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The Search for Extraterrestrial Intelligence (SETI)

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■ **Abstract** The search for evidence of extraterrestrial intelligence is placed in the broader astronomical context of the search for extrasolar planets and biomarkers of primitive life elsewhere in the universe. A decision tree of possible search strategies is presented as well as a brief history of the search for extraterrestrial intelligence (SETI) projects since 1960. The characteristics of 14 SETI projects currently operating on telescopes are discussed and compared using one of many possible figures of merit. Plans for SETI searches in the immediate and more distant future are outlined. Plans for success, the significance of null results, and some opinions on deliberate transmission of signals (as well as listening) are also included. SETI results to date are negative, but in reality, not much searching has yet been done.

INTRODUCTION

From the dawn of civilization, humans have looked skyward and wondered whether we share this universe with other sentient beings. For millennia we have asked our philosophers and priests to answer this question for us. Answers have always been forthcoming and have reflected the belief system represented by the person providing the answers (Dick 1998). In the second half of the twentieth century the rules for this inquiry changed. For the first time in humankind's history scientists and engineers have an opportunity to plan and conduct experiments—experiments that just might answer this old and important question. SETI, the Search for Extra-Terrestrial Intelligence, is now 40 years old as a scientific discipline. Cocconi & Morrison (1959) published the first scientific paper on the subject in *Nature*, while Frank Drake (1961) was preparing to conduct Project Ozma, the first observational program using a traditional radio astronomical instrument. It may take another 40 years, or 400 years, or even 4000 years for this exploratory scientific effort to find what it seeks or to conclude that there is nothing to be found. On the other hand, it could succeed tomorrow, and that tantalizing possibility is why scientists

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and the lay-public alike remain enthusiastic about the search. It is one of those scientific long shots, with immense payoffs but no guarantee of success.

This article is appearing in the *Annual Review of Astronomy and Astrophysics* rather than in e.g., the *Annual Review of Sociology* because searching for evidence of intelligent life on or near other bodies within our own solar system or in the vicinity of distant stars requires the observational tools of the astronomer. Claims by nonastronomers that intelligent creatures, or their surrogate machines, have visited the Earth in the past, or are resident today, have not withstood the evidentiary demands inherent in scientific methodology (Sagan 1996, Klass 1983). There have been no extraordinary pieces of evidence to substantiate previous extraordinary claims of this sort. Until this changes, SETI will be practiced by professional and amateur astronomers using astronomical instruments. It will involve remote sensing of distant environs and robotic (and then perhaps human) exploration of solar system worlds, and it will make use of what we think we know about the nature of the universe to distinguish between astrophysical and intelligent phenomena. Every new astronomical instrument that opens up pristine cells of observational phase space, and thus new ways of observing the universe, may potentially surprise us with unexpected manifestations of extraterrestrial technologies, and by inference, extraterrestrial technologists (Tarter 1983). Observers need to remind themselves to remain open to this possibility. They need to have the persistence of Jocelyn Bell and follow up on any "bit of scruff" that shows up in the data (Bell-Burnell 1977). Existing tools should also be used to search in a systematic way for signs of intelligent life elsewhere. If possible, such searches should be planned and conducted so that a null result might be significant.

We are, at present, an impatient species with little or no ability to sustain the long view of our activities (Brand 1999). SETI, as with any scientific exploration, must document rigorously the territory it has explored to date and the thoroughness of that exploration. Future explorers can then decide whether technological advances warrant a reexamination or whether previously unexplored territory should be selected for continuing investigations. This also allows for dissemination of information about how much has been done relative to how much might reasonably need to be done. It is the only way that the public, and funding sources, can be convinced to extend their horizons and continue what may be a very long process. SETI currently enjoys the great advantage of the public's interest; they want to know what is happening. It is to everyone's benefit to nurture this interest in the real science of SETI rather than in the pseudoscience that preys on the public's credulity.

SEARCHING FOR LIFE—THE NONINTELLIGENT KIND

There is a timeliness to science: Subjects become "hot" when both the theoretical underpinnings and the observational tools for exploring them are in hand. For example, the theory of rotating, collapsed stars was old (Baade & Zwicky 1934) and almost forgotten when pulsars were discovered with a newly commissioned telescope (Bell & Hewish 1967). Pulsar astronomy then exploded and continues to be an exciting field today because both the observational details and the theoretical tools for modeling the astrophysics keep improving.

In the 1960s and 1970s NASA's exobiology program was a stepchild specialty that sent two life-detection packages to the surface of Mars equipped primarily to detect some very terrestrial forms of life. Three decades ago, searching for planets beyond our solar system was a labor of love carried out by only a few dedicated individuals struggling against a checkered history of premature announcements (Gatewood & Eichhorn 1973). Back then, SETI was already drawing fire from Congressional critics in this country while being actively pursued in the former Soviet Union. However, today we know of many more planets beyond our solar system than within it, although planets like the Earth still elude us (Marcy & Butler 1998). NASA's Astrobiology program and the National Science Foundation's Life in Extreme Environments program have become the scientific rage, attracting new funds and many of the best and brightest of our students. Humans are beginning to have a bit more respect for microbial life in all its splendid diversity. We have begun to acknowledge its survivability under a surprising range of conditions, utilizing a number of different metabolic strategies (Chyba 2000, Wachtershauser 2000). We are even starting to agree on a workable, Darwinian definition for life that may or may not help us recognize it elsewhere (Joyce 1994)[1]. We are awaiting opportunities to return to Mars to search for fossils, or even extant life below the surface where liquid water may still persist (Malin et al. 2000). We are speculating freely about whether biology of any sort might occupy the (presumed) briny liquid water oceans beneath the icy carapaces of Europa, Callisto, and Ganymede and planning for missions that might inform us (Blankenship et al. 1999). The Allen Hills meteorite (McKay et al. 1996) has reminded us that it is sometimes difficult to distinguish biology from geology and further, that a second example of biology within the solar system does not necessarily mean a second genesis—we may all be Martians.

SETI continues to draw Congressional fire, but for the moment, it has found a willing base of philanthropic support in the United States. The nascent Astrobiology program actively promotes the study of the history of the biogenic elements from their formation in massive stars, through their incorporation into planetary systems, and into life as we know it here on Earth. That program is designing progressively more capable missions to find other Earths, whose atmospheres might eventually yield spectroscopic evidence for plausible biomarkers, but it draws the line at looking for evidence of intelligent life.

The coming decades will see the increasing inclusion of biology and biochemistry into what has historically been the observational, physical, mathematical, and chemical basis for astronomy. The inclusion of the word "life" in astronomical grant and observing proposals is rapidly becoming obligatory. Christian DeDuve (1997)

[1]"Life is a self-sustained [chemical] system capable of undergoing Darwinian evolution."

has declared that "Life is a cosmic imperative," and the next two decades could well validate that statement. The Taylor & McKee (2000) report on *Astronomy and Astrophysics in the New Millennium* recommended 7 major space- and ground-based projects, 12 moderate projects, and 8 small initiatives. At least 11 of these recommended projects directly or indirectly relate to our own origins and the study of life in the universe. The survey committee identified "Is there life elsewhere in the universe?" as one of five fundamental questions setting the scientific agenda for the decade to come. The committee further argued that within this millenium astronomers must "search for life outside of Earth, and if it is found, determine its nature and distribution in the galaxy." While references to the question of the origin of life on Earth and its prevalence elsewhere were scattered throughout the text of that report, in the chapter entitled "Benefits of Astronomy to the Nation" this idea was mentioned 11 times within 10 pages. Not only is this science timely, but the public is interested.

There is much we can look forward to in the next two decades. Because life as we know it is a planetary phenomenon, the search for extrasolar planets, a better understanding of how the Earth and our own solar system formed, and whether our system is typical are all relevant to the question of life elsewhere in the universe. Therefore, efforts are aimed not only at detecting planets (particularly terrestrial planets) close to home so we can probe them for potential biomarkers, but also at making a census of planets and solar systems associated with large populations of stars. Given sufficient angular resolution and methods for dealing with the extreme contrast ratio of stellar light to reflected planetary light in the visible or infrared—adaptive optics, speckles, and nulling interferometers—any planets around the nearest stars may be directly imaged. Other methods of detection are indirect. It is possible to measure the reflex motion of the star about the planetary system's center of mass owing to the gravitational tug of its orbiting planets. One can also measure the diminution of stellar luminosity as a planet transits, or the magnification of the light of a distant star by a properly aligned planet (sitting near the Einstein ring at a distance R_E from the foreground parent star), creating a short-lived gravitational microlensing event.

Measurements of reflex motions and direct imaging techniques will work best on nearby stars. Transits and microlensing require long-term monitoring of large populations of stars and thus can yield planetary statistics and demographics. Schneider (1999) has summarized the relevant observable parameters and the required instrumental precision for the various methods; they are elaborated here in Table 1. M_p, R_p, T_p, L_p, and A are the mass, radius, temperature, luminosity, and albedo of the planet, while a is the radius and P is the period of its (assumed circular) orbit. $L*$ and $a*$ are the stellar luminosity and radius of its reflex orbit, and the system is assumed to be at a distance D from the Earth. The left-hand column of Table 1 is the observable parameter, and the right two columns give values of precision necessary for detection of a Jupiter-mass planet in a 5 AU orbit, and an Earth-mass planet in a 1 AU orbit around a 1 $M_\odot$ star at a distance of 5 pc.

TABLE 1 Observables and measurement precision for extrasolar planet detection

Observable	Parametric form	Jupiter (D = 5 pc)	Earth (D = 5 pc)
Angular separation $\Delta\vartheta$	$(a/D) \times \phi(t)$	1 arcsec	0.2 arcsec
Astrometric wobble $\Delta\alpha$	$(M_p/M_*)(a/D) \times \phi(t)$	1 mas	0.6 μas
Brightness contrast Δm	$0.125A(R_p/a)^2 \times \phi(t)$	23 mag	24.75 mag
Radial velocity ΔR_V	$(M_p \sin(i_p)/M_*)\ (GM_*/a)^{1/2} \times \phi(t)$	13 m/s	3 cm/s
Time of arrival ΔT	$(a/c)(M_p/M_*)$	5 s	3 ms
Photometric precision η	$(R_p/R_*)^2$	0.01	8×10^{-5}
Transit duration D_T	$(P/\pi)(R_*/a)$	25 h	11 h
μ lensing amplification A_G for D = 4Kpc	$\leq 4[(GM_pD/c^2)/R_E]^{1/2}$	$\leq$0.1 mag	$\leq$0.01 mag
μ lensing duration T_G for transverse velocity V = 100 Km/s	$\approx 4[GM_pD/c^2]^{1/2}/V$	$\leq$3 days	$\leq$4 hrs

For calculating the last two columns in Table 1, the phase angle function $\phi(t) = 1 - \sin(i)\sin(2\pi t/P)$ was assumed to have its maximum value of 1, thus removing the unknown inclination angle i between the orbital plane and the plane on the sky.

Adaptive optics are now being pursued aggressively on the Keck, VLT, and other large aperture telescopes. Various tricks for achieving spatial resolution and suppression of the light from the central star may permit ground-based imaging of massive Jupiters. However, the real potential for imaging lies in space missions. With an appropriate high-resolution correction instrument, the Next Generation Space Telescope (NGST) could find Earth-like planets around the nearest dozen stars. Later, nulling infrared interferometers (Bracewell & McPhie 1979) like NASA's Terrestrial Planet Finder and ESA's Darwin—or some future amalgam of the two—should thoroughly survey more than 100 nearby stars for terrestrial planets and allow spectroscopic exploration for atmospheric biomarkers such as ozone (Woolf & Angel 1998).

