PANEL B

User Experience and Requirements of Hydrogen Masers

Panel Members

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PANEL B DISCUSSION

MR. FOSQUE: Would the panel members come forward, please.

Gentlemen, I would like to open this panel discussion by asking certain individual members if they will perhaps relate the user experience that they have had in a general way, that is to provide a background for the discussions, then we will move into some more specific questions.

I think I will just start over on my right with Mr. Rogers, and then proceed across the table. If you would be so kind, perhaps you could speak to us in a general way about your experience with the hydrogen masers and the uses that you put them to.

DR. ROGERS: Our main use is for very long baseline interferometry, and for geodesy and astronomy work it is extremely important to have frequency standard stability that is very good in sort of the medium term, that is, between say 10 and 10,000 seconds.

The reason why it is important in that time scale is that we make observations of a number of radio objects during a course of a typical observing session, and some of these objects act as calibrators. So really, what is important is that the frequency standard acts as a flywheel to carry us between the time that we observe one particular source, through, say, a number of other sources and back again, say, to the same source. One could, of course, carry this to an extreme and have a system with multiple antennas where one set of antennas are always looking at a particular radio source, in which case that would sort of become the Clarke star.

However, that is extremely costly in order to do that because in order to see the radio sources you need very large antennas. Our experience with hydrogen masers has been quite good. Our early experiences were not that good. We have used H-10 masers which have quite good stability at a hundred seconds. But seem to degrade very rapidly beyond that, mainly because of their extreme sensitivity to environmental factors, we think. At that time we were not really seriously measuring the sensitivity of the maser to various different environmental factors. Also, we had some difficulties with masers failing.

Early design masers had problems with the disociator and the lifetime of the disociator, and they used to develop the disease known as the whites. However, I am pleased to say that in more recent design masers, I don't think we have ever seen a case of the whites. We would, I think, like to see somewhat better, or less sensitivity to environment than we have now, although we are of course working to improve the rooms in which we operate these standards. But even so, it is very difficult to hold the temperature of a room much better than two-tenths of a degree C, and even that perhaps is optimistic, if you are going to have people going in and out to check this.

So certainly we would like to see improvements in this area. Dr. Clark indicated that we really could benefit by even better performance. I think better stabilities, in the range 10 to 10,000 seconds; and I think that is true.

We might want to set a goal of maybe a part in 10 to the 15. Our experiments become limited by the atmosphere probably at this level, although we are working on systems for calibrating the atmosphere that may mean even better frequency standard stability than a part in 10 to the 15 could in fact benefit us in the future.

MR. FOSQUE: I would like to ask Dr. Clark if he would comment on his experience.

DR. CLARK: Well, I should preface this by saying that Alan Rogers and I are really from the same group, although our affiliations are different. There has been a group of astronomers and radioscientists on the East Coast of the United States that have merged together to do a number of types of VLBI programs a long time ago, and we still continue to be working together. So, to some extent Al and I speak together; so we are getting twice as much time as any of the other people.

I thought it might be a little bit useful to trace in slightly more detail the history that we have had with hydrogen masers, just so you can see that our experience is fairly widely based. And I will take off some of the different kinds of units that we have used along the way.

The first masers which were used as Alan said were H-10s. The Haystack has its own H-10. We managed to pick out of the box with Harry Peters' help an H-10 that went to Green Bank, West Virginia.

The earliest use of two hydrogen masers at VLBI was on that particular baseline. Subsequent to that time we have had additional experience with the masers that Dick Sydnor has built and used them at the Goldstone tracking station in California. We have used the NP design masers of Harry Peters in a number of facilities, in California, Sweden, Alaska, in Massachusetts; and have gained quite a bit of experience and confidence in those units. And the Smithsonian masers were of the current generation masers, have been the only ones that have been "commercially" available. Commercially is said in quotes here because I don't think Bob Vessot would like to think of himself as a factory, but he did make at least a limited number of masers for sale to the astronomy community for VLBI purposes.

Currently such are in use at Green Bank, West Virginia; Maryland Point; Haystack; and one at the Onsala Observatory in Sweden and at Ft. Davis, Texas.

So based on that kind of set of experience, I think you can see that we have seen masers all around the world. The Goddard masers have also been used on VLBI in Australia and are currently also in use -- Pete MacDoran just walked in and I am sure he will make comment on the use of it within another VLBI program in NASA.

One of the environmental effects which Alan showed on his slide which was not expanded upon, perhaps as much as it should have been, which I think is a particular type of environment effect that we are very concerned about, long baseline interferometry work, is magnetic field effects.

The reason that this is of concern to us is that we typically use these masers at facilities that have large dish antennas which amount to large amounts of steel overhead which move to point at various directions in the sky, altering the earth's magnetic field in the environs of the masers.

Since the signatures we are trying to observe from the radio sources are in fact diurnal sinusoids, which the motion of the antennas with a diurnal period to track the radio sources could very easily mask themselves into erroneous geophysical and astronomical results.

So magnetic field effects have been of particular concern to us. In fact, based on Alan's testing at the Haystack observatory and reexamination of the shielding properties of the NP masers, Goddard Space Flight Center has recently embarked on a program to add additional magnetic shielding to the NP maser series. Two of those have now been upgraded, and additional ones are going through the mill having additional shields added.

We have seen such interesting effects as waste baskets and chairs alongside of hydrogen masers do in fact change tuning and hence the baseline results. That is certainly the kind of thing you do not want to see affecting high accuracy geophysical results.

