CHAPTER FIVE

A FRESH APPROACH TO THE SEARCH FOR EXTRATERRESTRIAL INTELLIGENCE (SETI)

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A fresh perspective is required in the Search for Extraterrestrial Intelligence (SETI). This chapter explains the importance of this innovative SETI approach, putting it in the context of important foundational questions being asked in science today.

Solving cosmic mysteries

Astronomy addresses questions that were previously in the realm of philosophy or religion, such as “How did the Universe start?” (Loeb 2015b), in other words, “When did the first stars form and light up the universe?”, which can also be framed as the scientific version of the story of Genesis (Loeb 2014b); and “What is the origin of life?” (Loeb 2015b), “When and where did life start to form?” or, in simpler words, “Are we alone, or is the universe teeming with life?”. We currently have the technology to build telescopes and instruments that will attempt to answer these questions over the next decade (Loeb 2014b). Therefore, in my own academic work at Harvard University, I actually have the privilege of addressing philosophical questions using modern scientific means (Loeb 2015b). I also have the privilege of sharing some of my thoughts on these topics as an extensive series of essays in *Scientific American*.

We are born into the world like actors placed on a stage without knowing what the play is about. After studying the universe, we realize that the stage is huge (10^{26} times larger than our body) and that the play has been going on for 13.8 billions years before we came to exist. Clearly, we do not serve a central role in this play and should adopt a sense of cosmic modesty. But it would also be prudent for us to search for extraterrestrial actors, since they might have been around longer and perhaps have a better understanding of what the play is about. Most stars formed billions of years before the Sun. Therefore, numerous technological civilizations could have formed before us. We can find their relics in the form of advanced technological equipment that they launched into space, by engaging in space archaeology.

My main interests involve the beginning and future of the universe, the nature of black holes and the search for extraterrestrial life (Loeb 2019c). I also like to think about perpetual change on the cosmic scale. The reality is that the cosmos changes all the time. The most stubborn hold back in my lifelong waiting game of scientific forecasts involves identifying a signature that the universe as a whole evolves in real time (Loeb 2021l).

The physical size of the Universe is not difficult to imagine, just the way an ant could imagine the size of a big city. It takes a long time to traverse but it is just bigger than the length scales one encounters on a daily basis. So, there is nothing qualitatively difficult to understand here. What is overwhelming about the universe is its content. First the fact that same laws that govern nature in laboratory experiments also control the way the Universe behaves on vast scales. This should not be taken for granted. The Universe could have been chaotic. Humans do not obey our societal laws at nearly the same precision as nature obeys the physical laws. This fact alone amazes me. But then, the Universe contains a remarkable wealth of phenomena far more extreme than we find on Earth, including exploding stars, accelerated expansion of the universe, dark matter, black holes, numerous planets of different shapes and compositions, and so on. What happened before the Big Bang is one of the unsolved mysteries of modern cosmology. Yet, we can search for clues from the beginning of the Universe. There are some observables such as gravitational waves or the statistics of density fluctuations that can inform us about what happened in the Universe before or shortly after the Big Bang (Loeb 2019c).

In November 1915, Einstein formulated the mathematical equations that established the foundation for his General Theory of Relativity. The first solution of Einstein's equations was derived by Karl Schwarzschild a few months later, while serving on the German front during World War I. This solution describes the curved spacetime around a point of mass, labeled a "black hole" by John Archibald Wheeler half a century later. The solution admits a spherical event horizon that surrounds the singularity at the so-called Schwarzschild radius. No information can escape from inside this event horizon (Loeb 2021k).

Black holes are simple and complex at the same time. They are described by mass, charge and spin, yet as Jacob Bekenstein first recognized, they carry a huge entropy (Loeb 2020c). In addition, black holes are hidden behind event horizons. No information leaks out (Loeb 2020d).

In Schwarzschild's solution to Einstein's equation, the curvature of spacetime diverges at the central point, called the "singularity," because that is the singular point where Einstein's theory breaks down. This breakdown is for an obvious reason: Einstein's theory of gravity did not incorporate Quantum Mechanics. However, quantum corrections must be important when the curvature is large, and most physicists expect the singularity to disappear in a quantum theory of gravity. Despite many attempts to unify General Relativity with Quantum Mechanics (such as versions of string theory or loop quantum gravity), we do not have an experimentally verified version of the theory as of yet (Loeb 2021k).

Popular speculations based on incomplete theories of quantum gravity (such as the anthropic argument based on the string theory landscape of the multiverse) have no empirical support, yet they suppress much-needed efforts to understand dark energy by means of an alternative theory that unifies quantum mechanics and gravity. The fact that we have not yet converged on such a theory is indicated by paradoxes in other areas of physics. For example, information contained in, say, an encyclopedia, is lost if it is swallowed by a black hole that ultimately evaporates into heat known as Hawking radiation. This contradicts a basic premise of quantum mechanics that information is preserved and is known as the “information paradox” (Hawking, Perry, and Strominger 2016; Loeb 2016).

Stephen Hawking demonstrated by means of a detailed calculation almost half a century ago that black holes evaporate by emitting thermal radiation. Nothing survives according to this calculation. There is a fundamental question that physicists are still struggling with: where does the information that went into the black hole go? Quantum mechanics implies that information cannot disappear, yet Hawking’s calculation implied that it does. Accordingly, the information paradox remains one of the unsolved problems in modern physics (Loeb 2019c).

Matter falling into a black hole ends up in a singularity where, as mentioned above, the curvature of spacetime diverges and Einstein’s equations of general relativity break down (Loeb 2020d). What would a singularity look like in the quantum mechanical context? Most likely, it would appear as an extreme concentration of a huge mass (more than a few solar masses for astrophysical black holes) within a tiny volume (Loeb 2018d).

Very few physicists discuss black hole singularities. The reason is simple: to explore the true nature of singularities we need a theory that unifies general relativity with quantum mechanics, and we do not have a unique, well-defined formalism for doing that. Even in the context of specific proposals for a unified model, such as string theory, which has no experimental verification, the nature of black hole singularities is rarely discussed because of its mathematical complexity (Loeb 2018d).

But can Einstein’s equations give rise to what are referred to as “naked singularities,” which are not hidden behind a horizon? If such regions of spacetime affect us, we would not be able to predict our future using general relativity. We should keep our eyes open. If detected by our telescopes, naked singularities would be the best laboratories for testing theories of quantum gravity, such as string theory. The problem is that candidate theories of quantum gravity do not predict how such sources would look. They have no “skin in this game.” Hence, observers should lead the way, including through the challenging pursuit of gravitational waves from the Big Bang singularity (Loeb 2020d).

One idea I had after a series of interdisciplinary meetings, and which I continue to think about is that an observable quantum signal from the embedded singularities from collisions of black holes could guide us in the search for a unified theory. We know that the singularities at the centers of astrophysical black holes mark the breakdown of Einstein’s theory of general relativity, and that Einstein’s theory of gravity breaks down due to quantum corrections at a universal curvature scale. Singularities represent the only breakdown sites accessible to experimentalists, since the only other known singularity, the Big Bang, is believed to be invisible due to the vast expansion that occurred afterwards during cosmic inflation (Loeb 2018d).

The Laser Interferometer Gravitational-Wave Observatory (LIGO) collaboration detected the first gravitational-wave signal from a black hole merger, for which the Nobel Prize was awarded to Rai Weiss, Barry Barish and Kip Thorne in 2017. The subsequent discoveries of many additional LIGO sources have begun to move astronomy from being focused on electromagnetic radiation—visible light, infrared radiation, microwaves and so on—to a multi-messenger discipline (Loeb 2019a).

What happens when two singularities collide? How do they merge to a single singularity, and does this process have an imprint on the gravitational wave signal that is observable by LIGO? If there are observable signatures of merging singularities, existing computer simulations are, by construction, blind to them. Nevertheless, when black holes collide, interactions between their cores might leave an imprint on the resulting gravitational waves (Loeb 2018d).

From a physicist perspective, black hole collisions generate gravitational waves, which are ripples in the fabric spacetime. The LIGO experiment detects those ripples. My hope is that there is some signature in this signal that could teach us about how to unify quantum mechanics and gravity. The same holds for the gravitational waves generated in the early universe, where again Einstein's theory breaks down. The only reliable papers are those predicting what Einstein's equations imply. But we need something beyond that from the point of view of a reliable quantum theory of gravity.

We can listen to the vibrations of a black hole horizon that is strongly shaken through its collision with another black hole, hoping to learn more about the nature of the singularities hidden inside. Future generations of LIGO detectors could serve as the “child’s ears” in extracting new information from these vibrations. This could motivate gravitational wave observers to develop more sensitive detectors. At the very least, we might be able to outline the landscape of possibilities (Loeb 2018d). We should invest in such high-risk, high-reward facilities, such as the space-based gravitational wave observatory, the Laser Interferometer Space Antenna (LISA) (NASA *n.d.*; Loeb and Tripathi 2019).

Space archaeology offers fresh ideas for SETI

Physicist Enrico Fermi asked: “Where is everybody?”, suggesting that advanced extra-solar civilizations may not exist because we do not detect obvious signals from them in the sky (Dvorsky 2018). Fermi’s paradox is pretentious in assuming that that we carry cosmic significance (Loeb 2021g). I prefer the Copernican principle that the present time is not special (Loeb 2021a). Nevertheless, there is a large contingency of scientists who think that we are unique or special. To me this is a sign of arrogance. I prefer to adopt the principle of “cosmic modesty,” by which the fact that we exist implies that we are not alone because a quarter of all the stars have planets with surface conditions similar to those on Earth (Loeb 2019c).

Rather than being guided by Fermi’s paradox, “where is everybody?” or by philosophical arguments about the rarity of intelligence, we should invest funds in building better observatories and searching for a wide variety of artificial signals in the sky (Loeb 2017a). Observation is central to science. Thanks to data collected by the Kepler space telescope, we now know that about half of all Sun-like stars, of which there are tens of billions in the Milky Way, host a rocky, Earth-size planet in their habitable zone. Within this zone, the planet’s surface temperature can support liquid water and the chemistry of life. The huge number of sites where life may exist begs the question: which fraction of these planets host technological civilizations like ours, capable of communication? (Loeb 2021c). The chemistry of “life as we know it” requires liquid water but being at the right distance from the host star for achieving a comfortable temperature on the planet’s surface is not a sufficient condition for life. The planet also needs to have an atmosphere. In the absence of an external atmospheric pressure, warming by starlight would transform water ice directly into gas rather than a liquid phase (Loeb 2017a).