From the ground, very long baseline interferometry astrometric studies of radio stars now achieve 100 μas precision (Lestrade et al. 1994). Within the next few years, narrowfield astrometry should begin on the Keck interferometer and yield 10 μas precision together with the long temporal baseline necessary to find Uranus-like planets in solar system analogs (Colavita et al. 1998). Those observations will complement the short-lived orbiting astrometric missions such as GAIA, which will achieve a precision of 1–10 μas (for stars with M_v from 5 to

15) and detection limits of 10 Earth-masses around 100 stars within 10 pc, and Jupiter-masses around 5×10^5 stars within 200 pc (Lattanzi et al. 1997). The Space Interferometry Mission (SIM) will conduct narrowfield astrometric observations yielding 1 μas precision and 2 Earth-mass detection limits around 50 nearby stars and 5–10 Earth-mass limits for another 200 stars, as well as being a technology demonstrator for the Terrestrial Planet Finder (TPF) and supplying it with a candidate target list (Peterson & Shao 2000).

Transit searches for Jupiters are possible from the ground with 1% photometry, and in the special case of eclipsing binary stars, detection of terrestrial-type planets is feasible using a network of small, ground-based observatories (Doyle et al. 2000). COROT is an ESA spacecraft that has recently been retooled to spend half its time looking for short-period transit events and is currently scaled to expect tens of planets of size $\geq$2 Earth-radii in orbits of $\leq$0.5 AU (Schneider 1999). Kepler is a dedicated photometric mission that would be capable of examining 100,000 stars continuously for 4 years, with the expectation of detecting hundreds of terrestrial planets and nearly a thousand hot-Jupiters (Koch et al. 1998). This spacecraft is currently competing for selection as a NASA Discovery class mission. Transits of hot-Jupiters also offer the first opportunity to study potential biological markers in their atmospheres. The CRIRES instrument on the VLT should be able to see CO, CH_4, and H_20 in absorption during a transit, and NGST may be able to find the signature of chlorophyll absorption between 400–700 nm (Boccaletti et al. 1999). The PLANET group has used the current lack of short-duration microlensing events to conclude that less than one third of low-mass stars ($\sim$0.3 $M_\odot$) have Jupiter-mass companions in orbits with semi-major axes between 1.5 and 4 AU (Albrow et al. 2001). An improved network of ground-based observatories for microlensing and the proposed transit surveys will help establish the population statistics for planets in the galaxy.

Most of the 67 exoplanets found to date have been discovered using ground-based radial velocity techniques (Vogt et al. 2000). These searches will continue with the HIRES spectrometer on the 10-m Keck telescope and the CORALIE spectrometer on the 3.6-m ESO telescope at La Silla; both will achieve a precision of 2 m/s. This precision will be adequate to detect sub-Saturn-mass planets and is critical to the eventual identification of low-eccentricity solar system analogs (Butler 2000). Because white-light fluctuations in the stellar disks set a limit on achievable precision before the atmosphere imposes severe limitations, this technique will not benefit from moving into space. Indeed the next challenge—finding the massive planets in long-period orbits within any solar-system analogs—demands the decades of continuing observations achievable so far only from the ground. With ground-based radio telescopes, terrestrial-mass planets have indeed already been detected during observations of pulsars. Because the astrophysical clocks intrinsic to pulsars are so precise, changes in the time of arrival of individual pulses can be measured to the extreme accuracy required for detection of these low-mass companions (Wolszczsan & Frail 1992). The existence of these terrestrial-mass bodies is undeniable; what they are, and how they formed or survived, is still much debated.

Figure 1 illustrates the discovery space for extrasolar planets with the different observational techniques mentioned above using some specific missions as examples. The extrasolar planets discovered at the time of this writing are included, along with the planets of our solar system, and the pulsar planets.

Table 3 of the latest decadal review (Taylor & McKee 2000) nicely summarizes the suite of proposed space craft and ground-based facilities that are recommended to help trace the origin of the biogenic elements and understand how they become incorporated into protoplanetary disks and planets themselves. There is an equally impressive set of Cornerstone and smaller missions being proposed by ESA and a number of astrophysical laboratory initiatives to provide ground-truth for interpreting the remote sensing. It is extremely tempting to link the ubiquitous organic chemistry of the giant molecular clouds with the raw materials for life on our planet, and perhaps even the chiral nature of our biochemistry (Bailey et al. 1998); however, the devil is in the details. The greater spectral and spatial resolutions, for fainter sources, over the wider range of frequencies that will be forthcoming with these new observing facilities should let us study in exquisite detail the transformation from cloud to protostar and disk to protoplanets and smaller planetary system bodies and subsequent delivery onto the surface of a mature planet. This detail should allow us to deduce what chemistry (if any) gets set back to elemental form along the path from cloud to crust.

In the coming decades many planetary scientists will be seeking evidence for a possible second genesis of life within our own solar system. Four billion years ago when life was getting started on Earth, early Mars was wet and warm with liquid water on its surface (Forget & Pierrehumbert 1997). Life might have originated on Mars, and even though the Viking Landers found no trace of it on the surface, there has been great interest in returning there to look for fossils or perhaps even extant life in any subsurface aquifers (McKay et al. 1998). Recently, the Mars Global Surveyor has provided possible evidence for the existence of liquid water quite near the surface in the not-so-distant past (Malin et al. 2000). Previous mission failures have delayed, but not deterred, the continuing exploration of this close neighbor. Another orbiter has just been launched in 2001, and two surface rovers will launch in 2003 along with ESA's Mars Express. Placeholders exist for launches in the Mars Surveyor program in 2005 and 2007, with many planetary scientists pushing for a sample return. Europa will be the destination of an orbiter originally scheduled for launch in 2003, with a goal of establishing whether a liquid water ocean (something that has been inferred from the Galileo Orbiter data) does indeed exist beneath the surface ice. An affirmative answer will bolster the case for a lander and accompanying aquabot to explore that ocean for signs of life. The Cassini mission will drop its Huygens probe into Titan in 2004 to sample the organic chemistry that may inform about the early Earth and chemical pathways to life.

The smaller bodies of the solar system will also get their share of attention. Radar observations from the newly upgraded Arecibo Observatory will explore the properties of nearby asteroids and their companions. NEAR Shoemaker recently ended its orbit of the asteroid Eros in a soft landing. The data it collected will help us to better understand the conditions in the early solar system. The Pluto/Kuiper Express was planned to fly by Pluto in 2012 and go on to closely approach primitive Kuiper Belt Objects. Increasing costs for that mission have caused its cancellation. An affordable substitute is now under study. The Stardust Mission will collect interstellar dust and then return samples from the vicinity of Comet Temple II in 2006. As with a sample return from Mars, new analysis tools will be required to tease out any biological signatures from the micro- and nano-samples. As yet there are no unambiguous methodologies for distinguishing terrestrial biology from an independent origin and evolution of life, but this challenge will be embraced enthusiastically should the sample returns demand it.

As for the origin of our own terrestrial life, increasingly sophisticated tools of molecular phylogony may more firmly root the much-uprooted universal tree of life, or at least determine whether the concept of a last common ancestor is valid. Recent claims that such an organism was an extreme thermophile are now being challenged as deeper-lying branches are proposed that appear to indicate moderate temperatures (Pace 2001). These new molecular tools, and their somewhat confusing results, have led to a useful reexamination of our theories about the nature of early life. Oceanic hydrothermal vents (and metabolic pathways based on Fe, Ni, and S) are now being studied as a possible site for the origin of life (Huber & Wachtershauser 1997), or as a last refugia from occasional ocean-vaporizing impactors (Sleep & Zahnle 1998) that persisted perhaps until 3.5 bya (Chyba 1993).

Today, astrobiologists seem to be adopting a more inclusive attitude towards the origin of the basic building blocks of life on Earth. They now embrace a combination of metallic sulfide–moderated synthesis around ancient hydrothermal vents and endogenous synthesis within various reducing environments on the early Earth and in its atmosphere, as well as exogenous synthesis and delivery by comets and asteroids (Lazcano 2000). An improved time sequence may allow us to better appreciate the cosmic and planetary phenomena that directly influenced the origin and survival of life within the solar system. Further studies of the extremes that can be tolerated by primitive life may at last bury (or give renewed life to) the panspermia hypothesis of Arrhenius (1903) or the directed-panspermia variation suggested by Crick & Orgel (1973). Just how biologically connected were the newborn terrestrial planets (Weiss et al. 2000)? What is the minimum time required for the transformation of chemistry into biology (Lazcano & Miller 1994)? What about the time required to turn microbes into mathematicians? The oldest microfossil dates back 3.5 bya, but differences in the isotopic abundances of carbon $^{13}C/^{12}C$ in the rocks of the Isua formation in Greenland argue for the existence of life on Earth as long ago as 3.8 bya (Mojzsis et al. 1996). Ward & Brownlee (2000) posit that this long interval between the ancient origin for life and its relatively recent blossoming into animal complexity during the Pre-Cambrian explosion 750 mya means that life may be abundant elsewhere in the galaxy, but animal or complex life will be rare. These authors may be correct, or they may simply be succumbing to a logical error of the type known as affirmation of the consequent. We only know our own history (barely); we do not know whether it is typical or perhaps quite unlucky. However, the principle of mediocrity espoused by Carl Sagan and many other scientists suggests that we use our own singular statistical sample as representative of the norm. Without data, we lack an $N = 2$ example. The ambitious programs of the coming decades may provide some of the required data. Once, or if, we have found a second example, the cosmic answer must be that $N =$ many.

SEARCHING FOR LIFE—THE INTELLIGENT KIND

When it comes to the question of intelligent extraterrestrial life, the most recent Astronomy Survey Committee (Taylor & McKee 2000) echoed the recommendations of all three previous decadal reviews (Greenstein 1972, Field 1982, Bahcall 1991), endorsing modest SETI projects. They further noted that whereas radio observations had dominated the field in the past, the likeliest path to success is uncertain. Why is it that radio astronomers have historically been the leaders of this field, and what other options are there?

Choosing a Search Strategy

NEAR OR FAR? "Intelligence" is just as difficult to define as "life" and impossible to detect at a distance. All searches for extraterrestrial intelligence are in fact searches for extraterrestrial technologies revealed through physical artifacts, energetic particles, or electromagnetic radiation. Although our searches must be conducted with current terrestrial technology, we can be reasonably certain that the extraterrestrial technologies we are seeking are far older than our own. The Drake Equation was conceived as the agenda for a National Academy of Sciences meeting to discuss possible communication with extraterrestrial intelligence (Drake 1962), and it does an excellent job of organizing our ignorance and clarifying this conclusion. We can estimate the current number of communicative societies N in our galaxy as $N = R{*}f_p n_e f_l f_i f_c L$, where $R{*}$ is the average rate of solar-type star formation in the galaxy, f_p is the fraction of solar-type stars with planets, n_e is the average number of Earth-like planets or moons that could support life within a solar system, f_l is the fraction of habitable planets that actually harbor life, f_i is the fraction of all life sites where intelligence develops, f_c is the fraction of intelligent sites that develop a technological civilization that communicates, and L is the average longevity of a communicative civilization. Astronomical observations can yield realistic numbers for the first two factors, and astrobiology is working on the third, but the latter terms remain in the realm of speculation. Nevertheless, we can recast this equation as $N = R_c L$, where now R_c is the average rate of emergence for communicative civilizations in the galaxy. Although R_c is unknown, an upper limit can be estimated at ∼10–100 per year (FD Drake, private communication). Therefore, this simple formulation shows clearly that communicative civilizations will not be close to us in both space and age. Having close neighbors, with whom we can communicate, demands that N be large, and thus requires that the average value of L is large—comparable to stellar ages. If, on the other hand, the average L is small because technological civilizations self-destruct or cease to communicate, then they could be close to us in age, but they will be few and far between in the galaxy. Technologies spatially close to us will be more advanced; therefore, searches limited to our locale should take advantage of this asymmetry.

