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Adventures in the unknown: A path of astronomical research and discovery

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1. Introduction

The path through the unknown of science takes many unexpected twists and turns and heads off in unexpected directions. As a graduate student at the Harvard College Observatory, 1945–1949, I then had little interest in comets or chemistry. This was the period when Fred Whipple was developing his icy conglomerate model of the cometary nucleus. Little did I realize the influence that work would have on my future research.

2. Early career

I had a background in physics, an AB degree and several years' experience in physics-related jobs including four years of wartime research in optics and radar. At the end of World War II I decided this was the time to follow my interest in astronomy. Harvard College Observatory was my choice for graduate study. At that time Harvard had one of the few graduate programs in astronomy. Another feature that appealed to me was that three members of the astronomy department, Professors Bok, Shapley and Mrs Gaposhkin, were actively involved in social and political issues of the time. After four years of war research I had no desire for an ivory tower existence.

Interstellar matter, a relatively new field, posed a number of fascinating problems and was an active field of research at the time. At Bart Bok's suggestion I began an observational program on interstellar extinction and reddening in clouds. However, the observing program did not appeal to me as much as questions dealing with the nature and origin of interstellar grains responsible for the extinction.

A prevalent idea at the time was that grains would be composed of the abundant reactive species, hydrogen, carbon, oxygen and nitrogen. H.C. van de Hulst (1949) proposed a formation mechanism whereby atoms were trapped on condensation nuclei at the very low temperatures of interstellar clouds. They then would diffuse over the surface and recombine to form molecules as water, methane, ammonia and carbon monoxide.

As I investigated these phenomena, it appeared unlikely that the proposed processes would occur. Although data was very limited, there was a variety of laboratory experiments which indicated that the reactions postulated by van de Hulst would not occur as he predicted. One part of my thesis dealt with condensation and reactions of atoms and free radicals on surfaces and was critical of the prevailing theory of grain formation. This work started my interest in astrochemistry.

During the period when I was working on my thesis, Bok, who was my principal advisor, left Harvard for three years to carry on observations of the Milky Way from the southern hemisphere in South Africa. This turned out to be a blessing in disguise. It forced me to make my own decisions about how to proceed in my study of interstellar matter. An important aspect was that his absence left it to me to make contacts with other scientists whose work might be relevant to my research. I soon discovered that this process was not as painful as I imagined it would be. Making decisions on how to proceed and initiating contact with others were valuable assets which I could continue throughout my career.

From Harvard I took a position in the Physics Department at Wayne University (now Wayne State) in Detroit where I continued working on interstellar grain chemistry. When Harold Urey came to Wayne to receive an honorary degree, this presented a good opportunity to discuss problems of low temperature chemistry in an astronomical context, as Urey had been working on chemical problems of the formation of the solar system (Urey, 1952). He was interested in discussing my work on interstellar chemistry and we had a productive discussion. The most significant outcome was his conclusion that this was an interesting problem and it would be good if I could come to Chicago and continue working on it in his laboratory. We prepared a proposal for the National Science Foundation and eventually received a grant to carry on the research. Thus was my career in astro-

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chemistry forever determined although my formal training in chemistry was almost nonexistent.

However, before the grant was finally approved I had to decide whether to take a leave of absence from Wayne and move to Chicago with my wife and three children without any assured means of support. Urey had left for a year to become a Visiting Professor at Oxford University. The chance to hold a research associateship under Urey was a once-in-a-lifetime opportunity and not readily passed up. We moved to Chicago in September, 1954 and were supported by my brother Bill Donn, for several months until the NSF grant came through.

3. The beginning of astrochemistry

As is often the case, serendipity played a large role in my future research. Urey had called my attention to work going on in Francis Rice's laboratory at Catholic University. Rice and his students were dissociating molecules as ammonia and methane in electrical discharges and condensing the fragments on glass surfaces with liquid nitrogen at 95 K. Professor Rice told me how this program came about. Several graduate students were working in the lab while he had left. As students sometimes do, they began fooling around with the liquid nitrogen. Some dropped on a discharge tube in which ammonia was being dissociated. A bright blue film was immediately deposited where liquid nitrogen had dropped on the discharge tube. At this point Rice returned to the room. The students' carryings-on were immediately overlooked while everyone examined the unexpected blue deposit. An investigation of this phenomenon became a major part of Rice's research program. One characteristic of the brightly colored deposits, depending upon which molecules were dissociated, was that they were energy-rich and would sometimes explode during warm-up.

