni. This last point is implicit in Hoyle's (1946) discussion of nuclear statistical equilibrium and subsequent beta decays.

The first thoroughly modern-looking plot of abundances of the nuclides is that of Suess and Urey data "as told to" Burbidge et al. (1957; B^{2}FH throughout the folklore), reproduced as Figure 4. Guided by confidence (well placed) that they had identified the correct production mechanisms for virtually all stable nuclides, they smoothed over observed values to reveal the physically important features of $N(A)$, including the excesses of nuclides that act like the sums of alpha particles, the double peaks at the closed neutron shells $N = 50$, 82, and 126, and the extreme sparsity of nuclides with the lowest stable n/p ratios among the heavier elements.

Extinct or fossil radioactivities are isotopic (and sometimes chemical) anomalies in meteorite grains whose chemical context says that some unstable nuclide must have been incorporated in a solid before it had time to decay. A classic example is an excess of ^{26}Mg in phases where Mg is rare but Al is common, meaning that the stuff was still ^{26}Al when it condensed. Depending on whether solidification occurred near the nucleosynthetic event (supernova or whatever) or in the early solar system, we can learn either about particular synthesis sites and reactions or about the history of solar system formation, including the possibility of a supernova or other trigger for the event.

Another interesting example is ^{22}Ne (called Ne-E by meteoriticists) which was once ^{22}Na. Since the half life is all of 2.6 yr, this must be a relic of the synthesis site in the form of pre-solar grains that survived, unvaporized, to be incorporated in the host meteorites. ^{26}Al has a half life of 0.72 Myr, and one can, therefore, imagine its being either a pre-solar grain component or still alive at the formation of the meteorites. Expert opinion favors the latter because of the potential of ^{26}Al for heating the meteorite parent bodies to permit their chemical fractionation early in the history of the solar system. Because decaying ^{26}Al leaves the ^{26}Mg in an excited state that de-excites radiatively, you can now also see ^{26}Al live at a gamma-ray observatory near you (Diehl et al. 1995).

Grains that are clearly pre-solar (with elemental as well as isotopic anomalies) have come from both carbon rich material (e.g. graphite grains) and oxygen-rich material (e.g. Al$_2$O$_3$) representing either different supernovae or different zones. Among the fossil radioactive nuclides that must have been alive at solar system formation are ^{129}I and ^{244}Pu, which decay and fission respectively to xenon with half lives near 10^8 yr. They are sporadically regarded as evidence for supernova-triggering of the formation of the solar system. For more about this fascinating but rather specialized topic, see *Wasserburg (1987) and Trimble (1994). Still longer-lived radionuclides, especially ^{232}Th, ^{235}U, ^{238}U and the isotopes of rhenium and osmium are the chronometers that tell us both the length of time since the solar system (meteorites, moon rocks, earth rocks) solidified

and something about the time since the galaxy began making heavy elements.
The topic is called nucleocosmochronology and has been excellently reviewed by
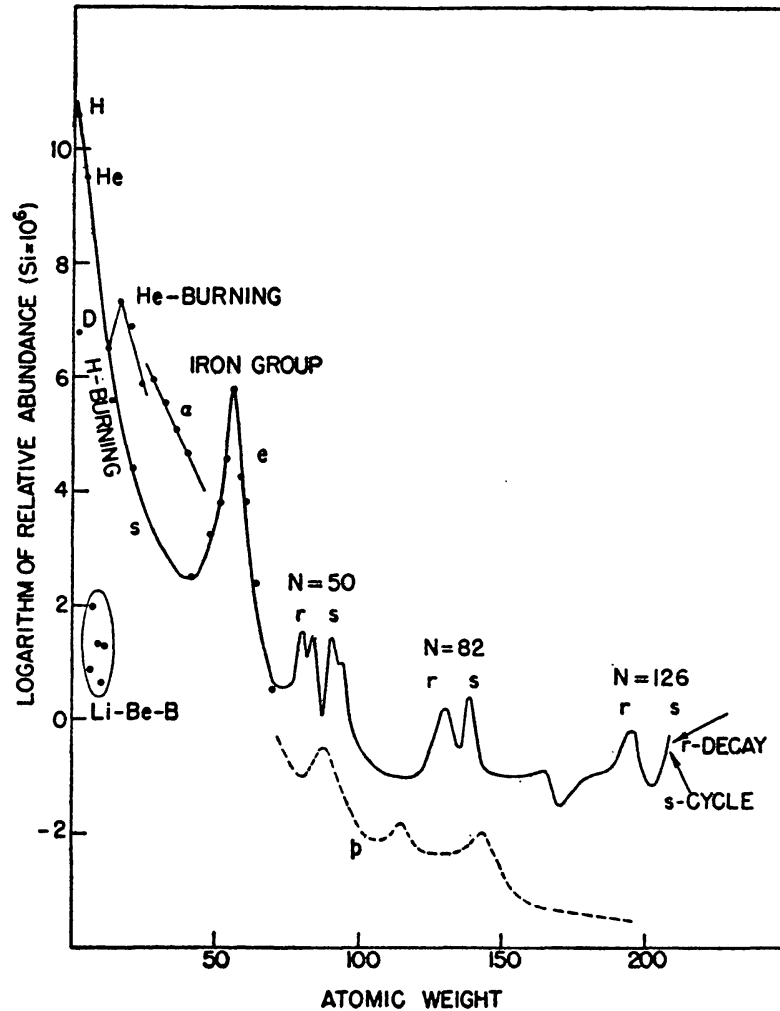Cowan et al. (1991a).

Figure 4.   The abundances of the stable nuclides from Suess & Urey
(1956) "as told to" Burbidge, Burbidge, Fowler, & Hoyle (1957). Mak-
ing use of their conclusions about which nuclear processes have pro-
duced which nuclides, they were able to produce a memorable plot
that still looks right. Normalization is again to $\log N(\text{Si}) = 6$. Purely
solar or stellar data are normally shown with $\log N(\text{H}) = 12$, which
moves everything up by about 1.5 dex.

4.2.   Abundances There and Then

Let's look first at the Milky Way. In a general sort of way, most stars have about
the same chemical composition, dominated by hydrogen and helium (Payne 1925
and others over the years). Not very many people solve a fundamental scientific
problem as part of a doctoral dissertation (though you will meet another one
in the galactic evolution section). *Cecilia Payne did, applying the then-new
Saha equation to the spectra of coolish giant stars. It took the astronomical

community a few years to absorb the dominance of hydrogen and helium, but the idea of uniformity quickly became so deeply imbedded that even her own efforts to modify it (below) were largely doomed to failure!

The most significant deviations from this uniformity are spatial and temporal gradients, with the bulge more metal rich than the disk (which also has a radial gradient, and perhaps a vertical one) and with the oldest, halo stars distinctly deficient in heavy element's. The overall pattern of $Z(R, z, t)$ is, however, hazed around with very large scatter that is not very well understood.

