Frank Shu

in conversation with Bo Reipurth

Q: *The Lin-Shu density wave theory for the spiral structure of disk galaxies was published in 1964. What was the genesis of this enormously influential concept?*

A: The project started when C. C. Lin of MIT spent a sabbatical year in 1961 at the Institute for Advanced Study in Princeton, to work with C. N. Yang on the theory of superfluids. Lin attended a symposium on the spiral structure of disk galaxies organized by Bengt Strömgren, who was then the Professor of Astrophysics at the IAS. At this meeting, Jan Oort gave the plenary lecture on the winding dilemma of material spiral arms. Per Olof Lindblad, the son of Bertil Lindblad (for whom Lindblad resonances are named), presented some early numerical N-body simulations of a system of self-gravitating stars in a flattened geometry. The idealized system exhibited transient spiral patterns that sporadically formed and dissolved. From those two lectures, Lin got the seminal idea that the spiral patterns were really a wave phenomena, and not material arms.

When Lin returned to MIT, he started to organize a team of young theorists to help him develop this idea. The group grew eventually to include Alar Toomre, Chris Hunter, Chi Yuan, Bill Roberts, James Mark, Y. Y. Lau, and Guiseppe Bertin. I was then a MIT physics undergraduate major, uncertain about how to have a career in science. Lin hired me as a summer research assistant to help him perform some numerical calculations on the problem of wind-driven ocean circulation, another scientific problem that interested him at the time. After spending a summer crunching numbers on an old mechanical calculator, I managed to finish the assigned calculations well enough that Lin agreed to be my adviser for the senior thesis that all MIT physics students had to write before graduation. This thesis was on density wave theory, and my primary job was to calculate asymptotically the gravitational potential of a small-amplitude density perturbation, in stars or gas, in a flattened axisymmetric galaxy. The perturbations were oscillatory in time and had a m-fold sinusoidal variation in azimuthal angle, with the radial variation to be determined self-consistently from the joint equations of dynamics for the stars and gas plus Newton's theory of gravity.

These calculations, supervised by Lin's sure vision that the actual phenomenon had to be quasi-stationary and not transitory, grew to become the foundations of modern spiral density-wave theory. It was pure dumb luck that brought me there for the beginning, but density-wave theory holds a special place in my heart. It is a topic to which I have returned many times in my career, as the ideas turned out to have important applications not only in disk galaxies (e.g., the study of flocculence resulting from the chaos induced by overlapping subharmonic resonances with Greg Laughlin and Sukanya Chakrabarti, and "feathering" as a parasitic instability behind self-gravitating, magnetohydrodynamic spiral shocks with Wing Kit Lee), but also in planetary rings (resonantly driven, linear and nonlinear, density waves with Jack Lissauer, Luke Dones, Jeff Cuzzi, and Chi Yuan), and in heavy protoplanetary disks (e.g., m =1 SLING instability for binary and giant planet formation with Scott Tremaine, Fred Adams, and Steve Ruden).

Q: *From the linear theory of density wave theory of the stars you began to study the nonlinear theory of the response of the interstellar medium and its implications for star formation. What influenced you to change your focus, given that there were major unresolved issues with the stellar theory?*

A: After getting my PhD from Harvard in 1968, I spent five years on the faculty at Stony Brook, which was just getting started with a newly formed astronomy group headed by Steve Strom. From Steve, I learned a lot about stars as points of light and not just as points of mass. I had become interested in the problem of OB star formation behind the two-armed shockwave patterns in spiral galaxies and was collaborating with Chi Yuan and Bill Roberts for a better astrophysical understanding of the triggering mechanism. It soon became clear that we had to have a much better model for the interstellar medium than the adopted default of a single-phase isothermal gas, so I went to Berkeley on a one-semester sabbatical to learn about the elegant two-phase model that had been developed by George Field, Don Goldsmith, and Harm Habing. Together with Vinny Milione, Don Goldsmith (whom I helped later to recruit to Stony Brook), Chi Yuan, Bill Gebel, and Bill Roberts, we wrote a paper that dealt with the problems of phase transformations and star formation in a two-phase ISM periodically exposed to shockwaves in a spiral galaxy.

