OPEN DISCUSSION; SESSION I (Chairman Martin F. McCarthy)

McCARTHY: With the authorisation of the chairman of this joint discussion I will first call upon Dr Jack Hills to give a five minute remark about the location of Population III stars.

HILLS: True Population III stars composed only of primordial hydrogen and helium do not appear to exist in our Galaxy. Bond (1981, Ap.J. 248, p. 606) finds that one-zone models only reproduce the metal distribution of the stars in the Galactic halo if the gas from which they formed was already contaminated at a level $z = 0.0025z_{\odot}$ before any halo stars formed. Without the initial contamination, the one-zone models predict that up to several tens of percent of the halo stars would have $z < 0.001z_{\odot}$, but almost no such stars exist.

I propose that true Population III stars may only be found in a small fraction of external galaxies in which star formation happened to occur relatively early. The universe is highly inhomogeneous on the scale of superclusters. The first galaxies to form will be those found in the densest parts of each supercluster since their higher self-gravitation will allow them to overcome the expansion of the universe and to collapse before galaxies form in the lower-density regions. True Population III stars will only be found in these first-formed galaxies due to the contamination of their neighboring galaxies by the supernova debris from their Population III stars. The densest region near our Galaxy is the Virgo cluster. The true Population III stars in the Galactic neighborhood may only be found in some of the galaxies of the Virgo cluster.

The close proximity of the Virgo cluster at the time of star formation in the Galaxy greatly helped the z-contamination of the Galaxy. From the observed redshifts of the most distant quasars and the fact that they are already metal rich, we estimate that the first star formation must have occurred in these objects when the age of the universe was about 0.1 of its present age. At that time the Virgo cluster was about 2.5 Mpc away compared to its present distance of 13 Mpc. The ejection velocities from Type II supernovae of $V = 6000$ km/s= 6 Mpc/10^9 years and Type I of $V = 20\ 000$ km/s = 20 Mpc/10^9 years would have allowed the z-contaminated material to travel to our Galaxy in under 10^9 years.

The main problem with contaminating a galaxy with the supernova debris of another galaxy is the fact that little of this debris escapes the galaxy of origin. This escape is greatly enhanced if dust grains form in the supernova ejecta. The formation of such grains is strongly supported by both theoretical work and by the observed presence of

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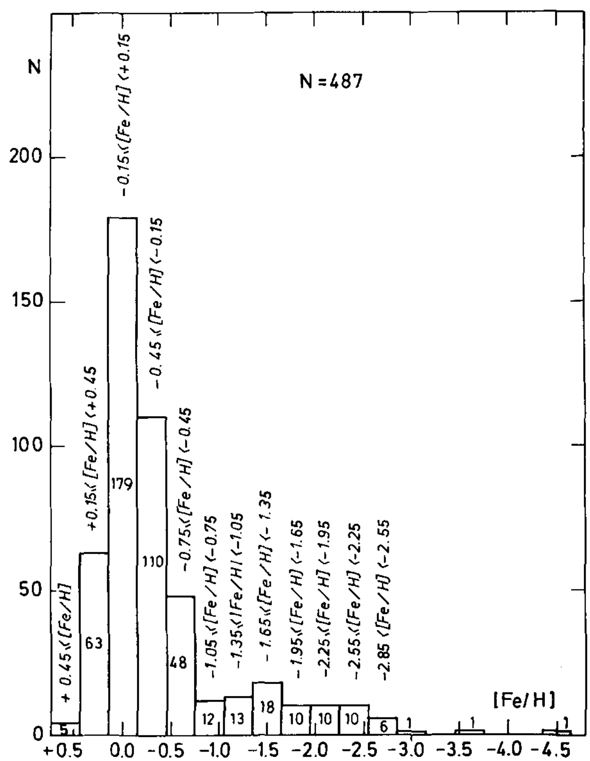
localized isotope anomalies in inclusions found in the Allende meteorite. The dust acts like shrapnel and has much greater penetrating ability than does gas ejecta. While most of the grains would be trapped in the parent galaxy, enough are expected to escape to account for the very small levels of z-contamination found in the early Galactic halo.

There is observational evidence for metal loss from galaxies. Bond notes that the nucleosynthetic yield in the Galactic halo was only 0.04 of the present yield in the galactic disk which indicates that halo star formation occured in a very leaky box. The high iron abundance observed in the intergalactic gas in clusters of galaxies also indicates the escape of metal-rich material. (A fuller treatment of this work is given in Hills: 1982 Astrophys. J. 258, L67.)