The recent discovery (Anglada-Escud et al. 2016) of a habitable Earth-mass planet four light years away and orbiting our nearest star, Proxima Centauri, revealed a new opportunity in the search for life. Due to the proximity of this planet, Proxima b, it is easiest to use it as a first target, either for remote sensing of the faint radiation signals from biologically produced molecules such as oxygen, or for sending spacecraft that will photograph the planet's surface from a close distance. The Breakthrough Starshot initiative, whose advisory committee I chair (Breakthrough Initiative *n.d.*), aims to launch a lightweight spacecraft to a fifth of the speed of light using a laser beam pushing on a sail (Manchester and Loeb 2017; Christian and Loeb 2017; Hoang et al. 2017; Hoang and Loeb 2017) so that the camera attached to the sail could reach nearby habitable planets like Proxima b within our lifetimes. The pursuit of this concept became feasible owing to recent advances in laser technology and the miniaturization of electronics. Starshot already has begun a five-year phase of technology demonstration at a generous funding level provided by the entrepreneur and physicist Yuri Milner through the Breakthrough Prize Foundation. By taking photographs during a flyby of Proxima b, we would like to find out whether the planet's surface shows signs of vegetation, volcanic activity or an oceanic glint (Loeb 2017a).

In addition to the discovery of Proxima b, astronomers also have discovered three more potentially habitable planets out of seven around another nearby star, TRAPPIST-1, which is ten times further (Gillon et al. 2016). If life has formed on one of these three, it has likely been transferred to the others (Lingam and Loeb 2017a; Loeb 2017a). But this also raises an important scientific question: 'is life most likely to emerge at the present cosmic time near a star like the Sun?' By surveying the habitability of the Universe throughout cosmic history from the birth of the first stars 30 million years after the Big Bang to the death of the last stars in ten trillion years, one reaches the conclusion (Loeb, Batista, and Sloan 2016) that unless habitability around low mass stars is suppressed, life is most likely to exist near dwarf stars such as Proxima Centauri or TRAPPIST-1 trillions of years from now. Based on all available data on dwarf stars, the likelihood for life around Proxima Centauri and TRAPPIST-1 is several orders of magnitude smaller than on Earth (Lingam and Loeb 2018; Loeb 2017a).

The Kepler satellite survey of nearby stars has allowed astronomers to infer that there are probably more habitable Earth-mass planets in the observable volume of the Universe than there are grains of sand on all the beaches on Earth (Loeb 2017a). This implies to me that we cannot be alone (Loeb 2019c). Since the dice of life was rolled in billions of other locations within the Milky-Way under similar conditions to those on Earth, life-as-we-know-it is likely common (Loeb 2021g). When you roll the dice so many times, there must be life elsewhere. We just need to find it (Loeb 2019c).

Our civilization has reached an important milestone. We now have access to unprecedented technologies in our search for extraterrestrial life, be it primitive or intelligent. The search for primitive life is currently underway and well-funded, but the search for intelligence is out of the mainstream of federal funding agencies. This should not be the case given that the only planet known to host life, Earth, shows both primitive and intelligent life forms. Once life develops on a planet like Earth, it is unlikely to be eradicated by astrophysical catastrophes other than the death of the host star (Sloan, Batista, and Loeb 2017). In fact, sub-millimeter animals, such as tardigrades, are capable of surviving the harsh environment of space, and could potentially travel between planets on asteroids and comets (Loeb 2017a).

The Drake equation quantifies our likelihood for receiving a radio signal from another civilization in the Milky Way galaxy (Loeb 2021c). However, it misses a crucial possibility: most technological civilizations that ever existed might be dead by now (Loeb 2020e). In other words, it does not apply to physical messages that may arrive at our doorstep long after their senders may have perished on their remote island in outer space, which means that any message they sent may not have been intended for us specifically (Loeb 2021c).

Classical Searches for Extraterrestrial Intelligence (SETI) focuses on the search for radio signals from an extraterrestrial intelligence. All in all, galactic nuclei offer launching sites for the fastest habitable platforms that nature offers for free. It would not be surprising if advanced technological civilizations choose to migrate towards galactic centers for the same reason that astronauts and spectators flock to Cape Canaveral during rocket launches. With that perspective in mind, classical SETI should check for radio signals coming from riders of hypervelocity stars kicked gravitationally by the black hole there at high speeds outwards. We might also notice celebratory fireworks from their relatives at the Galactic center whenever a high-speed star is shot out of there (Loeb 2021j).

In addition, the evidence for an extraterrestrial civilization might not be in the traditional form of radio communication signals. Rather, it could involve detecting artefacts on planets through the spectral edge from solar cells (Lingam and Loeb 2017c), industrial pollution of atmospheres (Lin, Gonzalo, Loeb 2014), artificial lights (Loeb and Turner 2012), or bursts of radiation from artificial beams sweeping across the sky (Guillochon and Loeb 2015). The latter type of signal is to be expected if another civilization has mastered by now the technology of lightsails that we are developing with Starshot (Lingam and Loeb 2017b; Guillochon and Loeb 2015; Loeb 2017a). We are basically developing the technology of pushing a sail with light in the same way that a sail on a sailboatis pushed by air reflected off its surface (Loeb 2019c).

As already mentioned, one of the areas I promote is space archaeology, namely the search for relics from ancient cultures that are not around anymore (Loeb 2019c), through the artifacts they left behind. Similar to the work of archaeologists who dig into the ground, astronomers can search for technological civilizations by digging in space (Loeb 2020e). My view is somewhat similar to a view articulated decades ago, which was referred to as the Search for Extraterrestrial Artifacts (SETA) (Freitas and Valdes 1985). It seems obvious to me that space archaeology, which today is a burgeoning field of research concerned with the search for extraterrestrial technological relics, should be funded as generously as the search for, say, the faddish Weakly Interacting Massive Particles (WIMPs), which were thought to be the constituents of dark matter (Loeb 2021e).

It is prudent to start the search in our back yard and look for technological equipment floating through the solar system (Loeb 2020e). The simplest way to detect extraterrestrial equipment is through its reflection of sunlight, namely by searching under the nearest lamppost, the Sun. The first interstellar object that originated outside the Solar System and was detected this way near Earth is 'Oumuamua (Loeb 2018b; Loeb 2020g). This 100-meter object showed weird properties, such as an extreme geometry—most likely pancake-like, an excess push without a cometary tail or spin change, an unusually shiny surface and an unlikely low speed relative to the rest frame of the local population of stars (Loeb 2020e). The most peculiar fact about 'Oumuamua was that it deviated from an orbit shaped purely by the Sun’s gravity (Loeb 2020g).

My book, *Extraterrestrial* (2021d), tells the story of the scientific discovery of 'Oumuamua, meaning ‘scout’ in the Hawaiian language, by the Pan STARRS facility in Hawaii on October 19, 2017. As the first interstellar object detected near Earth from outside the Solar system, it looked weird, unlike any comet or asteroid seen before within the Solar system. The book details the unusual properties of 'Oumuamua: it had a flattened shape with extreme proportions that had never been seen before among comets or asteroids, as well as an unusual initial velocity and a shiny appearance. It also lacked a cometary tail, but nevertheless exhibited a push away from the Sun in excess of the Solar gravitational force. As a regular comet, 'Oumuamua would have had to lose about a tenth of its mass in order to experience the excess push by the rocket effect. Instead, 'Oumuamua showed no carbon-based molecules along its trail, nor jitter or change in its spin period, as is expected from cometary jets. The excess force could be explained if 'Oumuamua was pushed by the pressure of sunlight, namely if it is an artificially-made lightsail: a thin relic of the promising technology for space exploration that was proposed as early as 1924 by Friedrich Zander and is currently developed by our civilization. This possibility would imply that 'Oumuamua is possibly artificial space trash (Loeb 2021c; Loeb 2021g), or, more poetically, a message in a bottle (Loeb 2021c). We wrote a regular scientific paper aimed at explaining an anomaly in the data, namely the extra force exhibited by 'Oumuamua’s trajectory in addition to the Sun’s gravity, in the absence of visible cometary outgassing. We suggested 'Oumuamua may be pushed bysunlight, like the light-sails we are currently developing in the Breakthrough Starshot initiative that I am involved in (Loeb 2019c). Our own civilizationhas dreamed of such a perfect spacefaring technology for decades, and I had recently helped to design a prototype of one for the Breakthrough Starshot Initiative (Loeb 2021e).

As mentioned above, since its discovery, 'Oumuamua showed unusual features. These features make 'Oumuamua weird, belonging to a class of objects that we had never seen before. In terms of the details, based on its reflection of sunlight, it was inferred to be much more elongated or flattened than any known asteroid or comet in the Solar System. We do not have an image of it. The information about its shape stems from the variation in reflected sunlight as it spins. Even a thin sheet that is folded as an umbrella might appear from a distance similar to a rotating cigar based on the variation of reflected sunlight. Moreover, 'Oumuamua's motion indicated that it originated outside the Solar System from the so-called “Local Standard of Rest" (obtained by averaging the random motions of all nearby stars), with less than one in 500 nearby stars being as slow in that frame. But most intriguing was the fact that its trajectory deviated from that expected based on the Sun’s gravitational force. Such a deviation could be caused through a rocket effect from cometary outgassing (due to the vaporization of ice by heating from the Sun), but no cometary tail had been seen around 'Oumuamua. Moreover, cometary outgassing would have produced a variation in its rotation period, which was not detected. If not cometary outgassing, what could be causing the excess acceleration of 'Oumuamua? We propose that the peculiar acceleration of 'Oumuamua is caused by the push (not heating) of radiation from the Sun. In order for sunlight to account for the observed acceleration, 'Oumuamua needs to be less than a millimeter thick but tens of meters in size. A pancake-like geometry was previously suggested to explain the light curve of reflected sunlight during the tumbling motion of 'Oumuamua. These inferred dimensions are unusual for rocks, but we do not know whether 'Oumuamua is a conventional asteroid or comet since we do not have an image of it. Our paper shows that a thin object of the required dimensions could survive its journey through the entire Milky Way galaxy unharmed by collisions with atoms or dust particles in interstellar space. We do not know how long its journey had been (Loeb 2018a).

What is the origin of 'Oumuamua? One possibility is that the object is a light-sail floating in interstellar space into which the Solar System ran, like a ship bumping into a buoy on the surface of the ocean. A light-sail is a sail pushed forward as it reflects light.Indeed, the Spitzer telescope detected no thermal emission from 'Oumuamua, indicating that its surface is highly reflective. If the reflectivity is high, the inferred size of the object drops from a few hundreds of meters (which was deduced based on the albedo of rock) to a few tens of meters. It is unclear whether 'Oumuamua might be a defunct technological debris of equipmentthat is not operational any more or whether it is functional. Radio observatories failed to detect transmission from it at a power level higher than a tenth of a single cell phone. If 'Oumuamua originated from a population of similar objects on random trajectories, its discovery requires the production of a thousand trillion such objects per star in the MilkyWay. This number exceeds considerably theoretical expectations for asteroids based ona calculation I published with collaborators a decade ago. However, the inferred abundance could be reduced considerably if 'Oumuamua was on a reconnaissance mission (Loeb 2018a).