So I think in terms of the reliability and requirements for masers, Alan indicated that levels of the order of parts in 10 to the 15 of a thousand seconds are certainly very desirable numbers for us. We do like to have longer term stability too, up to the one day level. Past one day it is relatively unimportant for most of the VLBI applications because our very long term time base is in fact TAI derived from LORAN-C.

However, since we are observing phenomena due to the rotation of the earth, we do like to be able to come back and observe that phenomena today and tomorrow, hopefully without any untoward behavior of the clock. Hence, time scales up to about 10 to the 5th seconds are of some importance to us.

Very short-term time scales are required, fairly good stability, though not past the level of the crystal oscillators included inside the H masers because we use them for local oscillators also.

So our most critical type regime is in the 100 to 10,000 second range with somewhat less criticality with one times 10 to the 5th range.

That is the reason for Alan's comment. In terms of the way the masers have operated, I think we have found at least two brands of masers currently available have shown very high degrees of reliability and movability. Not portability because then you think of a suitcase and these certainly aren't suitcases.

But the masers from Goddard and Smithsonian astrophysical observatory have both been moved around to a large number of places in the world and usually you can plug them in and they work when you get there at about the same level of transportability and reliability as cesium standards.

We have grown to trust them very much and have had very few failures. I think the question was asked this morning on mean time to failures. I think our experience has been very good. The worst failures we usually see are light bulbs burning out.

MR. FOSQUE: I would like to pause a moment before we go on and make sure that we ask Major Kittler of SAMSO to come forward if he is in the audience.

I guess he didn't come in late. Well, then, I would like to pass the microphone on to Mr. Easton and see if he will give us some flavor for his experiences with hydrogen masers and their uses.

MR. EASTON: We have had two different requirements for hydrogen masers.

Originally we started wanting one to compare rubidium and cesium standards to. For this purpose we got the first VLG10 ever built. And as Dr. Rogers has said, these had some problems with the disociator. But after the disociator was changed, it gave no further problems. We had another problem with the isolation amplifier, and when that was fixed, the thing has run now for a year with no problems. So we are quite pleased with the present VLG10, and our next use for hydrogen masers though is quite different.

We were now worried about flying them, so we are critically interested in small size, weight, power, but still have long life.

So I have been very encouraged today to hear that some of these standards have run for five years. That is about the order of life we would like to see. So I think, all in all, we are encouraged and think we can build good and long-life masers for space applications.

MR. DECKER: I am from Marshall Space Flight Center, and we have recently used four masers built by SAO in a redshift experiment.

These were three ground masers and one probe maser. These masers have undergone quite some severe tests especially the probe maser of course. Further there has been travel between SAO Wallops Island and the station at Cape Canaveral and have to work quite often in rather difficult environments. We have not experienced any reliability problem, any difficulties and these masers showed excellent short-term stability.

Our main concern for this experiment was stability over a period of about three hours because this was the main operational time required during the experiment. Our requirement was less than 1 part in 10^{14} drift.

You heard this morning from Mr. Bob Vessot about the design of those masers. The flight maser was tested in a vacuum chamber and its design was rather similar to the ground masers.

It was tested, for vibration, shock, ambient pressure changes between vacuum and atmospheric pressure; environments as you experience it in space, centrifugal force since we had a spinning payload and some variation of magnetic field, as well as combination of these various factors. In addition we did not only test but calibrated the maser for changes in these parameters during flight; we simulated the flight environment in the chamber and calibrated its various effects.

Through all these activities we never experienced any problem with a maser, and as you heard this morning, experiments worked very well and all the masers performed perfectly. I guess maybe we will come back later to talk about future requirements.

MR. FOSQUE: Maybe we could get Mr. MacDoran to give us some of his experiences, and I know he has had experience both in the areas of VLBI and spacecraft tracking. I will ask him if he would please make a few comments in both areas.

MR. MACDORAN: Thank you, Hugh. Well experience at JPL started out in the spacecraft navigation area, and the – some of the particular system elements that I was involved in was the effect of the time and frequency system upon the accuracy of the estimation of the Doppler signal and its decomposition to deduce the right ascension and declination of the spacecraft.

One of the elements that comes in that is generally foreign to the experience of most of the individuals involved in PTTI is the notion of long, round trip light time. People involved with, say, satellite timing, or something like that, are kind of used to the notion of, about a quarter second of round trip light time, out to geostationary orbit when you are talking about spacecraft tracking. In particular, my experience came with Mariner 6 and 7; where we were setting out ranging signals and waiting at that time for 45 minutes for them to get back from the spacecraft transponder.

And, as more ambitious missions are coming up, we have Mariner-Jupiter-Saturn that is getting ready to launch next year, and there some of those light times are going to be measured in hours.

The reference that you use for demodulating telemetry when you receive the return signal or the range code or the Doppler, you are obviously looking at a replica of what frequency system was doing three hours ago. And that is a very strange kind of environment in which to live.

So, you end up looking at a different kind of perspective of the way this error source gets into the data. For example, if this turned around very fast, you can tolerate a pretty poor kind of stability, you know, because it is going to be just differenced out in the next instant.

