

INTRODUCTION TO IMF@50

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Abstract I submitted my IMF paper in 1954 from a stay in Australia, but the seeds of the paper stem from the Ann Arbor Astrophysics Summer School in 1953. After reminiscing about the pros and cons of that paper, I also mention an aftermath at a Vatican Conference in 1957. I conclude with my advice to young scientists that they should NOT separate science and politics, but should be involved in national issues.

1. The 1953 Ann Arbor Summer School

I submitted my IMF paper almost exactly 50 years ago and I want to give some background to that paper. I should distinguish between “motivation” (why do you want to do it?) and “technique” (how can you do it?), but the two get mixed up and have multiple sources. The paper was written in 1954 during a one year stay at the (then almost brand-new) Australian National University in Canberra, Australia. However, I mainly have to talk about the beginnings in 1953 in the U.S.A. and a little about the aftermath at a Vatican Conference in 1957.

Early in 1953 I was preparing to write a book on “Energy Production in Stars” for Wiley/Interscience (I have missed the deadline of 1955 by a little already). At the time I considered myself purely as a theoretical nuclear physicist, not an astrophysicist, and the book was to be mainly on thermonuclear reactions plus nuclear photo-disintegrations. I expected that real astrophysicist would apply the results to real astronomy, but I also hoped that some physicists might read the book. For that purpose I felt I had to put some elementary astronomy in the book, including stellar structure and statistics, even though I knew little of that myself at the time. Learning some astronomy at the ripe old age of 28 was made easier by Martin Schwarzschild of Princeton giving patient and insightful answers to my many naive questions. Schwarzschild and Hoyle, both separately and together, had recently started to calculate stellar evolution away from the main sequence and into the red giant branch. It would take a

while before the calculations became fully quantitative, but it was already clear that stars leave the main sequence rapidly when they have burned roughly 12% of their hydrogen, almost independent of the mass.

My incentive to give myself a crash course in stellar astronomy in early 1953 was enhanced by the fact that I had to give a course that summer on “energy production in stars”, with similar aims to my planned book. My lectures were at the Ann Arbor Astrophysics Summer School at Michigan University. This summer school was probably the most important educational experience in my whole career, with brilliant and trustworthy “bigshots” like Walter Baade and George Gamow, but also youngsters like myself (see Fig. 1).

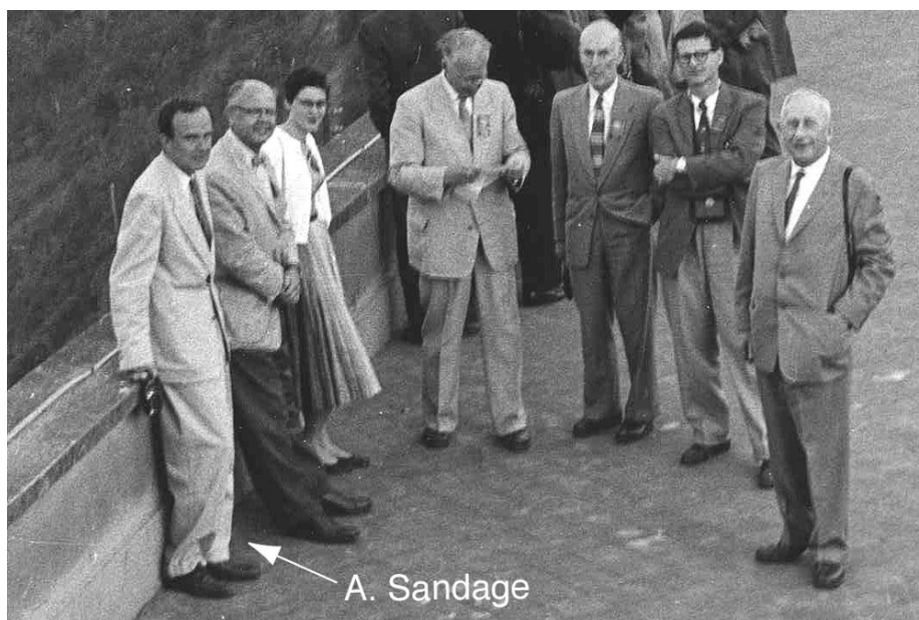


Figure 1. Some participants at the Ann Arbor summer school, 1953. Ed Salpeter is the second one from the right.