Some detailed differences in abundance patterns can cast light on the events leading up to the present. For instance, the ratio $^{13}C/^{12}C$ is about twice the solar value (1/90) in the present interstellar medium, at least in the inner galaxy. This suggests continuing operation of the CNO cycle. The ratio O/Fe is high (though both elements are deficient) in globular clusters, indicating a change in the dominant site of heavy element production in the galaxy from type II supernovae alone over the first 10^{6-7} yr to a significant input from SNe Ia after 10^{8-9} yr.

Some anomalies correlate in interesting ways. For instance, the trend of [O/Fe] decreasing as [Fe/H] rises persists to higher overall metallicity among bulge stars (and probably elliptical galaxies) than in the disk. This implies more rapid enrichment in the bulge. And the regressions of [Li, Be, B/H] against [C, N, O, or Fe/H] provide primordial values of the light nuclides to be used in calculations of big bang nucleosynthesis. None of these correlations is established with absolutely enormous firmness.

How did we come to even this limited level of knowledge? One always begins the history of any astronomical topic by asking what *Jan Oort thought about it. Oort (1926) established the existence of high velocity stars. He declared that they (a) had the same correlation of absolute magnitude, M_v, with spectral type as low velocity stars, (b) had no spectral peculiarities (admittedly there were no hydrogen lines in his data base), and (c) included no binaries. All three are wrong. He included globular clusters among the high velocity stars, which is, of course, correct.

Adams & *Joy (1922) had actually already found the first three examples of what they called "intermediate white dwarfs," that is stars falling on the HR diagram between the main sequence and the white dwarfs (a deviant correlation of M_v with spectral type!). They placed the stars too early because they used metal lines as their temperature,indicator. Adams et al. (1935) expanded the sample to six stars, which they described as displaying spectra with narrow, sharp H lines, faint metal lines (yes), and resemblances to the spectra of Sirius B and o Eri B (no!). From the proper motions and parallaxes they tabulate, I calculated an average transverse velocity of 234 km/sec for the six stars. They did not, perhaps because the average is so very "high velocity" that they distrusted it (this is a guess).

The first hints of chemical peculiarity are contemporaneous. *Lindblad (1922) noted that the giants of M13 had remarkable weak CN lines. This was confirmed by *Popper (1947) for the K giants of M13 and M3. *Morgan, *Keenan, & Kellman (1943) mentioned the weakness of CN features of high velocity stars in their classic atlas of stellar spectral types.
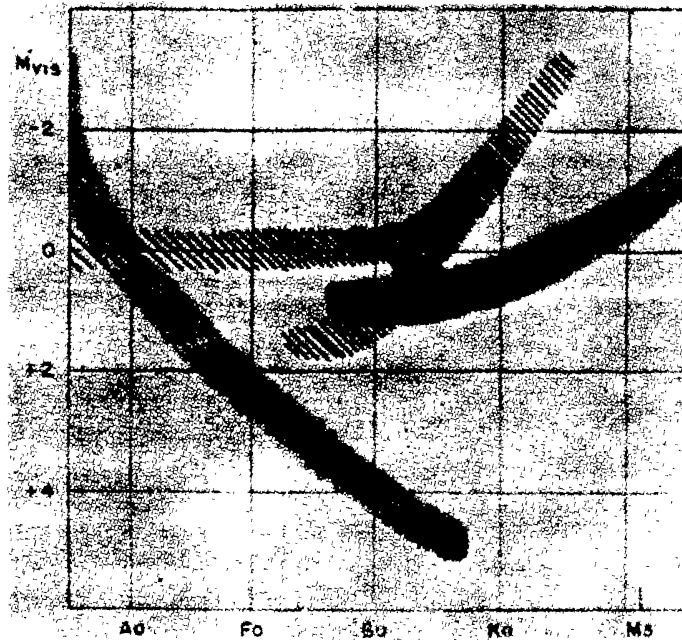
Figure 5. Baade's (1944) symbolic representation of HR diagrams of the two stellar populations. Because the globular cluster data did not reach down to the main sequence, the essential similarity of stellar evolution in the two contexts could not be seen.

The much-cited paper by Baade (1944) that defined stellar populations I and II on the basis of their color magnitude diagrams (Fig. 5) included a "prediction" that high velocity stars should display weak CN. The first large sample of high velocity, weak lined stars was collected by *Roman (1950, 1955), a participant in the present workshop. That line weakness implied genuine deficiency of calcium and iron was bravely enunciated by Chamberlain & *Aller (1951) somewhat before the world was prepared for it (though they moderated their deficiency factor from 100 to only 10 in deference to prevailing winds). They called their stars "subdwarfs or intermediate white dwarfs," and the former name has, of course, prevailed. Morgan (1956) added that the weak-lined globular clusters must also be metal poor.

The third part of the equation, "high velocity = metal poor = old" required evolutionary tracks for globular cluster stars. *Hoyle & *Schwarzschild (1955) and *Haselgrove & *Hoyle (1956) provided the first of these. They assumed initial stellar abundances $X = 0.93, Y = 0.07, Z = 0.007$, and $Z(\text{CN}) = 0.0025$, and, looking at an HR diagram for M3, derived an age of 6.5×10^9 yr. Thus, when B^{2}FH wrote in 1957, they were aware that globular cluster stars and high velocity stars were metal poor and that the former at least were old.

*Helfer, *Wallerstein, & *Greenstein (1959) dropped Z down to 1% of solar for M13 and M92, and *Wallerstein (1962) established the first correlation of anomalies, recognizing that the high velocity stars, though deficient in all metals, had Mg, Sc, Ca, and Ti less deficient than iron. These are the classic "alpha

nuclei," the ones you can think of as being made up of integral numbers of helium nuclei. They are important products of Type II supernovae.

The preceeding anomalies and correlations reflect the initial compositions of the stars displaying them (that is, the progress of chemical evolution before the stars formed). Some much weirder stars reveal nucleosynthesis in progress, since their surfaces show the effects of nuclear reactions, mixing, and mass loss in the stars themselves. Examples include the R and N stars with reversal of the normal C/O ratio (Curtiss 1926), the hydrogen-free Wolf-Rayet stars (*Aller 1943) and R Corona Borealis stars (*Berman 1935), the helium stars (*Greenstein 1940; *Popper 1942), some of which are stripped binaries (*Bidelman 1950), and the S-type stars, enriched in zirconium, barium, and other products of slow neutron capture (*Merrill 1947; Keenan & Aller 1951). Some of these (apparently the non-binary ones) flout technitium, requiring nucleosynthesis to have occurred within the last few million years (Merrill 1952). The report of promethium in HR 465 (*Aller & *Cowley 1970) has never been confirmed, but also never repudiated (Cowley 1995). The longest-lived Pm isotope has a half life of 17.7 years. And then there is FG Sge, whose surface composition as well as color and luminosity have wandered all over the map in the past century (*Herbig & *Boyarchuk 1968; *Wallerstein 1990).

Among globular cluster giants, there are correlated anomalies in oxygen, sodium, and aluminum that clearly require reactions that proceed considerably beyond the CNO cycle (*Kraft et al. 1995). Curiously, the most metal poor globular clusters cut off at [Fe/H] = −2.5, while field stars extend down to -4.0 (Sarajedini & Milone 1995; McWilliam et al. 1995; von Winckel et al. 1995).