But, when you are looking at hours of round trip light time, obviously, the requirements are changed quite a lot. Not only is the Doppler the prime data type in the Mariner-Jupiter-Saturn mission, but some new things have come up. One is that the declination of the planets, the outer planets at least like Jupiter and Saturn, are what they are. You might feel that you wish that it wasn't so close to zero declination, if you go through and write out the equations of Doppler, you will see that at zero declination, you don't have any sensitivity in a Doppler data system. So if you are talking about a flyby to a planet, and the planet is near zero declination, you have a very bad time estimating just where it is in that direction, relative to the equatorial plane of the earth.

There is now a move to try and determine angles by differential range. Now that differential range starts to put on another series of requirements, because if you are talking about range, good to a meter, there you are having to face now something like three nanoseconds of synchronization between two stations that are probably at intercontinental separations.

And my colleague, Brooks Thomas, was going to address that, but the press of work back at the laboratory prevented him from being here. But, there are lots of ways that this is being looked at. There are some schemes that are called near simultaneous ranging where you can sort of trade the fact that you can interpolate the orbit just a little bit and pretend as if it were simultaneous and, therefore, you don't have to have the synchronization on the ground.

But there is a lot of thought being given to synchronization at the subnanosecond level to do the navigation. The rippling effect this has on the design of a mission is really amazing. Ultimately, it goes right back down to what it is that you are going to fly. For example, if you have an ambitious kind of scientific package, you want to fly, but it is kind of too heavy, then it comes onto the trade-off with the propulsion people, just how much weight are you going to get; how much propellant are you going to off-load to put on this instrument. That goes back to the navigation and says, "Well, how well are you going to put me by the planet so I can decide what kind of motor burn I am going to have to do when," and it's a tremendous rippling effect and you see it coming down to: "Well, how well is the PTTI going to be done?"

The requirement's coming up for the Mariner Jupiter Saturn synchronization of something like 10 to 13 and it looks like VLBI is going to play a prime role both in the synchronization of the oscillators, as well as the epoch of the clocks, in the time interval of the Mariner-Jupiter-Saturn mission.

With regard to ARIES and the transportable VLBI work, the experience we have had now is with quite a few major systems, started out in 1971 doing experiments using two of the developmental JPL masers, and the very valuable experience, and we began to get a better handle on just what it was that we had to have.

From that experience and the experience of using rubidium, we managed to develop observing strategies that would allow us to live with some of the peculiarities of the rubidium system relative to a hydrogen maser. We then had an experiment in which we had an SAO maser in Madrid, which Bob Vessot very kindly took the maser to Madrid for us. And – he just smiles over there. And we got some very interesting data there. We were getting synchronizations of the oscillator rates that were equivalent to a part in 10 to 13. And the two components of the equatorial baseline between Goldstone and Madrid were precision of about one meter and agreement at about the two meter level with Doppler determination sustention positions. Then, about a year ago, we started doing experiments using a rubidium and ARIES station and modified H-10 masers at Owens Valley and almost 12 months ago, we started using a Goddard hydrogen maser in the ARIES transportable then.

The experience with the Goddard maser has been very gratifying. Our data reduction goes ever so much more smoothly that we don't have to figure out exactly what rubidium was doing in order to kind of back our way through the analysis. It is a very well behaved system. I think it is even NP-1 that we have in the station, so it, by no means, is the latest of the Goddard technology.

We have made all the moves successfully, and there have been a total of six moves that we have made during the last 12 months. And the Goddard maser has worked well. Once I have to admit the van got overly hot, and I guess we lost phase lock in one of the loops or something but we got the air conditioning back on and things settled down and it came right back up. But that is certainly not a problem with the maser; we had a truck break down and it was summertime and the thing got very hot and so on. But the maser has done very well.

And as we look forward to other systems, I guess we would like to see a maser that would settle down in just a period of a couple hours or something after a move and I don't know what the implications are for a system design for that kind of a wish. Synchronization needs for us are something like about 10 microseconds to get this started in the cross process, so we don't have to search too large a space and frequency synchronization, about one part in 10 to the 11 and then stability, once wherever the maser is going to be running or wherever the frequency device is of about one part in 10 to the 14 will sort of do everything we need to do, even up to X-band on regional baselines up to, say, 1000 kilometers. Or probably even more than that.

Even intercontinental baselines part in 10 to 14 will do it but let me not mislead you with a statement like that. When we are talking about transportable geodesy, what we are after are three baseline components, three dimensional relationship between the two stations and in instrumental terms. For that I will just mean something that looks like a clock synchronization and something that looks like a synchronization of the rates. So we are solving for just five parameters. In some of the work that Tom and Roger are concerned with, they have a more ambitious kind of solution problem. Not only do they have to do the five I just described, but they are talking about picking up you the one, polar motion, radio source positions and so on.

MR. FOSQUE: I wonder if at this time, there are some questions that the various panel members wish to bring up among themselves. If not, I think we can probably stimulate some discussion by opening the panel to questions from the floor.

I see Dr. Winkler with his hand up there. Perhaps we could let him have the first question.

DR. WINKLER: Can I ask the last speaker, MacDoran, would you repeat your statement about the planets being on declination zero? I am old enough to have seen most of the planets, declinations as high as 23-1/2 degrees up and down. I think there must be something wrong.

MR. MACDORAN: Okay, the problem is that outer planets will do those kind of things. You just have to wait around long enough. For the inner planets, sure, you don't have to wait too long to watch Venus kind of go through its whole range of declinations, but if you want to wait around to watch Pluto go through its whole range of declinations, it might chew up a lot of time.

