Secondly, my work provided early support to Fowler's strategic goal of making Kellogg Lab a hotbed of nucleosynthesis theory. Understand that very little nucleosynthesis theory had been attempted in Kellogg Lab when I joined that effort in 1957. That newness is not widely appreciated, even within the science community. The common assumption being that, because the new review paper (B^2FH) coauthored by Fowler was to become quite celebrated in nuclear astrophysics, Kellogg Lab must have been at that time quite active in nucleosynthesis theory, just as it had been in the laboratory measurements of nuclear reaction rates for the stars. But such is not the case. I had been the first research student taken on by Fowler to work on nucleosynthesis *theory*, as opposed to *laboratory* measurements of relevance to nuclear astrophysics. I was Fowler's wedge for creating a Caltech presence in the theory of the origin of the elements. Without that transformation, the theory of nucleosynthesis would remain Hoyle's theory, a product of Cambridge University. Willy wanted that leadership for Caltech. Oblivious myself to such issues of institutional reputation, I did help to make nucleosynthesis theory a Caltech specialty.

Thirdly, my creation of a quantitative description of the time-dependent abundances produced by neutron captures in the stars was a major step in the transition within Kellogg Lab. My work made it possible to calculate numerical answers for the *s*-process abundances starting from iron, a process that is responsible for the natural abundances of about 120 of the stable isotopes of the elements heavier than iron. I showed that these were created in low-mass stars as a byproduct while they are fusing helium into carbon. The quantitative theory, in contrast to simply its phenomenological ideas, was such a numerical success that the origin of our elements in stars was an idea now beyond doubt. What my Ph.D. thesis calculated was the temporal evolution[5] of *s*-process abundances. That new approach to the *s*-process abundances sought those superpositions of the time-dependent solutions that I had found that could, when summed together, reproduce the solar abundances. It was in those superpositions that the astrophysical circumstances lurked.

This opportunity was my good fortune and its success made my subsequent career possible. I had been given the chance to become a pioneer of nucleosynthesis by giving better definitions and time-evolving quantitative treatments to the static outline in (B^2FH). The structure of thermonuclear nucleosynthesis in stars, which had been introduced by very important papers by Fred Hoyle in 1946 and 1954, had not been improved upon until Fowler, with his inimitable infectious enthusiasm, created a bandwagon for nucleosynthesis theory. His first step had been publication in 1957 of the review paper by Burbidge, Burbidge, Fowler and Hoyle (B^2FH). This review achieved iconic status by being cited *pro forma* by future works as a catchall for the idea of nucleosynthesis in the stars. It came to be called simply B^2FH among astrophysicists, the square of the *B* standing for both Burbidges, and began a trend in journals by which influential works could be cited *by acronym*. B^2FH had been submitted as a review paper for the ideas of nucleosynthesis in stars, albeit with considerable new material concerning the synthesis of the heavy elements. A *review* paper is one submitted to a review journal to summarize the state of a research field. It had done that profoundly well for the elements heavier than iron; but, as I slowly realized[3] during the following decade, did not review Hoyle's picture for the origin of the common elements lighter than iron. This weakness went unnoticed as nucleosynthesis developed. B^2FH became a science icon, as well as the default citation for new researchers entering this field. Fowler described how Ed Condon accepted it for *Reviews of Modern Phys*ics without peer review, causing Fowler to quip, "Those were the days".

My primary research accomplishment had been making the s process something that could be calculated. To share what this means requires some technical discussion. The B^2FH treatment of the *s* process had focused on nuclear correlations between abundances in solar system material (earth and meteorites). It had not presented a calculable theory. It described a simplified correlation between the abundances of the isotopes of the elements and the nuclear properties of those isotopes. It did so by assuming the existence during *s*-process nucleosynthesis of a steady-state situation in which the abundances did not change with time[5]. They sought the values of abundances required in order that their values would not change during passage of time in a fixed environment. Each isotope was to be created at the same rate that it is being consumed, and that balance implied a specific value of the abundance of each isotope. That balance also required that every isotope in the chain be destroyed at the identical rate. One single destruction rate would characterize each of the 120 *s*-process isotopes. These were very restrictive assumptions, and B^2FH had made them for both the *s* and the *r* processes. The mnemonics *s* and *r* stand respectively for the *slow* capture of free neutrons in the star and the *rapid* capture of free neutrons in a separate stellar setting. Owing to the assumption that the abundances were not changing with time, their treatment could not address larger astrophysical questions. In the real world abundances do change with time. Their description, by assuming that each isotope was destroyed during the *s* process at the identical rate, required that the abundance must be inversely proportional to a purely nuclear property, the neutron-capture cross section. Those isotopes having smaller capture cross section must have larger abundance, and conversely. This inverse correlation was approximately correct over a small range of atomic weights, as my work confirmed it should be, but not correct over a large range of atomic weight. The inverse correlation of abundance with neutron-capture probability was also not new, although posterity tended to attribute it to B^2FH because they clearly expressed an astronomical context for it.