As Oppenheimer once said in a different context: “What we don’t understand, we explain to each other”. That summer school was instrumental for my IMF paper the following year in at least two ways. One pedagogical impetus came from my wanting to put together in one enormous Table, both for the book and the lectures, all the properties of main sequence stars as a function of mass M from the smallest to the largest. These properties had to include both visual and bolometric luminosity, central and surface temperature, and the radius. I used the observations and calculations then available, but I mainly had to use *Chuzpah*, or gall or guts, or unashamed guesswork. The real experts in the field would not have wanted to publish such a Table, since they knew the

enormous uncertainties at the time. As a mere outsider I did not mind making enormous extrapolations and guesses. Another quantity I put in that Table in 1953 was $\tau_H(M)$, the hypothetical lifetime a star of mass M would have if it could burn 100% of its H-mass at constant luminosity. It was not difficult for me to postulate, one year later, that the actual main sequence lifetime $\tau_{MS}(M)$ is simply $0.12 \times \tau_H(M)$.

Walter Baade explained to us simply and forcefully about the two stellar populations, with all population II stars of the same age, essentially equal to the age T_0 of our Galaxy, which was then estimated to be about 6 billion years old. The uniqueness of the stellar ages explained the sudden upper turnoff of the population II main sequence, but Baade told us that population I stars had all ages from zero to T_0 . I had also vaguely wanted to teach myself about stellar statistics for Ann Arbor, but had not gotten around to this yet. However, I now got a very strong impetus and motivation, even though in a negative way, from George Gamow.

Gamow was giving a course of lectures at the Ann Arbor summer school, talked about almost anything in any branch of science (even on the genetic code) and was most stimulating throughout. He (and his younger colleagues Alpher and Hermann) had already elucidated how to make the very light elements early after “The Big Bang”. He was still hoping to make all the elements up to iron in a big bang scenario, although he knew it was difficult. In particular he did not believe that these elements were made in population I stars. The following is the most relevant paragraph of Gamow’s lecture notes, verbatim but with my underlining: “Gamow considered the possibility that population II stars have original abundances of elements, and that population I stars have a mixture of elements which includes the original abundances and the abundances of elements formed in stars. This theory is excluded, however, by the observation that not enough stars have contributed much to interstellar matter during the age of the universe. The interstellar matter is of original pre-stellar composition”.

Of various kinds of motivations, a powerful one happens when an expert, whom you otherwise trust, makes an assertion which you do not believe. I just did not believe that the birthrate of massive main sequence stars (which are needed to make medium and heavy elements) over the last 6 billions was negligibly small. For my big main sequence Table I had already estimated (also very approximately) the main sequence lifetime as a function of mass and it was quite short for the very massive stars. However, to calculate an Initial Mass Function, I also needed observational data on the main sequence luminosity function.

2. The 1954 IMF Paper

Very soon after the Ann Arbor summer school I flew to Australia, with my wife Mika and new-born daughter Judy, to spend a year at the Australian National University (ANU) in Canberra. This new University was located in down-town Canberra, did not have an Astronomy Department and I was in the Physics Department. Fortunately, the Mt. Stromlo Observatory, although not officially associated with the ANU, was fairly close geographically and I made many trips to its library to learn about stellar statistics.

One curious bottleneck was the confusion between the main sequence (MS) luminosity function and the red giant (RG) luminosity function. The visual luminosity function was already known fairly well observationally, for MS and RG combined. The colors of the two types of stars are radically different, so two-wavelength photometry could distinguish them easily, but combining this with painstaking statistics counts was tedious. One frightening warning came from observations of stellar population II, where photometry had already shown a sharp upper cut-off to the MS (not counting the small number of blue stragglers): The *total* visual luminosity function for population II above the cut-off, although coming purely from RG (plus the horizontal branch), was qualitatively similar to that for population I, which included the MS! It took a particularly large dose of *Chuzpah* for me to estimate what fraction of the population I luminosity came from main sequence stars, but that estimate was not too far from the actual fraction.