This work drew the interest of Ron Allen, who then headed a radio group at Groningen building 21cm-line receivers for the newly commissioned Westerbork Radio Synthesis Telescope. Ron wanted to learn how WRST might be used to test density wave theory in the context of the response of the interstellar medium, and he came to MIT (where I was visiting) to question C. C. Lin and me about this problem. Ron later asked me to go for an extended visit at Groningen, an invitation that I accepted in the summer of 1973. It was the first time that my wife Helen and I had spent appreciable time living in Europe, a wonderful experience that we both still cherish. It was also a valuable learning opportunity for me, as a theorist, to interact closely with radio astronomers of the caliber of Ron Allen, Ron Ekers, Miller Goss, and Renzo Sancisi, who were all at Groningen at the time. They taught me up close the value of checking beautiful theoretical ideas with the hard empirical facts from observations.

Q *From density wave theory of the interstellar medium, you then began to study the internal structure of contact binaries. What initiated such a major change of research direction?*

The change was triggered by my move to Berkeley. While I was there on sabbatical in Fall 1971, George Field announced that he was moving to Cambridge, Masssachusetts to head the joint astronomy effort at Harvard and the Smithsonian Astrophysical Observatory. The revamped organization changed its name to the Center for Astrophysics under his leadership. The contacts I had made in the Berkeley Astronomy department encouraged me to apply for the position vacated by Field's departure. I succeeded in the application, and in the Fall of 1973, after spending the summer in Groningen, Helen and I drove across the USA to begin our new life at Berkeley.

My first PhD student there was Steve Lubow, a student of the Physics Department at Berkeley, and he wanted to work on a clean problem that could make use of his ample mathematical abilities. At the time, interacting binary stars were the rage because of the tremendous discoveries being made by X-ray telescopes launched into space. So we looked into the problem of interacting binaries and discovered a pioneering paper by Gerald Kuiper published in 1941 on the problem of mass-transfer in semi-detached binaries. Kuiper's analysis invoked particle trajectories to do the dynamical calculations and may have been overlooked for that reason since the mass-transfer rates are so large that collisions among the individual atoms making up the mass-transfer stream must be important. My experience in stellar dynamics and gas dynamics taught me the differences and similarities between collisionless and collisional systems, which was a perfect match with Steve Lubow's expertise in doing perturbational calculations using asymptotic methods. In 1975 and 1976, we wrote two papers on the subject of mass transfer in semi-detached binaries that are still considered benchmarks in the field.

From semi-detached binaries to contact binaries was a small step seemingly. But in semi-detached binaries, all the action is at the surface or outside the stars and can be observed. In contact binaries, all the important action is inside the stars, or within a common envelope, and cannot be observed (or so we thought). A naive idea for the structure of contact binaries is simply to jam two single stars together. This idea leads to the conclusion that two main-sequence stars cannot form a co-rotating contact binary because their mass-radius relationships on the main-sequence are inappropriate for them *both* to fill their Roche lobes except in the single case of equal-mass components. Observationally, W Ursa Majoris stars constitute the most common form of close binary systems; the two components are undoubtedly both on the main-sequence; they are co-rotating; yet no W UMa system is known that has equal mass components! Clearly, the naive theory is inadequate, and some drastic new ingredient needs to be added. Leon Lucy and others turned out to have very different thoughts on what drastic new idea was needed than Steve Lubow and I. These differences led to enormous controversy, not between Lucy and us, but with peripheral critics on the scene, that has not been settled even today.

Lawrence Anderson (who then had an office next to mine), Mal Raff, and I developed a technique of Doppler imaging of W UMa stars that we never followed up after our initial observational data taken at Lick Observatory because moving starspots badly contaminated the spectral shape of a line that should have ideally reproduced the spatial shape of a uniformly rotating dumbbell. Had I been in the mood to think more calmly at the time, I might have realized that the technique offered a chance to view the *differential internal circulation* that we had postulated to be at the heart of the resolution of the problem, but that seemed inaccessible to observation. Somebody needs to revisit this problem and technique, which I liken to being related to the theory of single stars in the same way that diatomic molecules are related to the theory of single atoms. In astronomy, we have a well-established theory of single atoms, but none for diatomic molecules.

Q: *In 1977 you published your study on self-similar collapse of isothermal spheres. This was again a major change of subject. From where came your interest in protostars?*

A: The impetus came from two different directions. First, I had always been interested in how stars formed in the context of OB stars being the delineators of optical and UV spiral structure in disk galaxies. Second, by 1977, I was very upset by the tone of the debate on contact binaries. When I complained about the unpleasant situation privately to Steve Strom and Bart Bok in a visit to Kitt Peak, both of them, separately and independently, advised me to switch fields – to the subject of star formation, not from the point of view of the interstellar medium, but from the point of view of the actual objects. Since Bok and Strom were my lifelong friends and mentors, I took their suggestions seriously and began to study in earnest the literature on protostars.