The time-dependent nature of nucleosynthesis was acknowledged by B^2FH only by another physically incorrect assumption; namely, that the actual rate of destruction of each isotope would decline smoothly with increasing atomic weight (instead of maintaining a constant value). They drew a cartoon-like smooth curve to illustrate that expectation. That sketch was not based on any calculation, as many believed. It was a hunch. They did not address whether this global decline happened in individual stars or was the net product of galactic growth of element abundances. Like much of B^2FH, one could not calculate abundances with their description.

Because observations by Margaret Burbidge, by Jesse Greenstein, and by others showed that *s*-process barium was enriched perhaps 20-to-50-fold in some red giant stars, Fowler had asked me if I could build a quantitative theory of the *s* process that would show if fifty-fold enrichment of barium is reasonable within a single star. Large overabundances could not be produced by merely postulating equal destruction rates for each isotope. Rather it required transmutation during time of some abundant seed nucleus to atomic weight A=138, the atomic weight of barium. Realizing this, Fowler asked me if neutron irradiation of iron, which was much more abundant than barium in the stars, could produce a large overabundance specifically of Ba. That was the speculation stated in B^2FH. We discussed that this would require a time-dependent *s*-process theory that could compute the spreading in atomic weight of the seed abundance as it moved toward larger atomic weight. I had welcomed that challenge, thankful to this day for Willy's having taken me along to Jesse Greenstein's colloquium.

I later years I explained how I generalized the *s* process by an analogy to the populations of insects. One can ask what the populations of insects would be if they exist in a steady state, that is if their populations are to remain unchanged as time passes. That requirement requires that the birth rate of each species be exactly balanced by its death rate. Only then can the population be unchanging. In that case one can calculate what the abundance (population) of every insect would be. It would be proportional to the birth rate of each insect (number born per year) and also proportional to the average time that each insect lived when all causes of mortality are taken into account (natural death, predation, disasters). When ecologists observe insect populations, however, they find something quite different. Many species go through dramatic population cycles— two slim years followed by a year of decided overpopulation, say, or in the case of the cicada, a seventeen year cycle! To understand these things, population biologists had to consider the time-dependent situation. They had to discard the simple idea that birth rate balanced death rate. They had to consider how the changing environment and the changing population of one insect influenced that of another. That sophistication was momentous for population biology. This is similar to what I achieved in my formulation of the *s* process (and soon thereafter of the *r* processes) of nucleosynthesis.

I had succeeded in answering Fowler's questions. My thesis devised new analytic solutions of the transport and spreading of nuclear abundances as time passed, giving the Ba overabundance produced by neutron irradiation of iron as a function of time. The answer was that Ba overabundances as large as 10,000 could in principle be achieved from such irradiation, but only if the neutron exposure had occurred for precisely the optimum time. But for other durations of irradiation the Ba abundance varied by huge factors. A second problem for the optimum neutron exposure was producing too many of those heavy abundances having atomic weight $A>130$. So such special history was ruled out. I was able to demonstrate that no single neutron exposure could approximate the solar abundances. The iron in galactic history had been exposed to varying numbers of free neutrons, irradiations that took no single value but instead covered a range of exposures[4]. I showed that this range had the requirement that the fractions of iron that had been irradiated had been smaller and smaller for increasingly larger values of the neutron exposure. And most iron had not been exposed to neutrons at all.

As a result I showed that the destruction rate of each isotope was not constant as a function of atomic weight, as B^2FH had assumed, but had regions of near constancy between the so-called magic numbers of nuclear structure, N=50, 82 and 126. And the destruction rate declined steeply in the vicinity of each magic number. It soon became apparent that the natural abundances showed exactly that same "ledge-precipice structure", as I dubbed it[4]. I had shifted the emphasis to the distribution of neutrons to which Fe has been exposed during the natural evolution of stars. By requiring a distribution that could produce the solar *s* process abundances in each red-giant star, I concluded that the Ba overbundance was about equal to 50, just as required by astronomical observation. Willy was ecstatic with these new insights, bragging about my work to others, "It's a solved problem"