Abundance anomalies resulting from in situ nucleosynthesis are also to be found in the ejecta of supernovae (*Whipple and *Payne-Gaposchkin 1941), novae (Aller & *Payne-Gaposchkin 1942), and planetary nebulae, though the mainstream astronomical community accepted these differences with painful slowness (see footnote 45 of Aller 1943 and Aller 1994). Of those who objected to the nova results in preprint form, the most vociferous opponents were *D.H. Menzel, O. Struve, and P. Swings (Aller 1995).

There are also stars of strange surface composition which do not belong in this chapter because the origins are not primarily nuclear. Thus if you want to know about Ap stars and their ilk, you will have to go elsewhere.

Stepping outside the Milky Way, we find that most spirals are rather like it, with relatively metal rich bulges, metal poor (old) halos, and disks with composition gradients easier to see in gas than in stars (Aller 1942 on M33). The nitrogen gradient is sometimes steeper than the oxygen or carbon ones, indicating that it is a secondary nuclide, whose production (in the CNO cycle) requires that there already be some C or O present.

The giant elliptical galaxies display strong lines and weak gradients, with their globular clusters normally bluer than field stars at the same radius (opposite to the Milky Way pattern). Disentangling the effects of age and metallicity remains a problem in this and any other context where individual stars are not resolved.

Magellanic spiral and irregular galaxies are systematically metal poor. For the clouds themselves, the average values of [Fe/H] are −0.5 and −0.8, and the current gas abundances are [Fe/H]= −0.3 and −0.6 (with the SMC being more

deficient). The older, more metal poor stars have [O/Fe] > 0 and, probably, others of the patterns seen in galactic metal-poor stars. I Zw 18, with [O/H]= −1.6, remains the least polluted galaxy that has enough gas to permit measurements of helium to be correlated with O, N, Fe (or whatever you think most appropriate) as an indicator of the primordial helium abundance. The goal, naturally, is to decide whether the value is one that big bang nucleosynthesis can produce and, if so, at what baryon density.

The dwarf spheroidals come as pristine as [Fe/H]= −1.9 for Draco and Leo II (but, in the absence of ionized gas, cannot be used to measure Y_p). Many have had several epochs of star formation, but none very recently. There is a clear correlation of mean metallicity with the mass or luminosity of galaxies, but also evidence for some second parameter, perhaps related to the local density of galaxies or other environmental conditions.

The best way to probe how current conditions came about might seem to be examination of stars and gas at large redshift. This has not proven quite as informative as you might expect. You see emission from very distant entities only when they are very bright and presumably not typical. Thus one does not quite know what to make of the apparent metal overabundances in QSO emission line regions or the approximate normality implied by emission line ratios in IRAS, radio, and emission line galaxies at $z \sim 3$. The gas producing QSO absorption lines is probably more typical of $z = 1 - 4$ material of some kind. Unfortunately, we are not quite clear about just what kind. That is, are we seeing mostly outskirts of galaxies; clouds that are in clusters but not part of galaxies; pieces of proto-galaxies; intergalactic clouds, or what? The majority of line systems in which heavy elements are seen have abundances of C, Mg, Zn and other tracers 1-10% of solar. You might expect a trend in metallicity vs. redshift, $Z(z)$, or even if you could locate clouds relative to the planes of their host galaxies a trend in $Z(z[Z])$. No strong one is seen. And, of course, QSO absorption lines tell us that the primordial value of D/H is .....

5. The Reaction Chains, Cycles, and Sites of Nucleosynthesis

The core of a correct understanding of cosmic abundances is figuring out which reaction(s) produce(s) each known stable and unstable nuclide and where each occurs. Historically we can approach the problem in four stages: the prehistoric, the golden age of *Cameron (1957) and *Burbidge et al. (1957), progress from then to the present, and prospects for the future.

5.1. The Eocene (Dawn of the Recent)

Two opposing ideas had already appeared by the end of the last century. Clarke (1889) as part of his attempt to describe the pattern of abundances had suggested that light elements, making up a primitive substance or "protyl", might be assembled into heavier ones, while Vernon (1890) had proposed a sort of primordial atom made of heavy things that would decompose into the lighter elements. In the years just after the first world war, J. Perrin, H.N. Russell, and A.S. Eddington recognized some sort of connection between accounting for the abundances of the elements and accounting for the sources of stellar energy.

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The next decade saw the establishment of a number of basic ideas. The mass of a helium atom is less than the mass of four hydrogen atoms (Aston 1927). Bringing hydrogen and helium into equilibrium would require very high temperature (Tolman 1922), but assembling the full range of elements requires both a range of conditions and non-equilibrium (*Urey & Bradley, 1931; Pokrowski 1931; Farkas & Hartock 1931). Walke (1934) and *Gamow (1935) both drew attention-to the importance of (non-equilibrium) neutron captures with intervening beta decays in creating the heaviest elements. Though we normally associate *Gamow with the idea of cosmological nucleosynthesis, this first of his papers on the subject in fact addresses only events in stars. He was, briefly, committed to the idea that the main source of stellar energy was the contraction of normal material first to degenerate electron densities and then to nearly pure neutrons, with nuclear reactions more or less incidental to the process. Landau briefly suffered from the same delusion; both recovered promptly and, it would seem, completely.

*Atkinson & Houtermans (1929) and Atkinson (1931) were the first to consider stellar nuclear reactions with barrier penetration (which, of course, greatly reduces the temperatures needed). They had in mind a catalytic, recycling process in which an atom of moderate weight would sequentially capture 4 protons and 2 electrons and spin off a helium nucleus (not far from what we now call the CNO cycle), but, with no knowledge of neutrons, they could not quite see where the first catalyst nuclei were to come from. The last effort to get from hydrogen to zinc in a single equilibrium marathon (with gradually increasing density) came from Sterne (1933).

We enter the modern era for hydrogen fusion with Von Weizsächer (1937, 1938) who, knowing about both protons and neutrons, emphasized that everything simply had to start with $p + p$, *Bethe & *Critchfield (1938) wrote down the details correctly, as *Bethe (1939) did the next year for the basic CN cycle. Neither set of reactions shows neutrinos explicitly.

Details of nuclear reactions in stars on beyond helium belong to the post-WWII years. In an ideal world, helium burning would have come first, then carbon burning, and so forth. In fact, *Hoyle (1946) was first off the mark with what we know now to be a much later stage, nuclear statistical equilibrium of the elements of the iron peak in highly evolved stars. Helium burning came next, in work by Opik (1951). *Ernst Opik, a greatly undersung pioneer of many astrophysical ideas, ended up with far too high a temperature for his triple-alpha reaction because of an inadequate appreciation of barrier penetration and incomplete knowledge of the properties of ^{8}Be and ^{12}C. That, plus publishing in Irish journals, was sufficient to keep helium burning from being primarily associated with his name. A more correct, and more appreciated, calculation soon came from *Salpeter (1952), who went on to demonstrate the possibility of additional alpha captures leading to ^{16}O and beyond (Salpeter 1953). *Hoyle (1954) looked at what would happen when two ^{12}C nuclei or two ^{16}O's came together at high enough energy. The dominant products are not, as you might expect, ^{24}Mg and ^{32}S, because of the need to conserve energy, momentum, and angular momentum simultaneously. Rather you get products like ^{20}Ne + ^{4}He and ^{23}Na + p from the former and ^{28}Si + ^{4}He from the latter.

