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Searching for sentience: SETI today

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Abstract: For more than four decades, a small group of researchers has sought to find evidence of extraterrestrial intelligence *in situ*, by detecting microwave signals that would betray its existence. Despite the failure to find these signals so far, there is continued and even accelerated effort to press the search. Recent advances include greater emphasis on experiments at optical wavelengths, and the construction of a new radio telescope that is deliberately designed for such reconnaissance. In addition to these instrumental improvements, several strategies have been proposed that might better the chances of 'looking in the right place, at the right time'. This review of the current state of SETI research concludes with a speculative look at the nature of the sought-for extraterrestrials, and when it is likely we might find them.

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Nature of the search

The premise of SETI (the Search for Extraterrestrial Intelligence) is that the existence of sentient beings elsewhere in the cosmos can be proved by detecting signals that are either deliberately sent our way or that inadvertently leak from their home worlds.

As a strategy for demonstrating that 'we are not alone', SETI has several advantages over other suggested approaches. Direct investigation, using interstellar travel (either by us or by them) seems impractical at best and impossible at worst. Several authors (e.g. Oliver 1990) have pointed out the enormous energy costs required to bridge the distances between the stars with acceptable travel times (i.e. a century or less). Searching for extraterrestrial artifacts, another proposed method for locating cosmic company, suffers from uncertainty in exactly what to search for. Communications, on the other hand, are energetically inexpensive, and consequently might be common.

How might interstellar communication be effected? While high-speed particles and gravity waves have been occasionally considered, electromagnetic radiation – radio or light – continues to hold the upper hand among those interested in this problem simply because: (1) the energy cost per bit is low; (2) the signals travel at the speed of light; and (3) both transmission and reception are relatively easy to implement. The first published analysis of the practicality of signalling between stars was that of Cocconi & Morrison (1959). This seminal paper pointed out both the practicality of radio communication at light-year distances (our own technology was on the verge of being able to do this), and the suggestion that microwave frequencies – and in particular, frequencies near the 1420 MHz (21 cm) line of neutral hydrogen – seemed particularly attractive as universal 'hailing channels'. Not only would this frequency band be known to anyone of technical sophistication, but the universe is both relatively quiet at centimetre wavelengths and fully transparent.

Although unaware of the work of Cocconi & Morrison, Frank Drake had independently reached similar conclusions, and in the Spring of 1960 used a 26 m antenna at the National Radio Astronomy Observatory in Green Bank, West Virginia, to look for signals from the vicinity of two, nearby solar-type stars. Project Ozma, as this effort was called, was the precursor of all modern SETI experiments (Drake 1961).

Since Drake's pioneering effort, approximately 70 radio SETI experiments have been reported in the literature (see, e.g., Tarter 1985). Many of these were *ad hoc* efforts made by radio astronomers with time to spare during their more conventional observing programmes. While these experiments often made use of large antennas, the receiving systems were designed for astronomy, not SETI. The difference is principally in the expected signal bandwidth. While hydrogen observations at 21 cm typically use channels that are 50 kHz wide (corresponding to a velocity dispersion of 11 km s^{-1}), SETI researchers look for narrow-band signal components. The effects of the interstellar medium limits the minimum width of such signals to approximately 0.01–0.1 Hz, and practical SETI receivers typically have channels that are 0.1–1 Hz in width. This is so much less than the channel widths used for radio astronomy that recourse to the receivers for the latter would, one presumes, dilute the detectability of any artificial signal many thousand-fold.

Consequently, and beginning in the 1970s, SETI researchers initiated the construction of digital autocorrelation receivers that sported truly narrow-band channels, and with as many of these channels as could be afforded. The