McCARTHY: Thank you, Dr Hills. I would like to call upon, again with the authorization of the President of the Joint Discussion, Madame Cayrel, who wishes to show us one slide.

CAYREL: After having followed with great interest the first papers of joint discussion, I would like to comment on two diagrams which I think are in the subject of the first session.



The first diagram is a histogram of metallicity determinations in terms of $[Fe/H]^*_\odot$ of 487 F, G and K field stars falling in the effective temperature range 7200 - 3800 K. This diagram is taken from a paper we have written recently on the "Status of evolution of F, G and K stars contained in the [Fe/H] catalogue" (Cayrel de Strobel G., Bentolila C., 1982). Let us remember that the $[Fe/H]^*_\odot$ values of the 487 stars all come from detailed high dispersion spectral analyses. Almost all of the $[Fe/H]^*_\odot$ values are means of several detailed analyses per star.

The Pop I disk and old disk stars are contained in the first five bins of the histogram. It is interesting to note that out of 487 stars there are 68 (14 % of the sample) which have a higher metallicity than the sun. Seven other bins are following on the histogram. These contain all halo population stars having very high spatial velocity parameters. However, the metallicities between the first halo star bin and the last differ by almost two order of magnitudes. The histogram has on its right hand side a tail of three stars having very high metal deficincies. The most metal deficient star is CD $-38^{\circ}245$ ([Fe/H] ~ -4.5) recently analyzed in detail by Bessel, 1982. The bins of the halo stars are not heavily but rather uniformly populated.

The histogram confirms that there exist a halo field population of stars having much lower metallicities than those of the most metal deficient globular clusters. But, there are also halo field stars having the same order of metal deficiencies as those of globular clusters, i.e. not all the halo field stars are more metal dificient than globular cluster stars. The question is now if the very metal-poor ([Fe/H] < -2.0) halo stars which populate the solar neighborhood are remnants of the first very metal deficient small globular clusters which have already evaporated.

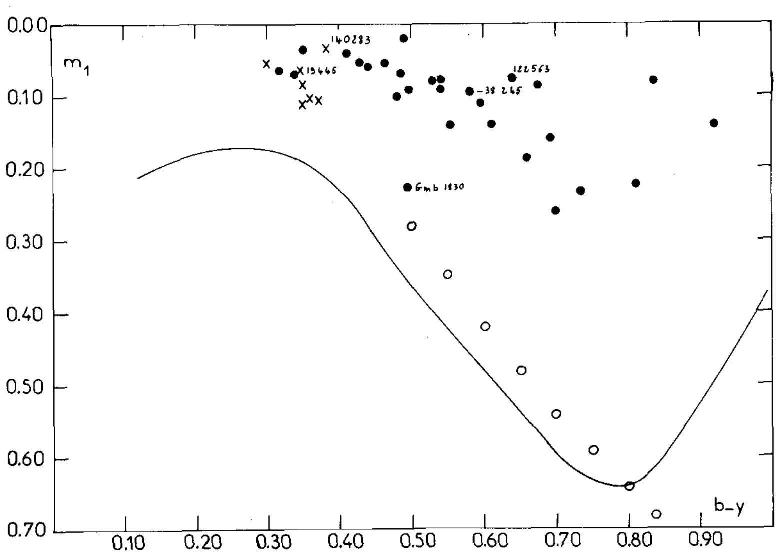


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Fig. 2 represents a Strömgren ($b-y$, m_1) diagram. The solid line and open circles are respectively the Hyades dwarfs photometric sequence and the metal normal bright field giant sequence in the ($b-y$, m_1) plane. The crosses are extreme halo turn-off stars in which the Spites have discovered the lithium resonance line (λ 6707) and have deduced from its intensity a primordial value of the lithium abundance:

$$A(^7Li) = (5 \pm 2) \times 10^{-10}.$$

The filled circles are extreme halo stars. Here it is interesting to note that the CD -38^{o} 245 analyzed by Bessel and Norris (1982), and which on the preceding histogram is the most metal deficient halo star, does not occupy an extreme position on the ($b-y$, m_1) diagram. It would be interesting to reanalyze in detail CD -38^{o}245.

McCARTHY: Thank you very much Madame Cayrel. I'd like to invite the open discussion now and recognize Jean-Claude Pecker for a question to Professor Kraft.

