Richard B. Larson

in conversation with Hans Zinnecker

Q: *Your 1968 dissertation was entitled "Dynamics of a Collapsing Protostar", and it was a pioneering study. What led you to do a thesis on that topic, and what was the influence of your thesis advisor, Guido Münch?*

A: As an undergraduate at the University of Toronto I first became interested in astronomy, like many students, for philosophical reasons: I wanted to know the answers to the big questions about the universe, and I realized that I would have to learn some astronomy to answer them. As a graduate student at Caltech I was fascinated by galaxies and wanted to understand how they formed. My first idea for a thesis project was to calculate how a spherical galaxy forms, using a simple treatment of star formation. When I discussed this idea with Maarten Schmidt, he seemed dubious about it as a thesis project and suggested that I talk with Guido Münch, who knew more about interstellar matter and star formation. Guido was also skeptical about my grandiose ideas, and he said "before you try to understand how a galaxy forms, why don't you try to understand how one star forms?" I quickly realized that Guido was right and that this would be a better thesis topic, although still challenging. But I decided to give it a try because I had already read nearly everything that had been written about star formation (which was not so much in those days), and I had also gained some experience in calculating stellar structure working with Pierre Demarque in Toronto. So I plunged into the project, not having any idea how far I would get with it. Guido again provided crucial advice at a later stage when I realized that I would have to deal with an accretion shock at the surface of the stellar core, and I proposed to include a treatment of radiative transfer in the vicinity of the shock. Guido said "no, don't waste your time with that, try using a simple approximation", and this gave me the idea that I should try to find a simple approximation that would allow me to continue my protostar calculation. I eventually came up with an approximation that seemed adequate, and it did the job and allowed me to calculate all the way through to a pre-main sequence star on the Hayashi track. After this work was published my treatment of the accretion shock was controversial, but eventually more detailed treatments showed that it had not introduced a serious error.

Q: *What do you think were the key findings of this work?*

A: Looking back, I think that the most important result of that work might have been the very first one that I found when I got my first collapse code working. I had written a simple code to calculate isothermal collapse, and the first successful run with it showed the runaway growth of a sharp central peak in density that appeared to be approaching a singularity. This result was not what anyone had expected, and it was also to prove controversial, but I eventually convinced myself that it was at least qualitatively correct and found a similarity solution showing this behavior. At about the same time, Michael Penston found similar results and independently derived the same similarity solution. This 'Larson-Penston solution', as it has been called, was perhaps the most enduring result of that early work, and it has been shown to have much greater generality. This basic qualitative result led to a change in thinking about star formation by showing that star formation begins with the runaway formation of a near-singularity in density, and then continues as an accretion process. Once you adopt the view that star formation is largely an accretion process, you can calculate many things about it by studying how the accretion process works.

Q: *You were among the first to suggest that most, if not all, stars are born in small multiple systems. How do you see that subject today, and how important is it to our understanding of star formation?*

A: Binary and multiple systems are clearly the normal way that nature makes stars, and most single stars are probably escapers from such systems. From a general point of view this is completely unsurprising because nature is complex, and star-forming clouds in particular are highly complex and have structure and motions on all scales. This has major implications for our understanding of star and planet formation because it means that most stars form in close proximity to other stars, and therefore that interactions will almost certainly play an important role in their formation. Our Sun and Solar System may not be typical, and indeed recent studies have found an enormous diversity in extra-solar planetary systems. The fact that star-forming clouds quickly become highly structured on all scales, probably because of both turbulence and self-gravity, means that their dynamics must necessarily be highly chaotic. To me this is a lesson that we as theorists have to respect the diversity and complexity and unpredictability of nature and be very cautious in applying simple theoretical models.