PROBES? Even in the case in which a civilization's communicative phase was short-lived but sufficient for them to disperse a large number of effectively-immortal robotic emissaries, the technologies represented by any local robots we subsequently encounter will still be far in advance of our own primitive capabilities. This idea of miniature probes is not new. They were first discussed (Bracewell 1960), immediately after the publication of Cocconi & Morrison's seminal paper (1959), as a way to circumvent the enormous energy costs of sending large spacecraft over interstellar distances (Purcell 1980, Oliver 1990) without requiring enormous transmitter powers from the technologists around distant stars. The Appendix [available in the Electronic Materials section of the Annual Reviews Web site (http://annualreviews.org)] includes several searches for such probes within our own solar system—primarily in regions of local gravitational minima, where demands for orbital station-keeping would be minimized.

To date, it is possible to rule out observationally only large shiny (in reflected light and radar) objects, and only in a few locations. Nevertheless, Hart (1995) and others claim that the absence of such probes and extraterrestrials on or near Earth is the only undisputed fact of SETI. They have resurrected the Fermi Paradox (Cullers et al. 2001), combined with this "fact" and concluded that there can never have been another intelligent civilization any place or any time during the history of the Milky Way Galaxy; we are the first. If we were not, then somebody's self-replicating probes or descendants would have colonized the galaxy in a time that is short compared to the age of the Milky Way, and they would be here by now. In spite of the surprising new observational discoveries made almost weekly, astronomers have a tendency to overstate how well we have explored the universe we live in, even our own local solar neighborhood. The rings of Uranus are much larger than most hypothetical probes, yet they were not discovered until infrared telescopes began to fly above the obscuring water vapor in the Earth's atmosphere (Elliott et al. 1977). Before our radars were sufficiently powerful, and before we had small spacecraft making close approaches, scientists could speculate that even large slow-ships of extraterrestrial colonists might be hiding among the asteroids, mining their mineral resources (Papagiannis 1982). Today we know that some asteroids do have orbiting companions. Most of these attendants appear to be chips off the parent body; none of them looks like a slow-ship, at least not yet! A group of futurists plus social and physical scientists have even joined together to search for extremely small probes hiding among us (Tough 2000). They assume that these advanced robots will be monitoring our burgeoning communication via the broadcast internet, and they have invited ET to log on and identify him/her/itself.

These few examples serve to illustrate that even when it comes to physical probes within our solar system, our observations have been sparse and incomplete in the extreme. Hart's fact is without factual basis, and future searches for such solar system objects are therefore not unreasonable. However, such searches would be extremely speculative, and in the current political climate they are more likely to be approved and funded if they are carried out as secondary objectives of other astronomical investigations. What about other evidence of technologies farther away?

PHOTONS OR PARTICLES? The *Cyclops Report* (Oliver & Billingham 1996) enumerated the properties necessary for a carrier to convey one bit of information over interstellar distances. The preferred means of information transfer should have

1. Minimum energy per quantum, other things being equal;
2. As large a velocity as possible;
3. Particles that are easy to generate, launch, focus, and capture; and
4. Particles that are not appreciably absorbed or deflected by the interstellar medium.

Charged particles are deflected by magnetic fields within the galaxy and they are also absorbed. From the point of view of our current technologies, photons are the easiest particles to generate, modulate, amplify, and collect, and they travel at light speed. This makes photons preferable to probes, neutral particles with mass, and even the massless "inos." Higher frequency photons are strongly absorbed by the dust and gas of the interstellar medium, and the energy per photon is higher. For decades, these simple arguments identified microwave photons as the carriers of choice. Townes (Schwartz & Townes 1961, Townes 1983) has argued for the use of optical or infrared lasers for interstellar communication because of the bigger gains and amplification available at these higher frequencies, but radio techniques have been the overwhelming choice of researchers to date. Serious efforts at optical SETI have had to await the technology developments of "Star Wars" to demonstrate the feasibility of powerful photon-generation by the transmitter [petawatt pulsed lasers are becoming a reality even for our young technology (Perry 1996)], and the development of affordable hybrid avalanche photodiodes with fast rise times to filter out the intrinsic stellar backgrounds at the receiver. Today there are active searches at microwave and visible frequencies. These will undoubtedly be extended into the infrared when detector technology permits affordable systems.

WHAT TO LOOK FOR, WHERE AND WHEN? Having selected a frequency regime, any researcher contemplating a search for electromagnetic signals still has more choices to make: what type of signal to search for and where to look. Some search strategies also involve knowing when to look.

If an extraterrestrial technology is deliberately broadcasting a signal, it is logical to assume that they will attempt to make the signal detectable. Two schemes have been suggested: (*a*) Generate a signal that violates the natural emission mechanisms of astrophysics so that it will appear to be an obvious technological artifact and (*b*) generate a signal that at first glance looks like some type of interesting astrophysical source. The latter strategy enhances the probability that emerging technologies, practicing observational astronomy, will detect it with their standard equipment and over time come to recognize the underlying peculiarity that reveals its technological origin.

ARTIFICIAL SIGNALS Examples of the first type of signal are those that are compressed in the frequency and/or time domains beyond what astrophysics allows—signals whose time-bandwidth product approaches the limiting value ($B\tau \approx 1$) set by the uncertainty principle. Obvious members of this class of signal are coherent, narrowband carriers, or impossibly short temporal pulses. The central limit theory enforces a thermally broadened line width for any atomic or molecular transition from an ensemble of astrophysical particles. In the absence of nonlinear amplification, the fractional bandwidth of natural emitters has values of $\Delta\lambda/\lambda \sim 10^{-3}$ or 10^{-4}. Water masers can achieve a limiting value of $\Delta\lambda/\lambda$ as small as 10^{-6}, but our primitive technology can easily produce signals with a purity $\Delta\lambda/\lambda \sim 10^{-12}$, and with effort, 10^{-14}. This led the contributors to the *Cyclops Report* to suggest one or more pure sinusoidal carrier waves as an obvious choice for an artificially broadcast signal. Furthermore, if such narrowband carriers were transmitted in one or the other sense of circular polarization, the *Cyclops Report* argued, they would propagate through the interstellar medium unmodified even by Faraday rotation. More recent work (Drake & Helou 1977, Cordes & Lazio 1991) has shown that multipath scattering against the ionized component of the interstellar medium will broaden the narrowest signals, so that for looking in most directions, and over long pathlengths through the Milky Way, there exists a minimum practical frequency resolution for any search equipment. For microwave frequencies, this resolution is ~0.01 Hz (Horowitz & Sagan 1993). This is not an issue at optical frequencies (Cordes 2001), although absorption by dust becomes significant over distances of 1 kpc or more. Studies of millisecond pulsars (discovered long after Cyclops) have shown that interstellar scintillation can cause 100% amplitude fading for a narrowband signal and occasionally even amplitude amplification (Cordes & Lazio 1991). In any direction the amplitude fading will be both frequency and time dependent, with the scintillation time $\Delta t_d \propto \nu^{-1.2}$ ranging from a few to 10^4 seconds, with the shortest times applying to directions of heavy scattering at large distances in the galactic plane. This variability has three implications for SETI searches. Immediate verification of a candidate signal should occur within Δt_d so that the signal strength will not have faded. Later attempts at verification should be done with significantly improved sensitivity and more than once to ameliorate the fading. Searches should be planned to observe a given direction or target multiple times to improve the probability of a favorable amplification (Cordes et al. 1997). Interstellar scintillation has prompted Cohen & Charlton (1995) to suggest that an extraterrestrial engineer would choose to transmit a comb of narrowband signals rather than a single tone in order that at least one of them remain unfaded at all times.

The exact frequency of choice for these narrowband signals has been the subject of continuing debate since the first SETI observations (Blair & Zadnik 1993). As more astrophysical lines were discovered, the charm of a neutral hydrogen 1420 MHz magic frequency first proposed in 1959 has diminished. However, for searches with limited access to telescope time or limited financial resources that preclude the construction of large spectrometers with the millions (or even hundreds of millions) of narrowband channels necessary for systematic searching, magic frequencies are still invoked to circumscribe the range of hypotheses being tested by a given experiment.

Unless the narrowband transmitter is aimed at our receiver and both the transmitter and the receiver compensate for their respective motions within some common rest frame, there will be relative acceleration between the sender and receiver. A search for narrowband signals therefore requires not only a search through frequency space, but the ability to detect signals with some unknown Doppler frequency drift. The frequency drift rate of a signal due to natural accelerations $d\nu/dt$ will be proportional to ν, and that signal will drift through an individual spectrometer channel of width B in a time $\tau = B/(d\nu/dt)$. In an instrument having $B\tau = 1$ (to fully sample the frequency-time domain), the optimal channel width B should increase as $\sqrt{\nu}$ (Morrison et al. 1977). Thus, the resolving power needed to search for narrowband optical signals is much smaller than that needed for microwave searches.

If these narrowband, artificial signals are generated in the vicinity of a star [as is the case for any civilizations occupying planets within the habitable zone of their planetary system (Kastings et al. 1993)] they will be spatially unresolved by our current technology. The artificial signals must therefore outshine the star to be detectable. At microwave frequencies this is an easy task. A star like our sun is a weak radio source producing only 1 kW/Hz. Thus, radio astronomy can be conducted 24 hours per day. Our primitive technology routinely outshines the sun. At the frequencies of broadcast FM and TV transmitters the embedded narrowband carriers are factors of 10^6 to 10^9 brighter than the quiet sun. Because stars like the sun have their peak output at optical frequencies $\sim 4 \times 10^{11}$ W/Hz, the power requirements for a detectable transmitter at those frequencies are more severe. In a long integration, the signal need only be detectable above the stellar fluctuations (rather than the total power), and Rather (1988) has argued that the transmitter power requirement is manageable, for sufficiently narrow lines, broadcast in the depths of the Fraunhofer absorption bands of the stellar spectrum.

In both frequency regimes the best signal-to-noise ratio (SNR) for narrowband signals is obtained with matched filters. The ideal frequency width of the filter (or spectrometer channel) is just adequate to contain all the power from the narrowband (drifting?) signal, while excluding additional background noise from frequencies containing no signal. Resolving powers for microwave searches are $R = \lambda/\Delta\lambda \sim 10^9$ to 10^{11}, with spectrometers having $\sim 10^8$ frequency channels, (see searches listed in the Appendix and the web sites listed in Table 2). Until the number of instantaneous channels becomes comparable to the resolving power, some sort of frequency scanning scheme, or a magic frequency strategy, is needed. Fortunately Moore's Law–type improvements in signal processors will make systematic searches through the frequency domain for narrowband signals increasingly affordable. Presently, the only optical search for narrowband signals is being carried out through reexamination of previously recorded spectra from radial velocity searches for extrasolar planets (G.W. Marcy, personal communication), with a resolving power $R \sim 5 \times 10^4$.