Gamow's (1935) brief discussion of neutron capture reactions assumed they would occur at a stage when stellar cores already consisted mostly of neu-

trons. *Greenstein (1954) and *Cameron (1954, 1955), by demonstrating that $^{13}C(\alpha, n)^{16}O$ could provide free neutrons during helium burning, moved neutron capture nucleosynthesis forward into the period of hydrostatic burning in stars.

What we now call cosmological nucleosynthesis began in a sea of pure neutrons, or ylem (Gamow 1946; *Alpher, *Bethe, & *Gamow 1948; Alpher & Hermann 1950, 1953; *Alpher, *Follin, & *Hermann 1953). The correct initial condition, a proton-electron-neutron soup expanding away from a high temperature equilibrium was the inspiration of *Hayashi (1950).

Other sites that we now expect to make some contribution were also explored early. These include pycnonuclear reactions on white dwarfs in novae, (*Schatzman 1947), cosmic ray spallation (Gurevich 1954), and reactions on active stellar surfaces (*Biermann 1956).

5.2.   The Golden Age

Burbidge et al. (1957) and Cameron (1957) not only contributed an enormous number of new ideas and calculations to our knowledge of nucleosynthesis but, of equal importance, superbly synthesized what had come before. Table 1 shows the processes and products proposed by each, with B^{2}FH in the left column and Cameron in the middle. "Fe peak" means Ti to Zn or thereabouts. The most stable heavy isobars, made by the s process, are also, apart from bypassed (p-process) nuclides, the lowest mass isotopes of each element. The excluded or bypassed nuclides do not dominate the make up of any elements, and, as a result, we have no evidence for their existence or abundances outside the solar system. By "photonuclear" Cameron meant mostly (γ, n) reactions.

Fig. 6 shows how the products of the three heavy-element reactions can be separated into r-, s-, and p-process products, including some made by s + r. A few might also be made by s + p, but the p component will always be swamped.

5.3.   From B^{2}FH to the Present

Whether 1957 is infinitely long ago depends entirely on how old you are. For me it is not. I was in the 9th grade at Joseph Le Conte Junior High School, studying algebra under *Mitsunori Kawagoye (still a good friend) and Christmas caroling with the Mixed Glee and Troubadours directly by *Mae Nightingale (long dead; but I can still sight read, clean piano keys, and tie a four-in-hand). Father (whose tie was always Windsor-knotted) was working for Papermate Pen, a brief, three-year respite in two decades of constant job changes that permitted a pool in 1956, a two-tone brand-new Chevy in 1957 (our first new car, ever), and a vacation in 1958 (four days in Yosemite). In many ways, it seems very close.

A good deal has, however, happened in the area of nucleosynthesis, without much disturbing the basic pattern shown in Table 1. Perhaps most important, the early universe has been admitted as a full partner with stars, responsible for making "all the elements up to helium" plus a bit of ^{7}Li . Second, the alpha process, in which ^{12}C, ^{16}O, ^{20}Ne, ^{24}Mg, etc. sequentially captured helium nuclei, fractured itself against a ^{20}Ne barrier. It has no low lying states with the right angular momentum and parity for $^{16}O(\alpha, \gamma)^{20}Ne$ to have a reasonable cross section. The alpha process thus broke into discrete stages of C, Ne, O, and Si burning (Table 2), which, however, are now beginning to blur again, because the full assortment of possible nuclides and reactions are included in the network

Table 1.   Processes and products as proposed by B^2FH and Cameron

|	Processes	Products
H burning		^{4}He, ^{13}C, N, ^{16,17}O, F, ^{21,22}Ne, Na
He burning	hydrogen and helium thermonuclear reactions in orderly evolution of stellar interiors	^{12}C, ^{16}O, ^{20}Ne, ^{24}Mg He, C, N, O, Ne
α process	heavy-ion thermonuclear reactions in orderly evolution of stellar interiors +	^{24}Mg, ^{28}Si, ^{32}S, ^{36}Ar, ^{40,44}Ca, ^{48}Ti
	neutron captures on slow time scale +	Ne to Ca
	hydrogen and helium thermonuclear reactions in supernova explosions	
e-process	statistic equilibrium in pre-supernovae and supernovae	Fe peak
r-process	neutron capture on fast time scale in Type I supernovae	unshielded isobars $A \geq 62$ including actinides
s-process	neutron capture on slow time scale in orderly evolution of stellar interiors	most stable isobars $A \geq 62$
p-process	proton capture and photonuclear reactions in Type II supernovae + photonuclear reactions on slow time scale in orderly evolution of stellar interiors	excluded/bypassed isobars $A \geq 62$
x-process	possibly made by nuclear reactions in stellar atmospheres	D, Li, Be, B
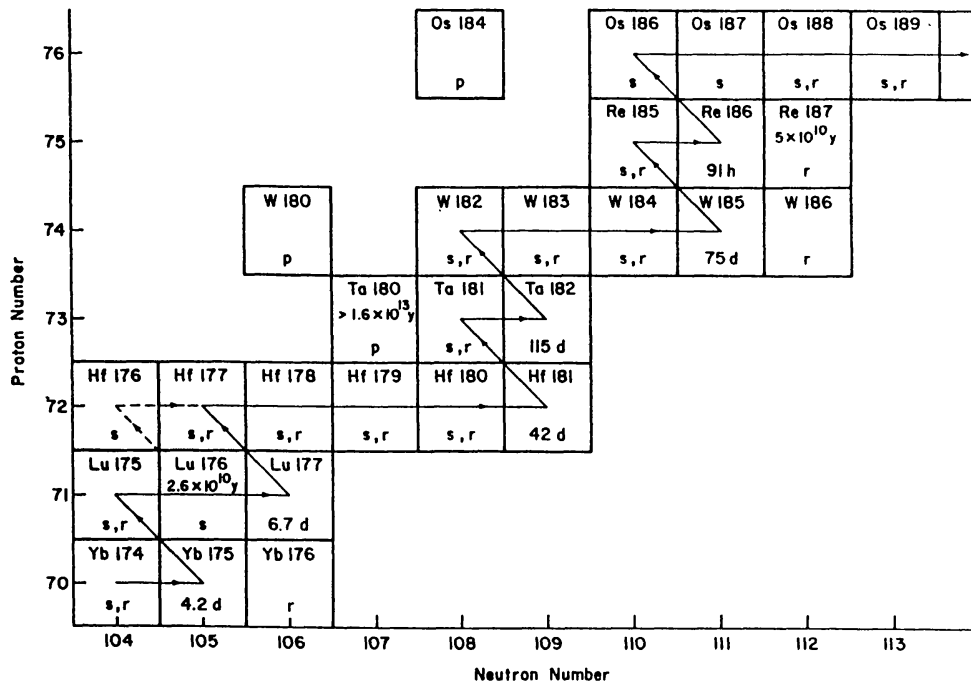
Figure 6. The region $A = 174 - 189$ showing the progress of the s-process through the stable (or very long lived) nuclides. Nuclides with n/p larger than those on the s-process path can all be reached by beta decays of unstable, neutron rich nuclides formed by the r- (rapid capture) process. The orphans of lower n/p ratio to the left of the s-process path are all very rare and must be derived from s- or r-process progenitors through addition of protons (hence the name p-process), removal of neutrons, or both.

at every stage. The lines in the table can be thought of as "the seven ages of a 20 $M_\odot$ star", by analogy with the seven ages of man. The eighth is also rather similar for both: "My, but you're looking well!" and "Gracious, you've become a pulsar!"