At nearly the same time, in the Free Radical Program initiated by Herbert Broida, at the old National Bureau of Standards (NBS) now National Institute of Standards and Technology (NIST), similar experiments were being carried out. Liquid helium at 4–10 K was used to condense discharge products of common gases as in the Catholic University experiments. Early conclusions based on both sets of experiments reported that approximately ten percent trapped radicals were contained in the frozen deposit.

These experiments indicated that with laboratory techniques then available, experimental investigations of interstellar phenomena could be carried out. Such a program was started at the University of Chicago. While I was developing this approach, Urey asked the question whether atoms and free radicals might be trapped in comet ices. If so, as they approached the sun and warmed up, preliminary conclusions from trapped radical experiments suggested that considerable energy could be released. This could account for a number of cometary phenomena.

4. Research on comets

We carried out calculations on the possible energy release and observable consequences. The results were encouraging and the work was reported in three papers (Donn and Urey, 1956a,b; Urey and Donn, 1957). One application was to the unexplained outbursts of Comet Schwassmann II at about six A.U.

Estimates of cometary temperatures presented a serious problem to trapping radicals in comets. At that time temperatures were obtained by balancing heat input from the sun against heat loss by reradiation assuming comets behaved approximately as black bodies. This yielded the relation $T \approx 300\, r^{-1/2}$, where r is the heliocentric distance. At the Earth's orbit, temperatures would be about 300 K, much too high to prevent trapped radicals from recombining. Few energetic species would survive within the orbit of Saturn.

As I looked for a possible way around this obstacle to the hypothesis, I realized that comets are not mearely black bodies. The effect of energy input from the sun was to cause the volatile ices to evaporate. This is a well known effective cooling process (Donn and Urey, 1956). When the temperature was calculated as a balance between solar heating, evaporative cooling and reradiation, it turned out that the result depended primarily on the volatility of the ices. The heliocentric distance had only a minor effect. For a comet with the average volatility of ammonia, the temperature at 1 A.U. was 125 K. More detailed calculations have since been published (Squires and Beard, 1961: Watson et al., 1963). Thus, atoms and radicals could not be trapped in the cometary matrix as the experimental limit for trapping was about 35 K. However, unsaturated, stable molecules as H_2O_2, and C_2H_2 which also have high heats of reaction, were suggested as possible energy sources.

Not long after this work was published, detailed investigations, mainly at NBS, showed that less than one percent atoms or radicals could be trapped even at liquid helium temperatures. Although the basis for the hypothesis, high energy storage, was no longer tenable for comets, the occurrence of trapped radicals and unsaturated molecules has an important role in interstellar grain chemistry (Donn and Jackson, 1970). This phenomenon has been studied and exploited particularly at Leyden by Mayo Greenberg and his associates.

Later research on comets showed that the problem was more complex. The surface of the comet is not a uniform mixture of ices and dust. Instead there are a few active, icy areas from which vaporization occurs. Most of the surface is inactive, hotter areas. The temperature is not well defined and the comet's behavior not readily determined.

About this time I received an invitation from Dr Broida to join the NBS Free Radical Program. Although I decided not to become a full-time permanent member, I agreed to spend summers with the program and serve as consultant, with monthly visits. This added to my experience with low temperature, atom and free radical chemistry.

A major outcome of the cometary trapped radical hypothesis was to get me actively engaged in cometary research. In following up that proposal I became aware of the many fascinating and significant problems associated with comets. As a result, I have retained my interest in comets to this day.