The other way to generate an obviously artificial signal is to make $B\tau = 1$ pulses. Such pulses can be narrowband, of long duration or short, broadband flashes. Narrowband pulses are intrinsically harder to find, requiring simultaneous searches for the unknown frequency, drift, pulse width, and repetition rate. At optical frequencies narrowband pulses are relatively energetically unfavorable (limited integration time means the pulse must outshine the total stellar output, not just the averaged fluctuations). Sullivan (1991) cataloged a number of interesting pulse durations and periods that seem to be avoided by natural sources but was unable to discover an obvious rationale (or any "magic") that would significantly reduce the multidimensional search space at microwave frequencies. There is an obvious choice at optical frequencies. A solar-type star at a distance of 300 pc puts 4×10^5 photons per second per square meter of collecting area into a broadband optical filter. For observing times $\ll 1$ microsecond the arrival of a single photon is unlikely from a distant stellar target being observed with a modest telescope. The arrival of more than one photon, coming from the star, within the same submicrosecond time window, is exponentially suppressed by Poisson statistics; therefore, such an event would be due arguably to an artificial laser beacon. At optical frequencies very short (nanosecond to microsecond) broadband pulses remove the receiver's problem of searching through frequency space and they circumvent the energetic costs of outshining the parent star (Ross 1965). The peak power required for the transmitter is still formidable, but nanosecond pulses with a low duty cycle make the energy requirements modest. A petawatt laser producing nanosecond pulses with a repetition rate of one per second has the same energy requirement as a 17-kilowatt continuous microwave transmitter (neglecting what are probably considerable differences in efficiency losses for generation). The most reasonable candidates for artificial $B\tau \approx 1$ signals seem to be narrowband continuous wave (CW) signals or long duration pulses at microwave frequencies and broadband, extremely short optical pulses.

QUASI-ASTROPHYSICAL SIGNALS An advanced technology trying to attract the attention of an emerging technology, such as we are, might do so by producing signals that will be detected within the course of normal astronomical explorations of the cosmos. Sooner or later the emerging technology will build the proper instruments to observe their surroundings and capture the signal. It may be some time before differences between the deliberate, quasi-astrophysical beacon and natural emissions are recognized, but the transmitting technology can be confident that detection will eventually occur without any SETI-specific effort on the part of the receiver. Some suggestions for signals of this type are (*a*) pulsars whose spindown rate remains precisely zero for a very long period of time, (*b*) pulsars that periodically "glitch" between two fixed values of rotation rate, (*c*) impossible emission lines in the spectrum of an otherwise normal solar-type star (perhaps as the result of disposal of nuclear fissile waste by dumping it into the star), (*d*) microwave emission at the tritium line frequency coming from a location far away from any recent supernova (the half-life of this radioactive element is only 12.5 years), and (*e*) isolated infrared point sources with no central object [as might be the case for a civilization capturing all the light from its host star to power a habitat (Dyson 1960)]. There have been many other suggestions in the refereed and popular literature and there will undoubtedly be many more. Those mentioned here have previously merited some limited observing time and archival searches (see Appendix). If this is indeed the strategy chosen by an extraterrestrial transmitter, the appropriate search strategy is to continue observing the universe in as many ways, with as many instruments as we can conceive and implement, thereby conducting a robust observational astronomy program.

If the signals being generated are not intended for reception by a distant technology, but rather are intended for use by the generating technology, then it is more difficult to guess the probable nature of such signals. We can, and have, used our current terrestrial technology as a guide for what such leakage might consist of, but this is a rapidly changing situation. The broadcast television of today will have evolved into something entirely different within the blink of a cosmic eye. Comparisons with current technologies are more useful in terms of estimating the completeness of any given search program or arguing the plausibility of a new strategy (see The Significance of Null Results, below). At best, it seems reasonable to assert that some form of resource conservation will dictate that signals generated for local consumption will be weaker than signals intended to be detectable over interstellar distances.

SKY SURVEYS OR TARGETED SEARCHES? Having chosen a preferred frequency for the search, one must decide whether to try to look in all directions or select a discrete subset of directions for which there is some a priori expectation of a higher probability of success. These two strategies are commonly known as sky survey and targeted searches, and the targets of choice are usually, but not always, solar-type stars. There are hybrids of these two approaches that broadly survey a limited (but desirable) volume of the sky, either in area or in depth. Which strategy has the higher probability of success depends on the (currently) unknown density of extraterrestrial civilizations and the luminosity function of their transmitters. If weak transmitters significantly outnumber strong ones, then the most detectable ones will be closer. If there are enough strong, continuously broadcasting, transmitters, a sky survey is more likely to find one at a greater distance (where target lists may be unavailable), even though such surveys are generally less sensitive than their targeted counterparts. Sky surveys are also a better match for sources of signals not associated with any known class of objects. These strategic choices were understood early on. Kardashev (1964) presented a classification scheme for technologies based on their energy consumption and thus the amount of power they might have available for transmission. Type I civilizations are postulated to consume $\sim 4 \times 10^{12}$ Watts (Earth-like), Type II harness all the energy of their parent star and consume $\sim 4 \times 10^{26}$ Watts, and Type III can control the energy output of their galaxy and consume $\sim 4 \times 10^{37}$ Watts. Sky surveys of limited power would suffice to find anything like a Type III beacon, but continuous sky coverage would be desirable if they are transient events. In analogy with astrophysical sources it has been suggested that the luminosity function of distant transmitters could be represented as a power law such that the space density of detectable extraterrestrial transmitters with transmitter power in the range P to P + dP is $\rho(\mathrm{P}) = \kappa\,\mathrm{P}^{-\alpha}$ (Drake 1973). For $\alpha < 5/2$, the number of distant, detectable, powerful transmitters will exceed the number of weaker local transmitters and a sky survey would be the favored strategy. We have no knowledge of α, or whether it even exists. Because engineered transmitters are not the result of a random, naturally occurring astrophysical process, it is likely that one or more design criteria will determine the output power they achieve. In this case a power law may be a very poor representation; a delta function could conceivably be more representative (J.W. Dreher, personal communication). If advanced technologies decided to deliver the same power flux density to every target they illuminate, then once an emerging technology is capable of detecting the closest such transmitter, it will be able to detect all such. Success will then depend on the sensitivity of our search and the probability that we are included in the list of targets to be illuminated by the distant technologies. That probability might well be distance dependent and therefore indirectly dependent on the transmitter power.

Instead of assuming a power law distribution for distant transmitters, Dreher & Cullers (1997) have taken a different approach to comparing targeted search and sky survey strategies by explicitly including the (unknown) transmitter power. Their SETI Figure of Merit is defined below and used to discuss the various SETI searches on telescopes today. Given the uncertainties about the nature of extraterrestrial transmitter powers, prudence would suggest that both targeted searches and sky surveys be pursued as complementary strategies at all frequencies. Hybrid search strategies are an attempt to reap the benefits of both sky surveys and targeted searches. If stars are the desired targets, the maximum number can be observed in the least time by limiting a sky survey to the galactic plane (Sullivan & Mighell 1984) or by observing distant galaxies that fit within one beam of the telescope (Sagan & Drake 1975). A targeted search of nearby stars, whose targets are chosen to lie in front of the galactic plane, also benefits from having distant, background stars falling within the observing beam.

TEMPORAL CLUES There may also literally be a timeliness in SETI. Because the only detectable signal is one that happens to be "on" when we look in the proper direction, it has been suggested (Makovetskii 1980, Lemarchand 1994) that any transmitting civilization will use cosmically brief and rare phenomena to synchronize its transmissions. The appropriate time for us to observe any specific target can be computed by using an ellipse passing through that target (with its assumed transmitter) and having the earth and the cosmic transient event as its two focii.

Figure 2 represents the sort of decision tree involved in selection of a SETI search strategy. The shaded blocks represent the choices for a microwave targeted search

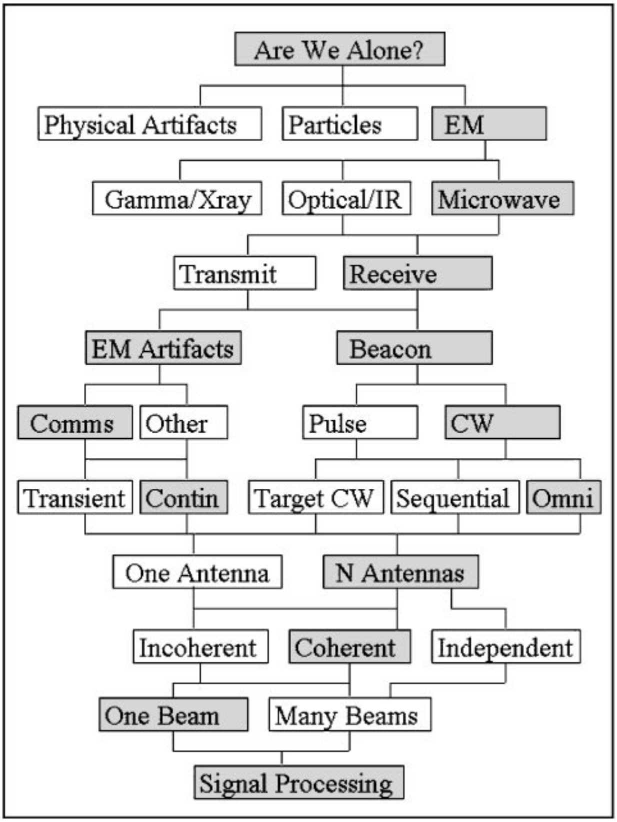

Figure 2 Strategic decision tree for SETI. This chart represents all possible search strategies discussed in the text. At each level it is possible to select one or more modalities for searching. Those highlighted correspond the search strategies recommended by the *Cyclops Report* in the early 1970s.

with an array of telescopes producing a single synthesized beam, as recommended by the *Cyclops Report* (Oliver & Billingham 1996).

All the strategies discussed in this section, and more, have been tried over the past 40 years. The lessons of those decades and the 99 searches in the Appendix is not how much has been done, but how vast the exploration may be, and how much is left to be done. We have not even scratched the surface yet.

A Brief History of SETI

Prior to the second decade of the twentieth century, when humanity began to grasp the spatial scale of our galaxy and its host universe, as well as the evolutionary timescales they implied (Hubble 1923), attempts to detect, or plans for signaling to, extraterrestrials presupposed a more or less coeval development for them and us. Historical schemes often involved humans bragging about such things as our mastery of geometry (Drake & Sobel 1992)! Percival Lowell's fixation on Schiaparelli's "canals" on Mars led to the construction of an excellent observatory in Flagstaff and ultimately to the detection of the planet Pluto (Tombaugh 1930). However, his over-interpretation of noisy data and excessive popularization of the resulting model of a dying Martian society had the effect of tarnishing the field for several decades. Journalists and the public remained enthusiastic about life on Mars and elsewhere, but astronomers tended to avoid the topic.

Modern SETI had to await the birth of the discipline of radio astronomy (Jansky 1933, Reber 1944), a significant technological boost from the radar studies of World War II, and the discovery of the 21-cm emission line (Ewen & Purcell 1951). Cocconi & Morrison (1959) ushered in the scientific era of radio SETI by recognizing that our own technology had advanced to the point that a signal could be transmitted at the "universal" frequency of HI (the most abundant element in the universe) and that signal would be detectable over interstellar distances against the relatively low natural background of the astrophysical universe. Compared with the rest of the electromagnetic spectrum, the microwave region is very quiet. Figure 3 represents the average background noise radiation from astrophysical sources across the observable spectrum (ML Lampton, private communication). Although very broad, the observable spectrum is finite. It is truncated at the low frequency end by the plasma frequency for the interstellar medium (ISM) (100 kHz), and at the high frequency end by pair production against the 3K background photons (10^{29} Hz) (Harwitt 1981). Figure 3 correctly portrays the background emission in W/m^2/strd, as long as there is no spatial, spectral, or temporal filtering applied. This last caveat is an important one, as was pointed out in the previous discussion of optical SETI pulses.