Table 1. *Current SETI experiments*

Experiment	Institution	Telescope	No. of frequency channels	Width of channels
Project Phoenix	SETI Institute	Arecibo 305 m radio telescope	58 million	1 Hz
SERENDIP IV	University of California, Berkeley	Arecibo 305 m radio telescope	168 million	0.6 Hz
Southern SERENDIP	University of Western Sydney, Macarthur	Parkes 64 m radio telescope	58 million	0.6 Hz
SETI@home	University of California, Berkeley	Arecibo 305 m radio telescope	33 million, using narrowest bandwidth	0.07 Hz and higher
Optical SETI at Harvard and Princeton	Harvard University, Princeton University	Oak Ridge 1.5 m and Princeton 0.9 m telescopes	Visible light	Broad band optical pulses
Optical SETI at Berkeley	University of California, Berkeley	Leuschner 0.76 m telescope	Visible light	Broad band optical pulses
Optical SETI at Lick	Lick observatory, SETI Institute, Univ. of Calif. Berkeley	Nickel 1 m telescope	Visible light	Broad band optical pulses

consequence of the development of these specialized signal processors is that SETI soon became a deliberate endeavour, and less an occasional 'schedule filler' for radio astronomers.

Today, there are approximately half a dozen organized SETI experiments, world-wide. These are listed in Table 1. Half of these are so-called 'sky surveys', in which large tracts of the celestial sphere visible to the telescope in question are surveyed. There are two advantages to such surveys: (1) they make no assumptions about alien habitats and (2) they can often be 'piggybacked' on to telescopes doing conventional research, and thus garner large amounts of observing time.

A second type of experiment is the 'targeted search'. In this case, the SETI researchers have control over the telescope, and can choose its pointing. In nearly all cases, this means targeting relatively nearby, solar-type stars, much as for Drake's Project Ozma. A current example of this type of reconnaissance is the SETI Institute's Project Phoenix, which over the course of 8 years will scrutinize approximately 1000 star systems within $\sim$150 light-years of the Sun. Several telescopes in both hemispheres have been used for this project; Phoenix is currently deployed at the 305 m antenna in Arecibo, Puerto Rico. It is particularly noteworthy for its wide frequency coverage: 1200–2700 MHz, and its specialized digital signal analysers able to search for both drifting CW (monochromatic) and slowly pulsed signals. In general, targeted searches have the following advantages. (1) Since they are undertaken with full control of the telescope, integration times are longer. This means increased sensitivity. (2) To the extent that it is reasonable to expect biology to occur in the environs of Sun-like stars, such a strategy maximizes the efficiency of the search.

Optical SETI

While radio searches have dominated SETI ever since Drake's pioneering effort, searches at optical wavelengths, either visible or infrared, are becoming more popular. In SETI's early days, optical SETI (OSETI) was often dismissed as a less promising approach because of a simple energetics argument. To send a single bit of information at microwave frequencies requires approximately 40 photons. Good photomultipliers are able to respond to single photons, so the corresponding requirement in the optical range is one photon per bit. However, a visible light photon has 500 000 times the energy of a microwave photon, so the energy cost to transmit a given amount of information is four orders of magnitude higher in the optical range compared with microwaves.

However, this simple argument ignores the fact that high-powered, coherent light sources (lasers) can be readily focused with $\sim$1 m diameter mirrors into beams that illuminate individual solar systems at distances of hundreds of light-years. Equivalent antenna arrays that could achieve a similar degree of focusing in the radio range are large and expensive. The ease with which optical photons can be directed means that, if a society chooses to deliberately target recipients of its transmission, the cost of sending that information in the optical range might be no more than doing so via radio. Even at infrared wavelengths, one can easily achieve a factor of 10^6 more gain in the optical than in the microwave range. The conclusion is that radio and optical signaling can achieve comparable signal-to-noise ratios at the receiving end for similar transmitter powers (Ekers *et al.* 2002).

A simple scheme that a civilization interested in 'getting in touch' might employ is to use an automated laser-cum-mirror system to systematically send a series of pulses to a few thousand promising star systems in its neighbourhood, repeating this optical 'ping' every few days or so. As an illustrative example of the required technology, we note that a laser capable of producing a 10^8 J nanosecond pulse, directed into a 1 m mirror, would – during the nanosecond when the pulse was on – generate four orders of magnitude more photons than a solar-type star at a distance of 10^3 light-years. In other words, short optical pulses can easily outshine a star, especially when distances are $>$100 light-years.

