

Advice for a young astronomer: Style & Mode

It is said that the choice of problems matters as much as the solution to problems. I agree with this sentiment wholeheartedly. I make the additional remark that science is like any other human activity and so the same maxim is true for all other fields of human activity.

This note is being written on the eve of a “mini” course that I am teaching to the graduate students of the Department of Astronomy & Astrophysics, Tata Institute of Fundamental Physics (TIFR), Culaba. The nominal course consists of three one-hour sessions (one-hour lecture, half hour of discussion) and deals with the Physics of the Diffuse Interstellar Medium (and includes Low Energy Cosmic rays).

However, the real purpose of this course is to share my enthusiasm for the pursuit of excellence in research in science and help a few young people achieve this goal. For this reason, the formal course described above is preceded by three “sessions”, each with an introductory writeup. This is the writeup for the first of the three sessions.

Modes

As with any aspect of life there are several modes of doing science. For example, you may aspire to become a theorist, or an observer or an experimentalist. The choice is yours. However, please be aware of the old maxim: you reap what you sow. This harsh statement simply means that all choices have consequences and so choices must be made with considerable care.

Growing up in India, most aspiring young people are strongly attracted to theoretical subjects. Perhaps this is strongest in the case in physics, which then naturally extends to astrophysics. There are multiple reasons for this phenomenon, some practical and some cultural (the most successful role models are theorists; think of Chandrashekar, Saha, Bose of yore, and Ashtekar, Sen, Padmanabhan of recent times). However, astrophysics, like biology & geology, is a *phenomenological* subject. It is the job of the astronomer to explore the Universe, identify and measure ecological niches and then construct a reasonable theory to explain the primary patterns. An astronomer has to be, first and foremost, a daring explorer, not a shy retiring mathematician.

Mathematics lies at the heart of theory. It is my observation that only a small fraction of the population has that peculiar mindset that is essential for success in mathematics. It remains untested whether a typical person, with sufficient training can become a great mathematician. In contrast, a much wider range of mindsets can undertake observational and experimental research. The fact remains that we are all born with some strengths and some weaknesses. With effort, weaknesses can be diminished and skills enhanced. Make no mistake about that. However, it is foolish to make a choice that plays on your weakness and diminishes your strengths.

As a teenager I liked mathematics very much. I was amazed by the power of mathematics and fascinated with calculus, in particular. I still claim that I can integrate any function that is integrable. However, I concluded that my skills were such that I would become a mediocre mathematician (and by implication a mediocre theoretical physicist). Fortunately, I avoided the usual trap. After considerable contemplation, between the age of 15 and 20, I determined that being the best at something in the world trumped over the particular problem I chose. Next, thanks to the first ever Astronomy &

Astrophysics summer school (RRI, 1976) I discovered that I had some talent in the field of experimental radio astronomy. So instead of pursuing theoretical astrophysics at RRI (with Rajaram Nityananda which I dearly wanted to do) I applied at the last minute to MIT, Caltech and Berkeley. I was rejected by Caltech and received admission at MIT without any scholarship. In 1978, I arrived at the Astronomy Department at UC Berkeley with full scholarship and immediately gravitated to the Radio Astronomy Laboratory.

As mentioned earlier, only you can make the choice for your style – but you need make it with your eyes wide open. The greatest difficulty in figuring out your style is yourself. Most people are least knowledgeable about themselves but have considerable opinions about others! It helps to have frank discussions with an older and experienced person (but who also has to be kind) to help you understand yourself better.

So, my dear students, over the next three weeks you will experience the course at two levels: the astronomical level (a minor puzzle in low energy cosmic rays with potentially important ramifications and some nascent ideas on physical conditions of cloudlets in the ISM) and at a deeper level, on some ideas on how to become a research scientist.