Hot hydrogen burning has acquired an identity of its own, extending upward in A from the CNO cycle to a Ne-Na cycle and probably a Mg-Al one. It contributes to isotopes that are not sums of alpha particles. For nova explosions on ONeMg white dwarfs, these reactions will be the dominant ones. The ongoing puzzle of just where most of the ^{26}Al comes from is part of this picture if the main reaction is ^{25}Mg$(p,\gamma)^{26}$Al. Sites proposed in the past include asymptotic giant branch stars, Wolf-Rayets, and novae. The distribution in the sky of the gamma ray line from the decay to ^{26}Mg (Diehl et al. 1995) suggests that production in supernovae is the most important. In this case, ^{26}Al may be made by the neutrino process discussed by S.E. Woosley elsewhere in this volume. Other potential ν-process products include ^{7}Li, ^{11}B, ^{19}F, ^{138}La, ^{180}Ta, ^{10}B, ^{22}Na, and the odd isotopes of Cl, K, Sc, Ti, V, Mn, Co, and Cu.

Table 2.   The Seven Ages of a 20 $M_\odot$ Star (from Arnett et al. 1987)

Fuel	Central Density g/cm^3	Central Temperature K	Photon Luminosity erg/sec	Neutrino Luminosity erg/sec	Duration years
H	5.6	4×10^7	3×10^{38}	small	10^7
He	940	2×10^8	5×10^{38}	small	10^6
C	3×10^5	8×10^8	4×10^{38}	7×10^{39}	300
Ne	4×10^6	1.7×10^9	4×10^{38}	1×10^{43}	0.38
O	6×10^6	2.1×10^9	4×10^{38}	7×10^{43}	0.50
Si	5×10^7	4×10^9	4×10^{38}	3×10^{45}	2 days
core collapse	10^{9-15}	4×10^{10}	10^{42-44}	$\sim 10^{52}$	10 sec

Explosive processes in general look more important (or anyhow more calculable than in 1957. The first discussions of reactions in supernova explosions (as opposed to in pre-SN massive stars) came in 1960 (*Hayakawa, *Hayashi & Nishida 1960; Hoyle & Fowler 1960). The next round of calculations treated explosive nucleosynthesis as a sort of fine tuning of abundances established by hydrostatic processes (Schramm & Arnett 1973). Current supernova models typically have all reactions available at all stages and let the star decide what it wants to do.

Some refinement of understanding of the s-, r-, and p-processes has occurred. For the s-process, there is a second possible neutron source during the double shell burning phase of intermediate mass stars, when convection zones move up and down, bringing the products of hydrogen and helium burning into contact: $^{14}N(\alpha,\gamma)^{18}O(\alpha,\gamma)^{22}Ne(\alpha,n)^{25}Mg$. But the ^{13}C source is probably still more important (Lambert et al. 1995). Many more accurate cross sections have been measured, many by groups working with H. Beer and F. Kappeler (e.g. Kappeler et al. 1994). A process called n, intermediate between s and r (in the sense that the time scales for beta decay and neutron capture are about the same) has been considered sporadically.

The nuclear data needed for r-process calculations have also improved greatly. Cowan et al. (1991b), for instance, tabulate cross sections and such clear up to $^{137}113$ (which I am inclined to call Cameronium, though this violates IUPAC rules). The "best buy" site remains core-collapse supernovae (Woosley et al. 1994), with the core, carbon burning, and helium burning zones all having been considered. Helium flash, novae, and mergers of neutron star binaries have also

been proposed. In the carbon detonation site proposed by Panov et al. (1995), there is somehow no need for intervening beta decays.

Supernovae of Type II remain also the most promising site for the p-process (Rayet et al. 1994). Lambert (1992) has provided an excellent review of what we know about these rarest of nuclides.

The so-called x-process, responsible for deuterium, lithium, beryllium, and boron, is clearly not a single entity. At minimum, it includes the early universe (responsible for all the ^{2}H and 10% or so of the ^{7}Li), cosmic ray spallation (for which the clearest evidence is that the cosmic rays are themselves greatly enriched in Li, Be, and B, as well as in rare odd isotopes), and probably one or two other contributors, like the neutrino process in supernovae, red giants (at least the lithium-rich ones), flares, and novae.

Finally, it has become clear that the several types of supernovae make very different contributions to nucleosynthesis. It is less clear just how many types (plus their rates, products, etc.) need to be considered. The current commonest assumption (to which I have no fundamental objections) is that there are two basic physical processes that make observable supernovae – nuclear explosions in degenerate material and core collapse in evolved massive stars.

Type Ia supernovae occur only about once per century in the Milky Way, show no hydrogen features in their spectra (the definition) and are blamed on explosive burning of carbon and oxygen in degenerate dwarfs. It takes $0.5 - 1.0$ $M_\odot$ of C and O burning through to iron peak elements to match light curves and spectral evolution. If you have to start with at least a Chandrasekhar mass (M_{Ch}) of degenerate stuff to make it happen, this leaves $0.5 - 1.0$ $M_\odot$ to be expelled as partly burned Ne, Si, S, Ar, etc. and unburned C and O. Observations do not, at any rate, contradict this. Type Ia supernovae therefore provide an additional source of iron and some other elements that kicks in after a stellar population is at least 10^{8-9} yr old (the time taken to get to,the relevant degenerate objects). The actual progenitors are essentially unknown. Stripped CO cores of moderately massive stars, cataclysmic binaries with massive white dwarfs (so that a little bit of mass transfer drives them over the edge; some recurrent novae may fit), and mergers of white dwarf pairs all have their advocates. The third is probably most popular. It requires a white dwarf binary with total mass at least M_{Ch} and orbital period less than about 12 hours (so that angular momentum loss in gravitational radiation will bring the stars together in less than the age of the universe). The number of such pairs in our catalogues after several years of intense searching remains precisely zero. The short period white dwarf binaries are all low mass; the few massive ones all have long periods.

Core collapse supernovae are somewhat commoner (at least here and now) and are generally regarded as including type II (with hydrogen lines) and types Ib and Ic (no hydrogen, helium optional, blamed on collapse of stripped cores, binary or single). Production of oxygen is the best nucleosynthetic signature for type II SNe. The progenitors are well known – all the stars you observe with main sequence masses of $8-12$ $M_\odot$ or more. Until very recently the main problem was getting the things to explode. Thus most nucleosynthetic calculations simply hit the base of the stellar envelope with a numerical hammer or piston. Two- and three-dimensional calculations of neutrino-driven turbulence, convection, and

instabilities appear to have revealed the solution (Herant et al. 1994; Burrows et al. 1995).