5. Interstellar grains again

In 1957 I returned to Wayne University. There I set up a laboratory to study low temperature chemical processes of interest in astronomy. Not long afterward, Gerald Sears came to Wayne to give a physics colloquium on crystal growth and whiskers, then a lively research subject. Discussions with Sears on problems of interstellar grain formation resulted in his pointing out that under the extremely low interstellar densities, grains are not likely to be compact, equiaxed, nearly spherical particles. A more plausible structure would be whiskers. These are one- or two-dimensional aggregates, defect free, that have one to two orders of magnitude greater strength than normal bulk solids. Defects in the crystal structure are sources of weakness which reduce the strength of the material. Their absence makes whiskers much stronger. Whisker-like structures also have a large surface to volume ratio which increases their aerodynamic drag.

In applying these properties to the accumulation of planets and comets (Donn and Sears, 1963), several features were pointed out. Grains would be efficiently carried along by the gas and could only collide with other grains in the same turbulent element and thus would have small relative velocities. This favors sticking. Because of their shape, this process leads to porous, low density objects. Such a structure would be an effective trap for larger colliding objects, producing efficient growth.

Dr Sears' untimely death ended this collaboration. His knowledge of crystal growth and skill at carrying out experiments would have added greatly to our understanding of the formation and structure of interstellar grains.

A model for the cometary nucleus based on the concepts introduced by Sears was developed (Donn, 1963). When the accumulation of whisker-like grains was combined with data on the compaction of snow and ice, the result was a comet nucleus with a predicted density of 0.4 g/cm^3. This mechanism was exploited later (Donn, 1990) in a more detailed analysis of comet formation and structure. This analysis supported the earlier result of a density for the nucleus of about 0.5 g/cm^3 but with a heterogenous structure having variable density and composition. Densities of 0.5 or lower were subsequently derived by Rickmann et al. (1987) and Rickmann (1989) by combining nongravitational parameters with jet forces obtained from gas production rates. Peale (1989) by a similar method obtained a value of 1 gm/cm^3 with a large uncertainty, thus not in contradiction with Rickman's result.

6. NASA and astrochemistry

1957 was the year of Sputnik, the first artificial satellite, launched by the Soviet Union. Thus began the Space Age and the start of the U.S. National Aeronautics and Space Administration (NASA). Getting some firsthand experience with space research seemed like a good idea to me. The first NASA center was the Goddard Space Flight Center in Greenbelt, Maryland. A number of scientists doing space research with German V2 rockets at the U.S. Naval Research Laboratory joined Goddard and started a program there. In 1959 I also joined Goddard with the intention of staying a year or two before resuming an academic career with additional knowledge of space science and space techniques. As it turned out, it was thirty years before I retired from Goddard in 1989.

The attraction for me at Goddard was not so much the space program but the support for astrochemistry that NASA and Goddard would provide. An indication of this support was that the Center established an Astrochemistry Branch for which I became Branch Head. This was probably the first explicit recognition of Astrochemistry as a distinct scientific discipline. I was still interested in being at a university where there would be interaction with students along with an active research program. However, I did not find any university that showed much interest in an astronomer who wished to carry on research in chemical problems related to astronomy.

The Goddard Astrochemistry Branch, for perhaps the first time, provided an opportunity for astronomers and physical chemists to collaborate closely on a continuing basis. When William Jackson, with a doctorate in physical chemistry, joined the program, conditions were ripe for studying a variety of chemical problems relating to comets. The importance of reactions in the coma were examined jointly (Donn and Jackson, 1966; Jackson and Donn, 1968). Jackson then went on to develop a laboratory program to study photochemistry and comets. Together we showed that icy grains would be transformed by photon and cosmic ray irradiation in interstellar space and their composition drastically changed (Donn and Jackson, 1970). Shortly afterward, Michael Mumma joined the Astrochemistry Branch as an ultraviolet spectroscopist. As he became interested in comets, Mumma realized that infrared spectroscopy was the way to study cometary molecules. He has since made major contributions to infrared spectroscopic instrumentation and using these instruments has been making significant contributions to cometary studies (Mumma et al., 1993).

While at Goddard I had three doctoral students and an occasional masters student working with me. This provided some interaction with students that I missed by not being at a university. I also had visiting appointments at the University of Maryland and at Catholic University.

