R225

A Novel Search for SETI Beacons

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From the Drake equation (Green Bank Lounge Plaque) one can deduce that the search for extraterrestrial intelligence (*SETI*) is an interesting game only if the typical lifetime of an intelligent, technological civilization is very long (> millions of years). A *SETI* beacon being broadcast by a typical (actually most likely a super-typical) civilization should have signal properties which make it easy for a primitive civilization (i.e., us) to detect. It would probably be very narrow in bandwidth. Further, it would have a frequency which is easy for us to guess. This kind of thinking motivated Morrison and Coconni to first suggest SETI and Drake to carry out the first search. We propose a limited search which differs in several significant ways from earlier searches [and, for that matter, the new NASA HRMS (high resolution microwave survey)].

The Rest Frame

One factor which has here-to-fore made beacon searches unattractive is that we had no knowledge of the reference frame in which the signals were being broadcast. Either one had to guess a frame or to sacrifice sensitivity by searching in frequency. We (and presumedly "they") now know a universal reference frame—that of the cosmic background radiation (Smoot *et al.* 1992, ApJ, 396, L1). We propose to observe in that frame—since the background fluctuations have been referred to by their discoverer as the "face of god," it seems reasonable to refer to this reference frame as the "frame of god."* The large motion of the Earth relative to the FOG ($\sim 360\ \mathrm{km\,sec^{-1}}$) assures that many earlier searches would have failed purely on this basis.

The Rest Frequency

Guesses at a beacon rest frequency have typically involved natural sources of radiation, often those of H I and OH because of the connection with the so called "water hole." We feel that such a beacon is most unlikely. To demonstrate this we ask the referees to imagine that this were a proposal to build a terawatt (yep 10^{12}) transmitter to be operated in L band with its frequency swept by 4 MHz. We imagine that "their" radio astronomers would have a similar reaction. Therefore we must consider a "magic" frequency that will not upset too many radio astronomers. Kardashev (1979, Nature, 278, 28) suggested the hyperfine line of positronium, but at 203 GHz it is not easily accessible to the most primitive civilizations. More attractive to us, seem the hyperfine lines of $^3He^+$ at 8.667 GHz and muonium at 4.463 GHz. Indeed, if H I is the most obvious, $^3He^+$ is the second most obvious magic frequency.

$^3He^+$ will probably attract the attention of a few radio astronomers early in the history of a civilization. The technology to observe it will be developed rather early and it will be studied for perhaps a few decades. It will never be a large scale survey tool important to a large fraction of the astronomical community. It is quite possible that beacon proponents could squelch the protests of the few astronomers foolish enough to observe $^3He^+$. It and muonium would be the ideal frequencies for reaching the most primitive civilizations, something that would appeal to extraterrestrial exoanthropologists.

Targets

Most directed searches to date have focussed on nearby solar-like stars. While some solar-type stars will be observed, we will in fact concentrate on another group. We do so after wondering, "What's in it for them?"

* Bob Vance has checked the 140 foot control system and it appears that with a minor software patch we can observe in the FOG.

We have taken as our fiducial broadcast beacon (FBB) one that is transmitting an omnidirectional signal with bandwidth 1Hz and producing a 1 Jy signal at 100 pc. This requires a 10^{12} w transmitter with a power bill of 10^{12}\$ yr^{-1}.† This is a lot of money. Building an FBB is not something our society would contemplate. Fundamentally this would be an expensive project for us because it amounts to a significant fraction ($\sim 2\%$) of the Earth's total power budget.

In an adjoining preprint we argue that such an energy expenditure will always seem expensive to a civilization that lives on a planet. Basically, a planetary civilization having significantly larger energy resources than we currently have on Earth will dump too much waste heat into a local environment. The only civilizations that will find FBBs affordable are those which have spread throughout their planetary system along the lines suggested by Dyson. While Dyson imagined a civilization tapping the entire energy output of a star, we imagine growth will stop well before then. Such a civilization would show itself via its waste IR radiation, i.e., we might observe its star to have an IR excess of 10^{-5}–10^{-3} $L_\star$. Vega is a good example: it is not associated in any way with star formation but still has an IR excess.*

Interestingly enough a civilization with energy resources such that a FBB seems affordable will also find colonizing interstellar travel only modestly more expensive. Hence, we feel the search should not be made exclusively toward stars which are candidate for indigenous *ETI*.

Following these arguments our prime targets will be relatively mature (not protostars; not pre-main sequence) stars with IR excesses. Civilizations in such systems might be energy rich enough to afford omnidirectional beacons. We will also observe a few nearby solar like stars searching for directed beacons.

We will select targets from various IRAF and stellar catalogs depending on the LST range of the available observing slots. There are a number of selection criteria which will surely evolve with time. For example, the highest resolution we can use is determined by the autocorrelator (AC) bandwidth and the error in the FOG velocity. Unfortunately Smoot *et al.* (1992) do not give errors for the velocity, but from Smoot *et al.* (1991, ApJ, 371, L1) one can guess that the error is ~ 9 km sec^{-1} and that the data exist for a higher precision value to be determined. For sources more or less in the same direction as the FOG we would now have to observe with an AC total bandwidth of 625 kHz. For source directions more orthogonal to the FOG we could observe with higher frequency resolution.