When the first interstellar object, 'Oumuamua, was glimpsed passing through our solar system, scientists quickly agreed that it was weird on half-a-dozen counts, as described above. However, despite these anomalies, the mainstream scientific community immediately declared business as usual and decreed the object to have been an unusual asteroid orcomet—albeit one that was unlike any asteroid or comet seen before (Loeb 2021e). The many anomalies exhibited by 'Oumuamua forced all natural interpretations of it to invoke object types that we have never seen before—all with major drawbacks—like a hydrogen iceberg, which will likely evaporate by absorbing starlight during its journey, a nitrogen iceberg that is unlikely to exist in sufficient abundance; a dust "bunny" a hundred times more rarefied than air, which might not have the material strength to withstand heating to hundreds of degrees by the Sun; or a tidal disruption relic, which would not possess the pancake-like shape inferred for 'Oumuamua. Moreover, the proposal, that it is an elongated fragment from the gravitational disruption of a bigger object by a star, faces the shortcoming that such disruptions are rare and that 'Oumuamua's shape was inferred to be pancake-like based on its light curve (Loeb 2021e). To learn more, we must continue to monitor the sky for similar objects (Loeb 2021g).

In other words, at the same time that conservative scientists argue for "business as usual" regarding 'Oumuamua, other reputable scientists admit that the object is weird and suggest "never seen before" explanations for it, each of which requires an imaginative leap much greaterthan the one necessitated by the lightsail hypothesis. At the same time, the mainstream orthodoxy contradicts itself by claiming that 'Oumuamua is not unusual and at the same time endorsing these notions that it could be explained by "never-seen-before” natural mechanisms. One cannot escape the impression that these exotic explanations are promoted simply to avoid a discussion on the possibility that 'Oumuamua might be of artificial origin. The response brought to mind a kid who has encountered many cats at home and, upon visiting the zoo and seeing an elephant, simply assumes it to be an unusual cat. Such naivete is charming ina child; it is less tolerable in a scientist. We ought to hold ourselves to a higher standard (Loeb 2021e).

Why is the study of alternative explanations for the anomalies of 'Oumuamua any different from the search for radical explanations for possible anomalies tied to the unknown nature of the dark matter? Given that between a quarter and half of all stars we examined host an Earth-like planet with a surface temperature that can support liquid water and the chemistry of life-as-we-know-it, the proposition that we are not alone is rather conservative and should have been endorsed by the mainstream by now. Yet our scientists—and, up until very recently, our elected leaders—would prefer not to look under this particular rock. In 1993, Congress halted federal for SETI, even though only a tiny fraction of all possible technological signatures of extraterrestrial civilizations had been searched for at that point (Loeb 2021e).

The data we gathered on 'Oumuamua is incomplete, and we must continue to monitor the sky for similar objects without prejudice (Loeb 2021c). One way to end any superfluous debates is to capture photographs of such objects. There is good reason to be optimistic, as there would be future opportunities to snap such apicture. The Pan-STARRS telescope discovered 'Oumuamua while surveying the sky for a few years (Loeb 2021g). Starting in 2023 the *Legacy Survey of Space and Time* (LSST) on the Vera C. Rubin Observatory will survey the sky for new objects (LSST *n.d.*). LSST is far more sensitive than any previous survey telescope like Pan STARRS (Loeb 2020e). An advance warning of more than a year from LSST about an approaching object of interestwould allow to launch a space mission from Earth that would intercept its orbit (Loeb 2021g). In other words, if we find another weird object like 'Oumuamua on its way towardsus, we could send a spacecraft to take a close-up photo of it and identify its nature (Loeb 2021e). This strategy resembles asteroid intercept missions, such as OSIRIS-Rex - which visited the asteroid Bennu and will return a sample from it to Earth in 2023. Photography of interstellar artifacts will usher the new frontier of space archaeology (Loeb 2021g). Let my hypothesis about 'Oumuamua be judged according to the evidence from this research, rather than by the popularity contest that guides so much of modern science (Loeb 2021e).

LSST should find many more 'Oumuamua-like objects, possibly a new one each month or every few years. Identifying artificial objects among the asteroids and comets in the Solar system is similar to searching for rare plastic bottles among the natural rocks on a beach. How can we obtain resolved images of weird interstellar objects to separate them from rocks? Two approachescome to mind. One is to deploy numerous cameras in advance within the orbit of the Earth around the Sun so that one of them will be close enough to the path of an interstellar objectof interest. Another strategy is to launch a dedicated spacecraft equipped with a camera as soon as LSST identifies a weird interstellar object on its approach towards us (Loeb 2021g).

While reading a newspaper, it is difficult to avoid the thought that our intelligence bar is not particularly high and difficult to surpass. We fight among ourselves in “lose-lose” situations, we do not promote long-term benefits in favor of short-term manipulations (Loeb 2020e). Our first encounter with alien technology would constitute a wake-up call for us to get our act together and collaborate internationally as a single species rather than waste our resources on short-sighted conflicts (Loeb 2021c). My hope is that finding relics from dead civilizations in space will teach us an important lesson to get our act together, treat our planet and other people better, so that we will avoid a similar fate (Loeb 2019c). Finding traces of civilizations that died from self-inflicted wounds, such as wars or climate change, will hopefully convince us to get our act together and avoid a similar fate. But it would be even more remarkable if flyby photography of an interstellar relic within the Solar System would reveal an advanced technology never witnessed before. No lesson is more valuable than the sense of awe and modesty that would accompany such a discovery (Loeb 2020e).

Searching for interstellar objects, artificial lights, industrial pollution, megastructures, and flashes

Territories that remain unexplored by classical SETI todayinclude industrial pollution of planetary atmospheres, artificial lights, solar cells or mega-structures in space (Loeb 2021e). Therefore, one approach to space archaeology is to use the Earth's atmosphere as a detector and search for artificial meteors (Loeb 2020e). Looking ahead, we should search for any interstellar objects in the sky (Loeb 2018a). Only a fraction of the interstellar objects might be technological debris of alien civilizations. But we should examine anything that enters the Solar System from interstellar space in order to infer the true nature of 'Oumuamua or other objects of its mysterious population (Loeb 2018a).

For example, we can search the surface of the Moon for extraterrestrial technological debris that crashed on it (Loeb 2020e). Both the Moon and the Earth serve as fishing nets to retrieve interstellar debris. In addition, Jupiter could serve as a gravitational fishing net which traps interstellar objects which pass near it. Most of the time we will likely recover natural rocks or icy bodies like asteroids or comets. But perhaps not always (Loeb 2020e).

Together with my postdoc, Manasvi Lingam, we calculated that currently there should be thousands of interstellar objects that were captured within the Solar System by their interaction with Jupiter and the Sun. The Jupiter-Sun system acts as a fishing net, which traps such objects as they pass near Jupiter and lose energy through their gravitational interaction with it. A few months after our paper, an asteroid occupying an orbit indicative of this origin, BZ509, was identified in a retrograde orbit around Jupiter (Dvorsky 2013).

Extending the search to the outskirts of the Solar System, one can look for artificial lights that originate from giant spacecrafts (Loeb 2020e). Venturing beyond the Solar System, one could search for artificial light or heat redistribution on the surface of a planet. The nearest star to the Sun is the dwarf star, Proxima Centauri, whose mass is only 12 percent that of the Sun. The habitable zone around this faint star is twenty times closer than the Earth-Sun separation. As it turns out, our neighboring star hosts an Earth-size rocky planet, Proxima b, at that distance. But since this planet is so close in, it is likely tidally locked like the Moon is to the Earth and so it faces the star with the same side at all times. Naturally, the permanent dayside would be hot and bright whereas the permanent nightside is cold and dark. But an advanced civilization might attempt to cover the dayside surface with photo-voltaic cells that would generate electricity to artificially illuminate and warm the night side. As the planet moves around the star, the varying level of light from its surface could inform us whether a global engineering project of this type took place. We could also search for the unusual reflectance and color expected from solar cells on the dayside. These studies can be done just by monitoring the planet's light and color as it moves around the star without any need to image its surface (Loeb 2020e).

In particular, the James Webb Space Telescope (JWST) will be able to detect artificial lights from Light Emitting Diode (LED) lamps on the nightside of the nearest habitable planet, Proxima b, if they make up 5% of the stellar illumination of its dayside. But even if artificial illumination is as faint as our civilization currently utilizes (0.01%) on the nightside of Earth, JWST could detect it as long as it was limited to a frequency band that is a thousand times narrower than the stellar light (Tabor & Loeb 2021).

But artificial activities may have other consequences such as industrial pollution of atmospheres. The contamination by a blanket of pollutants or aerosols may be intentional in order to warm up a planet that is otherwise too cold or vice versa. Our archaeological dig could include a search for artificial molecules, such as chlorofluorocarbons (CFCs). Some molecules and surface effects may survive long after the industrial civilization that produced them died (Loeb 2020e).

In addition, one could search for a swarm of satellites or megastructures that block a significant fraction of the light from distant stars, as envisioned by Freeman Dyson. However, such gigantic megastructures may be rare or non-existent as they face major engineering challenges (Loeb 2020e). Primordial inhomogeneities from the early universe led to the gravitational collapse of regions as large as tens of millions of light-years, assembling all their matter into clusters of galaxies. Each of these clusters contain the equivalent of 1,000 Milky Way galaxies. Therefore, an advanced civilization need not embark on giant construction projects of the type suggested by Dyson. It only needs to propel itself into the nearest galaxy cluster and take advantage of the available resources as fuel for its future prosperity (Loeb 2020f; Loeb 2020h).

At even greater distances stretching out to the edge of the Universe, we could search for flashes of light from beams sweeping across the sky. Such beams may be used for communication or ligh sail propulsion purposes. In particular, spacecraft launch systems which are based on the technology of light sails, would inevitably appear as flashes in the sky due to the inevitable leakage of light over the edge of their sail when the beam happens to be pointed in our direction for a brief moment in time. Whereas radio frequencies are ideal for transporting massive cargos at modest speeds between nearby planets such as Earth and Mars, infrared or optical lasers are optimal for launching lightweight probes to the speed of light, as envisioned by the Starshot project, whose scientific advisory committee I chair (Loeb 2020e).

Breakthrough Starshot

In July 2015, while I was Chair of the Harvard Astronomy Department, Yuri Milner and Pete Worden visited my office and offered me to lead the Starshot project, aiming to visit the nearest star within our lifetime (Loeb 2020h).

Space is the next frontier of our civilization. Starshot potentially represents the next giant leap forward after the Apollo mission: to fulfil humanity's dream of reaching the stars. My hope is that we could find a lot of traffic there and might even receive the friendly message, "Welcome to the interstellar club" (Loeb 2018e).

After six months of critical examination of various propulsion schemes with my students and postdocs and subsequent extensive discussions with Yuri, Pete, and, also, Pete Klupar, we converged on a plan to launch a lightweight spacecraft attached to a sail with a powerful laser beam. In 2016 we publicly announced the Breakthrough Starshot project in New York City. The Starshot Initiative (Breakthrough Initiative *n.d.*) is the first funded initiative for interstellar travel and is the first funded project to develop a light-sail technology that could propel a spacecraft up to a significant fraction of the speed of light (Loeb 2020f; Loeb 2020h). I currently chair its Advisory Board (Loeb 2018e).

