HOW TO APPROACH AN ASTRONOMICAL VENTURE?

S. R. KULKARNI

Buddha apparently once noted that "you can understand the Universe by deeply studying a leaf". This may well be true[1] but at any given time some problems are more solvable than other problems. And a good scientist identifies these problems and proceeds to work on them.

What follows are my views and made in the context of experimental astronomy. This means that, at the very best, these views have a limited application. There are other limitations that a reader should be aware of.

First, this note is meant for young scientists who are starting their research/faculty careers and with not too dissimilar than me in makeup, namely, not particularly gifted but willing and eager to work hard. This means that if you think you are in the league of young Fermi or Feynman then you need not read the rest of the note.[2]

Next the reader should be aware of different styles of research. Some researchers prefer to dig deeply and for a long time. This certainly makes sense if the topic chosen is sensible (see the opening paragraph for my sense of what is sensible). The reader should be aware that my preferred style of research is more "opportunistic" and short-term than the approach taken by a majority of scientists.

Finally, it is important to understand what motivates one do to science. I certainly agree that curiosity is what motivates all scientists. However, it is my very strong view that a fundamental reason why I chose to be a researcher is a very personal one, namely, I want to be the *first* to discover an effect, the *first* to see a connection and so on. Take this selfish motivation away and you take away a fundamental reason (certainly for me and most scientists that I have met) to do research. Now let me get back to the topic of this note.

Date: 1-November-2011. Latest revision: May 2, 2012.

[1] I can well imagine that an incredibly bright person, on noting the green color of a leaf, would make a connection to the peak wavelength of the Sun's irradiance and thence to how stars shine and rapidly, by pure thought, figure out our current understanding of astronomy but obtained by centuries of laborious experimentation, observations and theory. To my knowledge, the world has not seen such a bright person.

[2] This note was written for a lecture of the same title as this note I gave to astronomy seniors and first year graduate students at UW Madison (circa November 2011). After going through the material presented here I talked about an Hα survey that was recently initiated at Palomar. I analyzed the "business" plan for this survey including my plans to find the resources for the proposed survey.

1. Developing the Project

After much effort and with some luck you have finally obtained a good job at a first-rate research or teaching establishment. You are contemplating about your academic/research future. It is an exciting time but also worrisome since you will (like many young people) have many exciting ideas and feel that 24 hours is not enough to execute your ideas, perform your job, have a decent person life, sleep, eat and exercise. If you are at that phase in your life then you may find the rest of this write up to be interesting (and perhaps even educational).

First you need to develop the vision for your project. This means identifying an important problem whose answer will have an impact on the field. Merely saying that it interests you is not a criterion for choosing a problem. There should be a match between the problem you intend to choose and your core competence. Next, most of us are not so highly talented that we can identify a problem far ahead of the astronomical community. Thus, a familiarity with literature is essential. To counteract this suggestion I note that too often, young people identify problems based on their thesis topics or generally fashionable areas. However both options carry considerable risks. Fashionable topics are pursued by a very large number of groups and so the situation is asymmetric: a young person versus a large group of veteran researchers. Continuing one's PhD topic plays to the strength of the young person but most theses projects are conservatively chosen. Furthermore, usually working in the same field tends to wear off one's creative edge. Bearing these different issues I strongly advise young people attend talks and colloquia with an with an open mind.

Developing a good vision requires creativity. Once you have the rough outlines of your vision then immediately re-program yourself to be a critical person. Stand outside and see if the idea could have an impact (which means whether others will care to know the answer in the very near future) and of long lasting value. In this regard I have found the Bohr approach[3] to be very helpful. Get your colleagues to challenge your ideas. In this phase, *listen* and do not immediately start defending your proposed research. You want these discussions to help you identify weaknesses of your incipient vision.

Let us say that after suitable period of internal cogitation and external discussions you have gradually gotten convinced that your vision is sensible. Your next task is then to scope out the competition. Is there another program or project out there that has a large overlap with your proposed program? Do you truly have core competence in the mechanics of the project? Is there another person or a group which has far more experience and thus possesses a head start?

I know I have selected a good project when the project passes the "unfair advantage" test – that is when someone else says that I have an unfair advantage relative to the rest of the world.

[3]It is said that, during the early days of Quantum Mechanics, Neils Bohr used to argue in favor of the New Mechanics during the morning and in favor of Classical approach during the afternoon — all on the same day. Now that is what you should aspire to!

The choice of a project is based positive considerations, namely, the expected science is interesting and that you have a clear advantage relative to anyone else. Equally important is the concept of `Opportunity Cost`. This is a well known term to economists and business people but alas not so well known to most scientists. The entry in Wikipedia is wonderful. I reproduce the following two paragraphs[4]:

(1) *In the economic sphere an act, a habit, an institution, a law produces not only one effect, but a series of effects. Of these effects, the first alone is immediate; it appears simultaneously with its cause; it is seen. The other effects emerge only subsequently; they are not seen; we are fortunate if we foresee them.*
(2) *There is only one difference between a bad economist and a good one: the bad economist confines himself to the visible effect; the good economist takes into account both the effect that can be seen and those effects that must be foreseen.*

What the economist Claude Frédéric Bastiat, the father of this concept, was saying is that once you commit to a project then your capital and commitment will preclude you from developing a project until such time as the project you have launched comes to an end. Thus you will be precluded from undertaking new projects that may arise from new developments until your newly launched project is completed. Opportunity cost is a very hard concept to understand because the bad effect comes from what is *not* being pursued and so it takes a great degree of imagination to understand the concept.