PECKER: I thank you for allowing me to speak for the Devil for one second I'm addressing Bob Kraft and asking the following question. I have been impressed, of course, by the very nice fit of the observations and the computed spectra. But, as you said, you didn't explain and you didn't try to explain Hα or the Balmer lines. Well, obviously it is because you can not know what kind of chromosphere or corona to use. But remember that the CN lines are formed very near the minimum of temperature; and, depending on the importance of the non-radiative heating of the chromosphere and corona, the physical conditions of the regions where CN lines are formed may be very different. So in part - I don't know how much - a part of your effect might be due to the fact that in globular cluster stars the non-equilibrium outer layers might be different from one cluster to another.

KRAFT: Well, of course, you may very well be right. One can, of course, invent all kinds of scenarios, but the only thing I can tell you is that you have to explain, then, why two stars which are in exactly the same place in the HR diagram are basically different.

PECKER: The HR diagram is only two parameters and we know very well that....

KRAFT: But then you have to tell me why it is that all the stars in a cluster are somehow different from all the stars in some other cluster in a way that affects, somehow, the chromospheres. I somehow find that connection rather hard to make but, of course, you may very well be right. The whole abundance racket has been nettled for 30 years, it seems by statements like "Well, you know, you can't believe this and that because it might be out of LTE". I think that if we had believed that in the beginning we would never have done anything. I can only tell you what the differential effects are and you can, of course, turn it into anything you like.

McCARTHY: Now do we see some other questions, please? Yes, Dr Aller.

ALLER: I would like to comment on some of the difficulties in using planetary nebulae. Now, planetary nebulae are very valuable, particularly in the halo, because one can get elements like S and Ar and Ne. For example, Barker has shown that S and Ar are down in abundance compared with the Sun by about two orders of magnitude. When you turn to C, O and N, the story is otherwise, and we find that in the planetaries in the Large Magellanic Clouds, for example, in the small sample which we have, some have greatly enhanced N abundances at the same time as you have depleted O abundances. In all objects observed with the IUE in the Magellanic Clouds and in Fornax by the Goddard group C has greatly enhanced abundances. In the Small Magellanic Cloud, for example, it is up by a factor of 40 over the ambient medium. So what happens is that that these stars manufacture these elements, they manufacture C anyway, at the late stages of their evolution. Since we are observing giant stars (which are are destined in a short time to evolve, hopefully, into planetaries and certainly into white dwarfs and eject this material) we have to be awfully cautious in relating what has happened in the evolution of the star itself, and in comparing that with what we believed transpired in the medium from which the star is formed.

McCARTHY: Thank you very much, Professor Aller.
Would you please come up and mention your name so that we can get a good record. Oh! Nice to see you! (Laughter)

RENZINI: I'd like to mention a possible clue for the understanding of these CN variations among stars in a given cluster, field halo stars, and the presence of gaps on the HB of some clusters. One can easily verify that all clusters studied so far having HB gaps and/or CN bimodal distributions also have very high central densities. One can also theoretically estimate for each cluster the frequency of tidal collisions, and in the case of M80 one finds that about 10 % of stars have suffered one tidal collision during the cluster lifetime. Tidal collisions or close encounters can strip material and/or transfer angular momentum, which may account for HB gaps and for extra mixing in individual stars. A crucial test for this idea would be to observe the HB morphology and CN strengths in the high-density cluster M80, as well as in some very low-density clusters (e.g. NGC 5466).

McCARTHY: Thanks very much. Bob?

KRAFT: I would respond that, yes, I think that's a marvellous idea. The problem that I always have with this though, is why it is that M3 behaves itself? Is M3 a low density cluster?

RENZINI: Yes.

KRAFT: OK, that's good because one wanted a way to couple the abundance pecularities to some kind of thing that might produce some sort of spinup.

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McCARTHY: Dr van den Bergh.

VAN DEN BERGH: I have two short questions, one for Dr Hills and one for Dr Kraft. If it is indeed correct that the solution to the G-dwarf problem in the Galaxy is due to the contamination by grains ejected from the Virgo Virgo cluster, then one might expect Ar to be under-abundant in the youngest galactic stars because, of course, Ar does not condense on grains. I'd like to know whether or not that is observed? The other other question, to Dr Kraft, is: could it be that the peculiar N and C abundances in globular cluster stars are due to the fact that WN and WC stars are precursors to supernovae which occured in proto-globular clusters? In globular clusters the debris from these explosions might get trapped, whereas it might not get trapped in field stars.

McCARTHY: I'll ask Dr Kraft to answer the second question and for Dr Hills to come up and get ready to answer the first.

