The case for applied astronomy

Martin Elvis looks at our golden age of astronomy and gives his personal view of what the future may hold for space and astronomy research, as that golden age hits a funding wall.

We have just celebrated the 50th anniversary of the discovery of quasars. Maarten Schmidt’s half-page Letter to Nature announcing the enormous 15% redshift of 3C273 was published on 16 March 1963. Quasars were just one of the extraordinary discoveries in astronomy of the past 50 years. This is so obvious that it is a cliché to call ours the golden age of astronomy. The worrying thing about golden ages is that they don’t last long – otherwise they would not be special. But just how short these famous ages are is unsettling. Periclean Athens lasted 77 years, and the Renaissance just 27 years, using The Cambridge Ancient and Modern History volumes as an objective guide. At 50 years, then, our own astronomical golden age is getting long in the tooth.

More troubling than this numerology is the observation that our flagship telescopes are starting to cost very serious money by any definition. ALMA cost ~$1.5bn and the ELT will cost a similar ~€1bn. In space, the James Webb Space Telescope will cost ~$9bn. Major operating facilities are already threatened with closure to pay for ALMA operations and, until JWST is close to launch, no new NASA astrophysics flagship will get started. Dreams of a Terrestrial Planet Finder – a large interferometer in space – have been abandoned. Things are no better in planetary sciences, as the Mars Sample Return mission is slated to cost $6bn. In December, NASA Administrator Charles Bolden told the NASA Advisory Council’s Science Committee in Washington DC, that “we have to stop thinking about … flagship missions … The budget doesn’t support that.”

One mission, one waveband

For astronomy these costs mean that once the Great Observatories now operating are gone – Hubble, Chandra, Spitzer and Fermi in the US, and XMM-Newton and Herschel (already gone) in Europe – we won’t be able to afford a new generation of their successors. We will be limited to one flagship mission and one wavelength band at a time. The privilege of having comparably sensitive observations across the electromagnetic spectrum will start to decay.

That’s especially sad news for those of us following up Maarten Schmidt’s work by studying the growth of super-massive black holes, which appear as the objects we call quasars. Without more sensitive X-ray telescopes how will we know if that faint infrared source at redshift 15 is the first black hole growing fast, or a proto-galaxy of just-formed first stars? Pretty much every other object we study in the universe emits across the spectrum too, albeit less copiously than quasars. Going blind in some of our telescopic eyes is not a pleasing prospect. Is the end of our golden age looming?

We are victims of exponential growth, which always seems unimpressive at the start. Then suddenly the curves turn all but vertical – population growth and carbon emissions are two examples. For big science this has been called the “funding wall”. The funding wall scuppered US leadership in particle physics back in 1993 when the Superconducting Supercollider was cancelled by Congress for badly overrunning its budget.

I have seen these relentless exponentials at work over the course of my career. The first generation of space telescopes – Copernicus, UHURU and IRAS, in the ultraviolet, X-ray and thermal infrared respectively – were modest but pioneering missions. Each one found such great surprises in its waveband that the need for a more powerful generation of telescopes was obvious. The International Ultraviolet Explorer, the Einstein Observatory and the Infrared Space Observatory followed, each with a heftier price tag. To follow those we advocated for, and got, our current series of telescopes. In ESA these are Cornerstone missions, at NASA they are the Great Observatories. And each one costs a billion dollars or more. As each generation had to be at least ten times more powerful than the one before, this price tag should not be a surprise. Already the growing costs have meant that the time between generations has been increasing. It was 20 years from the launch of Einstein to the launch of the Chandra X-ray Observatory, for example, and if the next big X-ray mission, Athena+, goes ahead, it will launch almost 30 years after XMM-Newton.

How can we deal with this? The natural response is to team up, nationally, then internationally. But already many big telescopes involve Europe and the United States. ALMA also has Japan and other partners. The fact is that there is no more than one more doubling of cost-scale left, even if we include absolutely the whole world. Economists are familiar with this situation. Herbert Stein’s Law states: “If something can’t go on forever, it will stop.”

Faced with this funding wall, what can we do? There are three responses, apart from giving up: tactical, strategic and “meta-strategic”.

Tactical astronomy

Tactically, in the next few years, we can take a vow of restraint and not push billion-dollar telescopes, however great for science, because of their opportunity costs. Each one sucks the air out of the programme around it, pushing the next new start far into the future and diminishing the diversity that is a great strength of astronomy, as Simon White wisely argued in his article “Fundamentalist physics” (White 2007). For a billion dollars we can have five to ten experiments on the $100m to $200m scale. Several ground-based telescopes come to mind: the Event Horizon Telescope, the Cerno Chajnantor Atacama Telescope, a near-infrared interferometer (I’m thinking of the stalled Magdalena Ridge Observatory Interferometer) and even, at the high end, the Cerenkov Telescope Array. Each one of these gets us a factor of 10
or 100 in some capability for a relatively modest cost—probably not exceeding $1bn.

Once we start to build them though, we must be prepared to be ruthless and “kill our darlings” if they start to blow in cost. But if there are many projects under development, cancelling one will be easier. In space, a $200m price seems to be about the minimum. But at that level there are numerous ideas for Explorer-class missions, as they are known in the US, and these would be smart investments. It would be great if ESA’s similar new “S” programme could grow larger.

The problem with this vow of restraint is that both the US decadal survey and ESA Horizon processes tend towards choosing one big facility. The causes are both political and sociological. Large teams have more influence and more motivation to pull together. And it is far easier to present and sell one winner than a broad clutch of projects. Recognizing this as a problem is the first step.