If we wish the journey to take less than a human lifetime, the spacecraft needs to travel faster than a tenth of the speed of light. With currently feasible technology, that is only possible by leaving the fuel behind and using light to push a sail attached to our payload. This is the ambitious concept behind the Breakthrough Starshot initiative. Starshot aims to launch a miniature probe, or “starchip,” weighing at most a few grams, propelled by a laser to a fraction of the speed of light. For the first time in human history, thanks to recent advances in laser technology and the miniaturization of electronics the dream of reaching the stars can be fulfilled (Loeb 2018e).

With the Starshot technology we could send a camera to Pluto within a few days instead of the 9.5 years that it took New Horizons to reach it. We could easily chase 'Oumuamua, the first interstellar asteroid, which was discovered in the solar system last year, even though conventional rockets cannot catch up with it at all. Our team of distinguished scientists collaborated with the visionary funder of the project, Yuri Milner, in listing more than two dozen technological challenges that it needs to resolve. These can be found on the Starshot project website and include stable sail design and communication. We aim to dedicate the next decade to a feasibility demonstration of the required laser and sail technologies and have just started to engage experimental groups in related research. Without any doubt, the project is challenging, but if successful, it will move on to the construction of a prototype system and eventually to the construction of the full system (Loeb 2018e).

If Breakthrough Starshot is successful, our civilization could contemplate a future journey to the *Virgo* or *Coma* clusters (Loeb 2020f; Loeb 2020h). The nearest cluster to us is *Virgo*, whose center is about fifty million light years away. Another massive cluster, *Coma*, is six times farther. Other advanced civilizations might have already migrated towards clusters of galaxies in recent cosmic history, similarly to the movement of ancient civilizations towards rivers or lakes on Earth. Once settled in a cluster, a civilization could hop from one star to another and harvest their energy output just like a butterfly hovering over flowers in a hunt for their nectar. The added benefit of naturally-produced clusters is that they contain stars of all masses, much like a cosmic bag that collected everything from its environment (Loeb 2020f; Loeb 2020h). Interestingly, very advanced civilizations that are not fearful of being discovered could potentially be detectable out to the edge of the observable Universe through their most powerful ‘Starshot-like’ beacons (Lingam and Loeb 2017b; Loeb 2017a).

A project to land on trapped interstellar objects

My dream project is to organize a space mission that will land on the surface of trapped interstellar objects within the Solar System and check whether they have signs of life in their interiors. One could also wait for one-time passing objects like 'Oumuamua and chase them even though they are unbound to the Sun. This might require some drilling through their outer rocky ice surface. Or, instead of landing on the surface, one can study the composition of the cometary tail (if it exists) behind the object and verify by measuring its oxygen isotope ratio whether its origin is different than the rest of the material in the Solar System (Dvorsky 2018).

Also with spectroscopy, one can search for organic molecules or biomarkers. It is commonly thought that such a search should be conducted using a large telescope aiming to detect biosignatures—molecular products of primitive life in planetary atmospheres, such as molecular oxygen combined with methane—or techno-signatures—artifacts on a planetary surface, such as megastructures or photovoltaic cells—in distant habitable planets, or alternatively by launching a spacecraft that will visit such worlds. The latter approach would take tens of thousands of years using conventional chemical rockets even for the nearest habitable planet, Proxima b. Fortunately, there is a promising alternative that will save this lengthy travel time. Instead of our spacecraft traveling to another star, we can search for objects that were ejected from other planetary systems and spent the lengthy travel time by now on their journey towards us. Even if they were to originate from the nearest star system, Alpha Centauri, these rocks should have started their trip around the time when Homo sapiens came to the scene on Earth about three hundred thousand years ago (Dvorsky 2018). If biological or technological signatures on other objects would look the same or would appear to be unusually clustered in space, we might realize that it has a common ancestry (Loeb 2019d).

Maybe sometimes it is extraterrestrials!

One fact is clear. If we assign a zero probability for finding evidence for artificial objects, assome scientists did in the case of 'Oumuamua by stating “it’s never aliens”, then we will indeed never find any evidence for aliens (Loeb 2020e). The possible existence of extraterrestrials will not go away if we ignore them, just like the Earth continued to move around the Sun after the philosophers refused to look through Galileo’s telescope. Are there extraterrestrials smarter than us in the Milky Way? The only way to find out is by surveying the sky (Loeb 2021g).

As far as space archaeology is concerned, the key challenge to improving our awareness of other civilizations is whether we are intelligent enough to adequately interpret their products. Discovering a piece of advanced technological equipment that was developed by an extraterrestrial intelligence may resemble an imaginary encounter of ancient cave people with a modern cell phone. At first, they would interpret the phone as a shiny rock without realizing that it is a communication device (Loeb 2020e). Gaining scientific knowledge about our cosmic neighborhood brings an improved sense of realism (Loeb 2021h). The realization that we are not alone will have dramatic implications for our goals here on Earth and our aspirations for space (Loeb 2021c; Loeb 2021g).

If we recover artificial artifacts from our archaeological dig into space, the ramifications will be profound. Furthermore, once we find other intelligent civilizations, the key question is: “are they smarter than we are?” and if so “what can we learn from them?” (Loeb 2019c). We can learn a lot from our findings and perhaps short cut our own evolution by thousands, millions or maybe even billions of years (Loeb 2020e). Most stars formed billions of years before the Sun and may have enabled technological civilizations that are far ahead of us. If we find any of them, we could expand our knowledge by borrowing information from neighbors with more extensive cosmic experience (Loeb 2021h). In other words, we could shortcut our own gradual progress by learning from civilizations that have benefitted from a head-start of billions of years (Loeb 2017a). This could have important ramifications for our ability to answer foundational questions in physics, such as how to unify general relativity and quantum mechanics.

Given that most stars formed billions of years before the Sun, and, therefore, that the technological civilizations that predated us had the opportunity to develop more advanced equipment than our century-old technologies, we can learn from them even if most of the equipment they deployed in space is not operational after billions of years. Finding their relics in our backyard saves us the long trip to their point of origin. By putting our hands on such equipment and attempting to reproduce it on Earth, we can shortcut our own technological development. Our technological future may lie in front ofour eyes if we can only master the will to harness the evidence (Loeb 2021g).

If we do find extraterrestrial civilizations that are smarter than human beings, what can we learn, and how can we cooperate with them? One potential scenario even has us cozying up to extraterrestrials for warmth. The accelerated expansion of the universe pushes resources away from us at an ever-increasing speed. Once the universe ages by a factor of ten, all the stars outside our Local Group of galaxies will be receding faster than light. They will no longer be accessible to us. Is there something we can do to avoid this fate?

Following the lesson from Aesop’s fable “The Ants and the Grasshopper,” it would be prudent to collect as much fuel as possible before it is too late, for the purpose of keeping warm in the frigid cosmic winter that awaits us (Aesop *n.d.*). In addition, for the same reason that animals feel empowered by congregating in large herds, it would also be advantageous to reside in the company of as many alien civilizations as possible with whom we could share technology (Loeb 2020f).

This means of avoiding a cosmic catastrophe could come about by establishing an interstellar treaty, similar to the Nuclear Test Ban Treaty, signed first in 1963 by the governments of the Soviet Union, the United Kingdom and the United States. The objective of the “Planck Collider Treaty” would be to protect our cosmic environment from artificially produced domain walls. With no such treaty, we could only wish that all civilizations would behave responsibly when they acquired the technological maturity to build a Plank-energy collider. Collisions of particles at Planck energy (10^{19} times larger than the energy associated with the rest mass of the proton) has the potential of destabilizing the cosmic vacuum (dark energy) and creating a domain wall that burns the cosmological constant of the vacuum at the speed of light and endangers everyone. We would have to hope that our neighbors would exhibit cosmic responsibility.

In the long term, the need to sign a treaty is only pressing within our galaxy, the Milky Way, and its nearest neighbor, Andromeda; it does not extend beyond the Local Group of galaxies. Even without a treaty signed or honored on extended intergalactic scales, the accelerated expansion of the universe will ultimately save us from the risk of a Planck collider catastrophe. All galaxies beyond “Milkomeda” (the result of an eventual merger between Milky Way and Andromeda, which I named with my postdoc T. J. Cox in a 2007 paper) will eventually recede away from us faster than light. As I showed in a 2002 paper, once all other galaxies leave our cosmic event horizon, nothing happening within them could affect us because all causal signals propagate at most at the speed of light. Once the universe ages by another factor of ten, Milkomeda will only be surrounded by dark space (Loeb 2021f).

Beyond disciplinary boundaries

While a fresh perspective on SETI is exciting, what else can we do in parallel to improve the academic experience here on Earth? Firstly, we can move beyond dogma; and secondly, we can encourage open-mindedness and generalism so students and faculty alike can explore beyond the confines of narrow disciplines.

The philosopher Friedrich Nietzsche in his book *The Dawn of Day*, published in 1881, “The surest way to corrupt a youth is to instruct him to hold in higher esteem those who think alike than those who think differently.” And indeed, the psychological pressure on physicists to conform with fashionable trends promotes a herd mentality in which young scientists today feel obligated to work on far-fetched ideas promoted by senior colleagues just in order to secure jobs, thereby perpetuating the problem. These young scientists learn from the examples set by their elders, who often react to original thought with violent pushback and bullying. I know because I have been the subject of such assaults (Loeb 2021e).

I came to appreciate how limiting prevailing world views can be (Loeb 2016). Is today’s science similarly trapped by cultural and societal forces? Most research funding is allocated assuming that the highest-quality data will inevitably deliver useful scientific interpretation and theoretical concepts, which can be tested and refined by future data. The astronomy division of the US National Science Foundation, for example, devotes most of its funds to major facilities and large surveys, which are performed by big teams to collect better data within mainstream paradigms. Fields from particle physics to genomics do the same (Loeb 2016).

Data collection is one antidote to scientific dogma. A common flaw astronomers have 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 (Loeb 2014a).

The consequences of a closed scientific culture are wasted resources and misguided 'progress.' To truly move forward, free thought must be encouraged outside the mainstream. Multiple interpretations of existing data and alternative motivations for collecting new data must be supported (Loeb 2016). Science is a never-ending work in progress. We show integrity by entertaining multiple possible interpretations of evidence to the public. Rather than pretending to know the outcome in advance, we should admit what we do not know and study all possible interpretations, so that the public will believe our robust conclusions when new evidence brings clarity (Loeb 2021e).

These examples and many more like them (starting with the ancient view that the Earth is at the center 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 (Loeb 2014a). As Galileo reasoned after looking through his telescope, "in the sciences, the authority of a thousand is not worth as much as the humble reasoning of a single individual." I would add the footnote that sometimes Mother Nature is kinder to innovative ideas than people are (Loeb 2019b).

It is beneficial to mount challenges to scientific dogma at the blueprint level by careful analysis of conceptual anomalies and puzzling data. There is no better framework for critically challenging a prevailing dogma than at the architectural ‘blueprint level.’ Sometimes, a crack opens in a very particular corner of a dogma due to a localized discrepancy with data. But conceptual anomalies are much more powerful as they apply to a wide range of phenomena and are not restricted to a corner of parameter space. Quantum mechanics emerged as a surprising departure from the intuitive notions of classical physics. Albert Einstein’s thought experiments are celebrated steppingstones that identified earlier conceptual anomalies and paved the way to the theories of Special and General Relativity in place of the fixed spacetime blueprint of Isaac Newton. And before Galileo Galilei came up with his conceptual breakthrough, it was standard to assume that heavy objects fall faster than light objects under the influence of gravity (Loeb 2013).