I am somewhat ashamed of two other pieces of *Chuzpah*, or plain sloppiness:

- 1 Although it was already known that the distribution of stars perpendicular to the galactic plane depends on stellar mass, I ignored that fact. I simply made no real distinction between the luminosity function within a fixed distance from the Galactic plane and that for the extended galactic disk.
- 2 My worst sloppiness was to assume that the absolute rate of star formation has been constant over the last T_0 years. Assuming a constant rate per existing gas mass would have been more reasonable, even at the time, and would have led to a gas mass exponentially decreasing with time. The later discussions by Maarten Schmidt and others that the star formation rate may depend on an even higher power of gas column density was “beyond the scope” of my paper, but I could easily have handled an exponentially decreasing gas supply. For that calculation, I would have needed an observational value for the present-day ratio of gas to star masses, but again I was scared about not knowing how things changed with distance from the Galactic Plane.

There was a third piece of *Chuzpah* I was proud of 50 years ago and I am even prouder of today: As mentioned, the age of the Galaxy T_0 was then thought to be about 6 billion years, more than a factor of two too small. With $\tau_{MS}(M)$ the main sequence lifetime as a function of mass M , the values of mass and the “turn-off magnitude” where $\tau_{MS}(M)=T_0$ was obviously important for my calculation. The main idea of my paper was to say that, for brighter population I stars, the total initial luminosity function is larger than the observed one by the factor of $T_0/\tau_{MS}(M)$. Of course I was worried that the 1954 value for T_0 , as well as my function $\tau_{MS}(M)$, were inaccurate, so I invented a “fudge factor”. This fudge factor effectively eliminated the numerical value of T_0 from the calculation and instead took an empirical “turn-off magnitude” from the observational data for stellar population II.

For comparison with theories of star formation the shape of the IMF itself is most important, but I was more interested in two applications. The most important for me involved the integral of IMF times stellar mass, which showed that the mass from all stars that have died (presumably now in the interstellar medium) almost equals (about 80%) the mass in existing stars. Although I did not mention George Gamow, this near equality was my negation of the paragraph I quoted above. The other result involved the integral of the IMF itself and showed that the number of stars that have died is about 12% of the number of existing stars. We did not think of neutron stars or black holes in those days, so I identified those “dead stars” with present day white dwarfs. Since white dwarfs were estimated to constitute about 10% of all existing stars, I was rather pleased with this result.

3. The 1957 Vatican Conference

A definitive aftermath to my 1954 IMF work was a conference at the Vatican in May 1957 on “Stellar Populations”. The theoretical discussion was dominated by Fred Hoyle, but the observational half was more important in my opinion. Much of what the conference did was to vindicate Walter Baade’s general ideas on the two stellar populations, but the most important observational talks were by Allan Sandage (see Fig. 2).

Sandage himself had done much definitive work over the previous few years and some of this impinged directly on the IMF. The main sequence and its turn-off for stellar population II globular clusters was of course well known already in 1953, but by now it had also been observed for a number of population I star clusters of various shorter ages. Fig. 3 is a modification of one of Sandage’s figures at the Vatican conference.

Once we had the main sequence for a young cluster, the observed luminosity function for the cluster of course gave the initial luminosity function up to a certain mass. Compilations for many young clusters thus gave fairly direct



Figure 2. Inside the Vatican, May 1957. Ed Salpeter is in the center of the front row.

evidence that my IMF was correct at least in a very qualitative way, but now opened up the possibility for more quantitative work. The related question of the dependence of the star formation rate on interstellar gas abundance was also discussed.