Strategic astronomy

Strategically, on a decade-plus timescale, larger mirrors to collect more photons will be needed, however clever we get with smaller telescopes. For space, many of us pin our hopes on the innovative rocket company SpaceX to reduce launch costs. This would be good and seems to be even likely, but alone it won’t be enough. Launches are typically ~25% of a mission’s cost. If they were ten times cheaper—say 2.5% of the launch cost, and so essentially free—that would save a nice chunk off the budget. But unless we can cut 50% off the total price, we won’t get a second mission. To support a full fleet of Greater Observatories we need to lift 75% off current mission costs. That sounds impossible. Yet there may be a way. To do so we merely have to change the space engineering practice of the past 50 years!

Since the beginning of the space age, mass has dominated mission design. It costs between $10,000 and $20,000 to launch a kilogramme of anything to low Earth orbit, and has done so for decades. This single fact pushes space engineers into a corner. I have seen several highly paid engineers argue for a full afternoon over the placement of a 100 gramme, $2000 sensor. That discussion alone almost doubled the price, and it was the first of many. Ask anyone in the business and they will have similar experiences. If instead it is cheap to put mass in orbit then:

- We can have large solar panels so that our electronics can be standard avionics quality, not energy-sipping specialties.
- We could establish high reliability by flying more redundant systems than we do now. Extra reaction wheels on Kepler could have kept it operating at peak performance for years.
- We can use structures made of big steel girders instead of refined designs using light but expensive alloys that can only just survive launch.

This approach needs real study, but it looks like it would work for the spacecraft. It is not clear that it would work as well for the payload. Our telescopes have to be state-of-the-art and that is always expensive.

Meta-strategic astronomy

Eventually, on a generational timescale, the only way to keep up with an exponential function is by using another exponential with an exponent at least as large. Economies grow exponentially, and that’s where our funding ultimately comes from. The news always tells us that the economy grew x% this quarter which is, of course, exponential growth. If we can harness space missions to an exponentiating economy then we would have a long-term solution to the astronomy funding wall. I call this part “meta-strategic” because it requires us to think beyond astrophysics or even space engineering. We have to think of how to start and encourage an in-space economy.

Right now, space is a small industry with annual revenues of around $300bn. This is only two-thirds of the revenues of Walmart, the largest US supermarket company, and includes all ground-based GPS hardware; the in-space part of the industry is quite a bit smaller. If space were to grow at China-like rates of 10% a year for a generation (25 years) it would become 10 times larger. If this expansion was based on exploiting in-space resources—such as minerals on asteroids—the then the mining companies involved would have to prospect. Just 1% of their revenue applied to prospecting would be nearly double NASA’s total current budget. The pressure to make a profit would push investment into further cost reductions for spacecraft and launches, letting us build Terrestrial Planet Finders and other big telescopes that cost now relegates to fantasy. In business, “time is money”, so advanced rockets to bring the valuable ore back quickly (solar or nuclear powered) will be built to maximize profit. Once the Main Belt asteroids are being mined routinely, the incremental cost of stopping off at Mars for scientific expeditions will be small.

If you are a ground-based astronomer you may think this does not apply to you. But once the current generation of 25–39 m telescopes is built, we may find that the 100 m generation will be better built in space.

Applied astronomy

What can we do? Now is a good time to begin. The “NewSpace” movement is buzzing with novelty and has a huge emphasis on doing rather than studying. Planetary scientists can help directly, especially lunar scientists, by using their knowledge and expertise to help find valuable resources. Astronomers may think they have nothing to add. But a likely source of growth for the in-space economy is asteroid mining. There are already at least two NewSpace companies that have asteroid mining as their goal. And asteroids, like quasars, are quasi-stellar. Only a small minority of asteroids can be investigated by radar and spacecraft missions. But there are about 10 million near-Earth asteroids larger than 20 m across. That’s far too many for one-by-one visits.

Asteroids have to be primarily investigated by standard techniques of astronomy: astrometry, photometry and spectroscopy. It turns out that not only are our techniques valuable, but also that the large volumes of high-quality observations mining companies need require the professional skills of astronomers. They need thousands, perhaps tens of thousands, of high signal-to-noise, well-calibrated spectra, light curves and astrometric measurements. Moreover, over our theoretical understanding of asteroids and meteorites is too undeveloped to be a reliable prospecting guide, so true research is needed too.

This development could start now. In the current yard sale of mid-sized telescopes, including UKIRT, Lick, the Calar Alto Observatory and some Kitt Peak telescopes, lies an opportunity. Repurposed, these telescopes can provide investors in space mining the data they need, either publically or commercially.

Geophysicists are used to being employed by the extractive industries. Astronomers may well soon find themselves similarly employed. Before the invention of spectroscopy, astronomers were useful. They supplied astronomical almanacs for navigation, and long before that kept the calendar for farming. The past century and a half have been pure pleasure—understanding the entirety of the universe with no thought for practical application. Now applied astronomy is beginning again. Those astronomers who go commercial to provide this data will be ensuring the long-term future of astronomy. Then we may be able to look back in 50 years, as we do now on quasars and the explosion of new astrophysics that they epitomize, and realize that in our golden age we had, in fact, barely begun.

Martin Elvis is an astronomer at the Harvard-Smithsonian Center for Astrophysics. The views expressed here are his own and not those of the Smithsonian Institution or Harvard University.

References

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