If you have got a mission and you have got the funding and have everything put together, you are going to launch. And the planet you are going to go to, you are going to fly by the declination that it happens to reside in. If it turns out it is a declination of five or six degrees, rather than 20, that makes quite a lot of difference in the sensitivity of the Doppler tracking and its ability to estimate the declination of the spacecraft as it approaches the planet. Have I answered the question?

MR. FOSQUE: Other questions from the floor? I see Victor Reinhardt.

DR. REINHARDT: Victor Reinhardt, Goddard Space Flight Center. I was just wondering if you could sort of sum up your needs for the future in terms of frequency stability, environmental sensitivity, longer term stability, just to get some sort of consensus or, and also synchronization. I heard three nanoseconds here which raised my eyebrows a little bit considering past statements.

I am just wondering if any of you would just make some projections for the future about your needs and would likes.

DR. ROGERS: I will try and get my set of numbers. A crucial time scale, as we have already mentioned is 10 to 10,000 seconds. I think one needs to have a better than part in 10 to the 14. Perhaps, as good as a part in 10 to the 15, if we make the improvements in atmospheric calibration that we may be able to make. So somewhere between a part in 10 to the 15 and part in 10 to the 14 for that time scale.

I think we would like to have that same stability extending out to a day. As far as the environmental sensitivity, I think you can really just take that number and as I say, I think we can hold the room temperature to, well, two-tenths of a degree, which means that the kind of temperature coefficients we have now have a few parts in 10 to the 14 are just good enough. But one could, again, go for some improvement. As far as synchronization is concerned, I think I am not sure you quite understood what Peter MacDoran said on that.

The initial requirement for synchronization, I don't think, is anything like nanoseconds. I think it is the order of microseconds, and it is merely a matter of convenience. In fact, we have done some experiments where we did not know the initial clock synchronization to better than a millisecond, and one can search for the clock synchronization, but it does take processing time. I think that we can, through the very long baseline interferometry technique, provide very good synchronization at, I think, the nanosecond level, once we calibrate our antenna systems correctly.

We have now subnanosecond synchronization, but only relative from one experiment to another. We have unknown constants in the synchronization that we have yet to calibrate out at the nanosecond level.

DR. CLARK: I might expand on this just slightly. The tracking stations and radio astronomy facilities that are involved in doing long baseline interferometry work using hydrogen masers probably constitute the biggest challenge for frequency standards for very high technology frequency standards anywhere in the world. And the fact that these can be synchronized by the interferometric techniques, that can essentially become a very high accuracy network of time available to the users.

One point when we get around to the questions going in the other direction that I would like to make is the idea of in some way having the astronomical facilities doing long baseline interferometry serving as a worldwide grid of time. So, I am posing that a little bit early but it goes along with what Alan just said here.

MR. FOSQUE: I guess we have a question here, I think we will take the question, and then come back to the panel views on requirements again.

QUESTION: Samuel Ward, Jet Propulsion Lab.

Would MacDoran or the panel like to discuss the problem of establishing calibration and nanosecond level and maintaining sync for long periods, like five to eight years?

Most of the users at the panel are only doing this for a period of a few hours.

MR. FOSQUE: Who wishes to speak to that?

Pete MacDoran?

MR. MACDORAN: Well, the synchronization on the scale of years comes down to, what level of synchronization you need. I am going over in my mind what the interplanetary flight would really need, say, nanosecond over that kind of time frame and I can't see any driving requirement at the nanosecond level for years. You know for time spans continuously for a scale of years.

I could see it certainly during the differential ranging to get around the zero declination degeneracy and parameter estimation but that extends over a tracking path; that is in the scale of hours.

Now, you have a problem of clocks going down, you have to restart, you have a synchronization problem there; but I see that problem being solved by the VLBI technique itself, where for initial conditions you have sychronization of oscillator rates at about the part in 10 to the 11, which you can do by VLF techniques or whatever; and you have for convenience, initial clock synchronization at the few microsecond level.

Once you have those as initial conditions, you start then with the VLBI; and probably, as the JPL deep space network is now considering, they are going to bring that back either on a so-called high speed data line at rates of about 50 kilobits per second, or maybe through a satellite communications circuit, and do synchronizations at the nanosecond level of clock epoch. And from changes of epoch to deduce rate.

So, I see what you mean on scale of years, but I don't see that translating into nanoseconds. I see it much more at parts in 10 to the 13th, as I mentioned before, for the Doppler tracking with long-return-trip light-times. That is where I see the driving requirements coming from.

MR. FOSQUE: I would like to go back now again to the question of future requirements and ask our other panel members who are not involved in the VLBI, if they would address what they see in terms of future requirements.

I will start with Mr. Easton.

MR. EASTON: Our requirement, as Fred Walls made quite clear this morning, is not frequency stability requirement; it is a time requirement. We would like to keep a maser in space having unknown errors no greater than about 10 nanoseconds, when compared to Dr. Winkler's 32 hydrogen masers, which, I am sure, he will have in the near future.

MR. DECKER: If we talk about maser application in space, of course, size, weight and space requirements are very critical. Particularly, long operation lifetime without maintenance or with automatic control from the ground.

If we consider future relativity experiments to measure some higher order effects – we would like to see a frequency stability of 10 minus 16 or better over at least a period of several hours; as good as you can get.

MR. FOSQUE: There is a gentleman with a question. Would you identify the panel member that you are addressing the question to?