KRAFT: Well, yes, I think that's one of the distinct possibilities. Those would be examples of how you would produce the cluster contamination that might lead to the effects that were described. So the answer to that is, "it is possible", certainly.

HILLS: The question of the Ar, I think should be left to the observers. The question is whether, indeed, the Ar is not present or is underabundant in the very first contaminated stars in the Galaxy.

McCARTHY: Thank you very much. Now we have just time for two more questions.

INAGAKI: I'd like to ask to Dr Kraft: I'm interested in whether or not all the halo field stars are very old globular cluster stars? What do you think about that?

KRAFT: The question is whether or not the halo field stars are debris of globular clusters? Well, I'm sure I don't know the answer to that, but one of the things that one could say, perhaps, is that you could imagine that the field stars we see are the remains of globular clusters that have already dissolved in the Galaxy. So we are back to the Fall and Rees picture of globular clusters as survivors. The ones we see are the compact clusters that survive, and they undergo this self-contamination that produces these strange C and N abundances. Clusters that dissolve into the field are not so self-contamined because the densities were too low to begin with. The fact that M3 survives and looks like the field may only reflect the fact that at least in some calculations about the tidal truncation and tidal radius, M3 turns out to be on a circular orbit, if Peterson's calculation was right, so it survived the gravitational shocking as a cluster. So, yes, I think the answer to that is that these abundances might be a clue to the formation of field stars in the halo.

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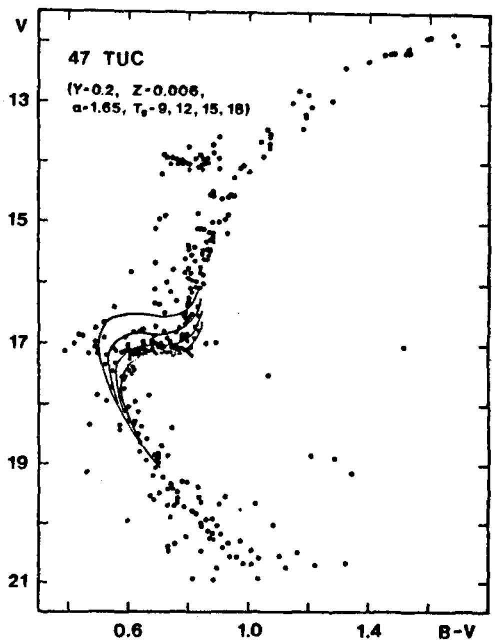
McCARTHY: Thank you very much and now the last question will be authored by Jim Hesser.

HESSER: I would like to comment on two aspects of Russell Cannon's paper. First I show a recently determined color magnitude diagram for 47 Tucanae which goes down to $M \sim +9$. The data from 17th magnitude to 21st magnitude were derived with the SIT vidicon detector at Cerro Tololo by Bill Harris, Bruce Atwood and myself; the data above 17th magnitude are the old photographic data from David Hartwick's and my study. The tracks are Don VandenBerg's isochrones. The comment is that Don's isochrones match the new color-magnitude diagram extremely well at the faint magnitudes. Our old photographic data tended to deviate in the same sense that Russell commented that most of the photographically determined color-magnitude diagrams deviate from the new tracks. I, myself, begin to suspect that it's the observational data and not the theoretical tracks that are in error at these faint magnitudes. The other comment concerns the age of the Magellanic Cloud clusters. From similar data obtained with the vidicon for NGC 2257 in the halo of the Large Magellanic Cloud, we have confirmed Linda Stryker's determination



B,V photometry obtained by Harris, Hesser and Atwood (1982, preprint) with the CTIO SIT vidicon on the 4-m telescope in six fields near 104 = 47 Tuc for stars with *V* > 17.2 mag are combined with Hesser and Hartwick's (1977 Astrophys. J. Suppl. 33, 361) p.e. + p.g. photometry for brighter stars. The photometry is compared with VandenBerg's (1982, Astrophys. J. in press) tracks for indicated helium (Y), heavy element (Z), mixing ratio (α) and ages (τ_9).

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of the turnoff, which she obtained by photographic techniques using the the RICHFLD program at Kitt Peak. With the vidicon we get an identical turnoff point from the visual magnitude luminosity function for NGC 2257, and I think that, within all the errors of interpreting such data, there can be no doubt that NGC 2257 is as old as M92, as she claimed.

McCARTHY: Thank you very much. I think that this is the conclusion of the first session.