We also should think outside the simulation box. Any ambitious construction project requires architects and engineers. As research shifts towards large groups that focus on the engineering aspects of linking data to existing models, architectural skills are becoming rare among young theorists. Conceptual thinkers are becoming an extinct species in the landscape of present-day astrophysics. I find this trend worrisome for the health of our scientific endeavors. In my view, it is the duty of senior scientists to encourage young researchers to think critically about prevailing paradigms and to come up with simplifying conceptual remedies that take us away from our psychological comfort zone but bring us closer to the truth (Loeb 2013).

Conceptual work is often undervalued in the minds of those who work on the details. A common misconception is that the truth will inevitably be revealed by working out the particulars. But this highlights the biggest blunder in the history of science: that the accumulation of details can be accommodated in any prevailing paradigm by tweaking and complicating the model. A classic example is Ptolemaic cosmology—a theory of epicycles for the motion of the Sun and planets around the Earth—which survived empirical scrutiny for longer than it deserved (Loeb 2013).

Today, the standard model of cosmology is merely a precise account of our ignorance: we do not understand the nature of inflation, dark matter or dark energy. The model has difficulties accounting for the luminous gas and stars that we can see in galaxies, while leaving invisible what we can easily calculate (dark matter and dark energy). This state of affairs is clearly unsatisfactory (Loeb 2016).

A modern analogue to the classic example of Ptolemaic cosmology is the conviction shared by mainstream cosmologists that the matter density in the Universe equals the critical value, $\Omega m = 1$. when a hemispherical asymmetry in the power spectrum of temperature fluctuations in the cosmic microwave background was reported a decade ago, it was quickly dismissed by mainstream cosmologists; this anomaly is now confirmed by the latest data from the Planck satellite, but is still viewed as an unlikely (< 1% probability) realization of the sky in the standard, statistically isotropic cosmology. Given that progress in physics was historically often motivated by experimental data, it is surprising to see that a speculative concept with no empirical basis—for instance, the ‘multiverse’—gains traction among some mainstream cosmologists, whereas a data-driven hypothesis, such as dark matter with strong self-interaction (to remedy galactic scale discrepancies of collisionless dark matter), is much less popular (Loeb 2013).

Although there is much work to be done in the analysis and interpretation of experimental data, the unfortunate by-product of the current state of affairs is that popular, mainstream models with which data are interpreted are rarely challenged. Most cosmologists, for example, lay one brick of phenomenology at a time in support of the standard (inflation + cosmological constant Λ + cold dark matter, or LCDM) cosmological model, resembling engineers that follow the blueprint of a global construction project, without pausing to question whether the architecture of the project makes sense when discrepancies between expectations and data are revealed (Loeb 2013).

Scientific conservatism is possible today because we now have the LCDM model, which was not the case several decades ago (Loeb 2013). In such a culture, the current model can never be ruled out, even though everyone knows that its major constituents (dark matter, dark energy and inflation) are not understood at a fundamental level. Instead, observers should present results in a theory-neutral way. Observations should not converge on one model but aim to find anomalies that carry clues about the nature of dark matter, dark energy or initial conditions of the Universe. Further observations should be motivated by testing unconventional interpretations of those anomalies (such as exotic forms of dark matter or modified theories of gravity). Vast data sets may contain evidence for unusual behavior that was unanticipated when the projects were conceived (Loeb 2016).

There are many examples of observational ‘discrepancies’ that do not fit the model. The LCDM model assumes that dark matter is made of exotic particles that have very weak interactions with light, ordinary matter or among themselves. The nature of these particles remains elusive. Numerical simulations of collisionless cold dark matter predict that collapsed objects (haloes) in an expanding universe generically develop a density cusp at their center. However, observations of low-mass galaxies (subhaloes) that are dominated by dark matter reveal a central density core. The discrepancy can be explained if the dark matter particles have a large cross-section for scattering off each other (but still interact weakly with light and are hence ‘dark’) (Loeb 2013).

Too few theoretical astrophysicists are engaged in tasks that go beyond the refinement of details within a commonly accepted paradigm. It is far more straightforward today to work on these finer points than to review whether the paradigm itself is valid (Loeb 2013). Such projects have a narrow aim: pinning down the parameters of one theoretical model. The model comprises an expanding Universe composed of dark matter, dark energy and normal matter (from which stars, planets and people are made), with initial conditions dictated by an early phase of rapid expansion called cosmic inflation. The data are reduced to a few numbers. Surprises in the rest are tossed away (Loeb 2016).

It is disappointing to see conformism among young cosmologists who were born into this standard model and are supposed to be least biased by prejudice (Loeb 2013). However, surprises are exciting because they hold the potential for increasing our knowledge base. Scientific research is fundamentally a learning experience, and therefore scientists are perpetual students of nature. Experimental clues and their theoretical interpretation constitute a classroom setting for our two-way dialogue with reality. When evidence does not conform with our pre-conceived notions, we learn something new (Loeb 2021i). There is no bigger privilege to being alive than this learning experience (Loeb 2019b). For physics, this might include the existence of an underlying self-contained theory from first principles, the potential for experimental tests of this theory and a track record of related research programs. Clearly, factors such as intellectual excitement cannot be quantified, the data about the growth of a field should echo this ‘excitement’ factor (Loeb 2012b, 279).

Research can be a self-fulfilling prophecy. By forecasting what we expect to find and using new data to justify prejudice, we will avoid creating new realities. Innovation demands risk-taking, sometimes contrary to our best academic instincts of enhancing our image within our community of scholars. Learning means giving a higher priority to the world around you than to yourself. Without the humble attitude of a child, innovation slows down and the efficiency of the academic pursuit of the truth grinds to a halt. We all become static museum items rather than dynamic innovators. By chasing self-interest, we often lose track of the real goal of academic pursuit: learning about the world. This conflict is apparent when the popular view advocated by authority is not aligned with the truth (Loeb 2019b).

In cosmology now, we have the emergence of a ‘standard model’—in which the composition, geometry, large-scale structure and expansion rate of the Universe are well constrained. When I was a postdoc two decades ago, the lack of a prevailing paradigm made it more socially acceptable for young cosmologists to invest in [risky] ideas. Investing in research time: Astrophysics has both safe and risky topics. The best approach for the fledgling researcher is to diversify his or her academic portfolio, always making sure to devote some of it to innovative projects with risky but potentially highly profitable returns (Loeb 2010).

The most efficient way to simplify the interpretation of data is to work at the metalevel of architecture, similar to the heliocentric interpretation developed by Copernicus in the days when the Ptolemaic theory ruled. An engineering project that aims to study the strength of rods and bricks might never lead to a particularly elegant blueprint for the building in which these ingredients are finally embedded (Loeb 2013).

Some argue that architects were only needed in the early days of a field like cosmology, when the fundamental building blocks of the standard model—such as cosmic inflation, dark matter and dark energy—were being discovered. As fields of study mature to a state where quantitative predictions can be refined by detailed numerical simulations, the architectural skills are no longer required for selecting a winning world model based on comparison with precise data.

Ironically, the example of cosmology demonstrates just the opposite. On the one hand, we measured various constituents of our Universe to two significant digits and simulated them with accurate numerical codes. But at the same time, we do not have a fundamental understanding of the nature of the dark matter, dark energy or inflation. In searching for this missing knowledge, we need architects who could suggest to us what these constituents might be in light of existing data and which observational clues should be searched for. Without these hints we will never be sure that inflation really took place, or that dark matter and dark energy are real and not ghosts of our imagination due to a modified form of gravity (Loeb 2013).

The problem with researchers focusing on the engineering aspects of a prevailing paradigm rather than on questioning its foundation is that their efforts to advance scientific knowledge are restricted to a conservative framework. For example, cosmological data are often analyzed in the programmatic mindset of a community-wide effort to reduce the error budget on measurements of the standard cosmological parameters. Previous generations of cosmologists, who were debating the underlying physical principles that control the Universe while entertaining dramatic excursions from conservative guidelines. It is true that numerical codes allow us to better quantify the consequences of a prevailing paradigm, but systematic offsets between these results and observational data cannot be cured by improving the numerical precision of these codes. The solutions can only be found outside the simulation box through conceptual thinking (Loeb 2013).

The orthodoxy exhibited by young cosmologists today raises eyebrows among some of the innovative ‘architects’ who participated in the design of the standard model of cosmology. Too soon after the enigmatic ingredients of the standard cosmology were confirmed observationally, they acquired the undeserving status of an absolute truth in the eyes of junior cosmologists. In the scientific quest for the truth, we still need architects (theorists), not just engineers (who implement/test theory) (Loeb 2013).

Encouraging open-mindedness

It’s unfortunate that some of the most exciting frontiers in modern astrophysics were initially ridiculed as a waste of time (Loeb 2019a). This time lag can be perilous. When an innovative idea is ahead of its time, the experts often label it as “unlikely” and sometimes even “crazy.” Then, when the accumulating evidence starts to make it relevant, the experts say that the idea is trivial. And when evidence demonstrates beyond doubt that it is true, the same experts argue that they thought of it first (Loeb 2020j). Historical memory would encourage innovators to persevere despite the headwinds they encounter (Loeb 2019a).

Discoveries in astronomy—or, in fact, any branch of science—can only happen when people are open-minded and willing to take risks (Loeb 2014a). In hindsight, the paths that lead to scientific breakthroughs seem inevitable; they are carved indelibly into the landscape of ideas. But the ability to spot them first, follow their twists and turns, and keep going when fellow travelers shout that you are going nowhere is the trademark of a truly exceptional scientist. Most importantly, this requires character and not just technical skills. It may be necessary for pioneers to face the headwind of rejection for a while. An innovator has to persevere through an initial denial phase during which the mainstream rejects the idea so publicly that the proposer can later wear the rejection as a badge of honor. Unfortunately, this lesson is often forgotten in today’s world of theoretical physics, where mathematical gymnastics gets more attention than success in navigating to the right path based on empirical evidence. This is partly because the long stagnation in detecting new physics from particle accelerators has enhanced the popularity of complex mathematical structures such as string theory. But it is also because mathematical skills are easier to quantify on a short timescale, just like skill in athletics; whereas a good sense of direction takes a while to recognize (Loeb 2020j).

History repeats itself when it is forgotten by new generations of scientists. These days, the search for extraterrestrial technological civilizations, in which I am engaged, encounters even more hostility than that experienced in many other areas. This has little to do with professional conservatism but more with social trends, since SETI is less speculative than some dark matter searches that are federally funded. And contemporary bullying is amplified even more by the megaphone of social media. In order to relieve future scientists from the recurring pain associated with the birth of new frontiers, it would be prudent to educate fledgling researchers to act as independent individuals rather than to close ranks with traditional group thinking.