QUESTION: I am still very unsatisfied with the answers in terms of requirements and need. I am not that familiar with the VLBI, but can you give some concrete examples of what difference it would make to you, and what specific missions, whether you can order 10 to the minus 14 or 10 to the minus 15. It is very nice to have 10 to the minus 15 or 10 to the minus 16, but is there really a need and does it make a difference?

You would like to have 10 nanoseconds but in terms of applications, in terms of real need, now, what difference, what system would it make any specific difference whether you have 10 nanoseconds or 20.

There may be a substantial expenditure to achieve such a result. It is all very nice to have it, you know, but it still costs money to get it. Is it worth it, I mean to go from 10 minus 14 or 10 minus 15?

MR. EASTON: The reason why we want and need 10 nanoseconds is that at that level, the error budget due to the clock becomes large, compared to the other errors in the system. And very soon the system starts going to pot. This is the GPS system. So, that is why 10 nanoseconds is important.

MR. DECKER: In terms of time synchronization or time, the requirement is not very critical for the relativity experiment.

MR. FOSQUE: I think he's asking you about the level of stability that is required.

MR. DECKER: This depends on the type of measurements you want to make. If we go, for example, to the sun, we can with the present maser design do some very good measurements on relativity experiments. That depends how close you can come to the sun, how much change in the gravitational potential you can experience with a maser.

But there are some other experiments where you would like to get up to about 10 minus 16, if you can.

Does this answer the question?

DR. VESSOT: I can answer that question.

Prof. Kenneth Nordtvedt has just calculated what would happen if you did a solar grazer, not a solar plunger. That is a device that goes in a parabolic trajectory within four solar radii of the center of the sun and is measured at an angle of 45 degrees to its trajectory in the plane of the ecliptic, so you are not looking out of the plane appreciably.

He comes out with the following results. If you had a clock at one part ten to the 14th, you would get that value of the redshift would be on the order of 10 to the second-8, and the value of beta the second-order redshift would come out at about 10 to the minus 5 from the trajectory determination. You would measure beta from the second order redshift directly at about 10 to the minus 1. You would measure gamma, I think, at about 10 to the minus 6 level; that is the parameter that has to do with the way the spatial part of relativity is altered by the presence of the solar mass.

The oblateness of the sun could be measured at about 10 to the minus 10 level, that is the fact that the sun is flattened owing to its rotation.

There is also an experimental test of the frame dragging, which is the property space has when it is near moving matter, in this case the rotating matter of the sun where the actual coordinate frame is dragged by the rotating object, and that Lense-Thirring Effect. This effect would be measured at about .6 to .2 level -- enough to see its existence.

Your question can be answered by saying all of these can be improved by a factor of 10 if you got to 10 to the 15th, instead of part 10 to the 14th, where, I think, we are now at. These are all measurements which, I think, are very, very important to people in astronomy, relativity and astrophysics in general.

MR. FOSQUE: I think we will take the gentleman real seriously and ask all our panel members if they can sharpen up their requirements and identify how they come about. So, let's go now to our VLBI contingent.

MR. MACDORAN: After having heard the implications of the general relativity aspects, I would like to acquaint you with a much more prosaic problem: one of solid waste management. You think I am putting you on, but I am not.

There is a mission that is about to fly called SEASAT. SEASAT is going to carry a radio altimeter and it is going to measure the ocean's surface from orbit. One of the driving requirements of SEASAT is to find the open ocean circulation. That happens because there is something called geostrophic flow because when the water sort of turns a corner, it stacks up.

The assumption is that by flying over you will be able to see the stacking up of the water and identify the center of the thing called the amphidrome. That is the oceanographic aspect.

The geodetic aspect is that there is a systematic problem with the apparent sloping of mean sea level.

Something is inconsistent. It doesn't seem to be on the level.

MR. MACDORAN: Unless you can figure out what is happening there, you will get confused between what is systematic slope and what is the open ocean circulation.

Now, ARIES Project with the transportable VLBI is involved now with the National Geodetic Survey in developing a relationship between differential leveling and geometric geodesy.

Now, we come back to the positions requirement. How good do you have to do this thing? And the answer is about 10 centimeters. What is 10 centimeters, what does that actually translate into when you look at even a simple solving for five parameters? And you keep going through this, and what you end up with is about a part in 10 to the 14th stability.

So, now we have traced it back to a frequency stability requirement and that have to do with open ocean circulation and dumping of solid waste from the coastal states and how long it takes before it washes up on your beaches.

It wasn't a put-on.

DR. ROGERS: Are you people looking for an actual number?

VLBI is basically differential ranging, and we are hoping to be able to measure strains on the crust of the earth of the order down to the centimeter level across the continent and we hope perhaps down to the millimeter level for around a hundred kilometers, and we have already done the millimeter level on distances of about one kilometer. If you just take one millimeter and you say, you want to get down to one millimeter, and let's say, your antenna takes a thousand seconds to move from one object to another, you want the frequency standard to drift an equivalent to one millimeter, which is the order of one picosecond in a thousand seconds. That is a part in 10 to the 15th right there. That is the kind of calculation that is a very crude calculation, but that's the kind of numbers you come up with.

DR. CLARK: I was going to amplify a little on Alan's analogy. There are a number of the geometric turns. He was talking about the one where you make a differential measurement between two different radio sources to determine differential arrival time from the two sources at the two stations so that is, in a sense, a double difference measurement he was describing there, which required that kind of level of stability.

But if the kind of numbers we are trying to get down here are down in these centimeter and subcentimeter category then it's another story.