In the interpretation of interstellar and cometary grains, the basic assumption that has been made is that one is dealing with spherical particles. Although grains in nature are not likely to be spherical, spheres do have the convenient property that scattering and extinction can be accurately calculated by the use of Mie Theory. In order to test the reliability of the results, I initiated an experimental program with Robert Powell, a Catholic University graduate student (Donn and Powell, 1963; Powell et al., 1967) to compare the results of Mie Theory calculations with the measured scattering from well characterized nonspherical particles. Grains condensed from magnesium oxide and zinc oxide smokes were studied. The former yield cubes and the latter are described as fourlings. These resemble four spikes emanating from a single center.

7. The cometary nucleus

The next major development occurred when Jurgen Rahe received a National Research Council Research Associateship to spend two and a half years working with me. Together we continued his thesis work with additional studies of the ion tails of comets (Rahe and Donn, 1969; Burlaga et al., 1973). About a year later, Karl Wurm joined us as a Senior Research Associate. The major collaborative output was publication of an 'Atlas of Cometary Forms: Structures near the Nucleus' (Rahe et al., 1969). We also prepared a review of the origin and structure of the cometary nucleus (Donn and Rahe, 1982).

8. Comets and space research

Space science made its first contact with cometary research on January 14, 1970. I called the attention of Arthur Code and the Wisconsin group, who were the investigators of the Second Orbiting Astronomical Observatory (OAO-2), to the apparition of the bright comet Tago-Sato-Kosaka, 1969g. Several discussions of the vacuum ultraviolet spectra of comets were held. It appeared likely that the spectrum could be detected and several observations were successfully carried out (Code et al., 1972). Intense Lyα and OH 3080Å were observed in addition to weaker expected features.

Under William Fastie, a Johns Hopkins group had been conducting rocket ultraviolet spectrophotometry of the upper atmosphere. In the course of their visits to Goddard, I became acquainted with them at about the time that Comet Kohoutek was approaching the sun. We discussed the prospects of obtaining cometary spectra during a rocket flight and an attempt was made. The results were similar to the OAO-2 spectra but at higher resolution. Ultraviolet spectra were obtained over the next 15 years by a variety of instruments, mostly on rockets under the direction of Paul Feldman.

With the launch of the International Ultraviolet Explorer satellite (IUE) in 1978, cometary observations could be carried out by the same instrument in a systematic fashion. This enabled reliable comparisons to be made of cometary spectra and, from them, molecular abundances. The first IUE observation of a comet was for Comet Seargent, 1978M (Jackson et al., 1979). Because IUE observations could be made closer to the sun than was possible from the ground, this was the first spectrum of Comet Seargent that was obtained.

The development of the manned space program in NASA provided another approach to observing comets in addition to the use of unmanned rockets or satellites. I was involved in three attempts to use manned spacecraft for cometary research. This program was carried out in collaboration with Larry Dunkleman. In each case something unfortunate occurred which prevented the program from being carried out.

The discovery of comet Kohoutek coincided with the first attempt to rendezvous two spacecraft in earth orbit. Plans were made to have the astronaut continually photograph Comet Kohoutek during orbital flight. However, the target rocket did not get into orbit and so the second, manned launch, was canceled. In 1976, the plan for a Gemini spacecraft flight to the moon coincided with the appearance of another bright comet, Comet West. Again, an even more detailed observational program was prepared for the two astronauts. This launch was the near disastrous Apollo 13. Needless to say, the astronauts had much more on their minds than observing a comet. In 1986, a third opportunity to study comets from space occurred. NASA planned to observe the long awaited apparition of Comet Halley with a specially instrumented version of Skylab designed to carry out a number of stellar observations. The launch immediately prior to this was the Challenger mission, which exploded shortly after lift-off with the loss of the seven astronauts. All manned launches were canceled for the next two years while the cause of the disaster was investigated, to prevent a repetition. These three experiences ended my efforts for cometary observations from manned spacecraft.

9. The Cornell period

In the fall of 1974 I had a year's leave of absence from Goddard to serve as Visiting Professor of Astronomy at Cornell University. This was an extremely productive period for two reasons. It took about a month for me to grasp that at Cornell the time was my own and I was really free of all the chores and interruptions that filled my time at Goddard. In addition there was the opportunity to interact with the Cornell faculty in Astronomy, Physics, Chemistry and Material Science. Particularly valuable were discussions with Edwin Salpeter in Physics and Simon Bauer in Chemistry. I could now concentrate on a problem I had been thinking about off and on for some time at Goddard.