Drake (1961) independently recognized the appeal of the microwave region and conducted the first SETI radio search of two nearby solar analogs, Tau Ceti and Epsilon Eridani, using a receiver that would later be used to study natural HI. This first search had four characteristics that were to become hallmarks of many of the searches appearing in the Appendix: target stars like our own sun were selected,

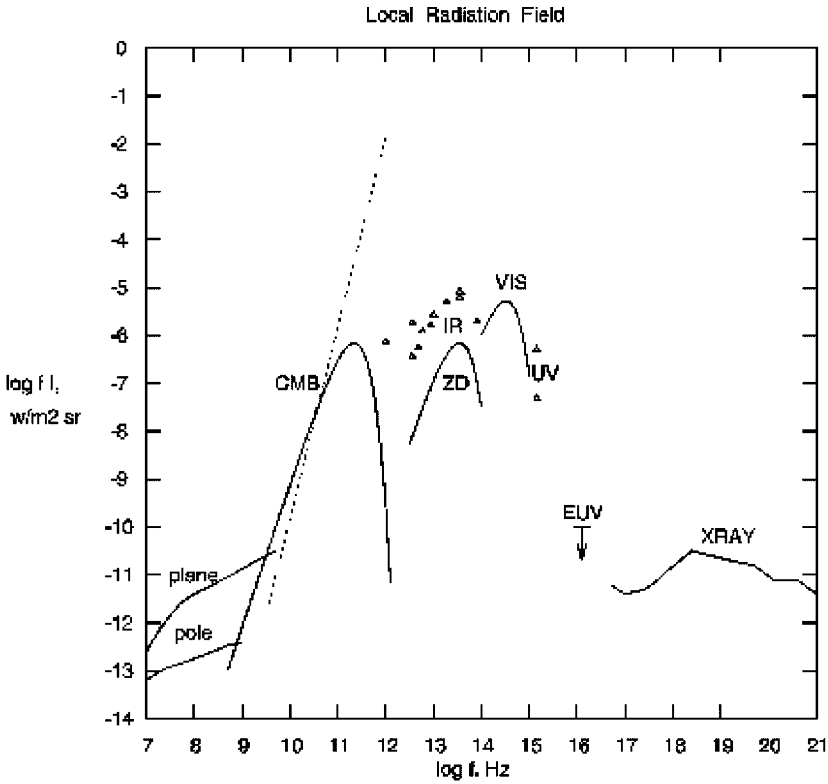

Figure 3 Electromagnetic background radiation in free space. The Power flux density (in Watts/m^2/steradian) resulting from natural astrophysical background radiation is plotted as a function of frequency. Latitude-dependent contributions are indicated from Galactic synchrotron emission (plane and pole); other sources are more uniform over the sky and include the 3K cosmic microwave background (CMB), IR Cirrus (IR), zodiaical dust (ZD), integrated visible starlight (VIS), limits to the ultraviolet (UV) and extreme ultraviolet (EUV) backgrounds, and the X-ray background (XRAY).

the receiver was optimized to detect very narrowband signals, the observations encountered radio frequency interference (RFI) from terrestrial technologies, and only a limited amount of observing time could be allocated to the search project. The Cocconi & Morrison paper (1959) and Drake's Project Ozma (1961) began to reverse the negative legacy of Lowell. The US National Academy of Sciences took note and convened a meeting at Green Bank, WV (site of Project Ozma) in 1961. There the agenda-turned-equation was discussed by 10 scientists, and rather optimistic values were estimated for the various terms. Although a few additional SETI observations were conducted at Green Bank over the next decade, the observational balance shifted abruptly to the Former Soviet Union. In 1965 overly enthusiastic Soviet observers, who shared their speculations with *Tass* reporters, eventually caused the editor of the *New York Times* to report the detection of a radio source thought to be due to extraterrestrial intelligence (*New York Times* 1965). In fact, the Soviet astronomers had detected synchrotron self-absorption modifying the spectrum of the quasar CTA 102, thereby producing an emission profile that resembled what Kardashev had postulated for an advanced technological civilization (Kardashev 1964).

Although little telescope time was being devoted to SETI in the United States, it became the subject of a number of scholarly and popular books such as AGW Cameron's *Interstellar Communication* (1963), Walter Sullivan's *We Are Not Alone* (1964), and Shklovskii & Sagan's *Intelligent Life in the Universe* (1966). In 1971 John Billingham convinced Bernard Oliver (then Director of Hewlett-Packard Research Labs) to head up a NASA Ames–Stanford University–sponsored engineering design summer study on the optimal technology for detecting extraterrestrial intelligence. Oliver then edited the remarkable *Cyclops Report* detailing the results of that summer study and providing a superb tutorial on antenna arrays and signal processing theory. That same year, the Academies of Science in the United States and the USSR jointly sponsored a meeting in Byurakan, Armenia (Sagan 1973), a decade after the Green Bank meeting. This time there were over 50 participants, and their assessments of probabilities were somewhat less optimistic. Such joint meetings became a decadal tradition that was repeated in 1980 in Tolinn, Estonia, and in 1991 in Santa Cruz, CA (just a few weeks prior to the breakup of the former Soviet Union). 1971 was also the year in which the first US decadal self-assessment, the Greenstein Report on *Astronomy and Astrophysics for the 1970s* was written (Greenstein 1972), containing a recommendation that SETI be pursued on a modest level. On the international stage, the first annual review session on SETI was held under the auspices of the International Academy of Astronautics SETI Committee in Vienna in 1972 (the 29th such review was held in Rio de Janeiro in 2000). Targeted-star SETI observing started again at Green Bank (Verschuur in 1971, Palmer & Zuckerman 1972), and the Big Ear radio telescope at Ohio State University Radio Observatory began the first dedicated SETI sky survey in 1973 (continuing through the efforts of volunteer observers until 1997 when the university sold the land for development of a golf course). SERENDIP I, the first commensal SETI sky survey started in 1976, piggybacking at UC Berkeley's Hat Creek Observatory. By 1974 SETI was sufficiently credible to permit John Billingham to establish the Interstellar Communication Study Group at NASA Ames Research Center.

Funding for SETI was marginal and often borrowed from related activities; nevertheless, NASA-sponsored workshops commenced in 1975 with the aim of figuring out how to implement the ideas in the *Cyclops Report*. The combination of SP-419 (the report from these workshops) (Morrison et al. 1977) and the *Cyclops Report* proved to be powerful recruiting tools, and young scientists began to embark on careers in SETI, even though the road was a bit rocky. Early in 1978 Senator Proxmire gave the NASA SETI program his Golden Fleece Award, saying it was wasting taxpayers' money, and he followed that up in 1981 by eliminating NASA's fiscal year 1982 funding for SETI. Publication of the Field Report on *Astronomy and Astrophysics for the 1980s* (Field 1982) reaffirmed the scientific credibility of SETI [as have the two subsequent decadal reviews (Bahcall 1991, Taylor & McKee 2000)] and funds were reinstated in fiscal year 1983, but a pattern of Congressional interference had begun.

At the same time, public interest was growing. In 1980 The Planetary Society was founded by Carl Sagan and Bruce Murray with SETI as a primary focus, and it became an extremely successful and fast-growing membership organization. Optical SETI observing programs started in the Former Soviet Union in 1978, and France began an observing program at Nancay Observatory in 1981. The International Astronomical Union created Commission 51: Bioastronomy in 1982 to coordinate activities across a range of related astronomical disciplines including searches for extrasolar planets and interstellar molecules relevant to life and studies of the solar system and the primitive earth, as well as SETI. Interdisciplinary meetings have been organized triennially by this group since 1984. NASA published the results of its SETI Science Working Group (Drake et al. 1983), recommending that a combined program of sky surveys and targeted searching be commenced using existing radio telescopes belonging to the scientific community and to NASA's Deep Space Network (DSN). Each telescope was to be equipped with modern digital signal processing equipment specifically developed for SETI. The SETI Institute was incorporated as a California not-for-profit organization in 1984 to stretch the limited NASA research and development funds available for SETI and to provide an institutional home for the increasing number of "soft-money" scientists and engineers becoming involved in the SETI efforts and NASA's exobiology program.

The recommendations of the SETI Science Working Group evolved through many iterations of agency planning until they resulted in the recommendation of a "new start" for the SETI Microwave Observing Project [later the High Resolution Microwave Survey (HRMS)] within the NASA Life Sciences Division in the fiscal year 1989 budget. As planned, the Microwave Observing Project would have built special purpose signal processing equipment and taken it to large radio telescopes around the world (Arecibo, Parkes, Green Bank, Nancay) in order to conduct a targeted search of 1000 nearby stars over the frequency range 1–3 GHz. At the same time, similar signal processing equipment would have been built to move between the NASA DSN 34-m antennas at Goldstone, Tidbinbilla, and Madrid. This equipment was to have operated 16 hours a day to complete an all-sky survey over the frequency range 1–10 GHz. On Columbus Day, October 12, 1992 the HRMS successfully launched observations at Arecibo Observatory and Goldstone, CA using engineering prototype detectors. Dick (1993) published a scholarly history of the development of SETI, particularly NASA's SETI program, to celebrate the start of systematic observations. During the next Congressional budget cycle, Senator Bryan succeeded in convincing his colleagues to remove funding for HRMS from NASA's fiscal year 1993 budget. The letter terminating the project was received on October 12, 1993. Thus the era of exclusively private financing for SETI commenced.

While the NASA project had been evolving, The Planetary Society helped fund sky surveys by groups at Harvard (META), UC Berkeley (SERENDIP II and III), and the Institute for Radio Astronomy in Argentina (META II). During this period plans for dealing with the successful detection of a signal began to take shape within the International Academy of Astronautics SETI Committee, and the SETI Institute began a series of workshops to assess the Cultural Aspects of SETI (see below).

In 1994 the SETI Institute entered into a bailment agreement with NASA Ames that permitted the Institute to use the NASA-developed targeted search equipment on a long-term loan. Another agreement transferred wideband feeds and receivers developed at Jet Propulsion Laboratory (JPL) to the Arecibo Observatory for use by the radio astronomy community. Armed with permission to keep using and improving the NASA-developed targeted search equipment, and with assurances that observing time on major telescopes, already secured by the NASA HRMS, would be available for use by a replacement project, the SETI Institute launched Project Phoenix and aggressively sought major philanthropic funding to complete the HRMS targeted search. Because of the loss of DSN telescope resources, the HRMS sky survey could not be continued, so the SETI Institute became a major sponsor of the SERENDIP projects, now in version IV and running at Arecibo. The Planetary Society funded major upgrades at Harvard and at the Argentinian Institute for Radio Astronomy. META became BETA, using increased processing power to remove the simplifying hypothesis that signals being broadcast to Earth had been Doppler corrected to a guessable frame of reference (Leigh & Horowitz 2000). In so doing, a natural discriminant against interference was removed and clever new techniques were substituted (see below). The Argentinian (META II) sky survey signal processing systems were enhanced to permit longer integration times, wider channel widths, and searches for signals amplified by transmission through OH masers (Lemarchand 2000). The Phoenix project introduced a new technique to SETI observations, the use of two widely separated telescopes linked together as a pseudo-interferometer (Cullers & Stauduhar 1997) to help discriminate against human-generated RFI. The first use of this technique was a six-month deployment of Project Phoenix to Australia to study southern stars with the 64-m Parkes Telescope coupled to the 22-m Mopra dish (Tarter 1997). Subsequently, the equipment was used on the NRAO 140-ft telescope in Green Bank, WV linked to a resurrected 30-m antenna in Woodbury, GA (DeBoer & Steffes 1998), and is now observing twice a year at Arecibo Observatory in conjunction with the Lovell Telescope at Jodrell Bank Observatory.