I did not pay much attention to theories of star formation after 1957, partly because it was - and still is - such a difficult problem. Partly, my reluctance to tackle the theory stemmed from my belief that more progress would come from observations. In particular, although I was hoping that my IMF was roughly

right on the average, I expected that it would vary extremely strongly with varying conditions. For instance, I (and others) thought that massive stars would be strongly favored in regions of strong turmoil and possibly in regions of high gas column density in general and the young Galaxy in particular. These are just the controversies which we will be debating here, and there surely will be variations but just what is still not clear. This uncertain state of affairs has been good for me personally over the last 50 years - in the absence of clearcut answers, my IMF still gets quoted! However, this absence has been bad for the theorists - we need clearcut variations to decide between rival theories.

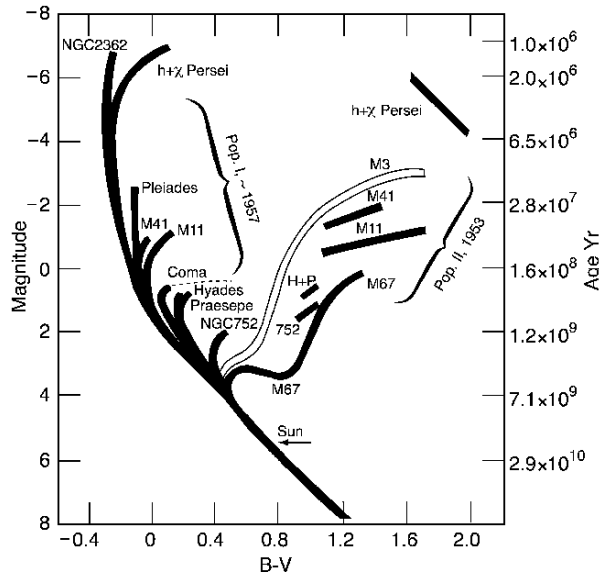


Figure 3. Adapted from a slide by A. Sandage, May 1957.

4. The ANU versus Cornell and the threat of fascism

During my year 1953/54 in Canberra I had to decide between two Universities. I had been offered the newly established Chair of Theoretical Physics at the Australian National University, but I had also been offered tenure back at Cornell University. Although I submitted my IMF paper from the ANU, this paper actually pushed me towards Cornell: Theoretical physics meant quantum electrodynamics and High Energy Theory in those days and as the chairman of a department (albeit a small one) I just could not goof off into Astronomy. As I mentioned earlier, I had thought of my previous nuclear astrophysics work as basically theoretical physics, to be applied to astronomy by others. My IMF

work, on the other hand, was getting me into real astronomy and by now I was hooked. At Cornell I was also in a physics department, but the atmosphere was more flexible for branching out into other fields.

There were other considerations besides my own purely academic ones. My late wife, Mika, had just gotten her Ph.D. in psychobiology, so she was at the beginning of her career. Unfortunately, male chauvinism was pretty bad at the time both at the ANU and at Cornell, but a little bit less so at Cornell. There was also real politics, with Joe McCarthyism pretty rampant in the U.S.A. just before we came to Australia. There was no equivalent political hysteria in Australia in the early fifties, but the “White Australia Policy” sounded pretty racist. In the middle of 1954 I judged that McCarthyism was on the way out, but the White Australia Policy was likely to last a long time. So, on the political side also we opted for the USA and we returned permanently to Cornell.

The political decision in favor of the USA is not so clearcut with 50 years of hindsight: Joe McCarthyism did indeed disappear fairly quickly, but the disaster of Vietnam took its place and, on the other hand, Australia’s immigration policies became more benign surprisingly quickly. Although I am still not sure of my wisdom 50 years ago, I am sure of my advice to young US scientists for the future :

Work hard on star formation, but work even harder on getting involved on science policy and national technical issues, which will help to maintain democracy. I may be a minority of one in advocating that one should NOT separate science and politics—partly because I am old enough to remember the Weimar Republic before 1934: Citizens there were not against democracy, they just did not want to get involved in politics, so they lost democracy and gained World War II. The end result was particularly disastrous for science: Germany’s industry recovered surprisingly quickly after the war, but basic science took very, very much longer to recover. Let us not allow politics to demolish democracy and basic science now or in the future.