This was the problem of the formation of grains from atoms and molecules in circumstellar shells around late type stars. It is here that observations indicate grains form and are then ejected into interstellar space. Prevailing theory used thermodynamics and classical nucleation theory. However, under the low densities in these regions, extreme disequilibrium prevails. How does one treat grain condensation from a cosmic distribution of elements under these conditions? It seemed clear to me that, as in the case of interstellar molecule formation, the procedure to use is chemical kinetics, not thermodynamics. A critique of the problem and an analysis was developed (Donn, 1976a, 1979).

Not long after returning to Goddard from the year at Cornell, Kenrick Day from the University of Arizona's Lunar and Planetary Laboratory received a Summer Faculty Fellowship to work with me. He had been experimenting with sodium silicate grains formed by precipitation from solution. At Goddard we assembled apparatus to condense them from a vapor. The results were formation of amorphous sodium silicate grains similar to the precipitates. Vapor condensation also required a high supersaturation (Day and Donn, 1978a,b) These experiments opened a continuing field of expanding experimental and theoretical research on grain formation (Khanna et al., 1981; Donn et al., 1981; Nuth and Donn, 1981, Nuth et al., l988). The vapor condensation experiments and their follow-up was continued by Nuth, as a graduate student, as NRC Research Associate and as a member of the Astrochemistry Branch staff. A satisfactory theory of grain formation still does not exist.

A second experimental program, this one directly related to comets, also developed from the year at Cornell. I was preparing a paper on the comet nucleus for the colloquium: 'A Study of Comets' to be held at Goddard. While thinking about comets in the Oort Cloud, I realized that cometary nuclei at the heliocentric distance of the Oort Cloud would be exposed to the full intensity of galactic cosmic rays. From my experience with the Free Radical Program at NBS, I was familiar with the chemical effects of energetic ions on condensed gases. I began to investigate the effects of cosmic rays on Oort Cloud comets. These calculations were presented at the Goddard Comet Colloquium (Donn, 1976b). They indicated that appreciable chemical changes would be expected in the outer several meters of the nucleus as new and generally more complex molecules were generated.

I presented a second paper proposing that comets may form, in situ, at the distance of the cloud. It was likely that the sun was part of a star cluster. The nebulosity associated with the cluster would include small cloudlets, too small to form stars but out of which comets could be created (Biermann and Michel, 1978). As the cluster evolved, the stars moved radially out at low velocities. Comets that form in cloudlets with sufficiently low velocities relative to the sun would have been captured and become part of the sun's family at Oort Cloud distances.

As with the case of the cosmic grain research, the ice irradiation analysis also was followed by an experimental program back at Goddard. Marla Moore, a graduate student in astronomy at the University of Maryland, was looking for a thesis subject. She had been in Ithaca the year I was there and had attended my course on comets. As a result she expressed interest in working on the ice irradiation problem. Goddard had a two Mev van de Graff accelerator for testing and calibrating spacecraft instrumentation and components. That is a very convenient energy for studying chemical effects in thin ice films. Marla Moore was given space in the laboratory next to the accelerator room where she assembled a facility for carrying out the experiments. Her thesis was reported in Icarus (Moore et al., 1983). This was the beginning of a continuing program in the Astrochemistry Branch by Moore and her associates, (Moore and Khanna, 1991; Moore et al., 1991; Moore and Hudson, 1993; Hudson and Moore, 1995).

The origin of life has always been a subject that attracted attention. It received renewed interest with the advent of the space age and the exploration of other worlds within and beyond the solar system. Because comets are composed of a high proportion of biogenic molecules they have been looked upon as a potential contributor to the origin of life on earth. In 1980 the Laboratory for Chemical Evolution at the University of Maryland organized a colloquium on 'Comets and the Origin Of Life".

As a contribution to that meeting I proposed a model for the structure of the comet nucleus. Based on the grain accumulation mechanism for the formation of planets, this model took into account the development of a broad size distribution of planetesimals. Therefore the nucleus was pictured as a compact array of cometesimals with a variety of sizes. A possible structure is displayed in Fig. 1. The nucleus is no longer a homogeneous body but one in which both the density and composition may vary throughout.