DAMIT

In the spirit of the NASA SETI program which is now a planetary search, we point out that this experiment is really a cosmic background radiation experiment. Since we know our velocity relative to the CBR to a precision of ~ 9 km sec^{-1} the beacon will most likely lie away from the center of our band. Since "they" presumedly know the velocity to far greater precision, by simply shifting the signal we can determine the velocity to a precision as good as that known elsewhere in the galaxy at a cost far less than a satellite experiment. We will refer to this follow up of COBE as: Dipole Anisotropy Measurement: Iteration Two.

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† Directed beacons would be a factor of 10^6 cheaper but would only be employed by nearby *ETI* who know of or strongly suspect our presence. There is certainly a tradeoff between number of directions, number of potential targets, beacon duty cycle, etc., but on the whole we feel more comfortable contemplating omnidirectional beacons than wondering why a civilization has picked just us as a broadcast target.

* The perspicacious referee might recall that the temperatures of the material producing a typical stellar IR excess are small by terrestrial standards. Even now there is a shift toward hydrocarbons as the "material" of the future. The civilization we imagine will be primarily interested in hydrocarbon rich (i.e., volatile) material in bodies having little self gravity. This will, of course, be found where $T < 100$ K.

Time and equipment required

We feel that projects such as this should not displace "traditional" radio astronomy. Hence, we request about 1 day of time as a test to see if observing in the FOG is feasible. Once we have determined this we request that the project be scheduled as a "filler" when gaps appear in the schedule while the appropriate equipment is on the telescope. After a few sessions to establish a routine we hope to observe remotely. (We have remotely observed successfully with the 140 foot in the past.)

A Note on Sensitivity

If we address the question of whether we could detect our FBB the result is rather sobering. With the X-band system at GB for 6 minutes on source, we get $RMS = 1\,\mathrm{Jy}/\sqrt{BW_{ch}/305\,\mathrm{Hz}}$ where BW_{ch} is the bandwidth per channel. The best resolution we can get with two receivers using the Model IV autocorrelator at GB is 152 Hz.

So our FBB at 100 pc would produce a 7 mJy signal with $RMS = 1.4\,\mathrm{Jy}$. At the distance of Vega the signal would be $\sim 1\,\mathrm{Jy}$. To reach $RMS = 300\,\mathrm{mJy}$ would require 2 hours.

Now, if we could observe with a resolution of 1 Hz we would have a 160 Jy signal from Vega with $RMS = 17.5\,\mathrm{Jy}$ and could reach 50 Jy in less than a minute.

If we integrated for 100 hours with the present GB system we could detect a FBB only to a distance of 22 pc. With a 1 Jy bandwidth we could detect FBBs to almost 80 pc.

So while the situation is not hopeless it is not terribly encouraging either. We chose our FBB because of nice round numbers and its association with a Reagan Era debt rate. We feel it is a rather more realistic way of looking at things than is normal for *SETI*. It still may not be a very good way of guessing beacon strength. We basically hope to blunder into something.

There are several compelling reasons to pursue *SETI* at the level proposed here. First, the requirements are modest and would not adversely impact normal radioastronomy experiments. Second, the strategy proposed is quite different from that of the much larger scale NASA HRMS program. Third, our FBB sensitivity estimates could easily suffer from human chauvinism: extraterrestrial exoanthropologists may be quite energy rich and enjoy transmitting with their megaFBBs. Fourth, we should continue to search using a wide variety of strategies. The probability of successful detection of *ETI* signals will be greatly increased if the search is not confined to the traditional *SETI* community. Finally, it's fun both for us and, we suspect, the GB staff involved.

PROPOSAL: R255 TELESCOPE: 140-Ft.

Title: A Novel Search for SETI Beacons

Authors: Rood, Robert T., Bania, Thomas M.

FREQUENCY: LST: 0 - 24h DAYS: 1

REFEREE A Rating: Good Time: 100%

When I read the title of this proposal I shook my head sadly and thought that two more astronomers were reaching their dotage. However, as I read through the proposal I found myself grinning and nodding my head. I don't like SETI searches from philosophical considerations, but this proposal is innovative enough to merit a try, if for no other reason then to reward the imagination and wittiness of the proposers. They won't find ETI's of course, (there's a bigger chance of me winning the lottery or getting NSF funding....) but who knows what will turn up? In addition, the proposal is a welcome slap in the face to the traditional SETI searches which are flawed by the search strategies involved. Finally, the proposers know human nature very well; how can a referee not be won over by statements from the authors such as "... the few astronomers foolish enough to observe $^3He^+$...() .

In all seriousness, the innovative methods outlined in this proposal, the rest frame augument, the fact that the observations won't interfere with normal observations, and also the fact that the two proposers haven't set up a huge bureaucratic organization to accomplish their objective makes this proposal worth scheduling. Kudos to Rood & Bania.

REFEREE B Rating: Average-Poor Time: 40-60%

Its a good use of dead time.

REFEREE C Rating: Good-Average Time:

Low, low priority!

REFEREE D Rating: Time:

PROGNOSIS:

Proposal will be scheduled, probably in late winter 1993. Intention is to schedule the pilot day, and then discuss filler time with authors as appropriate.