There is a distinction between legitimate physics-based criticism on the one hand and unprofessional skepticism or intimidation as a tool for preserving traditions on the other. Our community should avoid the ostrich gambit; if we bury our head in the sand, we will not see new horizons. Hostility to new frontiers delays progress and reduces the efficiency of the discovery engine that propels us forward (Loeb 2019a).

My advice to young scientists is to define your path not by looking at the surrounding geography and restricting your expertise to intellectual boundaries, but by following your internal compass. Unwarranted resistance by experts might signal that you are on track for an important breakthrough. In the end, wearing early rejection as a badge of honor might give you more pride than any prize awarded to you afterwards by the same experts (Loeb 2020i).

Puzzling data also has great potential value, and even failures to explain puzzling data are essential to the scientific process, with the challenges that data pose encouraging creative individuals to develop new ways of thinking about physical reality. Over extended periods of time—decades or longer—a data-driven culture without programmatic reins offers such extensive benefits that profit-oriented businesses often choose to support it (Loeb 2014c).

Scientific discovery is unpredictable. History reveals that scientists rarely anticipate the nature or the source of new breakthroughs before they happen. One might think, then, that it is impossible to cultivate an environment that promotes discovery. But I argue otherwise: By encouraging open research without a programmatic agenda, we can establish a fertile ground for unexpected breakthroughs (Loeb 2012a).

A scientist must go where the evidence is, but too often, our scientists do not (Loeb 2021e). False notions and unsubstantiated claims on a subject should not prevent scientists from exploring it using the scientific method and tools, especially when the implications are of great significance to the public. This motivates acquiring knowledge about alien neighbors in our Galactic environment. Such knowledge will shape our aspirations on Earth and in space (Loeb 2021h).

This observation, to go where the evidence is, applies well to the topic of Unidentified Aerial Phenomena (UAP). Here, rather than formulating opinions and becoming mired in controversy, we should conduct a scientifically controlled series of experiments using appropriately tailored instrumentation in order to collect the data we need to determine whether UAP are human-made or not.

Science relies on reproducibility of results. In order to believe evidence, it must be possible to reproduce it as an outcome of similar circumstances (Loeb 2021b; Loeb 2021m). The situation, therefore, gets complicated with eyewitness testimonies of one-time events. Recently, the Pentagon was asked by lawmakers to disclose all it knows about Unidentified Aerial Phenomena (UAP) by June 2021. But this focus on past eyewitness reports is misguided. It would be prudent to progress forward with our finest instruments, rather than examine past reports. Instead of declassifying documents that reflect decades-old technologies used by witnesses with no scientific expertise, it would be far better to deploy state-of-the-art recording devices, such as camera or audio sensors, at the sites where the reports came from, and search for unusual signals. A scientific expedition focused on reproducing old reports would be more valuable to unraveling the mysteries behind them (Loeb 2021b; Loeb 2021m). Personally, I will be glad to lead scientific inquiry into the nature of these reports and advise Congress accordingly. This could take the form of a federally-designated committee or a privately funded expedition. Its most important purpose would be to inject scientific rigor and credibility into the discussion (Loeb 2021m).

To overcome dogma in science on any topic, including the study of UAP, we should develop a good internal sense of direction and should follow it, even when the crowd is going the opposite way. There is a famous quote by Nachman of Breslov: “The whole world is nothing but a very narrow bridge, and the key is not to be fearful at all” (Loeb 2019b; Loeb 2019c) The primary task of scientific research is to identify the right path to take. The ability to imagine new directions is surprisingly rare for many, but others have it in abundance. For great physicists like Albert Einstein, John Wheeler, Richard Feynman or Yakov Zel’dovich, it was an outstanding sense of direction that led to their success (Loeb 2020j). One wrong strategic turn canbring irreversible outcomes, and therefore strategic vision is more important than tactical maneuvers. One should define the goals and then navigate towards them, rather than being guided solely by short-term political considerations (Loeb 2017b).

This state of affairs is particularly apparent in my field: the world of physics. For instance, in theoretical physics, a phalanx of untestable notions—about the multiverse, hypothesized extra dimensions, the idea that we live in a simulation, and the argument that there is no need for experimental evidence to justify the string theory strategy in unifying quantum mechanics and gravity—occupy centerstage. At the same time, there is a taboo on an open discussion of certain common-sense questions, such as whether there are other intelligent civilizations in outer space and whether our civilization is the smartest kid on the galactic block (Loeb 2021e).

The only way to work out whether we are on the wrong path is to encourage competing interpretations of the known data (Loeb 2016). But even though this holds true in principle, a sense of direction in science is often dictated by consensus, with young researchers competing, like athletes on a track, on their technical skills. This gives the false impression that mathematical virtuosity, for example, is the measure of success in theoretical physics. Based on my experience, coming up with attractive ideas for new research directions is a rare commodity that is as valuable as technical virtuosity for leading scientific research (Loeb 2020j).

To move forward we must think outside of the box and avoid prejudice about what we expect to find based on past experience (Loeb 2020e). However, the tendency to establish large projects and firm up mainstream ideas is a signature of a mature scientific discipline. In such a culture, the low-hanging fruit has already been picked by small, versatile teams that are long gone. Critics argue that when funds are limited, the focus of research should be on coordinated approaches that are likely to produce results in a predictable way. This advocacy fails to appreciate that our mainstream paradigm might be heading in the wrong direction (Loeb 2016).

Most of the time frontier science involves uncertainty due to lack of data. However, innovation and risk-taking are essential for making discoveries. Prejudice therefore must be banned from the scientific discourse, and mistakes should be tolerated in order for innovation to prevail (Loeb 2019c). Of course, it is difficult to know which exploratory path will bear fruit, and there is no shortage of novel scientific ideas that were proven wrong. Failure should thus be accepted as a natural ingredient in a culture of innovation. The fact is that high-risk research, just like high-risk capital investments in the business world, has the potential to be more profitable than safer approaches. If even one non-mainstream idea bears fruit, it could transform our view of reality and justify all of those heterodox hypotheses that do not (Loeb 2014c).

History is full of unexpected discoveries that changed the world. Despite the obvious risks, science funders should therefore allow researchers the flexibility to pursue new, exciting insights rather than just play it safe and stick to rigid research goals. Assembling new data is essential to scientific progress. Data play the important role of guiding scientists toward new discoveries and solutions, as well as to new puzzles that need to be solved, thereby keeping the scientific process honest and dynamic. Extended periods without new data facilitate—indeed, foster—the unrestrained growth of speculative-theory bubbles (Loeb 2014c).

One would have naively expected scientific activity to be open-minded to critical questioning of its architectural design, but the reality is that conservatism prevails within the modern academic setting (Loeb 2013). Nevertheless, I find it alarming that today's young astrophysicists often invest their time conservatively in mainstream ideas that have already been extensively explored. There is a better path for doing science (Loeb 2010). The unfortunate reality of young astrophysicists having to spend their most productive years in lengthy postdoctoral positions without job security promotes conformism, as postdocs aim to improve their chance of getting a faculty job by supporting the prevailing notions of senior colleagues who serve on selection committees. Logically, however, one might argue that an opposite strategy of choosing innovative projects should improve the job prospects of a candidate, as it would separate them from the competing crowd of indistinguishable applicants that selection committees are struggling through (Loeb 2013).

More young scientists should dedicate a portion of their attention to high-risk research despite the potential downside (Loeb 2010). What would the future look like if risks were not a concern? (Loeb and Tripathi 2019). Young researchers in particular should pursue innovative research. The window of opportunity in a career is short: although tenure should allow scientists to take more risks, most senior researchers get distracted by administration and fund-raising. Tenured professors often maintain a conservative profile that promotes old ideas. A change in attitude, supported by policy changes at our funding agencies, is crucial for the future health of astrophysics and other disciplines, and taking risks, in moderation, benefits young careers as well (Loeb 2010).

The tendency to play it safe is driven by peer pressure and job-market prospects and is sometimes encouraged by senior researchers. Past decades have seen the same phenomenon, but it is more prevalent today because scientists are increasingly pursuing projects in large groups with rigid research agendas and tight schedules that promote predictable goals. This trend towards conservative science is particularly unsettling given that so much of the Universe remains a mystery. And astrophysics is not a special case—scientists in many fields feel pressure to subscribe to the prevailing dogma (Loeb 2010).

The problem is exacerbated by the existence of large groups with a rigid, prescribed agenda and a limited space for innovation when unexpected results emerge. In large groups, such as the Planck or SDSS-III collaborations, young cosmologists often decide not to challenge the established paradigm because other group members, and particularly senior scientists who are considered experts on the issue, accept this framework. If hundreds of names appear on the author list of a paper, the vast majority of them have limited space for maneuvers, resembling a dense swarm of fish in a small aquarium. The fact that letters of recommendation are written by a few group leaders adds pressure to conform to mainstream ideas. True, some scientific goals require a large investment of funds and research time, but under these circumstances - efforts should be doubled to maintain innovative challenges to conventional thinking (Loeb 2013).

This is not to say that agenda-driven projects do not also lead to important breakthroughs. The recent discovery of the Higgs boson was the culmination of a programmatic experimental effort to confirm a theoretical idea (Loeb 2014c).

My advice should not be interpreted as encouragement to study every idea that comes along. I suggest that young scientists avoid ideas that are speculations on top of a speculation, such as postulating a modified theory of gravity, then adding the notion of an undetected form of matter (dark matter) to make that theory fit all existing data. And research priorities may need to be reallocated depending on what is discovered. Those who uncover an unexpected, intriguing result at the end of a project should consider making it the focus of their main research paper, despite the time invested elsewhere (Loeb 2010).

Although adaptation to changing circumstances is easier with a diverse knowledge base, “Renaissance people” or polymaths are rare, because specialization is rewarded more generously in the short term. By crediting focused accomplishments, the existing reward system of grants, awards and promotions creates silos of knowledge with suppressed cross fertilization. This unfortunate backdrop only highlights the essential role played by generalists. Those who cross boundaries of disciplines act as butterflies that pollinate flowers unintentionally by carrying pollen stuck to their bellies. Occasional random winds can accomplish the same outcome but with a reduced efficiency and vigor (Loeb 2020k).

Professors should approach their job as mentors of future leaders in science, technology, the arts, and humanities, rather than attempting to mold students in their own intellectual image (Loeb 2018c). Martin Schwarzschild (the son of Karl who derived the black hole solution just months after Albert Einstein formulated general relativity) was an emeritus professor at Princeton when I was pondering my career strategy. He gave me the example of binary star systems, which were very popular in his time and then lost their appeal. His perspective convinced me to avoid transient fads and instead explore a diverse collection of topics tied together by the unifying theme of common physical principles. Unlike phenomenological trends that could be gone with the wind, fundamental principles are here to stay. They offer a stable launchpad for a career engaged with diverse interests (Loeb 2020k).