Having operated under the basic ideas for microwave searching laid out in the *Cyclops Report* for nearly 30 years, in 1997 the SETI Institute took the initiative to sponsor another series of workshops to determine a roadmap for observational SETI for the next two decades. The recommendations of those workshops were so compelling they began to be implemented while the workshops were still underway! Optical searches for nanosecond pulses began at Berkeley and Harvard to bolster the pioneering work of Stuart Kingsley, who inaugurated the Columbus OSETI Observatory in his backyard in 1995.

The time had definitely come for other backyard SETI. The nonprofit SETI League incorporated and launched Project Argus in 1996 to coordinate the efforts of radio amateurs in an attempt to listen to the entire sky at all times (requiring about 5000 small antennas) for strong transient signals. They now have 105 stations operating in 19 countries. The public has also become involved in SETI data reduction via the UC Berkeley screensaver (SETI@home). This is by far the most successful distributed computing project ever undertaken. More than 2.9 million people in more than 226 countries have downloaded this screensaver!

While all this was happening in the United States, SETI Australia began its Southern SERENDIP project in 1998, piggybacking on the multi-beam feed of the Parkes Telescope, and SETI Italia began operations on the 32-m Medicina telescope as another commensal sky survey. Today there is more energy, in more countries, devoted to SETI projects, and less federal funding than at any time since the mid 1970s. At least in the United States, the specific challenge has now become the establishment of an endowment fund to capture this energy and turn it into a long-term, systematic scientific exploration. Fortunately, astronomy has an established tradition of patronage, stretching back to the royalty of Europe. In this country visionaries such as Carnegie, Rockefeller, and Keck have endowed some of our finest scientific establishments, and it may be possible for SETI to continue in that tradition.

SETI Now

The online Appendix is a compilation of 99 SETI observing projects that have been mentioned in the literature. There are undoubtedly other projects whose negative results have never been written up. What appears in Table 2 is a subset of those searches that are ongoing today. Although not all of them have yet established regular observing projects, all of them have active web sites, and their URLs are provided for convenience (literature citations appear in the Appendix).

The 14 searches listed in Table 2 should serve to demonstrate the current interest and involvement in SETI around the world. However, the searches should not be considered interchangeable or equivalent; they test different hypotheses, with differing degrees of thoroughness. Four of the searches (Columbus OSETI, BAMBI, SETI Italia, and Oz OSETI) have not yet established regular observing programs, so it is impossible to compare the merits of these searches with the others listed. To compute the merit of the remaining 10 searches, we use the figure of merit devised by Dreher & Cullers (1997). They argued that stars are the important targets for both sky surveys and targeted searches. If the unknown transmitter power P_T Watts EIRP (average power for CW searches and peak power for pulsed searches) is used as an explicit variable, then given the sensitivity threshold and the protocol of a particular search, it is possible to calculate $N_{stars}(P_T)$, the number of stars that would be explored during the course of the search (including stars that show up in the background of individual antenna beams pointed at targeted stars, for large enough values of transmitter power). The other important factors for search merit are the fraction of the available bandwidth that is covered and N_{looks}, the number of times each star is interrogated. Dreher & Cullers SETI Figure of Merit is then given by:

$$\text{SETI Figure of Merit } (P_T) = N_{stars}(P_T) \times \ln(F_{hi}/F_{lo}) \times \eta_{pol} \times N_{looks},$$

where F_{hi} and F_{lo} are the upper and lower frequency ranges of the search, and η_{pol} is 1 for observations using dual circular polarizations, a singular linear polarization (note that the corresponding sensitivity limit will be halved), and for incoherent optical observations, but it is set to 0.5 for observations using a singular circular polarization. Figure 4 shows how the various SETI programs on telescopes compare with one another. For the very large range of powers plotted, all the microwave surveys eventually explore all the stars in the fraction of the Milky Way Galaxy that they interrogate. At the large values of (peak) power that are included for the optical searches, some of the microwave searches would become sensitive to sources in external galaxies. Figure 4 shows only the merit for stars within the galaxy, because the detailed observational history of each program would have to be known to include the merit of the extra galactic sources. The actual stellar density of the 100 nearest stars and an averaged stellar density for more distant stars in the disk have been used to estimate N_{stars}(PT). The merits displayed in Figure 4 refer to the cumulative observing programs to date, with the exception of Project Argus. The curve labeled Argus-5000 represents the theoretical potential for this project when it has all 5000 stations on the air all the time.

Clearly, the Phoenix project is superior for detecting lower-power microwave transmitters. All the sky surveys are superior for detecting much higher power transmitters. Surveys with larger bandwith coverage and larger fractions of the sky observed are judged the most meritorious. The UC Berkeley archival search for optical CW signals takes advantage of integration time to exceed the fluctuations in the stellar spectrum and thus can detect lower (average) power transmitters compared to the (peak) power pulses of the optical targeted searches. Depending on the actual duty cycle of any fast optical pulsed signal, their actual average power could be many orders of magnitude less. The number of target stars in the current optical searches is limited by dust obscuration beyond about 1000 light years.

The inclusion of optical SETI searches and coordinated “amateur” efforts in Table 2 and Figure 4 represent innovations that would not have been seen in an article written just 5 years ago. The microwave projects in Table 2 have also implemented a number of other innovations in the past few years to deal with worsening RFI. The BETA group improved on the dual-beam OSURO scheme to require that, as the sky rotates overhead, a source appears first in the output of BETA’s east-facing beam and then in the west-facing beam output, but never in the output from an omni-directional antenna located nearby. The quarter-billion spectrometer channels of the BETA system are split among these three beams in order to reject RFI more efficiently. For a while META II in Argentina and

TABLE 2 List of active SETI projects on telescopes today (with URLs)

Date	**1990–**
Observer(s)	**Lemarchand "META II"**
Site	**Instituto Argentino de Radioastronomia (IAR)**
Instrument size (meters)	30 (one of two)
Search frequency (MHz)	1420.4, 1667, 3300
Frequency resolution (Hz)	0.05
Objects	Sky survey of southern skies and 90 target stars, OH masers
Flux limits (W/m^2)	1×10^{-23}–7×10^{-25}
Total hours	Ongoing
Reference	http://www.planetary.org/html//UPDATES/seti/META2/default.html
Comments	Search for signals that have been Doppler compensated to rest frame of SS barycenter, Galactic Center, or CMB. A duplicate of META system built by Argentinian engineers under the guidance of Prof. Horowitz at Harvard and financed by the Planetary Society. Simultaneous observations with META over declination range −10° to −30.° Major upgrades in 1996 to permit long integration times and switching between antennas. Search through OH masers looking for amplified signals
Date	**1995–**
Observer(s)	**Kingsley**
Site	**Columbus Optical SETI Observatory, Ohio**
Instrument size (meters)	0.1
Search frequency (MHz)	0.55 microns
Frequency resolution (Hz)	none
Objects	Nearby solar-type stars
Flux limits (W/m^2)	Transmitters with peak instantaneous power $>10^{18}$ W
Total hours	Ongoing
Reference	http://www.coseti.org
Comments	Broadband optical search for short pulses (~1 ns) that instantaneously outshine the host star. No formal program of observation is currently being conducted, owing to equipment damage
Date	**1995–**
Observer(s)	**Horowitz et al. (BETA)**
Site	**Oak Ridge Observatory**
Instrument size (meters)	26
Search frequency (MHz)	1400–1720
Frequency resolution (Hz)	0.5
Objects	Sky Survey from −30° to +60° declination
Flux limits (W/m^2)	2.2×10^{-22}
Total hours	Temporarily suspended in spring 1999
Reference	http://mc.harvard.edu/seti/beta.html
Comments	Waterhole search, using dual-beams and omni antenna to discriminate against radio frequency interference. Project

(Continued)

TABLE 2 (*Continued*)

	BETA is follow-up to META. Project interrupted when wind blew antenna off its mount. Repairs under way
Date	**1996–**
Observer(s)	**SETI League Project Argus**
Site	**Multiple sites world wide (currently ~100)**
Instrument size (meters)	~3–10 (satellite TV dishes)
Search frequency (MHz)	1420–1720
Frequency resolution (Hz)	1
Objects	All sky
Flux limits (W/m^2)	$\sim 1 \times 10^{-21}$ (varies)
Total hours	Ongoing
Reference	http://www.setileague.org
Comments	Plan to organize up to 5000 radio amateurs to provide continuous sky coverage for strong, transient signals using systems that can be bought and built by individuals. SETI League currently has 1243 members running 105 sites
Date	**1996–**
Observer(s)	**Werthimer et al. (SERENDIP IV)**
Site	**Arecibo**
Instrument size (meters)	305
Search frequency (MHz)	1420 +/−50
Frequency resolution (Hz)	0.6
Objects	Survey of 30% of sky visible from Arecibo
Flux limits (W/m^2)	5×10^{-24}
Total hours	Ongoing
Reference	http://seti.ssl.berkeley.edu/serendip/serendip.html
Comments	Commensal search occurring at twice sidereal rate in backwards direction while radio astronomers track targets using Gregorian system. Covers sky every 3 years. Looks for signals recurring at same frequency and location on rescan
Date	**1997– (A), 1999– (B)**
Observer(s)	**BAMBI (Bob And Mike's Big Investment), SARA (Society of Amateur Radio Astronomers) members**
Site	**A in California, B in Colorado**
Instrument size (meters)	2.6 (A), 3 (B)
Search frequency (MHz)	3700–4200
Frequency resolution (Hz)	0.6
Objects	Northern sky survey
Flux limits (W/m^2)	No formal observing program has yet begun
Total hours	Ongoing
Reference	http://www.bambi.net/sara/bambi.htm
Comments	Amateur radio enthusiasts using TVRO components and software FFTs to try coordinated search
Date	**1998–**
Observer(s)	**SETI Institute Project Phoenix**

TABLE 2 (*Continued*)

Site	**Arecibo Observatory and Lovell Telescope at Jodrell Bank**
Instrument size (meters)	305 and 76
Search frequency (MHz)	1200 to 3000, dual circular polarization
Frequency resolution (Hz)	1
Objects	600 nearby stars
Flux limits (W/m^2)	1×10^{-26}
Total hours	1300 h to date
Reference	http://www.seti.org
Comments	Continuation of NASA High Resolution Microwave Survey targeted search of 1000 nearby stars, using real-time data reduction and a pair of widely separated observatories to help discriminate against radio frequency interference
Date	**1998–**
Observer(s)	**SETI Australia Southern SERENDIP**
Site	**Parkes**
Instrument size (meters)	64
Search frequency (MHz)	1420.405 +/– 8.82
Frequency resolution (Hz)	0.6
Objects	Southern sky survey
Flux limits (W/m^2)	4×10^{-24}
Total hours	Ongoing
Reference	http://seti.uws.edu.au
Comments	Commensal search that uses 2 out of 13 beams of Parkes focal plane array to discriminate against radio frequency interference
Date	**1998–**
Observer(s)	**Werthimer, Berkeley Optical SETI**
Site	**Leuschner Observatory**
Instrument size (meters)	0.8
Search frequency (MHz)	300–650 nm
Frequency resolution (Hz)	None
Objects	800 solar-type stars
Flux limits (W/m^2)	1.5×10^{-9} peak during 1 ns pulse or 1.5×10^{-20} average per 100 second observation
Total hours	200 (ongoing)
Reference	http://sag-www.ssl.berkeley.edu/opticalseti
Comments	First optical search to use two high-time resolution photodetectors in coincidence to look for nanosecond pulses.
Date	**1998–**
Observer(s)	**Horowitz et al, Harvard Optical SETI**
Site	**Oak Ridge Observatory**
Instrument size (meters)	1.5
Search frequency (MHz)	350–700 nm
Frequency resolution (Hz)	None