The strategy for finding such pulsed signals is straightforward: a photon detector (photomultiplier) is fitted to an optical telescope that is aimed at likely stellar targets, and any extraordinary photon bursts occurring in, say, a nanosecond time frame are noted. Actual experiments underway today, in which multiple detectors are used to minimize false alarms, could respond to bursts as small as several tens of photons.

The advantage of OSETI is that it is relatively inexpensive and little observing has been done so far, so the field is still 'open'. The disadvantage is that it is reasonable to presume that optical transmissions would be directed and intermittent, meaning that repeated observations of many star systems are a likely requirement for success.

In reviewing the amount of sky that has been scrutinized by SETI to date, a crude summary is to note that most of the sky has been surveyed to a sensitivity of $\sim 10^{-24}$ W m^{-2} in a small band near 1420 MHz, and ~ 500 nearby star systems have been observed by Project Phoenix to a sensitivity of $\sim 10^{-25}$ W m^{-2} over a wider band (1200–2700 MHz). Note that 10^{-25} W m^{-2} is the flux produced by a 100 m telescope sporting a 100 kW transmitter at 100 light-years distance. OSETI experiments have so far examined several thousand nearby stellar systems for nanosecond pulses.

None of these experiments has yet produced a confirmed extraterrestrial detection.

How we might improve the search

There are two approaches to improving SETI experiments. The first is to adopt clever strategies that might sharpen our choice of direction, frequency or when to observe.

While many authors (e.g. Blair & Zadnik 1993) have suggested novel wavelengths that might be monitored, such as twice the hydrogen frequency, none have proved compelling to the SETI community, and most experiments continue to observe near 1420 MHz.

Suggestions with regard to where to point the telescope (other than at stars) have included searching in the directions of pulsars, the galactic plane, the galactic centre and even nearby galaxies (where typically tens or hundreds of millions of star systems can be examined simultaneously). A few observing programmes have addressed at least the last three of these categories (Sagan & Drake 1974; Shostak & Tarter 1985; Shostak *et al.* 1996; Sullivan *et al.* 1997).

A second type of clever target is typified by supernovae (Makovetskii 1977; Lemarchand 1994). The point has been made that if an alien society were to witness a supernova, they might promptly broadcast a signal in the anti-direction of this event. Consequently, if we discover a supernova ourselves, we should then look in its direction for signals sent our way by such enlightened societies (more generally, we should observe in a direction approximately the presumed width of a transmitting antenna, in order to pick up off-axis societies). The clear advantage of this idea is that it tells us both where and when to observe. The disadvantage is the paucity of targets.

Another possibility is to observe long-period eclipsing binaries during transit (Shostak 1997). If any of these have technological civilizations, then those societies will surely have colonized both of the stars in their system, and the line-of-sight connecting the two suns will be an obvious communications pipeline which would be aimed at us during transit. A similar idea is to make SETI observations in the anti-Sun direction, following the ecliptic, for it is in these locales that the Earth will be seen in transit by other societies, and this event will be an obvious synchronization device for their broadcasts (Castellano *et al.* 2000; Shostak & Villard 2002). Once again, both schemes tell us where and when to search. The former is currently limited by the lack of data on long-period (>10 years) eclipsing binaries, and the latter, while eminently feasible now, may not sample sufficient numbers of nearby stars.

The second approach to improving SETI is simply to refine our instrumentation by taking advantage of the rapid advance of digital electronics. As semiconductor pioneer Gordon Moore noted more than a dozen years ago, this advance – if measured by the number of transistors on a chip – is exponential. The computing power available per dollar expended is currently doubling each 18 months.

The present capabilities of this technology allow a new type of radio telescope to be built: an array of hundreds of small (and therefore, relatively inexpensive) antennas. In the past, the savings that could be garnered by using small dishes was more than offset by the cost of the electronics to connect them together (e.g. correlators for beam formation and spectral analysis, as well as the amplifiers at the foci). This situation has now changed, and the Allen Telescope Array, currently being built by the SETI Institute and the Radio Astronomy Laboratory of the University of California, Berkeley, and consisting of 350 antennas each 6 m in diameter, will cost one-third or less that of a conventional radio telescope of similar collecting area, approximately 10^4 m^2.