Q: *Thirty years ago you suggested the existence of scaling relations for molecular clouds linking velocity dispersion and density with size. What is your current view of these "Larson relations"?*

A: There are real trends like the ones I discussed, but it has to be kept in mind that they are only broad correlations with a lot of scatter, and that different studies can find different results. These relations have been endlessly debated and I think they have often been overinterpreted. What is clear is that the internal motions in molecular clouds are very complex and that they are at least in part hierarchically structured like turbulence. But these facts are not diagnostic of any particular origin for these motions, or of any particular mechanism for sustaining them. Even the basic energy source is still debated, sometimes heatedly. Probably all of the suggested mechanisms contribute at some level. The main implication of all this for star formation is that the initial conditions for it are likely to be characterized by chaotic supersonic motions on a range of scales, and therefore that idealized models may not be relevant. But once gravity takes over, as it must eventually if stars are to form, gravitational dynamics becomes increasingly dominant and develops characteristics of its own that become independent of the initial conditions. For example, the properties of binary and multiple systems, and perhaps even planetary systems, may depend more on the universal properties of gravitational dynamics than on the initial state of star forming clouds.

Q: *Ten years ago you wrote an influential paper on "The Role of Tidal Interactions in Star Formation". What were your key points?*

A: I had earlier suggested that gravitational torques in disks are likely to play an important role in star formation, and in the paper you mention I suggested that tidal interactions between stars and disks in a system of forming stars could also be important for redistributing angular momentum and thus helping to solve the angular momentum problem. Recent detailed simulations of star formation do indeed show strong gravitational interactions between stars and disks, sometimes to the extent that disks are completely disrupted. What seems clear is that gravity must often be an important player in the dynamics of disks and in the redistribution of angular momentum. Magnetic and thermal pressure forces can be equally important, and many interesting phenomena probably involve a complex interplay of forces. But non-radial gravitational forces alone can already go a long way toward solving the classical 'angular momentum problem', in which case this problem can be seen as being at least in part just an artifact of oversimplified models. This is another case of the complexity of nature not being fully appreciated in early work.

Q: *You have also been interested for a long time in the properties and origin of the stellar Initial Mass Function. What is your current view of this subject?*

A: I have been interested in the stellar IMF from the beginning of my career because I was always surrounded by people who wanted me to explain the IMF. So I made a number of attempts over the years based on various ideas, and I have tried to keep up with the observational status of the subject. The main thing I have learned after all these years is that when looked at in any detail, this subject is a can of worms. In general terms, we know that for massive stars the IMF looks something like a power law, perhaps not too different from that originally proposed by Salpeter, and that at the low end the IMF shows a turnover below one solar mass. Whether any feature of the IMF is universal has been debated inconclusively for decades, and the subject has a long history of claims that didn't stand the test of time. On the observational side, it is clear that sample definition is of critical importance, but in the end arbitrary choices always have to be made about what to include in the sample. Theorists then have to be careful to theorize about what the observers actually observed if they want their work to be relevant. Observers have learned from hard experience to pay careful attention to sample selection, but theory isn't there yet. Concerning the physics behind the IMF, I have found appealing the idea that the low-mass turnover is determined by fundamental atomic physics through the thermal properties of star-forming clouds. It seems clear that the thermal physics is indeed important, but other effects can also be important, and we don't yet have a full understanding of the origin of the IMF and can't yet make the predictions that observers would like us to make, for example how the IMF might vary in different circumstances. What's needed is big simulations that include as much of the relevant physics as possible, but this is difficult work.

Q: *Computational star formation has made amazing strides since you began your work almost 50 years ago. What do you see as the main challenges today, and where do you think this field is heading?*

A: See above for some of the challenges. I don't want to predict where this field is heading, but continuing advances in hardware and software will surely continue to make bigger and better calculations possible. I hope that enterprising young people will continue to push ahead with such calculations. But it will require a large effort and expertise in a range of areas of astrophysics and computation, and big projects will have to be organized. I hope I live to see many more advances, but I wouldn't attempt to predict what they will be. It will be an adventure to find out.

Q: *What are you planning to do in your retirement?*

A: Less astronomy and more of everything else.