There is almost no data on the internal structure and

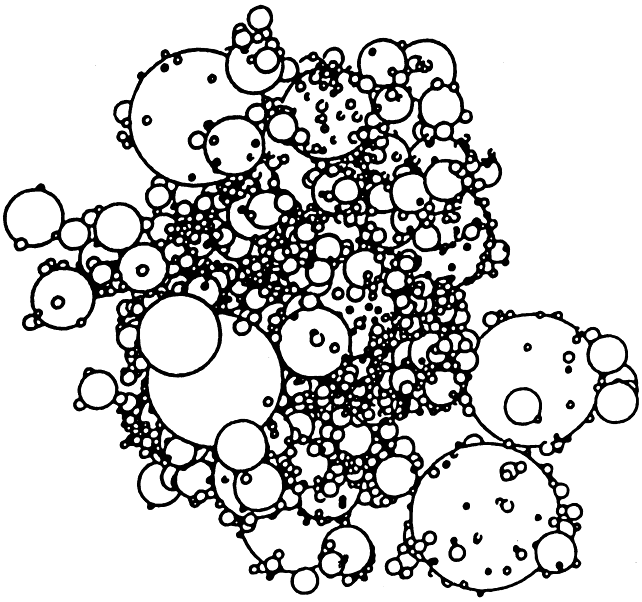

Fig. 1. Schematic depiction of the comet nucleus. The circles are simplified representations of cometesimals formed by random accumulation of grains. Each has a structure resembling that of Figure 3. Modified from Donn (1981).

composition of the nucleus. When NASA proposed a comet rendezvous mission and solicited experiments for it, the Astrochemistry Branch at Goddard organized an international team that prepared a proposal to investigate the internal structure. Low frequency radio waves can penetrate a considerable distance into ice. This penetration is the basis for investigating snow fields and glaciers from an airplane. Because of cost, power and weight requirements, the proposal was not accepted. Thus, no time or effort was put into the experiment. This turned out to be fortunate when, after several years, but before launch, the mission was cancelled.

10. Fractals in astrophysics

About 1983 or 1984, in the review of 'Highlights in Physics' in *Physics Today*, an account was given of fractals and their role in various physical phenomena. The concept of fractals intrigued me and I looked up the references given there. Pictures of fractals in the references cited were similar to the figures in the article by Daniel and Hughes (1981) depicting their simulation of meteoroids formed by grain collisions. This suggested that comets, formed by a collision process among a distribution of cometesimals with a variety of sizes may also have a fractal structure. I began to investigate this possibility. An effort was made to collaborate with Hughes in England, but this did not work out. My next step was to contact Dr Paul Meakin, one of the scientists whose work on fractals was referred to in the *Physics Today* article. Meakin was a member of the DuPont staff in Wilmington, Delaware and therefore not far from Goddard. He expressed considerable interest in applying the fractal concept to astrophysics and we arranged for several visits together.

Several papers arose from this collaboration including one on fractal aerodynamics (Meakin and Donn, 1988). In this paper, we showed that the initial stage of grain aggregation in space would be fractal aggregates. Growing aggregates are self-similar in a statistical sense. The number of primary particles within a distance R is given by $N(R) \approx R^D$. D is the fractal dimension and in this case would be about 2. Such particles would be nearly transparent, that is, there is very little self-shielding. Thus, the ratio of cross-section to mass is almost constant and only decreases slowly with the number of grains. A further consequence is that gas drag also remains large as the grains grow. This effect, combined with an anticipated low density, fluffy aggregate structure for the growing body, produces a model of efficient planetesimal formation (Donn and Meakin, 1989; Donn, 1990; Donn and Duva, 1994). This allows further development of the proposal, first made in 1963, based on whisker formation and accretion.

11. Investigations of planetary accumulation

The last two papers were published after retiring from Goddard in 1989. At the time I held a position as part-time, visiting professor in the Engineering Physics Department at the University of Virginia at Charlottesville. I also continued my association with the Goddard Astrochemistry Branch as visiting scientist under the Universities Space Research Association (USRA).