Let's talk about the one-day level of stability briefly. If we are talking of about a part in 10 to the 14th over 10 to the 5th seconds, that is a nanosecond per day. We all know a nanosecond is a foot. So, if we have instabilities at the part in 10 to the 14th level at the one-day level, that means that there is 30 centimeter type noise that is masked in all of the rest of the stuff we are really trying to observe.

Some of that noise, if it averages out, really doesn't hurt us very much. But the thing that we are trying to observe with the very long baseline interferometry techniques, we use the quasars up in the sky as inertial reference frame. They are very fixed; we haven't much worry about their stability. We are trying to measure the geometry of the earth underneath those quasars.

We see that geometry change once per day as the earth rotates underneath the quasars and the apparent geometry of the baseline, as seen from the quasar then rotates once a day. Therefore, what we get as an output signature for the observable from one source is a sinusoid that varies over a period of time of one day.

But we can't observe it over a full day because the earth isn't transparent, unless we find some very specialized sources that are up all of the time and all of the antennas that are involved, which doesn't happen.

So, what we are trying to observe is over a significant fraction of a day, and hopefully come back the following day and make sure the measurement lies on top of that first measurement, be able to stack all of these things together, coming out with that numbers that are accurate at the centimeterish level, we don't want to have, then, frequency standard effects that have diurnal signatures masked into the data.

For instance, if the frequency standards have temperature sensitivity, the temperature sensitivity in the room is going to have a diurnal signature to it typically. So that is a kind of effect that can be very bad. Similarly magnetic field effects could be diurnal because we have this big moving mass of telescope overhead, which is pointing at the different stars and taking out the earth's rotation.

These are some of the reasons why we require all of this freedom from environmental parameters at the same kind of level Alan showed for the double difference measurements. So, it does convert to a requirement also for precision out at the nearly one-day level or one-day level.

Now, obviously, we could then do what Pete did, and say what is the implications of measuring these distances to that level.

Well, that obviously has implications in earthquake prediction areas and things like that.

I don't think we want to go into the economic benefits. Pete already did in his dumping of waste matter analogy.

MR. FOSQUE: Any questions?

DR. REINHARDT: I would just like to make a comment in line with what the BIH people said about wanting hydrogen masers. And our problem at the laboratory has been holding on to them long enough.

Since the VLBI are using the masers and use them for long periods and have the network, the ideal place for reporting to TAI is the masers at the VLBI stations rather than at the laboratories that are doing the research on the masers themselves.

MR. FOSQUE: Other questions?

DR. KNOWLES: Steve Knowles, NRL.

The questions about the supercooled oscillators I have heard about. What is the current state of the art on those; who makes them; and is there a possibility of a prototype being available for use at a VLBI telescope? I would certainly like to see that. We all have good facilities for testing oscillators.

MR. FOSQUE: Who wants to tackle that problem?

DR. CLARK: We have some of the NBS people here in the audience.

MR. FOSQUE: It has been suggested we have people very knowledgeable in the audience. Perhaps Mr. Allen might, if you give him a microphone, maybe he could comment on that.

DR. ALLEN: The best results to my knowledge that have been obtained at Stanford by Dr. Stein, and Dr. Turneaure. Those results have been published.

We have not yet – had systems operating to the point where we can compete with those. Those results are, one-second stabilities.

DR. WALLS: Now seconds

DR. ALLEN: Yes, 4 and 10 to the 16. 1000 seconds. One-second stability, as I recall, is about 10 to the minus 14. going down. one over Tau. That is what has been documented. As far as their availability, we are not interested in a production unit. The thing that needs to be done is for someone to pick that up and try to capitalize on what research has been done. We have high hopes for the superconductor cavity.

I think one thing has to be kept in mind, and that is that if I understand the need in VLBI, it is the total accumulated phase over whatever integration period is of interest, whether it is 1000 seconds or whatever.

And if you talk about that, then you talk about the stability at that sample time. If it is at 1000 seconds, then, due to the environment sensitivity of superconducting cavity you nominally have comparable stability between hydrogen and superconducting cavity.

So that is an important point I think that needs to be brought out.

MR. MACDORAN: I guess going back about a year and a half ago I had a meeting with Turneaure when we thought we were in better financial shape than we are.

We talked about the possibility of putting together a transportable device which he thought he could do. My recollection, it was something like, at that time at least, around the 50 K category for a demonstration field unit. And there were some really fascinating possibilities with it.

The fact that you have to go down to below the usual for Kelvin liquid helium temperature, you have to pump it down I guess 1.6 or something. It is cold, cold outside. But not much. Being from Southern California, it is a real shock to me. But one of the other things you could get, a kind of synergy going is with the cryogenic traveling wave maser.

In the masers now used in the deep space network, there is a bath that runs about 4 Kelvin, and – or maybe a little cooler. So there is this traveling wave maser structure so what you begin to envision is the structure and this Gunn diode running in this resonant cavity and it is all one integrated unit.

When you take the maser from 4 degree Kelvin, the maser gain is something like 45 dB, but when you pull the maser down to about 1.6 Kelvin, the gain goes to about 90 dB and it is just fantastic, so you get visions of this tremendous receiver one could have operating, you know, probably at around 10 Kelvin for the operating temperature of the receiver with an integrated local oscillator. The Gunn diode wants to run at X-band anyway so you don't have to do any multiplication.

