commentary

Benefits of diversity

Abraham Loeb

Discoveries in astronomy — or, in fact, any branch of science — can only happen when people are open-minded and willing to take risks.

According to Mark Twain, "It ain't what you don't know that gets you into trouble. It's what you know for sure that just ain't so." This illustrates a very common flaw astronomers have, which is to believe that they know the truth even when data are scarce. This fault is the trademark of a data-starved science. It occasionally leads to major blunders by the scientific community causing the wrong strategic decisions, and bringing about unnecessary delays in finding the truth. Let me illustrate this phenomenon with ten examples, in chronological order.

Large telescopes. In 1909, Edward Charles Pickering, who served as director of the Harvard College Observatory from 1877 until 1919, argued that telescopes had reached their optimal size of 50–70 inches and there was no advantage to be gained from seeking larger apertures. In December 1908, in an article titled 'The Future of Astronomy', he wrote[1]:

> *"It is more than doubtful, however, whether a further increase in size is a great advantage. Much more depends on other conditions, especially those of climate, the kind of work to be done and, more than all, the man behind the gun. The case is not unlike that of a battleship. Would a ship a thousand feet long always sink one of five hundred feet? It seems as if we had nearly reached the limit of size of telescopes, and as if we must hope for the next improvement in some other direction."*

Pickering's blunder led to a major blow for observational astronomy on the east coast of the USA. On the west coast, just before Pickering's article was published, George Ellery Hale obtained first light on the 60 inch telescope at Mount Wilson Observatory in California, which became one of the most productive telescopes in astronomical history. Around the same time, Hale received funding from John Hooker and Andrew Carnegie to create a larger telescope. The 100 inch telescope was completed in 1917; Edwin Hubble and Milton Humason later used it to discover the expansion of the Universe. It was surpassed in 1948 by the 200 inch telescope at the Mount Palomar Observatory in California, which played a key role in the discovery of radio galaxies and quasars and in studies of the intergalactic medium[2]. Clearly, bigger telescopes continued to benefit astronomy as technology improved.

Composition of the Sun. While working on her PhD thesis in 1925, Cecilia Payne-Gaposchkin (who became the first to be awarded a PhD in Astronomy at Harvard-Radcliffe) interpreted the solar spectrum based on the Saha equation and concluded that the Sun's atmosphere is made mostly of hydrogen. During the review of her dissertation, the distinguished Princeton astronomer Henry Norris Russell convinced her to avoid the conclusion that the composition of the Sun is different from that of the Earth, as it contradicted the conventional wisdom at the time[3].

Maser and complex molecules. When Charlie Townes worked on his experimental demonstration of the maser in 1954, two Nobel laureates, Isidor Isaac Rabi and Polykarp Kusch, tried to stop him by saying[4]: "Look, you should stop the work you are doing. It isn't going to work. You know it's not going to work, we know it's not going to work. You're wasting money. Just stop!" Three months later, the maser worked. Similar circumstances repeated when Townes was determined to discover complex molecules in space and experienced resistance from astronomers who argued that the interstellar gas density is so low and the ultraviolet illumination so intense that any surviving molecules would be too scarce to be detectable[5].

X-ray astronomy. In the early 1960s a panel of experts was assembled by NASA to evaluate the merits of a proposal to launch an X-ray telescope into space (as the Earth's atmosphere blocks X-rays). The panel concluded that the scientific motivation for an X-ray space telescope was weak, as most of the X-ray sources would be flaring stars. The launch of an X-ray telescope by NASA was therefore delayed by half a decade, after which astronomers discovered X-ray emission from numerous other sources, such as accreting black holes and neutron stars, supernova remnants and galaxy clusters.

Dark matter. In the early 1970s Jerry Ostriker gave a talk at the California Institute of Technology describing the case — developed by him in collaboration with Jim Peebles and Amos Yahil — for spiral galaxies having dark matter haloes that comprise most of their mass[6]. Members of the audience were contemptuous of the idea and dismissed it as wild theoretical speculation.

Gravitational lensing. Around 1980, shortly after the discovery[7] of the first gravitational lens (QSO 0957+561 A/B), Ed Turner at Princeton University was advised by a highly distinguished astronomer not to spend much time working on gravitational lenses because they would turn out to be useless curiosities. For a few years, lenses were widely regarded by astronomers as unimportant and it was almost impossible to get observing time or grants to study them.

Cosmology. Around 1990, during my term as a postdoctoral fellow at Princeton, I asked a prominent astronomer from another prestigious academic institution whether they would consider hiring junior faculty in the field of theoretical cosmology. He replied: "we might contemplate this possibility if we could only convince ourselves that cosmology is a science". Two years later, in 1992, the COBE satellite reported the detection of microwave background anisotropies[8].

High-redshift galaxies. Piero Madau at the University of California, Santa Cruz, once told me that he had great difficulties publishing a paper he wrote in the mid-1990s on intergalactic absorption and the colours of high-redshift galaxies because the referee kept arguing: "we all know that there are no normal galaxies above a redshift of two".

Kuiper Belt objects. David Jewitt at the University of California, Los Angeles, could not get telescope time or funding for attempts to detect the conjectured population of Kuiper Belt objects[9]. He used observing time and funding he received for other projects until he finally discovered the first of these objects in the outer Solar System with Jane Luu in 1992, using the 88 inch telescope at Mauna Kea, Hawaii.

Close-in Jupiters. The first planets ever discovered around a main sequence star other than the Sun had masses similar to Jupiter but were orders of magnitude closer to their host star than Jupiter is to the Sun. This can be simply understood as a selection effect, because the reflex motion of a star due to a close-in planet is much easier to detect than the motion induced by a distant planet. But because Jupiter is considerably farther out from the centre of the Solar System, time allocation committees on major telescopes declined proposals to search for close-in Jupiters for years based on the argument that such systems would deviate dramatically from the architecture of the Solar System and hence are unlikely to exist.

Theoretical prejudice prevented HD 114762 b, discovered in 1989[10], from being recognized as a planet for over six years. It was only acknowledged following the announcement of the discovery of 51 Peg b in 1995[11] and after others had found similar examples. As it turns out, Otto Struve had already suggested[12] in 1952 that close-in planets may exist and would be easy to find through both radial velocity and transit observations, but his paper was completely ignored because of theoretical priors.

These examples and many more like them (starting with the ancient view that the Earth is at the centre of the Universe and that the Sun revolves around it), demonstrate that progress in astronomy can be delayed by the erroneous proposition that we know the truth even without experimental evidence. Lapses of this type can be avoided by an honest and open-minded approach to scientific exploration, which I label as having a 'non-informative prior' (known as a Jeffreys prior in Bayesian statistics). This unbiased approach, which is common among successful crime detectives, gives priority to evidence over imagination, and allows nature itself to guide us to the correct answer. Its basic premise is humility — the recognition that nature is much richer than our imagination is able to anticipate.

Uniformity of opinions is sterile; the co-existence of multiple ideas cultivates competition and progress. Of course, it is difficult to know in advance which exploratory path will bear fruit, and the back yard of astronomy is full of novel ideas that were proven wrong. But to make the discovery process more efficient, telescope time-allocation committees and funding agencies should dedicate a fixed fraction of their resources (say 10–20%) to risky explorations. This can be regarded as affirmative action to promote a diversity of ideas, which is as important for the progress of science as the promotion of gender and ethnic diversity. ❐

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