Was broadening my interests a worse career choice than specializing on a trendy topic? Over the past three decades since my postdoc years, the landscape of astrophysics changed dramatically. Topics that were regarded as speculative, such as theoretical cosmology, exoplanets, or gravitational wave astrophysics, became the mainstream and even received Nobel Prizes. In retrospect, my breadth was not a bug but a feature that allowed me to adapt to changing circumstances. There is no doubt that Darwinian survival in an evolving research environment selects for breadth of interests. Martin’s perspective was a blessing (Loeb 2020k).

A foundation of general education enables wide maneuvers and unexpected discoveries. It provides the tools needed to venture into unexplored territories or to correct misguided specialists based on general principles (Loeb 2020b). A broader view offers a better appreciation of the full landscape of our humbling learning experience. It is easy to become arrogant based on a narrow focus simply because of its limited field of view. However, the vast universe is so rich in stunning details that a narrow view misses its full splendor. There is much more for us to treasure by looking up into the full sky than by looking down at each other (Loeb 2020k).

One of the main drawbacks of specialization is that by drilling down narrowly one often encounters the bedrock of a subject, where no further progress can be made (Loeb 2021i). Subsequently, there are only subtleties to explore, but no fresh substantive ground. At this phase of maturity, the niche is ripe for a summary in a textbook but is no longer intellectually stimulating. There is little room left for innovation. Under these circumstances, a broad range of interests enables a sideways shift in research focus to territories where the bedrock was not exposed as of yet and where creative thinking still has an impact (Loeb 2020k). Under these circumstances, the potential for a breakthrough improves with a broader perspective which identifies the boundary of the bedrock and enables "out of the box" opportunities for drilling deeper around it. This is especially helpful after the discovery of unexpected anomalies that cannot be explained within the prevailing paradigm (Loeb 2021i).

Progress in science is sometimes triggered by surprises. Data collection resembles gathering of new pieces in a jigsaw puzzle and placing them together. Sometimes one of the pieces does not quite fit. It is natural for scientists to instinctively argue that such a piece does not belong; perhaps it is an artifact driven by uncertainties in the data or a misinterpretation of the experiment. This might indeed be the case in most instances. But every now and then, an anomaly of this type signals a real discrepancy from expectations, either a violation of a highly respected but incomplete law of nature—namely, an exception to the rule or an unexpected surprise—signaling the possibility of "new physics." Most anomalies are found to be associated with faulty interpretations or systematic errors in the experiments. Recent examples for such outcomes involve the experimental claims for faster-than-light neutrinos and unusually strong gravitational waves from cosmic inflation. However, some anomalies appear resilient to scrutiny and flag new discoveries.

Daring scientists who pursue an anomalous perspective that deviates from the mainstream dogma serve as agents of progress (Loeb 2018f).

Multidisciplinary initiatives

A healthy dialogue between different points of view should be fostered through multidisciplinary conferences that discuss conceptual issues, not just experimental results and phenomenology. A diversity of views fosters healthy progress and prevents stagnation. Over the past five years, I had the privilege of serving as the founding director of an interdisciplinary center, the Black Hole Initiative (BHI at Harvard University in Cambridge, Massachusetts, which brings together astronomers, physicists, mathematicians and philosophers. Our experience is that a mix of scholars with different vocabularies and com-fort zones can cultivate innovation and research outside the box (Loeb 2016).

In 2017, I obtained approval from Harvard's graduate school to start an interdisciplinary graduate program on black holes, where students can take courses in both the humanities and sciences as part of their Ph.D. education towards a thesis focused on the exciting research frontier of black holes (Loeb 2017b).

I was motivated to bring together astronomers, physicists, mathematicians and philosophers under the interdisciplinary umbrella of the BHI. That way, "non-experts" might help "experts" realize what they had been missing on unsolved puzzles about black holes. Despite the notion that is often advanced by textbooks, our knowledge should be regarded as a small island in a vast ocean of ignorance (Loeb 2019b). A fresh perspective could identify distant unexplored lands (Loeb 2020i). The most efficient way to add landmass to this island is by not being afraid of the consequences of originality, by being dedicated to the thrill of finding the truth (Loeb 2019b).

The dangers of intellectual territorialism are significant. A set of narrow experts, each focused on a single intellectual territory, provides a fragmented view similar to early maps of the world, which depicted a set of regions with unrealistic proportions and boundaries. Narrow expertise has its value, but it's also vital to let scientists step out of their "lanes." So-called experts exhibited the unfortunate tendency to discourage younger scientists with fresh ideas from entering into their field. The behavior appeared similar to the way animals protect their territory: they wish to remain dominant, minimize competition for available resources, and never expose their weaknesses—in the case of the experts, the important insights they might be missing. The perspective of an outsider poses a threat to conventional thinking. The consequences are particularly acute when the outsider raises foundational questions to which there is no good answer. The only way to obtain a proportional view of the complete intellectual world is to allow scientists to cross boundaries between separate intellectual continents and venture into uncharted oceans (Loeb 2020i).

Much of the tension in our professional life originates from boundaries, protected by gatekeepers that limit access across them. In my personal case, I was fortunate to cross the boundary between the humanities and sciences thanks to the kindness of a few gatekeepers. The pool of opportunities is infinite, and the talents of an individual can be realized equally well in completely different disciplines. Creative freedom is all about adopting the infinity pool's point of view, blurring the significance of interdisciplinary boundaries and continuing to create professionally despite the "localized" opinions of some gatekeepers (Loeb 2015b).

Scientists are comfortable in their own communities but other groups working on similar phenomena at different length scales could provide unexpected insights. Collaborations are more likely to uncover common underlying principles (Loeb and Imara 2017). Exciting science often blossoms these days at the interface between traditional disciplines; as a result, many students are being advised by faculty outside their home departments. We tried to come up with a funding scheme that takes interdisciplinary research into account and also rewards scientists who raise outside funds to support their students (Loeb 2014b).

Another way to ensure that academia continues to innovate in useful and relevant ways is to blur the traditional boundaries among disciplines—the frontiers where invention so often happens. To that end, universities should update their organizational structure, moving away from clearly delineated departments in order to create a kind of continuum across the arts, humanities, and sciences. Students should be encouraged to take courses in multiple disciplines, so that they can weave those lessons and experiences into new patterns of knowledge (Loeb 2018c).

Is there any evidence that spreading your focus across multiple scientific disciplines necessarily leads to a superficial impact? To the contrary, history demonstrates that polymaths like Leonardo da Vinci, René Descartes, Gottfried Leibniz, Isaac Newton, Charles Darwin, Benjamin Franklin, Marie Curie and Nikola Tesla were all responsible for foundational breakthroughs in science (Loeb 2020i).

Finding the answer to the question ‘are we alone?’ will change our perspective on our place in the Universe and will open up new interdisciplinary fields of research, such as astro-linguistics (how to communicate with aliens), astro-politics (how to negotiate with them for information), astro-sociology (how to interpret their collective behaviors), astro-economics (how to trade space-based resources), and so on (Loeb 2017a).

Academia and society

Academia should reflect society, which means it should be diverse. At the same time, academia should communicate effectively with society, which means that academics should develop meaningful ways to discuss their findings with the public at large. As mentioned in the opening paragraph, I have the privilege of sharing some of my thoughts on topics that interest me in a series of essays I have written for *Scientific American*. As a scientist, it is important for me to engage in a dialogue with the general public in this way.

On the topic of diversity, we can start by recognizing that in science, as in any complicated and creative endeavor, uniform opinions and approaches will always prove sterile. The coexistence of disparate ideas cultivates competition and progress (Loeb 2014c). Uniformity of opinions is sterile; the co-existence of multiple ideas cultivates competition and progress (Loeb 2014a).

Improving the diversity of our faculty and students is of utmost importance. This includes diversifying in terms of socio-economic starting conditions, gender, ethnicity, race and **ideas**. The promotion of diversity of ideas implies accepting the legitimacy of opposing opinions and criticisms. This does not mean that all opinions are equal but rather that alternative opinions should be discussed and filtered based on merit and clear reasoning. Debate is healthy as we iterate towards a better future (Loeb 2017b).

Truth and consensus are not always the same. Diversity of opinion—which implies diversity of gender, ethnicity, and background—is vital to support creativity, discovery, and progress. That is why it is so important for prizes and professional associations to be used not to reinforce mainstream perspectives, but rather to encourage independent thought and reward innovation. This does not mean that all opinions should be considered equal, but rather that alternative views should be debated and vetted on merit alone (Loeb 2018c).

How do we select a cohort of promising scientists before they have made their discoveries? The above faults are sometimes driven by the misconception that scientific success is largely down to raw talent, which would be evident in any early snapshot of an individual. After all, Albert Einstein showed brilliance at a very young age. But this presumes a static view of science itself, while in reality the landscape of science has evolved dramatically over the century since Einstein’s day. Today, scientific information changes constantly and there are many more scientists around. In this climate, success is often linked to acquired skills, such as being able to adjust to rapidly changing intellectual landscapes—for example, big data—and to identify the right problem to work on while others are still searching in the dark. Today’s science also requires good “soft” skills, such as the ability to lead other scientists and to communicate results so that they promote progress. These skills take time to develop, so any model that attempts to forecast success reliably needs to include evolution and refrain from static images (Loeb 2015a).

One obstacle to an honest evaluation process is that prominent scientists often seek to promote their own research program in an effort to link it permanently to the main stream. This tendency takes the form of senior scientists promoting their own students or group members well beyond what may count as fair play, which in the process sup-presses independent thinking. Put simply, senior scientists too often measure success by how much a younger colleague replicates their own research agenda or set of skills. For example, if they are fluent with mathematical subtleties, they will identify success with mathematical skills. In faculty recruitment, this tendency for self-replication is dangerous because it might not stop at academic qualifications but could easily spill over to an unconscious bias based on the replication of one's own gender, race or ethnicity (Loeb 2015a).

There are multiple paths to success in science. Some paths are mathematical and quantitative while others are qualitative and require conceptual vision. Rather than replicating ourselves and preserving a static past, to secure a vibrant future we should aim for diversity and promote scientists of all varieties. Anyone serving on committees should resist static images of our younger colleagues and replace them with dynamical models by paying special attention to initial conditions and embracing evolution in our assessments. To cultivate innovation, we should always encourage creativity beyond the comfort limits that we establish for ourselves (Loeb 2015a).

There are also geniuses who were discovered by chance. A notable example is the mathematician Srinivasa Ramanujan who was plucked out of obscurity based on the recognition of his raw uneducated talent by Professor Godfrey Harold Hardy of Cambridge University. One is left to wonder how many Ramanujans are living in third world countries without the resources and educational facilities to fully realize their talents (Loeb and Lingam 2018).

These examples make a convincing case that there must be many intellectual treasures that were lost in the peripheries of human history. A simple way to nurture raw talent is to offer prestigious fellowships to brilliant children with limited opportunities from the most common environments of insufficient education or low socio-cultural-economic status. In particular, governments could allocate more funds towards the nourishment of talented individuals who originate outside the established intellectual clubs of elite universities or academic societies, thereby allowing them to access better training. In this spirit, it is also important to establish incentives that reward individuals for creativity outside the mainstream dogma (Loeb and Lingam 2018). Academic planning committees that will adjust the promotion and tenure policies of colleges in order to ensure that diversity and equity will endure (Loeb 2020a).