Frankly, I think one can see stabilities that we haven't even thought about how we were going to exploit them if they were available because our thinking has been restricted right now to what we can get or by whatever means we obtain, to use what is physically available. But that is going to be changing, I think, in the next few years.

DR. ROGERS: I would like to just comment on Pete's statement and, those kind of stabilities.

I think we know we are limited by atmospheric stability a long time before the part in 10 to the 16 level.

DR. FOSQUE: Are there other questions?

DR. WARD: I would just like to add something to Pete's statement. He forgot that that maser also has a superconducting magnet.

MR. MACDORAN: That's right.

MR. FOSQUE: Any more questions?

Well, I have a piece of paper here which somebody gave us a written question. I think I would like to turn that over to the panel now.

The question is: what are hydrogen maser user experiences in comparison with cesium and rubidium standards? And have these different devices really been compared on an equal basis, or under equal conditions?

I will start over here on my left and see what comment the gentlemen would like to make on that.

MR. DECKER: I don't have any experience. We have no comparison.

MR. EASTON: Well, we have had experience with all of them but I don't know how to say equal bases, since they all use different types of atoms.

There is no doubt that the hydrogen maser is the most stable. The cesium is right now the most accurate. Rubidium has very good short-term stability and not so good long-term stability.

We are in hydrogen masers because we were told to look into them. And, strangely enough, it now appears to be a very good decision. But it wasn't one we had to make.

MR. FOSQUE: Would you gentlemen care to comment?

MR. MACDORAN: Well, a year ago PTTI, the paper I had in there, discussed specifically how we had used an HP50-65 to get baseline solutions at S-band, where the requirements are less severe than going to X.

We evolved observing strategies so that the frequency system would not do us in. But that doesn't mean those strategies were not compromised to what it is we ultimately want to do. You know, it was good to begin to demonstrate a feasibility, to develop software and the whole systems analysis.

Now that we have had experience with the hydrogen maser in the transportable station, we are looking at covariance limits right now, sort of the best we could do theoretically from the data quality being generated, and those covariance limits are in the range of three to five centimeters. We have some other systematic things going on that are kind of limiting us at 10 or 12 centimeters right now, but we think we will get those cleared up within the next few months. So I think we will start moving down where we can get out the three to five centimeters and that will be in the time scale of the next year.

The hydrogen maser plays a prime role in that. It is kind of interesting. If you don't have a really clean data system, or a clean frequency and time reference, then the entire system, and ARIES is a very multidisciplinary kind of thing, and if you are getting confused right at the time and frequency level you have trouble distinguishing what your dependences are on the radio source positions or the ability to do the transmission media calibration, both the charged particle effects and the neutral atmosphere, or the ability to do the modeling. Enumerable numbers of things.

So it sure is a real blessing to us with the hydrogen maser that now we can get in there and say we just don't have the problem from the time and frequency system and now these other errors are so much more clearly standing out and amenable to a systematic solution.

DR. ROGERS: We have had a little experience with rubidiums. The HP50-65A at Haystack Observatory is in fact used as the primary source of five megahertz.

In fact, we lock the rubidium to the maser.

That, we may change. That was somewhat historic because we wanted to keep that five megahertz on line at all times.

We have had rubidiums going in and out of our standards room as we switch from one rubidium to another. We have seen very good stability with that particular HP standard. It approaches a part in 10 to the 13. I think at about 1000 seconds.

I have a sigma tau plot. I have it in my briefcase if anybody would like to see it afterwards.

We did have a cesium supertube that Dr. Klepczynski brought to Haystack Observatory, but unfortunately we were only to look at it for a matter of about an hour.

It came as part of a clock synchronization service, normally we would just look at the second tickout to synchronize our clocks, but we tied it into a frequency comparison system. It looked like it was not too good at all at 100 seconds. Worse than the rubidium.

It was about a part in 10 to the 12 at 100 seconds. I am not quite sure why that was so. Maybe he would like to comment on that. But that is the only test I know of a supertube cesium.

The older cesiums we know are totally inadequate for any kind of VLBI application. Stability at 100 seconds is totally inadequate. We can't even get coherence at X-band.

DR. CLARK: There was one supertube cesium experience we had on the air that I think Allen may have forgotten about. It was at a time when we were running VLBI between Haystack and the Goldstone Tracking Station and the normal site maser was not available at that time.

So we did take one of the very early supertubes that Goddard had bought to the Goldstone Tracking Station to use as a local oscillator. Our on-the-air experience with that, very early one, was that it gave stabilities at the critical time scales for VLBI roughly comparable with what we would have gotten with the rubidium.

In terms of answering this question, in terms of a geodesy application, Pete gave part of the answer. There are some errors which are instrumental errors which are independent of baseline length. There are others, VLBIers, which are proportional to baseline length.

The fact we don't know before the fact the geometry of earth, which way is the pole pointing and how fast is the earth rotating, that introduces errors to us.

On very long baselines, the way in which the frequency standard maps into our ability to measure these terms, it will introduce a few centimeters of uncertainty if the frequency standards depart in the 10 to the 14 level.

That uncertainty goes essentially that is for the critical numbers around 1000 seconds.

Therefore, if we are talking about a part in 10 to the 13, that is introducing 30-centimeters errors and it gets very difficult to do 5-centimeter geodesy if you have 30-centimeter clock-error noises masking themselves into the data.

So I think the VLBI applications for high accuracy astronomy and geophysics, to produce numbers at the output at the subdecimeter level clearly require hydrogen masers or comparable performance from some other technique, whatever that Brand X technique might end up being, and I don't see any way around that.

