

## Prof Fungus: Olympus Mons Observatory

You made your name by detailed statistical studies of the cepheid population in redshift two galaxies. As such, you had to worry a lot about samples. On the TAC, you frequently see sloppy proposals with little attention taken to sample size. Watch out for these proposals and reject them!

The basic question every proposer should address is: how big a sample do I need? Typical errors to watch out for include:

- **Poorly justified sample size.** Proposers often ask for time to do lots of targets, without showing why they chose this sample size, and what they can learn from it. They should have thought this through — they should know what sample size is needed to solve the puzzle in question. It is not enough for them to say “we want 300 quasar spectra”; why 300? What will they learn from 300 that they wouldn’t have learned from 100, or 10, or 1? Will 300 be enough, or will they need 20,000 to solve their puzzle?
- **Unique Object Syndrome.** You hate the word “unique” in proposals. It usually means that people are proposing to observe only one target, and are trying to bullshit about how exciting it is. How much can you possibly learn about the universe from one object? Imagine trying to study galaxies by looking only at the LMC – it would be hopeless. You need to have enough targets that you can say whether they are typical of the population in question.

Be merciless on proposals showing these grave errors!

## Dr Guzzlebeer: Institute d'Astrophysique de Luna

You hate blobologists! You meet them everywhere – astronomers who spend their lives taking images of blobby things (nebulae, galaxies, supernova remnants, radio galaxies etc). Not that these targets are uninteresting. It's just that you cannot usually learn much about the astrophysics of an object from a picture.

A typical proposal from a blobologist goes something like this. Such and such a type of blobby thing is really interesting. We want to image some of these blobby things at such and such a wavelength/resolution. Nobody has done this before for blobs just like this. These images will be used to constrain models of the blob physics.

That word “Constrain”! Arghh! What this really means is that they couldn't think of any good observing projects. So, they decided to take a few pictures and hope that someone else will think of something to do with them once they are published. They usually have no idea of what they will learn from their images, so they waffle on about how important their type of blob is, and use the magic word “constrain” to pretend that some astrophysically interesting results will come out.

These proposals are usually *very* specific about the details of the observation, and the properties of the targets, but the suddenly get awfully vague and waffly when it comes to talking about how the resultant images will be used to learn anything about the physics of these objects.

Watch out for proposals like this and reject them! Help stamp out the curse of blobology!

## Prof Beakers: Ganymede Telescope National Facility

These are difficult financial times for observatories all around the solar system. Economic rationalism is back in fashion, and it is hard to justify astronomy in financial terms. Just last week, the government of Mars announced major cuts in its funding for astronomy, claiming that astronomy is useless and never benefitted anyone.

What this means is that every observatory needs to justify its continued existence. And that means papers! You have to show that many influential publications come out of observations made with this telescope. The far-side telescopes have been doing rather badly in publication numbers recently. Only about 30% of observing runs are leading to papers at all! This must stop.

You believe that the culprit is poorly thought-out, “Fishing Trip” type projects. A good proposal should read like this. Here are two or more rival theories. Nobody can tell which is true. The following observation will clearly and categorically discriminate between these theories, as described in the proposal. We ask for time to make this observation. This sort of proposal will automatically lead to a paper.

A “fishing trip” proposal is along the lines of: nobody has made observations like this before. So we thought we’d make them and see what shows us.

They usually throw in a few vague ideas of what they might see, but there are no clear theoretical predictions that these observations will test.

Be vigilant! The observatory needs publications. Ask whether a given proposal is certain to lead to a paper, or whether the proposers are just hoping.

## Dr Sofa: University of New South Tycho

You study clusters of galaxies at redshifts above ten. This is hard work; exposure times are long and signal-to-noise ratios poor. This has made you somewhat of an expert on signal-to-noise ratios.

Getting a high signal-to-noise ratio is expensive. The exposure time needed to get  $s/n=100$  is 400 times longer than that needed for  $s/n=5$ . All proposers need to think carefully about what  $s/n$  ratio they need to do their science. If they want a high  $s/n$ , they will either need vast amounts of observing time, or a tiny sample size.

How much  $s/n$  do you need? To detect something,  $s/n=5$  is usually plenty. You need more if you want accurate photometry or spectroscopy of weak lines. What are the proposers trying to do? How much  $s/n$  do they need? Could they get the same result with less?

Time after time you see proposals which are vague on this crucial point. You are fed up with them. Be ruthless!

## Prof Spider: University of Miranda

There are lots of smart astrophysicists out there, from Mercury to Charon. A new volume of ApJ comes out every 23 minutes. It's hard to do anything really new. All work builds on what is already known, and should acknowledge this fact.

You see many proposals that are not really doing anything new. They are just repeating some well known observation on a new sample or a new object. For example, they might be measuring the stellar population of some new galaxies. Why is this interesting? Stellar populations are already known for millions of galaxies. Why does adding a few more help?

Another example: for decades, astronomers have been debating whether type 5b supernovae are preferentially found in ring-shaped superclusters. Survey after survey have found inconclusive results, basically because nobody could agree on a formal definition of the shape of a supercluster. Until this fundamental problem can be resolved, there seems little point in adding yet more inconclusive surveys.

For each proposal, ask whether it is truly new, and what it will add to our understanding of the universe. Will it lead to a conclusive result, or just add a new inconclusive result to the large collection in the literature?

## Prof Bubble: Anglo-Martian Observatory

You have the honour of chairing the Lunar Farside Observatory TAC. Your job is to get this bunch of fractious astronomers to agree on a ranking during the next hour. You cannot allow them to discuss each proposal for too long, or you will be here all day. Let everyone concisely have their say, then once you sense that minds have been made up, take the scores.

Each member of the TAC should rank each proposal on a scale of 1–5, where 1 is good and 5 is bad.

Your personal bias is towards clear proposals: proposals that state concisely and lucidly what they want to do, why it is interesting, and how they will do it.

You also have to look at the big picture: does this field of research really matter? There may be tens of thousands of astronomers studying turbulence in the atmospheres of M-dwarfs, or photoionisation in extragalactic planetary nebulae, but does anyone really care? If you go to your political masters, or to the general public, and say “Yes, after spending twenty billion dollars on this observatory, we now know the turbulent energy spectrum of M-dwarf atmospheres to 10% accuracy, rather than 30%.”, they will laugh at you.