Mass loss before the explosion and similar mass loss by smaller stars during their asymptotic giant branch and pre-planetary-nebula phases can add carbon, nitrogen, and s-process material to the galactic inventory in a peaceful fashion. Not all models of chemical evolution yet incorporate this source.

5.4. Topics to be Reviewed in 2015

Trimble (1975) ended with a set of residual puzzles, worries, etc. Time has actually taken partial care of some of them. We do now, for instance, have empirical evidence for newly-produced iron in supernovae and their remnants. One can similarly hope that some of the items listed above as unclear, disputed, or just plain messy will be sorted out in the next decade or two. First among these is figuring out the correct relative contributions of multiple sites to synthesis of elements like lithium and zinc that can be made in many different ways. Another issue to be resolved is just how many physically distinct kinds of supernovae exist and what each contributes to nucleosynthesis.

Another unanswered question is the source composition of cosmic rays, which is associated with the dispute about whether initial acceleration takes place in flare stars, supernova-driven shocks, or someplace else. This might or might not be part of mainstream cosmic chemical evolution, depending on what the answer is. Finally, cosmologists will wait with anxiety to hear (or pronounce) an answer to whether the standard hot big bang (presumably with small baryon density) can "predict" the observed prestellar abundances of hydrogen, helium, and lithium. This issue has two parts, both discussed extensively elsewhere in this volume: deciding what the observed abundances are, and doing the calculations correctly.

6. Galactic Chemical (and Chemo-Dynamical) Evolution

The final task is to put all the reactions and sites together over the history of the galaxy, add up their products, and see whether the sum as a function of time and place agrees with the data we have on abundances vs. time and place, subject to the constraint that the evolved galaxy must have luminosity, color, and residual gas fraction matching the real galaxy you are trying to model. This is far and away the most immature part of our discipline, with even the basic ideas going back 40 years or less, and the first major step toward a synthesis dating from *Beatrice M. Tinsley's 1967 PhD dissertation (Tinsley 1968: remember, I promised you would meet someone else who had solved a fundamental problem in a thesis). With the wisdom of hindsight, it is obvious that the most important aspects of her work were the demonstrations that galactic evolution is (a) calculable and (b) important. I have approached the subject plonkingly, year by year.

6.1. The First 25 Years

1955.   *Salpeter (1955) determined that star formation acts so as to produce numbers of stars as a function of mass proportional to $M^{-2.35}$ over the range 0.3 to 30 $M_\odot$. He called this the original mass function. We now call it

the initial mass function, or IMF. Modern forms are often Gaussians, peaked at $0.3 - 0.5 M_\odot$ whose declining right edges look quite a lot like a Salpeter function.

1957. *Von Hoerner (1957) attempted to predict the number of stars, integrated over all ages, that would have a particular metallicity, $N(Z)$, on the assumption that the ratio of mass of metals coming out of stars to mass of gas going into stars was a constant. We would now call this the constant yield approximation. He handled the effect of decreasing mass in interstellar gas incorrectly and predicted a flat $N(Z)$, thereby missing the discovery of the G dwarf problem (of which more shortly).

1958. *Van den Bergh (1958) corrected the Von Hoerner calculation to allow for the increase of metallicity of the ISM as its mass decreases. *G.R. Burbidge (1958) pointed out that only 10% of the known helium could be made in stars over the age of the universe or galaxies would look much brighter than they do. This is a very important argument in favor of the universe having gone through a hot, dense early state (big bang) that clearly predates the discovery of the 3K background radiation.

1959. *Schmidt (1959) examined numbers of stars being formed under various conditions and concluded that normal star formation rates scale as (gas density)^2. Thus the rate in the young Milky Way was five times the present one, and enough gas remains for 10^{10} more years of star formation. He spoke of the "initial luminosity function."

1961. *Sandage (1961) estimated how much the luminosity of a giant elliptical galaxy would have changed since $z = 0.46$ (the largest then known) due to stellar evolution, and concluded that, if $N(M)$ were flat, it would be only 0.38 *mag* in bolometric magnitude (enough to make a true $q = 0.2$ universe look like $q = 1.0$ in a Hubble diagram) and much less if $N(M)$ rises to small masses (since there are more red giants coming along with time to make up for each one being somewhat fainter). The effect is considerably larger in any one observed wavelength band because you are looking at photons originally emitted at shorter wavelengths in large redshift galaxies.

This was also the year of IAU Symposium 15 (*McVittie 1962) on problems in extragalactic research. Among the highlights there, Allen Sandage announced a best value for H_0 of 98 ± 9 km/sec/Mpc (at least the error bars haven't changed), Fred Hoyle said that the luminosity evolution of an elliptical galaxy depends primarily on its IMF (true), and *Ivan King said that the present age also matters (also true). *Robert Christy asked whether some seemingly young globular clusters in the outer halo might be material recently added to the Milky Way (could be!), and *H.C. (Chip) Arp showed the time evolution of galactic metallicity as then understood (Fig. 7). Some of the individual points have probably moved around in the intervening 35 years, but the basic picture hasn't changed much.

1962 was something of a banner year. *Sandage (1962) provided a picture of the rate of increase of metallicity in the Milky Way that fed directly into the classic *Eggen, *Lynden-Bell, & *Sandage (1962) model of galaxy formation as a monolithic and fairly rapid collapse, leaving metal poor stars behind in the halo, not sharing the rotation of the disk. An unpublished manuscript by *Schmidt (1962) suggested that the low metallicity in dwarf elliptical galaxies might be a result of outflow carrying away the metals made by their first generations of