I knew about the controversy between Hayashi and Larson concerning where pre-main-sequence stars would appear in the H-R diagram after a phase of rapid gravitational collapse lasting on the order of 10^5 yr, but it was not until I found the papers by Larson and Penston on self-similar collapse that I saw a way in which I might make a contribution to the problem. The L-P solutions, applied to gravitational collapse, correspond to states that are initially far from equilibrium. For example, they had supersonic inflow toward the center at infinity. I could not imagine how such a state could be set up by natural processes occurring in the ISM (but later, Susana Lizano, Daniele Galli, Jorge Cantó, and I found a way to use reversed L-P solutions for modeling the champagne flows of H II regions). On the other hand, I knew from our work using Bonnor-Ebert spheres and their cousins in the two-phase model of the ISM that such objects became singular isothermal spheres in the limit of high central concentration. Thus, I was motivated to study the problem of how an unstable equilibrium starting with a singular isothermal sphere would gravitationally collapse. To my delight, the inside-out collapse is exactly self-similar without having to assume self-similarity as a hypothesis, and the central product is a point that had a mass which grew linearly with time! By then, I had enough experience as a real astronomer to realize it was important to compute also how much light such a point would generate as a protostar. This became the starting point of my studies with Steve Stahler and Ron Taam that led to a satisfactory resolution of the controversy between Hayashi and Larson, in agreement with contemporaneous numerical simulations by Winkler and Newman.

The self-similar collapse of singular isothermal spheres was also the beginning of a long series of fun generalizations that allowed us to find analytical or semi-analytical solutions when we added rotation (with Susan Terebey and Pat Cassen), departures from axial symmetry (Daniele Galli and Susana Lizano), magnetic fields (Zhi-Yun Li and then Fred Adams), combined rotation and magnetic fields (Tony Allen, Zhi-Yun Li, Daniele Galli, and Susana Lizano), and general relativity (Mike Cai). Looking at the implied spectral energy distributions of the corresponding collapsing objects led Fred Adams, Charlie Lada, and me to our classification of Class I, II, and III objects based on the appearance of their SEDs. (Later Phillipe André and his colleagues added a Class 0, to which I objected not so much because it is not a valid scientific addition, but because Roman numerals do not have a zero. That was an invention of Indian and Chinese mathematicians!)

Q: *In 1987, you and Fred Adams and Susana Lizano published an ARAA review on star formation in molecular clouds. With about 2000 citations, this is one of the most influential articles ever in the field of star formation. What accounts for this profound impact?*

A: My guess is that the article satisfied a need from both observers and theorists to have a comprehensive discussion that unified what had previously been regarded as distinct subfields. Fred, Susana, and I synthesized the work done at Berkeley (which included the optical/infrared observations of Len Kuhi, Gibor Basri, and Martin Cohen, as well as the radio work of Jack Welch, Dick Plambeck, Mel Wright, and Carl Heiles), together with the rich variety of work done at the Center for Star Formation Studies that included UC Santa Cruz and NASA Ames (with too many names to mention individually), and other organizations (such as by the strong star formation group at the CfA). Rightly or wrongly, we offered a rational organizing framework that (a) linked the theory and observations, and (b) gave an outline of future directions where additional progress might be made. The most important concept that we put forward in a single cartoon is the idea that star formation occurs in four stages: a first stage that involves the formation of molecular cloud cores (e.g., Myers and Benson); a second stage that involves the gravitational collapse of an unstable, slowly rotating, core to form a protostar, an infalling envelope, and a centrifugally supported circumstellar disk (e.g., Sargent and Beckwith); a third stage in which the infall would be reversed by a bipolar outflow (e.g., Snell, Loren, and Plambeck or Rodríguez, Ho, and Moran); followed by a fourth stage in which a pre-main-sequence star emerges surrounded by a circumstellar disk that might give birth to a planetary system.

Although we were criticized at the time for focusing on the problem of the formation of single stars (mostly of low mass), and not discussing much the issue of the formation of binary or multiple stars or clusters, nor emphasizing the importance of interstellar turbulence, I still think we made the right decision. Concrete progress in science is not made by attacking all important problems simultaneously: for example in quantum mechanics, one must learn to solve the hydrogen atom, and then one can move to diatomic molecules, triatomic molecules, and eventually DNA.