At Virginia I was primarily involved with applying the fluffy aggregate concept to the problem of accumulating large solid bodies from grains in the primordial solar nebula. Although grains readily stick upon contact, large compact objects, such as rocks, do not. This was an obstacle to theoretical simulations of planet formation in which it was implicitly assumed that colliding grains formed dense, compact, rock-like objects. However, the fractal growth concept and experiments on grain aggregation showed that colliding grains produce fluffy, low density aggregates, not rock-like bodies.

A serious drawback to following up this concept is that there is no data on the effect of impacts of fluffy objects upon each other. Blum and his collaborators (1996) have performed experiments on collisions of mm size fractal particles of uncertain characteristics. At Goddard and later at Union College, David Peak attempted to investigate this problem. He dropped styrofoam balls into a variety of low density, refractory powders and measured the penetration as a function of energy per unit area of the balls. These results enable approximate calculations to be made on the growth of planetesimals to kilometer sizes (Donn and Duva, 1994; Donn, 1997, unpublished observations).

At the University of Virginia, Jeff Beaudry, a graduate student, attempted to create cm size, compressible aggregates to obtain more reliable collision data (Beaudry, 1991). This turned out to be extremely difficult under terrestrial conditions. Fluffy compressible particles would not retain their integrity and fell apart. Such particles may occur in a microgravity environment where they would not collapse under the Earth's gravity.

The only recourse, in spite of its deficiencies, was to use Peak's penetration data (Donn and Duva, 1994; Donn, 1997, unpublished). A detailed Monte Carlo calculation, although far from rigorous, suggested that km size planetesimals could grow before they became so compressed that they behaved like rigid bodies. This result is given in Fig. 2 which shows the density as a function of size. Several experimental and theoretical results show that maximum density of a random accumulation of grains is 0.65 of the bulk density of the material. At a size of about 1 km, gravitational attraction takes over and growth continues. Two cases are shown in which the compaction functions dependence on the diameter and the initial density differ. Both results asymptotically approach a bulk density of 0.65 For the more favorable case of Fig. 2, linear extrapolation yields this density at a size of about 340 m. This is becoming comparable with the size at which gravitational accumulation occurs.

At the University of Virginia I have also been participating in research on ion bombardment of astrophysical ices with Bob Johnson and Raol Baragiola.

Although I still continue to have some interest in various aspects of astrochemistry, my major activity since retirement has been to return to my long-time interest in peace and social justice, particularly conflict resolution, antiviolence and race relations.

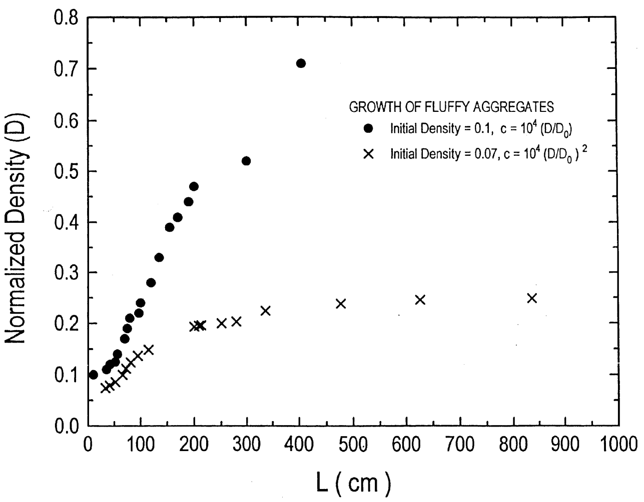

Fig. 2. Size-density relation for fluffy aggregates. Results are displayed for two runs with parameters, initial density and impact compaction c defined by: impact energy = cx penetration squared. The maximum density obtained under random accumulation is 0.65 of the solid density beyond which the aggregate is no longer compressible. A linear extrapolation through the last three points of the lower curve yields a size of about 340 m.

There have been other colleagues, in addition to those mentioned here, with whom I have been associated over the past half century and I apologize for not having been able to include them. Whatever I have been able to achieve owes much to my wife, Marjory, who supported, encouraged and put up with my idiosyncracies over most of that time. It has been her companionship that made my journey along this adventurous and exciting path possible.

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