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stars. Finally, *van den Bergh (1962) concluded that the rate of metal creation in the galaxy has decreased faster than the rate of star formation. This is the first explicit description of what becomes called the G dwarf problem. He also noted that, on the basis of data from *Wallerstein (1962), enrichment in the alpha nuclei went faster than that in iron.
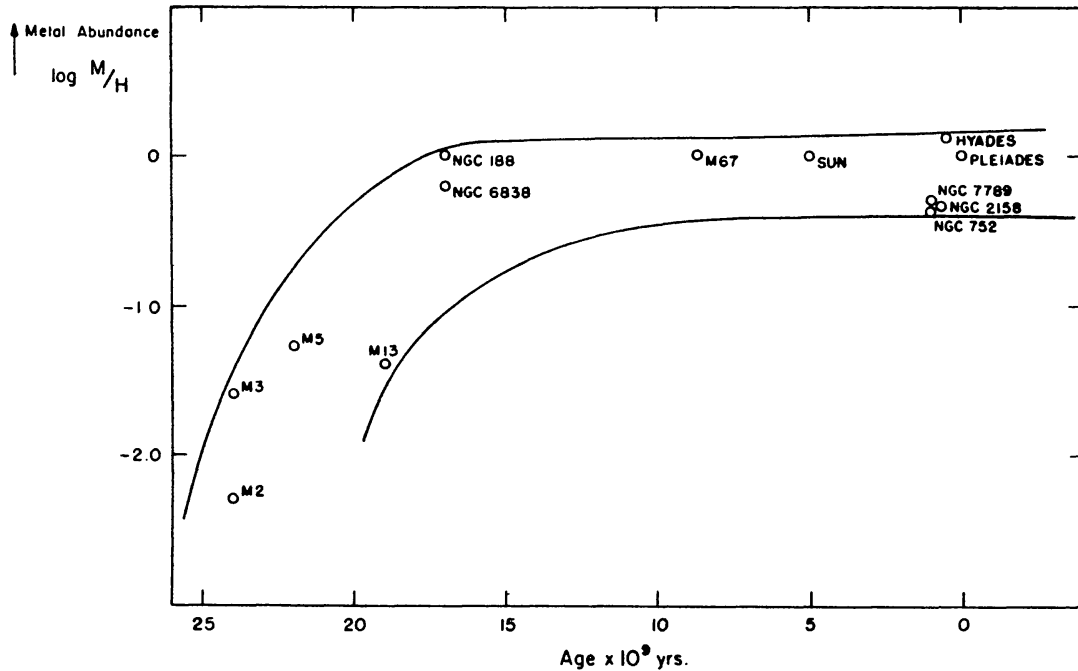

Figure 7. Arp's (1962) version of the gradual increase of metallicity in the Milky Way, based on globular and open clusters (from IAU Symposium 15). The current version of the plot would not look very different because the real scatter is large and only partly explained by spatial gradients.

1963. This was the year that Schmidt concluded, from counts of G dwarfs with different ultraviolet excesses, that "relatively more bright stars formed in the past." This both enunciates the G dwarf problem and suggests a variable IMF as a solution. For better or for worse, 1963 was also the Year of the Quasar (3C 273). Many astronomers, including Schmidt, were thereby diverted from work on the Milky Way and other normal galaxies, ushering in a few years of "dark ages" in the field.

1964. The 13th Solvay conference was devoted to structure and evolution of galaxies (Solvay 1964), but no new ideas can be discerned in the proceedings, for which J. Robert Oppenheimer provided the concluding remarks. A portion of that year's IAU General Assembly was also concerned with structure of galaxies. In a much under-appreciated calculation, McVittie (1964) demonstrated that the large mass to light ratios already then known for spiral and elliptical galaxies would require that a power law (Salpeter) IMF continue down to 0.01 $M_\odot$, or

that there be some other sort of invisible stuff. He suggested objects inside their Schwarzschild radii.

1966. *Lequeux (1969) produced another volume entitled *Structure and Evolution of Galaxies* and containing no new ideas. The three year delay in publication makes the printed version seem even more backward-looking. At IAU Symposium 26 (Hubenet 1967), on abundance determinations in stellar spectra, *J.L. Greenstein delivered an uncharacteristic sort of fin-de-siecle message expressing a dislike of supernovae and neutron stars as part of galactic chemical history and doubting the possibility of ever learning anything useful about the primordial helium abundance.

1967. The year 1967 saw the completion of Tinsley's thesis, but the real renaissance of galactic astronomy (in our narrow sense) was still a few years away.

1970. *Sandage, *Freeman, & Stokes (1970) proposed that continuous infall (in contrast to a single, rapid epoch of collapse) might be important in the growth of the Milky Way.

1971. Prompt Initial Enhancement, or PIE, became the first solution to the G dwarf problem with a cute nickname (*Truran & *Cameron 1971). The problem, shown in Fig. 8, is simply that the local population of stars old enough to trace the entire history of disk star formation (G dwarfs and later types) includes far fewer low-metallicity stars than would be there if the disk had evolved as a homogeneous, closed system with constant initial mass function (hence constant yield). The S's represent the data available to Schmidt (1963) and van den Bergh (1962), when they first called attention to the discrepancy. PIE is the idea that star formation in our part of the galaxy began with a sudden burst of massive objects which enriched the gas that, only later, made stars of lower mass that survive to the present time. A pre-galactic generation of supermassive objects would also work as the "initial enhancer."

1972. *Larson (1972), *Fowler (1972), and *Quirk & *Tinsley (1972) proposed an alternative solution based on infall (Sandage et al. 1970). The idea is that local gas metallicity has been kept at essentially its current value for billions of years because fresh hydrogen and helium gas continuously rains down into the disk, mixing with the material enriched by previous generations of stars. Fowler described this at a 1974 conference as: "Here these stars are burning their hearts out trying to make heavies, and those bastards keep diluting it." Outflow, if it removes metal-rich gas preferentially, will have much the same effect.

1973. Metal-enhanced star formation (MESF, *Talbot & *Arnett 1973) was another G dwarf solution, in which the first few supernovae enriched gas in their immediate surroundings, and future star formation (at least of low mass stars) occurred only in the enriched gas. This is not unreasonable because higher metallicity means more rapid cooling of gas, facilitating star formation. In the same year, *Leonard Searle and *Bernard Pagel (in not very accessible venues) showed that you can get an analytical solution for number of stars as a function of metallicity for the homogeneous, closed, constant yield system, provided that you make one additional approximation, called instantaneous recycling. This means you pretend that the new metals come out as soon as the massive stars

form, (not far wrong at least for type II supernovæ). The answer is

$$\frac{N(Z)}{N(Z_0)} = \frac{1 - \mu_0{}^{Z/Z_0}}{1 - \mu_0}$$

where Z_0 is the metallicity in the gas now, Z is any lower value, $N(Z)$ is the number of stars with metallicity Z or less, and μ_0 is the present residual gas fraction. The derivation is reproduced in Trimble (1975) and breaks down for large Z (more than about 10%). The continuous curve in Fig. 8 is this expression. That the data points for stars with much less than solar metallicity fall well below the curve is fairly obvious.

1974. Early attempts at coupling dynamical and chemical evolution together came from Talbot & Arnett (1974 for disk galaxies) and *Larson & *Tinsley (1974 for spheroids). *Ostriker & *Thuan (1975) considered the transformation of spheroids into disks both dynamically and chemically. This was also the year of the three-week NATO workshop "Origin and Abundances of the Chemical Elements" that was my introduction to the subject (Trimble 1975). The transformation of my lecture notes from the workshop into a review article happened primarily because a participant who was not a native speaker of English asked for a copy of my notes (which meant typing them up), and I would like to thank *Anna Zytkow, very belatedly, for her unintended but pivotal role in the process.

For the first 15 years or so, discussions of galactic chemical evolution focussed largely on passive processes. That is, you started with a given amount of gas, turned it into stars, and let the stars die away, allowing, at most, moderate amounts of gas flow into or out of your system. The modern study of active evolution, in which galaxies or parts of galaxies interact, merge, trigger each other's star bursts, and generally fail to mind their own business began with a classic numerical simulation of galaxy mergers by *Toomre & *Toomre (1972), which produced a very persuasive simulacrum of the pair NGC 4038/4039, complete with the insect-like antennae. (Radio astronomers have antennas; insects have antennae.) The paper is also, I think, one of the best-written ever in the astronomical literature.