Q: *In the nineties you and your collaborators developed a detailed theory of the magnetocentrifugally driven flows from a young magnetized star and its accretion disk, which has had a major influence on the way we understand young stars and their mass loss. How did this concept develop?*

A: Again, it was a matter of paying attention to what the observers were saying, and then sieving through the different ideas put forward by theorists, *without being prejudiced by the motivation to explain the most striking observational fact, which were frankly the beautiful images that you, Hans Zinnecker, and others were making of jets from YSOs and their associated, rapidly moving, Herbig-Haro objects.* For my own part, I have always preferred motivation by fundamental theoretical issues to being over-influenced *at the start* by observational data. From the start of discussions of pioneers in the field like Lyman Spitzer and Leon Mestel, these issues have concerned the *obstacles* to star formation presented by rotation and magnetic fields.

While rotation by itself could and would give rise to protostellar disks, it cannot solve the angular momentum problem of the central object. Processes like spiral density waves might help transport angular momentum in the outer disk, but they become ineffective in the central regions. In these regions, it almost certainly must be magnetic fields, combined with rotation, that would give rise to the bipolar outflows that act as the process by which protostars reveal themselves as optically visible objects. The efficacy of the combination of strong magnetic fields coupled with rapid rotation for producing massive outflows was realized by early workers like Lee Hartmann and Keith MacGregor building on work by Leon Mestel concerning mass loss from rotating magnetized stars, or Ralph Pudritz and Colin Norman building on the work of Roger Blandford and David Payne on extragalactic radio jets. As a byproduct, we conjectured that the outflow process would help a star to define its own mass – another theoretical conundrum since molecular clouds and even their cores do not have stellar masses as a natural characteristic mass scale.

Once one has this motivation, discovering the right set of equations to solve is relatively easy (in fact, a literature search showed that the appropriate formulation had already been given by a physics MIT professor, Stan Olbert, who taught me E&M), and then it was just a matter of time to find a way to solve the posed problem in a completely satisfactory way (which took ten years, and the help of five graduate students – Joan Najita, Eve Ostriker, Sienny Shang, Mike Cai, and Subu Mohanty).

In the meantime, we discovered that by considering the heating and cooling of such outflows, a subject on which we had the help of Steve Ruden and Al Glassgold, we got as a gratifying bonus the (seemingly) highly collimated, pencil-beam jets that you and others were, rightly, so excited about!

Q: *More recently your interest turned to chondrules. Do studies of present-day star formation and of the distant formation of the solar system illuminate each other?*

A: The study of chondrules and calcium-aluminum-rich inclusions (CAIs) came from another old scientific friendship: this time with Typhoon Lee, the discoverer, with Jerry Wasserburg, of Al-26 in the Allende meteorite. Typhoon was the person who persuaded me to help bring astronomy to a higher level in Taiwan, and I helped to persuade other Chinese-American astronomers including Chi Yuan, Fred Lo, Paul Ho, Sun Kwok, Ron Taam, You-Hua Chu, and many others to lead this effort and make it a success. But the founding and nurturing of ASIAA is another story.

Carbonaceous chondrites like Allende are a curious mixture of a grainy matrix that has never experienced temperatures higher than 600 K if we are to judge from the fragile organic molecules that they contain, and inclusions like chondrules and CAIs that have undergone conditions hot enough to melt rock (i.e., 2000 K or more). Yet such meteorites are supposed to originate from parent bodies in the asteroid belt, which astronomical models and observations suggest should never have had temperatures that can melt rock. CAIs also contain extinct radioactivities like Al-26 that are surely telling us something important about the early solar system, information that we have no way of accessing by remote astronomical observations.

Typhoon Lee, Sienny Shang, Al Glassgold, Mathieu Gounelle, Ernest Rehm, and I had the simple idea that the curious mixture of hot and cold rocks may literally be a mixture beginning with hot rocks from the interior of the primitive solar nebula flung out to large distances by an X-wind responsible for an ancient bipolar outflow in the solar system. The entrained material would undergo aerodynamic size sorting in flight, with mm-sized and larger objects landing typically in the asteroid belt, where they would seed the cold matrix of protoplanetary dust already there with a sprinkling of chondrules and CAIs (which can make up a major portion of the total mass of chondritic meteorites). If this were the case, then we need not invent other exotic mechanisms that would modify or completely damage current promising ideas about how the parent bodies of chondiritc meteorites, i.e., planetesimals, originate (e.g., work by Andrew Youdin and collaborators).