(*Continued*)

TABLE 2 (*Continued*)

Objects	13,000 solar-type stars (4000 done to date)
Flux limits (W/m^2)	4×10^{-9} peak in <5 ns or 4×10^{-20} average per 500 second observation
Total hours	Ongoing
Reference	http://mc.harvard.edu/oseti/index.html
Comments	Search for nanosecond laser pulses, with hybrid avalanche photodiodes in coincidence. Piggybacks on nightly searches for extrasolar planets
Date	**1998–2000**
Observer(s)	**Marcy, Reines, Butler, Vogt**
Site	**Lick, Keck**
Instrument size (meters)	3 and 10
Search frequency (MHz)	400–500 nm
Frequency resolution (Hz)	Resolving power = 50,000
Objects	600 FGK stars within 100 pc
Flux limits (W/m^2)	1×10^{-13}
Total hours	500
Reference	http://albert.ssl.berkeley.edu/opticalseti
Comments	Search through archival data for narrowband continuous optical laser emission lines
Date	**1999–**
Observer(s)	**Werthimer and Anderson (SETI@home)**
Site	**Arecibo**
Instrument size (meters)	305
Search frequency (MHz)	1420.405 +/−1.25 MHz
Frequency resolution (Hz)	0.6 Hz
Objects	Data taken from SERENDIP IV (sky visible from Arecibo)
Flux limits (W/m^2)	5×10^{-25}
Total hours	Ongoing
Reference	http://setiathome.ssl.berkeley.edu
Comments	Hugely successful experiment in distributed computing. Permits more sophisticated processing of a fraction of SERENDIP IV data by harnessing idle CPU cycles in millions of personal and corporate computers
Date	**2000–**
Observer(s)	**Montebugnoli (SETI Italia)**
Site	**Medicina**
Instrument size (meters)	32
Search frequency (MHz)	1415–1425 and 4255–4265
Frequency resolution (Hz)	0.6
Objects	Northern sky
Flux limits (W/m^2)	No routine observing program established yet
Total hours	Ongoing
Reference	http://www-radiotelescopio.bo.cnr.it/setiweb/home.htm
Comments	Commensal sky survey using Medicina telescope and SERENDIP signal processing boards

TABLE 2 *(Continued)*

Date	**2000–**
Observer(s)	**Bhathal and Darcy**
Site	**Campbelltown Rotary Observatory, Oz OSETI**
Instrument size (meters)	0.4 and 0.3
Search frequency (MHz)	550 nm
Frequency resolution (Hz)	None
Objects	200 solar type stars
Flux limits (W/m^2)	No routine observing program established yet
Total hours	Ongoing
Reference	http://www.coseti.org/ragbir00.htm
Comments	Dedicated telecopes built for SETI. Uses high time resoution photodiodes in coincidence to search for laser pulses, soon to use two telescopes in coincidence and to be teamed with microwave search of same objects

the original META search in Massachusetts spent two days each week jointly surveying the same piece of sky looking for coincident signals to filter out RFI. The Southern SERENDIP project attempts to reject RFI signals entering the telescope sidelobes by rejecting signals appearing in both of their beams simultaneously. This has proven less effective than originally expected (Stootman et al. 2000) and is currently being studied. The Phoenix Project requires that a signal be detected by two widely separated antennas, and further that the detected signals exhibit a predictable differential Doppler signature calculated for each target star and pair of observatories. Nothing is known about the nature of a real extraterrestrial signal, but Project Phoenix assumes that it will arrive from a point source moving at sidereal rate on the sky. This pseudo-interferometer is an extremely efficient filter that can work well even with a pair of antennas having as much as a 10 dB difference in sensitivity. Furthermore, the Phoenix project is unique in being able to identify candidate signals in near real-time and immediately follow up on these detections.

Since the beginning of Phoenix observations in February 1995 through the March 2001 observing run, the Phoenix project has identified 1,074,402 candidate signals, of which only 685 were detected by the pseudo-interferometer with the proper differential Doppler signature. In all 685 cases the automated observing sequence was interrupted to allow off-source observations and other verification strategies to be performed. To date, no candidate signals have survived the immediate verification procedures (most often, the signal persists when the telescope is pointed off source). Thus, the Phoenix project can uniquely claim that there are no outstanding (or unconfirmed) candidate signals resulting from its observations to date. The insistence on sidereal motion is likely to eliminate any fast moving signal arising within the solar system (extraterrestrial intelligences' as well as our own satellites), but the improvement in search efficiency arguably justifies this restriction. As spectrum usage increases, more aggressive discrimination techniques will become necessary. Although interference is not an issue for optical pulse searches, instrumental background noise is necessitating new discrimination schemes at optical frequencies as well.

SETI In the Near Future

Each of the projects listed in Table 2 are actively engaged in improving their searches, by enhancing their signal detection equipment or by adopting a more complex observational strategy to discriminate against interference and instrumental backgrounds. The Harvard optical SETI project will soon begin operation in tandom with the Fitz Ruldolph 36-inch telescope on the Princeton campus (similarly instrumented with a pair of hybrid avalanche photodiodes). They will use Global Positioning Satellite (GPS) receivers to synchronize the detection of pulses to within a microsecond over the millisecond of delay between the two sites (Howard et al. 2000). In addition, the Harvard group has broken ground for a dedicated 1.8-m SETI optical sky survey telescope utilizing a pair of 8×64 multi-pixel photodiodes and cleverly folded optics for compact construction and simplifying output coincidence detection. A survey of the northern hemisphere should take about 1200 hours ($\sim$150 clear nights). OSETI is also moving onto more telescopes. The Lick Observatory will soon begin searches for short optical pulses using available time on the 1-m Nickel Reflector and the 0.9-m Crossley reflector. The system will feature three photodiodes in coincidence. Unlike the Harvard OSETI-targeted project that captures 30% of the photons collected by a nightly search for extrasolar planets, the Lick project can expect to have dedicated use of this new OSETI instrument on one of the telescopes about 30% of the time (F.D. Drake, personal communication).

All the targeted optical searches could benefit from larger collecting areas, without requiring good imaging capabilities. Therefore, preliminary discussions are underway with the builders of the Whipple high-energy Gamma-ray telescope to determine whether a commensal OSETI project could make use of this very large light-bucket and focal-plane photodiode array while it looks for Cherenkov radiation. The trick will be to capture the single photodiode events from the focal plane array that are discarded in the current search for extended air showers. The event rate could eventually be reduced to a manageable level by demanding coincidence from multiple telescopes in the proposed VERITAS array.

The SETI League will construct Array2k (an array of 16 3.6-meter dishes) on private land in New Jersey. This will add a large observing station to their network. For the northern hemisphere, the array can also serve as a verification system for signals reported by other members of Project Argus. SETI@home will increase the fraction of the Arecibo SERENDIP IV bandwidth that is recorded and subsequently distributed for deeper processing by users of the screen saver. This concept will soon be duplicated in the southern hemisphere. There, contributed computing cycles will permit additional analysis of the Southern SERENDIP data being collected from the Parkes multi-beam observing programs. Having lost the original

Big Ear telescope, Ohio State University is working with the SETI Institute to prototype an omni-directional, radio fly's eye antenna (called the Argus Project) to search for strong, transient signals. This antenna, first proposed in the 1980s (Dixon 1995), combines the outputs of many small elements to form all possible beams on the sky. Until costs of computing drop significantly the number of elements, and thus the effective collecting area of such an array, will remain small, but prototyping efforts are underway.

Project Phoenix will increase its processing bandwidth by a factor of five and build two new signal processing systems in anticipation of continuous access to a dedicated facility in 2005. The Allen Telescope Array (named after its philanthropic sponsor, Paul G. Allen; see Figure 5) is being designed and built as a joint project between the SETI Institute and the UC Berkeley Radio Astronomy Lab. It will be constructed from ~350 6-m parabolic dishes with ultra-wide simultaneous frequency coverage (0.5–11 GHz). Combining the outputs from all the dishes with different sets of time delays permits the synthesis of multiple high-resolution beams within the broad primary field of view of the small dishes. Each independent beam can be steered onto different types of targets, and the output from that beam can be scrutinized by special purpose processors, e.g., for pulsars, for SETI targets, or for astrophysical lines. At the same time, the entire primary field of view can be imaged by another processor. The beam-forming properties of the array should also permit subtraction or nulling of interference from orbiting constellations of satellites. Although it will be impossible to observe in the effected bands when a satellite is within the primary field of view of the array, tracking the satellite in a null beam or subtracting its contribution to the sidelobes should provide 30 dB of suppression and make most of the sky usable even at the worst frequencies. All these operations will require extremely fast computing that will be housed in a heavily shielded electronics laboratory that has been funded by a donation from Nathan Myhrvold. Starting with 3 beams on the sky (and increasing to 12), ~100,000 stars will be examined 3 times, from 1–10 GHz, for narrowband signals over a 10-year period. More beams or a high spectral resolution mode on the radio astronomy imager will enable a sensitive survey of the galactic plane. After 40 years of competing for telescope time, it will be possible for SETI scientists and radio astronomers to actively use the same instrument all the time.

SETI In the Foreseeable Future

Because OSETI is just beginning, with small targeted and sky survey telescopes, there has been no systematic planning for the long term. The Terrestrial Planet Finder might be able to detect signals (Howard & Horowitz 2001). Because good imaging quality is not required for OSETI, large, dedicated antennas at optical and IR wavelengths may turn out to be affordable in the not too distant future. The need for coincidence between separated sites has to be better understood, and the potential for doing IR searches 24 hours per day will be exploited as soon as the technology becomes affordable. Targets will be visible at greater distances through the galactic dust using the IR rather than the optical spectrum.

In the next few decades the costs of computing should continue to decline. Several more generations of Moore's Law improvements are reasonably forecast, and the promise of quantum computing remains robust. This should permit the construction of an Omni-directional Sky Survey telescope for SETI observations focused on temporally sparse signal events, at least at the low frequency portion of the terrestrial microwave window. An array of 1024×1024 half-wavelength dipoles would have a collecting area equivalent to an 85-m dish at 1.4 GHz and would require $\sim 10^{21}$ ops of computer power to tessellate all the sky above the horizon. If this does become affordable, the addition of a data storage buffer would permit reformation of specific beams to image prior events after an alert triggered by transient detectors at other wavelengths. This offers yet another positive synergism between SETI observations and more traditional astronomical studies.