*Press & *Schecter (1974) proposed a complete "bottom up" scenario for galaxy formation, in which nearly all large galaxies had been assembled from much smaller units, perhaps like the remaining dwarf irregulars. This became nearly everybody's favorite model during the recent heyday of cold dark matter, which produces its first bound structures on small scales.

1977. Another important conference took place at Yale (Tinsley & Larson 1977) on galaxies and stellar populations. Pre-galactic stars were widely blamed for setting a metallicity floor in the disk and for leaving behind brown dwarfs in the halo to produce the large mass to light ratios that were by then widely accepted. The latter, at least, has now been ruled out by the sparsity of gravitational microlensing events that can be attributed to compact halo objects in the brown dwarf mass range.

1978. *Searle & *Zinn (1978) suggested that even the halo of the Milky Way had been gradually assembled out of many entities, in contrast to the monolithic model of Eggen et al. (1962).
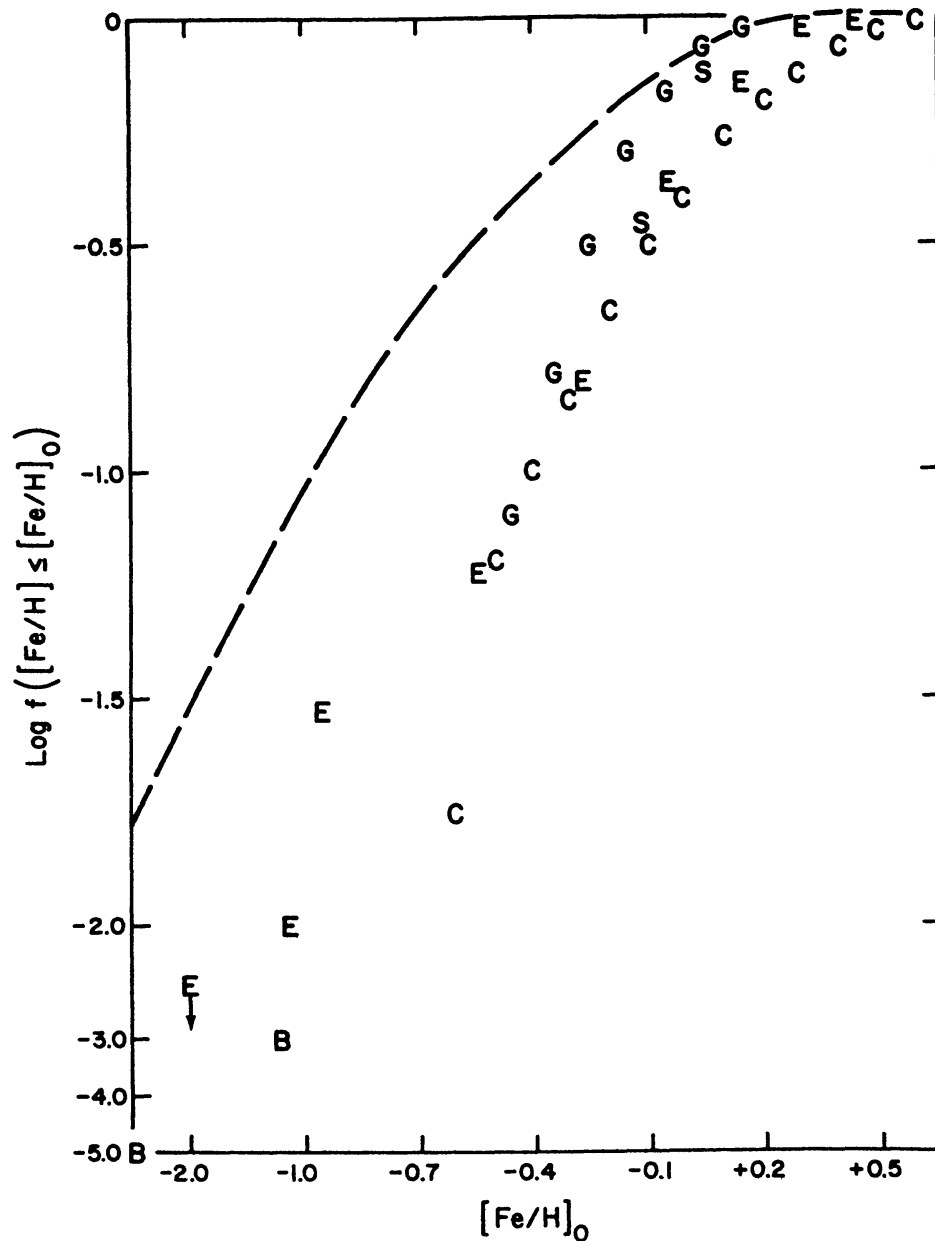
Figure 8. A visual presentation of the "G dwarf problem" from Trimble (1975). The solid curve is the prediction of fraction of all stars with metallicity equal to or less than Z as a function of Z in the simplest possible model, for a single, closed system with constant IMF or yield and instantaneous recycling. The letters show measured values of the fraction at different metallicities. S's are the data available to Schmidt and van den Bergh in the early 1960's. Of the other letters, B = Bond, G = Gliese & Pagel, C = Clegg and Bell, E = Eggen (references in Trimble 1975).

6.2. Current and Future Issues

First the G dwarf problem. It remains true that the simplest model is not a good fit to local disk stars. Current models of galactic chemical evolution tend to incorporate at least several of the traditional solutions (variable IMF, gas flows, perhaps some initial enhancement provided by thick disk stars...). But the simple model does fit a number of other systems, including the halo globular clusters, disk globular clusters, K giants in Baade's window (Pagel 1987), other bulge stars (Ibata & Gilmore 1995) and local halo stars with metallicities below [Fe/H]$= -2.5$ (Beers 1992), provided that you choose a suitable value of the yield for each population. Other issues that one might expect to see sorted out (not necessarily very soon) include the existence and nature of population III (that is, where did the very first metals come from?) and, closely related, what is the source of the metals in X-ray cluster gas and in the clouds producing QSO absorption lines. A number of people know the answers to these, but they don't all quite know the same answer.

Next, just how important are mergers in galactic evolution? Are all giant ellipticals the result of multiple mergers of disk systems? Is most star formation, including globular cluster formation, triggered in such events, or are they a fairly minor perturbation on isolated evolution. And, when you add up both active and passive evolution, do big galaxies turn out to be brighter or fainter at redshifts of $0.5 - 1.0$? This information is essential if the deceleration parameter, q_0, is ever to be determined by looking at a Hubble diagram for distant galaxies.

A very old puzzle is the ratio of helium to heavy element production by a generation of stars. Regressions on the data for metal-poor, gas-rich galaxies seem to suggest quite high values of $\Delta Y/\Delta Z = 3$ or even 6, while model stars produce ratios of 1 or 2.

Finally, of course, one would like to triumph over the curse of the variable parameter and learn to calculate or predict star formation rates, the initial mass function, the numbers and properties of binaries, and so forth, as a function of the amount of residual gas in a galaxy, its distribution, turbulence, and whatever else matters.

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