To minimize the number of adjustable parameters in this theory, we tried to explain the wild variety of extinct radioactivities – Al-26, Mn-53, Ca-41, Be-10, etc., that one finds in the CAIs of carbonaceous chondrites as products of irradiation by ancient solar flares before such material became entrained and flung out to interplanetary (and interstellar) distances by the X-wind. This effort had mixed success (to make a bad pun), as we admitted in our papers.

However, we did make a spectacularly successful prediction, which is that cometary material, which was then thought to be pristine, when collected and brought back to Earth, should also contain chondrules and CAIs, but of smaller sizes. This prediction was borne out by the analysis of Kevin McKeegan (who discovered Be-10 in CAIs) of the dust samples returned by the Stardust mission to Comet Wild. Kevin has also analyzed the oxygen isotopic ratios of an O-16/O-17/O-18 sample in the solar wind returned by the Genesis mission. They are in accord with Robert Clayton's prediction that if X-wind theory is correct, these ratios should be what are found in CAIs (which had previously been considered "anomalous") and not as they are found on the Earth (which had previously been considered "normal"). These successful predictions do not "prove" that the origin of CAIs and chondrules by X-wind transport is true, merely that the idea has been tested by serious scientists and not found to be false.

Q: *In addition to your research you have written several textbooks, the undergraduate text book 'The Physical Universe' and the two-volume graduate textbook 'The Physics of Astrophysics'. What motivated such a major undertaking?*

A: I have always liked teaching, and I have always contended that there are really only two ways to learn a subject well: one is to do research in it; the other is to teach it. So I have always regarded teaching not as an unwanted chore, but as an opportunity to learn about exciting developments in subjects apart from my personal research interests. I have also always enjoyed writing, so written exposition comes naturally and quickly for me. Writing the "Physical Universe" took two years (in the days when a tremendous innovation was the invention of electric typewriters that had balls with which one could type Greek letters). Most of the two years, apart from tending my normal duties as a Berkeley professor, was spent making revisions by the literal method of "cut and paste." When word processors became available on personal computers, it took me only one year to write the two-volumes of "The Physics of Astrophysics." But part of this efficiency derived from my always keeping complete written notes whenever I taught such courses at Stony Brook and Berkeley.

I have now retired three times (from Berkeley, from Tsing Hua, and from UC San Diego); if I ever retire permanently, I will go back to writing textbooks. I have two-thirds of a book written entitled "The Story of Astronomy" that I am itching to finish when I free up some time from more urgent tasks.

Q: *Upon retirement from the University of California you have devoted your time to studying a range of energy issues in Taiwan. What are your goals?*

A: My generation of scientists has been acutely aware of the global energy problem since the first Oil Crisis of 1973. And since 1982, I have had a growing concern about the threat posed to civilization by climate change. However, I always thought that science and technology would rise to the two connected challenges and solve the problem before it got really bad. Three to four decades passed, and the problem is still not close to being solved.

As a senior scientist with some influence in Taiwan, I finally felt that I had a social responsibility to not only give advice, but to roll up my sleeves and try to help find practical solutions. Thus, I retired from the University of California in 2009 and started a research group using high-temperature molten salts to advance two technologies: supertorrefaction and thorium breeder reactors. (Videos of our projects on "biochar" and "modular thorium reactors" can be found on the YouTube website of "Raw Science".) With biochar produced at rates achievable with supertorrefaction using waste biomass resources that do not impact on food production, we estimate that it should be possible to reverse climate change in about 40 years (i.e., get CO_2 concentrations in the atmosphere back down to 350 ppm). However, this reversal is possible only after other technologies bring the net CO_2 emission from all primary sources of energy generation down to zero.

To help reach the target of zero emissions, many people (including James Hansen and Bill Gates) believe that **safer**, **superior** (in cost), **securer** (in terms of weapons proliferation), and **sustainable** forms of nuclear power need to be developed and deployed. I am betting my last hurrah in scientific research that molten salt breeder reactors that run on the thorium cycle can be that reactor.

When I feel confident that these two projects can reach successful completion without my continued active participation, perhaps I can really retire and finish writing "The Story of Astronomy."