The discussion of Opportunity Cost leads us to three conclusions. First, it is just as important to spend time thinking deeply about your proposed project as doing the project. Next, you should always design your project with a clearly stated lifetime. A well defined duration is an insurance plan to minimize the penalty resulting from the choice of a mediocre project. A short horizon at least gives you a new chance a few years later. [My horizon for a project is three years].

Finally, you should be careful to leave significant spare time to have the ability to keep abreast of new developments and perhaps even start small initiatives, should interesting opportunities arise in the near future. My observation is that most scientists think being super busy is a badge of honor. They are keen to share war stories like "I just flew in from Atlanta where I chaired a NASA panel and am shortly heading for a run in Hawaii followed by a workshop in South Africa where I am giving an invited talk" and "I will work on this project over my weekends and on the main project during the weekday" or "I wish I had more than 24 hours in a day" (duh!). Perhaps the silliest (and alas a common) reason is that "I have too many on-going projects that I cannot afford the time to write up a paper". A careful scientist would ensure that no more than three quarter of day is assigned to duties and on-going projects. Do not confuse working hard with working smart.

[4] `http://www.econlib.org/library/Bastiat/basEss1.html`

2. The Practicalities of launching a project

The second part of scoping a project or program consists of understanding the practicalities. Are you in a position to commandeer the necessary resources (telescope time, computational power, assembling a suitable team, etc). This involves asking questions: how do I sell this project to the Time Allocation Committee, how do I raise the funds for the project and how do I motivate others to join the project? For this second part you need to have tremendous enthusiasm. You need to reprogram yourself as a salesperson.

Now we approach the penultimate phase of the project. Say you have garnered support. Rarely it will be all the support you need. You have to decide when (and whether) to start the project. This decision is a very personal choice (and related to whether you are a risk-taker or not). My criterion is obtaining 75% of the resources. I say this on the experience of having started projects with less support (and hoping for the best)[5] and then suffering with extreme pain (and failure). This percentage is really a tough call.[6]

This brings me the important issue of "mentorship". It is essential that you identify a senior and distinguished figure who is knowledgeable about your field but not too personally interested in your project and convince them to be your mentor. A good mentor provides an outside perspective as well as identify potential failures in the project (based on his/her past experience). Also a mentor may be kind enough to alert you about new opportunities and nominate you for various kinds of recognition[7] and prizes.[8]

3. The Endgame

Let us now assume that you have decided to proceed with the project. The first thing you should do is to reprogram yourself and drop the "sell phase"[9] and reprogram yourself for the "do" phase. This phase is the exact opposite of the sell phase. You need to develop an accurate understanding of what is feasible and the timescale for completion. Amazingly enough you will see many prominent astronomers and especially those leading large projects continue to be salespersons even after their project is funded and in the process they oversell the project and lose perspective of their own project.

[5]Surprisingly this "aspiration based planning" is quite common. The thesis advocated by the proponents of this model is that if one aspires sufficiently hard then it will likely succeed. It is understandable for a young person to feel this way but quite sad to see older scientists behave this way and almost comical (and tragic) to see famous institutions initiate project based on the "fumes of aspiration". Many local examples abound.

[6]As J. Watson of the Helix fame said "You should as high as possible because you do not know how high you can jump". To this, based on personal and painful experience, I add the clause "but not so high that you will likely break your limb".

[7]You may be surprised to learn that many prizes do not receive a large number of nominations. The prize winner is simply the best choice, on average, amongst the small number of nominees.

[8]Prizes in academia usually carry nominal financial value and only your sibling may appreciate that you have won the Prize for finding the 1000th Pulsar. However, prizes bring recognition and hopefully increase your chance for your next NSF proposal.

[9]Having studied in Berkeley, I would say that "do not smoke the dope you peddle."

Finally, once the experiment starts working and the data starts rolling in you may falsely conclude that you are done. No! No! The final product is the paper(s). The usual excuses for not writing papers – "Understanding is what drives me. Now that I understand I am not motivated to write the paper(s)" or "I really enjoy the methodology so much that writing the paper is boring" or "Writing does not come naturally to me" etc. Only your mother (or father) would overlook and forgive your non-completion. I view astronomers who cannot *truly* complete their project – writing the papers – as being failures and not worthy of future funding or telescope allocation.[10]

Writing (quality) papers requires great discipline. You should set up a schedule including clear goals and stick to it.[11] When I was young I used to wake up at 4 am and write and write until 10 am and then the rest of the day was for other activities. I was a very productive astronomer.

Why is writing papers so important? Perhaps the most important reason is that the very act of writing clarifies your thinking and as a result increases the depth of your understanding. A second reason is more practical: there are many astronomers who would like to get lots of telescope time and lots of funding. Only those who have produced should receive these resources.

A Final Caveat: I realize that I have given you a rather "business-like" view of astronomical research. I would readily agree that this is only one of many views and that there are many ways to do research in astronomy. I have met several young people who hold a different view of what motivated them to become a researcher. I have no quarrel with these other views including views which hold that research is a noble activity and should only be pursued by very pure people. The above approach has worked well for me and it may work for some fraction of aspiring astronomers.

[10]And also not worthy of my time as a mentor.

[11]All successful writers – people who make their living writing – follow a variant of this approach.