The international community of centimeter wavelength radio astronomers wishes to construct an array having a million square meters of collecting area (the Square Kilometer Array) beginning around 2010. The principle science driver for this large collecting area is the study of the structure of the early universe just prior to the formation of galaxies, using highly red-shifted HI. Current strawman goals for this array specify a frequency range of 300 MHz to 30 GHz, a primary field of view of one square degree at 1.4 GHz and the ability to synthesize up to 100 pencil beams simultaneously. An international consortium of 10 countries and 24 institutions is currently grappling with the technical challenges of building such a facility in an affordable manner. Using 10 of the 100 available beams, a SETI targeted search could be extended to a million stars with a decade of searching, and all the beams could be combined to survey the plane of the Milky Way for intrinsically bright, but distant technologies.

The success of SETI with an Omni-Directional Sky Survey Telescope or the Square Kilometer Array will depend strongly on the ability to work around the ever-increasing RFI environment. Establishment of SETI observing facilities on the lunar farside may ultimately be required for success in microwave SETI. It seems unlikely that this could be afforded for SETI alone, but it is conceivable in conjunction with a multi disciplinary, remotely operated, scientific station sited on the farside.

Plans for Success

With more SETI observing programs being conducted, the possibility of success must be taken seriously. Plans for scrutinizing discovery data purporting to prove the existence of extraterrestrial intelligence have begun to be formulated. Even though the circumstances of the detection, and/or the information content resulting from the detection cannot be constrained in advance, some generalizations are possible. The "Declaration of Principles Concerning Activities Following the Detection of Extraterrestrial Intelligence" is a protocol that has been voluntarily adopted by most groups conducting SETI observations. It was developed by the International Academy of Astronautics and the International Institute of Space Law, following a number of symposia at international congresses (Tarter & Michaud 1990). The protocol suggests some very sensible guidelines: work hard at verifying the detection at the discovery site (being mindful of possible hoaxes), attempt to get an independent confirmation (if appropriate), use the IAU Telegram system to alert observatories around the world, tell the world, make the discovery data available for inspection, and finally, make no reply until there is some global consensus that this should be done.

Building upon that framework, the International Academy of Astronautics and the International Institute for Space Law developed a position paper on "Transmissions to Extraterrestrial Intelligence" to address the question of how a global consensus might be achieved. After deciding whether we should reply (or transmit de novo), the more difficult questions concern who will speak for Earth and what they will say. This topic, and a plan to develop a plan, were introduced during the summer 2000 session of the UN Committee on the Peaceful Uses of Outer Space, a global geo-political body that might eventually decide to take up these questions in advance of any successful detection. Even in the event that this committee does deliberate and specify a course of action, there will be no way to enforce a ban on transmissions. If any extraterrestrial technology has not already detected the cacophony of terrestrial twentieth century electromagnetic leakage, they might receive a reply that is a commingling of separate and conflicting voices, an unfortunately accurate portrayal of humanity at the beginning of this new millennium.

The SETI Institute sponsored a series of workshops in 1991–1992 that brought together humanists, sociologists, religious leaders, members of the diplomatic service, and media specialists. They met with the scientists and engineers working on SETI to consider what the cultural impact of a successful detection might be and to discuss ways to ameliorate any potential negative consequences. The workshop report (Billingham & Heyns 1999) suggested a number of studies that could be carried out by social scientists to get a more accurate picture of possible consequences and stressed the benefits of public awareness and proactive education about the possibility of detecting extraterrestrial intelligence. Plans were made to internationalize this work to better reflect the cultural diversity of humanity, but these plans were an early victim of the termination of NASA's SETI project. Thus, the only studies bearing on this global question remain those made through the lens of a single, first-world nation.

What If Everybody's Listening and Nobody's Transmitting?

Many (but not all) of the searches in Table 2 and the Appendix are predicated on the assumption that there are intentional or leakage radiation signals being generated by other technological civilizations. We ourselves do not routinely transmit intentionally, and our leakage radiation will become more noise-like over time, so why should we expect anyone else to transmit? It is necessary to remember the asymmetry that characterizes our current situation; we are a very young technology in a very old galaxy. Because transmitting is harder than receiving (in terms of both cost and cultural commitment), and must be a long-term effort to have any chance of success, it is reasonable to place the burden of transmission onto the more advanced technologies. Emerging technologies such as our own should listen first. As our technology matures and our civilization stabilizes (failure to do so makes the question moot), we may add a transmitting strategy to our listening, but only when we are capable of conceiving and implementing programs with lifetimes of order tens or hundreds of thousands of years. The Long Now Foundation is currently attempting to design a clock that will work for 10,000 years and to build a living library to accompany it (Brand 1999), but this is a cultural anomaly rather than the norm. We are still too young for a serious transmitting program. A successful detection would encourage us to transmit to the others we would then know must be there. Continued failure might eventually cause us to transmit with the expectation that we are the first, or to leave a record of our passage (if we decide that the lack of signals implies that the average longevity L is quite small). There is no rigorous answer. Transmitters are a possibility. A search is the only way to find out.

The Significance of Null Results

Well-designed experimental protocols should permit something to be learned from a null result. At current levels of sensitivity, targeted microwave searches could detect the equivalent power of strong TV transmitters at a distance of 1 light year (within which there are no other stars), or the equivalent power of strong military radars to 300 ly, and the strongest signal generated on Earth (Arecibo planetary radar) to 3000 ly, whereas sky surveys are typically two orders of magnitude less sensitive. The sensitivity of current optical searches could detect megajoule pulses focused with a 10-m telescope out to a distance of 200 ly. Only a small fraction of the stars within these detection-limited volumes have been surveyed. Improvements in the sensitivity of surveys by four or five orders of magnitude will be needed before a meaningful statement can be made about the prevalence or absence of emerging technologies such as our own within the Milky Way Galaxy. Although it will never be possible to prove the negative case, null results from surveys with such improved capabilities would be very sobering. The conclusion that we are, for all practical purposes, alone could then be justified. Given the sensitivity improvements required, this state of affairs is not likely any time soon. Whether continuing negative results lead to the decision to transmit for the benefit of those emerging after us, or to the conclusion that L is inevitably small, will surely depend on what we have learned by then about the distribution of life of the nonintelligent kind.

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NOTE

The Appendix appears only on the Annual Reviews web site, where it will have active links to ongoing searches. It will be periodically maintained for currency.

Visit the Annual Reviews home page at www.AnnualReviews.org

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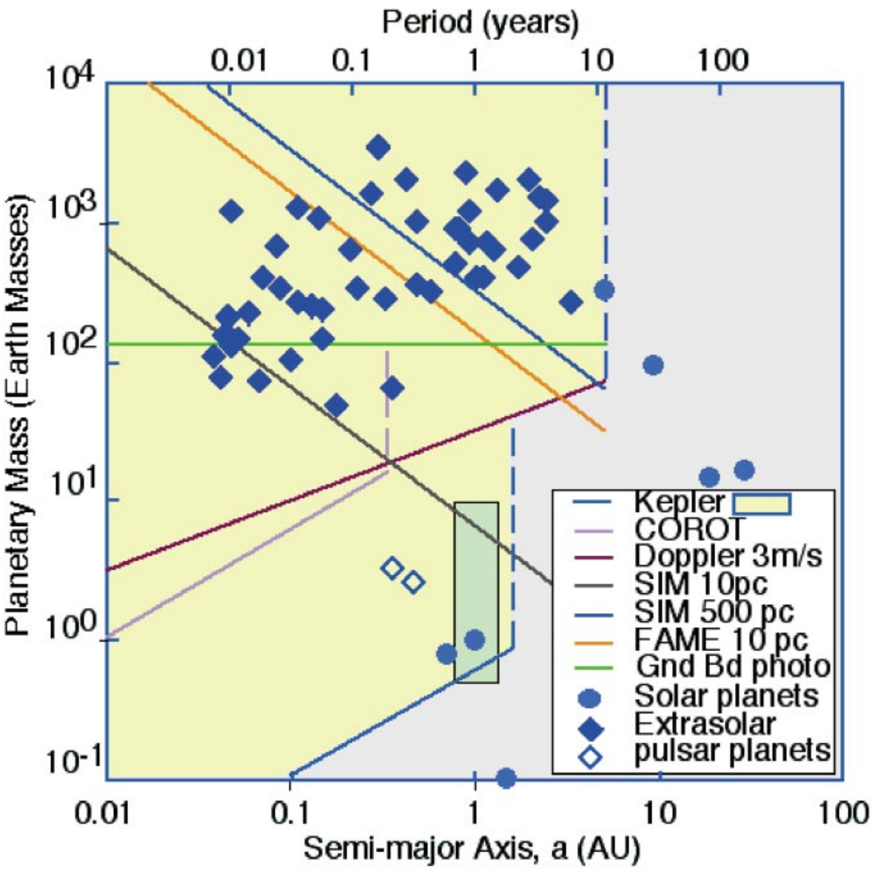

Figure 1 Observational Precision for Extrasolar Planet Searching Techniques (courtesy of Kepler web site http://www.kepler.arc.nasa.gov/capabilities.html). Limits on detection of extrasolar planets using photometry (Kepler, COROT, groundbased), radial velocity (Doppler 3m/s), and astrometry (SIM, FAME) around a solar mass star. Known Solar System and extrasolar planets are plotted as blue dots and diamonds. The green rectangle represents the position of terrestrial size planets within the continuously habitable zone of their host solar-type star.

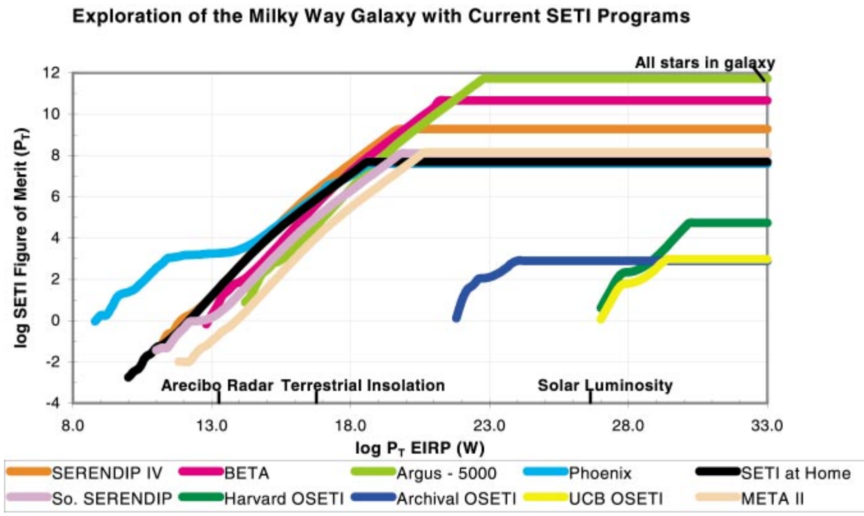

Figure 4 SETI Figure of Merit for Searches on Telescopes Today. This Figure of Merit that is plotted is due to Dreher & Cullers (1997). The power axis represents the average transmitter power EIRP for narrowband continuous wave searches, but is the peak transmitter power for short optical pulses.

Figure 5 Artist's concept of the Allen Telescope Array at Hat Creek Observatory. 1000 3 m or 500 5 m dishes will be arrayed together to produce a radio telescope whose collecting area is 10^4 m^2. The array should become operational in 2005, and will be constructed from emerging telecommunications and other consumer technologies in order to keep the price as low as possible. Because the array will be able to form multiple beams within a large primary field of view, with an instantaneous frequency range of 0.5 to 11 GHz, it will be possible to do SETI and more traditional radio astronomy continuously and simultaneously. Recent design reviews have concluded that the most cost effective antennas for the ATA will be 350 6.1 meter offset Gregorian reflectors with 2.4 meter secondaries and a ground shield. The current design concept can be seen at http://www.seti.org/science/ata.html