Because the Allen Telescope Array can be simultaneously used for radio astronomy mapping, as well as for SETI, it will allow targeted searches to be conducted continuously. In addition, by forming several beams (radio pixels) on the sky at once, star systems can be examined in parallel, rather than serially, as is now the case. This array is scheduled for completion in 2005, and when operational, will be approximately two orders of magnitude faster at searching nearby star systems than Project Phoenix is now.

However, an important point is that this instrument is easily upgraded, both with regard to the number of simultaneous beams (i.e. target stars), as well as the number of instantaneous frequency channels. One expects that such an improvement will follow the exponential law noted by Moore, at least for as long as that rate of technological progress is maintained.

What does this mean for SETI? Traditionally, the degree of optimism or otherwise among SETI researchers has been determined by their estimate of the terms in the Drake equation (Drake 1965). This simple formula gives the number N of co-existent, transmitting societies in the Galaxy. Published estimates of N range over at least six orders of magnitude (Dick 1996), but a logarithmic average of these (speculative) values is $N = 10^4$. If we assume that solar-type stars are the most likely to incubate intelligence, then one in $\sim 10^6$ such stars will have a co-existent civilization. This is the number of star systems that can be examined by the Allen Telescope Array in ~ 15 years, assuming improvement following Moore's law persists for that length of time.

In other words, if the premises of SETI are correct with regard to likely signal types, and if $N \sim 10^4$, then a detection is likely to occur by about 2020. This would suggest that SETI is not, as sometimes averred, a project for which discovery probably lies in the distant future. Either it will succeed relatively soon, or if that proves not to be the case, we should reconsider the premises that underpin our motivation to search.

The big view

All contemporary SETI experiments are, to a great degree, grounded in the assumptions of the Drake equation. The latter assumes that 'they' are in many fundamental respects similar to 'us', a point of view that gains credence from its obvious conservatism. In particular, we assume that intelligent aliens have been spawned on a planet surrounding a dwarf star, and – of greater consequence for the search – still dwell in that star system.

In addition, and more subtly, we adopt an anthropomorphic view of the aliens themselves. They are, we assume, members of a highly evolved species, consisting of many individuals (billions, at least), each with relatively small brains. They have lifetimes that are short compared with geological time, so that they cannot easily travel among the stars. And on the basis of our own experience, we expect that aggression plays an evolutionary role on their planet as well as ours, and consequently they may be afflicted with a short, mean technological lifetime (L in the Drake equation).

But all such assumptions are based on the recent development of intelligence on Earth, and take no account of our future evolution. It has been frequently suggested that we will soon re-engineer our own biology, something that will surely be initiated with the intention of eliminating defects and effecting improvements to our health and abilities. Once having done this, it seems but a short step to inventing our successors: artificial intelligence. This development may be less than a century in our future.

The advantages of artificial intelligence (AI) are considerable. Unlike biological sentience, AI can improve the individual, and can tightly control the direction of evolution. Artificial intelligence evolves in a Lamarckian, rather than a Darwinian fashion. The timescale for improvement can be short. In other words, machine intelligence could very quickly outstrip its biological creators.

Assuming that such a scenario has frequently transpired, it is worth noting the practical aspects of AI. Unlike soft, squishy beings with short lifetimes, machine intelligence could be robust and of indeterminate lifetime. Consequently, it could far more easily traverse interstellar distances. After all, when you are immortal (or nearly so), all trips are the same length. Societies comprised of billions of individuals, each having small processing units (brains) that can but poorly communicate information to its brethren, would not be necessary or even desirable. Small numbers of very sophisticated AI machines might quickly become dominant (in an information sense), with the 'winner taking all'.

Would such thinking devices remain near the stars of their birth? That is unclear. Given their ability to travel, they could choose the most interesting locales (those with abundant supplies or matter, energy or both). Possibly, they might find some attraction to interstellar space, where the $\sim 10^{-6}$ W m^{-2} stellar flux could be sufficient to supply their needs.

Needless to note, this is speculation of a high order. But it is hardly radical to suggest that the premises that underlie our SETI experiments could well prove to be too provincial. The universe, after all, is old enough to have truly deep intelligence, and of a form far removed from what we have always assumed.

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