Scientists should communicate with the public clearly and transparently. We in academia cannot continue to pat ourselves on the back, celebrating our own privileges and failing to look at the world in new and relevant ways. If we are to defend the freedom of our enterprise, we must restore dialogue with the broader public and ensure that the relevance of our work is well understood—including by us. Academics can no longer afford to pat themselves on the back and celebrate their own privileges. If they are to defend the freedom of their enterprise, they must restore dialogue with the broader public and ensure that the relevance of their research—and how research actually occurs—is well understood. Academic freedom is a precious commodity, critical to ensure that discovery of the truth is not encumbered by political or ideological forces. But this does not mean that intellectuals should hide in academic bunkers that, by protecting us from criticism by "non-experts," allow ego to flourish and enable a focus on questions that are not actually relevant to anyone else. We experts should have to explain ourselves (Loeb 2018c).

Society can support science by ensuring it has all the ingredients that would ultimately make scientific research successful (Loeb 2012b, 279). In the same spirit, scientific research has a strong impact on society, and the public deserves to know what we are doing with the funds allocated to us. Communicating our results is as important as deriving these results for securing a healthy dialogue with the public. The graduate students in astronomy pioneered a website called Astrobites that explains to non-experts the latest cutting-edge developments in astrophysics. They also organized a conference series on science communication (called ComSciCon) that brings a select group of interested students to the Boston area annually (Loeb 2014b).

This means, first and foremost, that researchers should be communicating their results in a way that supports accountability and confirms that public funds and education benefits are being used in ways that are in taxpayers' interests. The duty to communicate findings also ensures that the public is educated, not only about the topic itself, but also about the way research actually works. Scholarly books and journals often give the impression that the truth is revealed through a neat, orderly, and logical process. But research is far from being a pristine landscape; in fact, it resembles a battlefield, littered with miscalculations, failed experiments, and discarded assumptions. The path to truth is often convoluted, and those who travel along it often must navigate fierce competition and professional intrigue. Some argue that it is better to hide this reality from the public, in order to maintain credibility. Would hiding the truth from the public really do more for scientific and academic credibility than cultivating a culture of transparency? Probably not. In fact, being honest about the realities of research might enhance trust and create more space for innovation, with an informed public accepting that risk is the unavoidable and worthwhile cost of groundbreaking and broadly beneficial discoveries (Loeb 2018c).

As the future of research is contemplated, similar visionary goals—with broad engagement—must be considered. What should our next grand vision be? And how can we similarly involve all of society in this mission? Naturally, one could be guided by the interests of national security and economic prosperity. But historically, the burning front of innovation has advanced most vigorously when practical applications stimulated blue-sky ideas in basic research. Funding of practical challenges motivates innovators to come up with new ideas that are also stimulating for their pure academic value. Notable examples are the development of the first computing device by Alan Turing while he was aiming to crack the Enigma code of the Nazis, or the discovery of the Big Bang as the byproduct of the goal to improve communication, or many other examples in the remarkable history of Bell Labs (Loeb and Tripathi).

Putting money where our science is—The beauty and value of innovation

In July 1945, Vannevar Bush addressed a report to President Franklin D. Roosevelt arguing that basic research needed to become a priority supported by the federal government. As an engineer, businessman and government administrator, Bush recognized that each of these three worlds—academia, industry and government—plays a vital role in promoting scientific innovation. Crucially, he stated, the government's role should be to provide the guiding vision for basic research, seed the related effort and sustain its pool of talent. Bush's report led to the establishment of the National Science Foundation (NSF), and its legacy ultimately carried over to another federal agency that would become known for innovative research and development: NASA, which landed humans on the moon. Having recently celebrated the 50th anniversary of the lunar landing, it is timely to reflect upon the current research landscape and the enduring role of federal support and direction (Loeb and Tripathi 2019).

A striking development since Bush’s writing is the expansion of basic research beyond universities and national laboratories. Today, companies like Amazon, Google, Facebook, Microsoft and SpaceX maintain a trend in business, funding the majority of Research & Development (R&D) in the U.S. Unencumbered by middling budgets and boundaries between disciplines, industry has established an innovation ecosystem with the capacity for exponential growth. At the same time, some mainstream academic research has morphed into a conservative paradigm or an agenda-driven format, stagnating the free spirit of innovation (Loeb and Tripathi 2019). Long-lived bias of this type in the physics world could lead to similarly devastating consequences—such as an extended period of intellectual stagnation and a community of talented physicists investing time in research ventures unlikely to elucidate our understanding of nature (Loeb 2012b, 279).

Today, we need a combination of federal and private sector funding for science, and we should not rely wholly on one or the other. For example, federal funding agencies should not always insist on predictable research products, but rather allocate a small fraction of their resources to risky projects. It is imperative that individual scientists feel comfortable expressing critical views in order for the truth to ultimately prevail (Loeb 2013).

High-risk research, like a Venture Capital investment, has the potential to be more profitable than other investments. Even if only one in a million non-mainstream ideas bears fruit, it could transform our view of reality and justify the entire effort. Research-investment trajectories in astrophysics include radical concepts that gained acceptance with the collection of empirical data (Loeb 2010). Moon shots are invoked to solve a seemingly intractable problem with profound inspirational effects. The society of the future will be powered by as-yet-unknown scientific and technological breakthroughs, based on research today. Stable, long-term federal support for basic research to invigorate this future, is a moon shot of its own (Loeb and Tripathi 2019).

The external threats of a decline in federal funding could be partly compensated for by an increase in funding from the private sector. I have successfully applied this approach by securing funding for the Institute for Theory and Computation (ITC), Black Hole Initiative (BHI), and the Starshot Initiative (Loeb 2017b). New funding streams should be established in other fields. The LIGO discovery of black-hole mergers should encourage a 'template-free' search for new sources of gravitational waves that were never imagined. The Kepler satellite's discovery that roughly one-quarter of all stars in the Galaxy host a habitable Earth-mass planet should lead to a renewed effort in the search for extraterrestrial life, including new methods for finding intelligent civilizations (Guillochon and Loeb 2015; Loeb 2016).

All this means that selection and promotion committees and grant-awarding agencies in all fields of science must find new ways to reward creative thinking. For example, funding agencies could allocate a fixed fraction of their budget to risky but potentially highly profitable projects outside the mainstream. We will all benefit from the implementation of a strategy that encourages innovation. And for those who take the road less travelled, keep your spirits up. After all, is there any point to doing science other than to take that road? (Loeb 2010).

It is remarkable to witness for-profit organizations now taking the lead over nonprofit organizations on some risky projects. These private investments remind us that scientific R&D is not a zero-sum game analogous to pulling a short blanket up to cover your head while taking the risk of exposing your toes. Instead, it is an infinite-sum game with an infinitely stretchable blanket, where one discovery inspires many follow-up discoveries, and innovation generates revenues that could fund additional research (Loeb and Tripathi 2019).

In a time when software and rapid prototyping opportunities are ubiquitous, students, manufacturers and entrepreneurs can carry out R&D on numerous frontiers from gene editing to small satellite deployment. Visionary grand challenges, such as the Quantum Information Initiative, extend opportunities for R&D from outside the traditional halls of knowledge, continuing the bipartisan legacy of American innovation and exploration. They may lead us to tackle looming, overlooked topics, like the food-energy-water nexus or the ethical challenges associated with emerging technologies such as gene editing, artificial intelligence or robotics (Loeb and Tripathi 2019).

Invitations for diverse communities to get involved in grand challenges are needed not only for the public but also within established research organizations. A similar strategy for accommodating the rapidly changing landscape of innovation is to allocate federal funding to academic researchers based on larger themes rather than organize it by discipline. Federal agencies such as the National Science Foundation (NSF) could accelerate scientific discovery by allocating a predetermined fraction of the available funds to risky projects that could open up new horizons if successful (Loeb and Tripathi 2019).

This implies the generation of funding streams for research groups that have a demonstrated track record of creativity rather than a focus on narrowly defined projects with anticipated outcomes. This approach would benefit from peer review of proposals by the same innovators who are getting funded, so as to build a community for "out of the box" thinking. Fostering interactions among community members would make its impact bigger than the sum of its parts; for example, "massively collaborative mathematics" as envisioned by the Polymath Project, provides a new path for proving theorems or conjectures by a community effort rather than individuals. Engaging a larger community increases the diversity of ideas and the likelihood for success (Loeb and Tripathi 2019).

As we actively expand our community of innovators, it is vital for federal investments to support environments that cultivate excellence, namely our R&D infrastructure. Funding newer visionary projects cannot be divorced from funding the ongoing maintenance and updates of existing facilities. As one of its 10 Big Ideas, the NSF set forth the goal of investing in mid-scale research infrastructure, which was previously unfunded. This seemingly mundane objective is part of a larger, vital need to invest not only in ideas for research but in the environments that enable these ideas. Carefully crafted federal research budgets should include steady allocations for scientific infrastructure, increasing the longevity of facilities and data products. Equally global, and even more vital to research, are the talented minds in the R&D workforce. Government involvement is needed to recruit and sustain talent, by ensuring our institutions are the finest available and there are opportunities for students and researchers from diverse backgrounds to make use of them (Loeb and Tripathi 2019).

Furthermore, I have been arguing for many years that funding agencies should promote the analysis of data for serendipitous purposes beyond major programs and the main-stream dogma. At a minimum, when funding is tight, a research frontier should maintain at least two ways of interpreting data so that new experiments will aim to select the correct one. This should apply to alter-natives to inflation when dealing with new cosmological data, and to alternatives to cold dark matter when discrepancies are observed in the properties of dark-matter-dominated galaxies (Loeb 2016).

Balanced assessment of the level of risk and potential benefits from emerging research frontiers can increase the efficiency of the workforce, leading to stronger growth. Maintaining balance and ensuring diversity among subfields, taking some risks and avoiding funneling resources into a small number of successful but conservative programs are important considerations for funding agencies (Loeb 2010, 358; Loeb 2012b, 279). This approach demands an understanding that progress will not be steady over time, because discoveries rest on extensive preparatory work. It is thus inappropriate to measure success based on the contemporaneous level of allocated resources. Lost resources (time and money) should never be a concern in a culture that is not tied to a specific programmatic agenda, because an unexpected discovery could be far more valuable in the long term than these lost resources (Loeb 2014c).

It would not be prudent for agencies to allocate all of their funding to high-risk research. But they should allocate a small fraction—say, 20%—of their resources to research that is not tied to specific goals. Such a funding scheme is essential for promoting breakthroughs in the long run, because it encourages researchers to take on risky projects with fundamentally unpredictable outcomes but potentially high gains. Most important, it would give individuals the freedom to respond to unexpected insights as they arise, rather than compelling them to follow a prescribed agenda (Loeb 2014c). In other words, 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 (Loeb 2014a).

The triad of visionary leadership, infrastructure investments and talent development, can pave the way to a brilliant future. With secure federal research opportunities, the possibilities are endless and unexpected. In the end, the future may not be forward, but upward—among the stars (Loeb and Tripathi 2019).

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