Caveats: Please be aware that your lecturer is a moderately successful astronomer. If you are expecting brilliant insights into physics (let alone advice on quantizing gravity or deep thoughts on the physics of multiverses) or even brilliant anything then you are likely wasting your time. *The class is best suited to students with great zeal for life and who want to accomplish something substantial in their lifetime, consistent with their natural endowments.* The experiences of my research lifetime clearly limit the scope of advice that I can give – the topic next discussed.

Styles

As mentioned before, early in my life, I elected to eschew a theoretical approach in favor of an experimental approach (which, in astronomy, could include observations and modeling). Even having chosen the mode (experimental) there remains the question of style. Perhaps the most common style is to choose a field and stick to it. An outstanding example of this approach is that provided by J. Taylor who, along with Hulse, discovered the first binary pulsar and then devoted this lifetime to timing the pulsar to measure the orbital decay and compare that with expected from the General Theory of Relativity. The exact opposite approach to this is a curiosity driven program. My dear colleague and friend, E. Sterl Phinney, whom I regard as the brightest astronomer/astrophysicist of my lifetime, falls in this category.

My approach is not common. It can be kindly referred to as “low hanging fruit” (LHF) or less kindly (although accurately) as an “opportunistic” approach. In this approach, the scientist maximizes her/his achievement for the effort/time expended. In commercial terms, this is like attempting to make money via “startups”. In fact, I make this analogy deliberately because the approach is exactly the same. In this LHF approach, the scientist identifies important areas of research that are not being pursued by leading players, makes a rapid entry and obtains early results (usually of considerable importance) and then, upon seeing hordes of new entrants, moves on to the next venture. On occasions, I have been caught saying “Never undertake a project unless you have considerable unfair advantage over the competition”.

It is rumored that only one in ten startups succeed. A single person cannot afford such a loss.¹ For this reason, choosing a new endeavor requires considerable “market” analysis and accurately forecasting improvements in technology/methodology to know what and when to start.

Another issue of style is whether to work alone or with others. I would prefer to work alone but I lack many attributes, particularly theoretical insight and numerical modeling, to complete a project all by myself. So, to compensate these weaknesses, I usually team up with theorists and modelers. Very, very, occasionally I write a single author paper and occasionally I have deep physical insight².

My IPO approach has been quite successful. I have had the following successful startups: [1] an air-linked interferometer for studying cold Galactic HI, all built by myself, [2] the discovery of the first millisecond pulsar, [2] optical observations of binary pulsars which led to some great advances (multiwavelength astronomy is now taken for granted), [3] the discovery of the first cluster pulsar, [4] proposing that soft gamma-ray repeaters are highly magnetized Galactic objects and not related to gamma-ray bursts, [5] the discovery of the first brown dwarf, [6] the demonstration that gamma-ray bursts, both long and short, are of extragalactic origin and [7] initiating a modern optical industrial-scale synoptic survey (with machine learning classification, robotic spectroscopy and distributions of alerts in real time). This amounts to switching sub-fields about once every five years. I have always had one terrible failure and a few misses. The failure helped me understand myself better and improved the success of my future projects. The misses, whilst most annoying, helped me gradually raise the bar of my successes. Failures and misses are so important that I will discuss them in a separate note.

If you wish to know more about this exuberant style of research then you will benefit from attending this course. On the other hand, if you wish to dive deeply into theoretical physics or theoretical astrophysics then you will be disappointed by the course. Caveat emptor.

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¹ Venture capitalists are willing to take the risk but only with a fraction of their wealth. They hope that one of their many investments will result in a fabulous unicorn (a lush initial public offering or IPO). In contrast, as a single person you need to seek far better odds.

² “Modelling Supernova-like Explosions Associated with Gamma-ray Bursts with Short Duration”
<https://ui.adsabs.harvard.edu/abs/2005astro.ph.10256K/abstract>
“Optical Identification of Binary Pulsars: Implications for Magnetic Field Decay in Neutron Stars”
<https://ui.adsabs.harvard.edu/abs/1986ApJ...306L..85K/abstract>