MR. EASTON: Perhaps there is one thing that has not been brought out.

The reason the hydrogen maser is so good compared to cesium and rubidium is that the bandwidth of the line is so narrow. It is about one hertz in the ordinary hydrogen maser – and in the passive maser, Fred, what is it, a quarter?

DR. WALLS: It could be.

MR. EASTON: Something like a quarter of a hertz. With the very long tube cesium, 30 hertz?

VOICE: 25.

MR. EASTON: With the ordinary run of the mine, 500 hertz.

Perhaps this gets to the point of this question. If we are comparing a hydrogen maser to an equivalent cesium standard, it is perhaps 300 meters long, and even the H10s weren't that big.

DR. KLEPCZYNSKI: Bill Klepczynski, National Observatory.

Tomorrow we will be reporting on a paper which is co-authored by the people from NRL, Ken Johnston being one of them.

We did do some experiments with radio astronomy where we have results where we were comparing rubidium on a maser and cesium on a VLBI experiment.

As expected, you find the maser is the superior performer.

However, according to specs as you see them, the short period term, rubidium outperforms cesium, about 1000 seconds. In this instance the cesium did pretty well.

You will see some of those results tomorrow concerning the one problem. We had the cesium up at Haystack a while back; I don't really know how to answer that because I think Dr. Costain is going to make a comment.

DR. COSTAIN: We had the same thing at NRC before we came to Haystack.

We were doing a phase comparison there against their cesium 5. We found very good agreement with it.

I was surprised at the performance we were getting at Haystack with this particular cesium.

It is within the spec, though, of what HP prints.

I was expecting, and when we got back the closure indicated the cesium itself had performed a lot better than it did, the time of the performance of the phase track we were doing.

So I am not completely sure about what happened there.

MR. FOSQUE: I believe we have a question over on my left, Dr. Costain.

DR. COSTAIN: There still is a question in our mind as to the place of cesium. Certainly we use the hydrogen maser still to, in our evaluation of our primary cesium standards. But we are beginning to wonder that if we can — we think we can or have achieved a part in 10 to the 14 stability in about three hours which is just going outside the requirement of many of these; but we think that is maintained for a year.

I think there is probably, certainly in the time-keeping business, it's going to be wide open, long-beam cesium standard type.

It's in my mind still a question in operational conditions - of whether the absolute long-term stability is not going to be an important factor.

Or the question is, is it going to be an important factor with sort of timekeeping of a microsecond per year?

I would just give a brief comparison. We have, against some high performance at Hewlett-Packard for selected intervals. This is not in performance seen between our primary standard and the high performance figures down into the several parts in 10 to the 15.

MR. FOSQUE: I believe there is a question down here on my right.

Mr. Peters.

MR. PETERS: I think it's pretty important or I wouldn't bring up perhaps what might be a touchy subject.

But as the question of accuracy relating to the type of standard has been brought up, when we are talking about accuracy with cesium superior to that of hydrogen masers, we are always referring to the National Standards Laboratory cesiums.

The available cesiums which are used as standards in the field, the commercial cesiums, I'm sure everyone will confirm this, are not specified in accuracy to near the accuracy capability documented for the usual hydrogen maser. Typically, five parts in 10 to the 12.

We are talking about fundamental accuracy now, and in the sense that they can be evaluated. I would like to question the statement that cesiums are more accurate than hydrogen in the sense that they are referred to the National Standards Laboratories and are even in their specifications. So for hydrogen in that sense, its capability at present is more accurate by a factor of 2 to 5 at least, I believe; as everyone is well aware, or should be.

MR. FOSQUE: I will take a question from Dr. Winkler first, then come back to Dr. Costain.

DR. WINKLER: I have no question. I have to make a comment.

I think I begin to see the point of Dr. Decker, that accuracy is becoming a much abused and misunderstood word.

I have to say I honestly begin to see the reason why it would be better to refer, to split the meaning which is inherent in that word, split away the part which he considers as uncertainty of a standard. Whatever that may be, Peters is of course correct. If you talk about uncertainty of a standard, you mix together several unknown systematic effects. If you talk about the other benefits which were inherent in your discussion here of long-term stability, you talk about your control over systematic effects which may come in over long periods of time. These are really two different things, I think, which one has to consider, and I believe maybe we shouldn't talk so much about accuracy and consider more the systematic effects.

There is something which Harry has said two days ago or three days ago at NCIR meeting which may be useful to mention here. That is the ability to model these long-term systematic effects; it may be here that there is an advantage in the hydrogen maser. Maybe Peters would like to make a comment on that.

This, of course, is ridiculous and a complete misunderstanding of the meaning of band width, and long-term performance.

The reason why the hydrogen maser is a – standard at least in a region of sigma tau plot, certainly between ten seconds, and somewhere, tens of thousands of seconds, I would certainly agree with that, there is no question about it, is that there is – it is inherently phased stable - coherent phase output avoids the random accumulation of steps or what we call the random walk-in phase which is inherent in the performance of a cesium standard; which is why frequency noise dominated, as opposed to the hydrogen maser, which is why phase noise dominated until it reached the frequency – so I think it is completely wrong to make that comparison.

I wish that you, Roger, would recant immediately, and –

I think it would be very interesting if you would comment on the ability, your expectation of modeling the systematic effects in the hydrogen masers.

I think this is a thing which is definitely pertinent here.

Of course, as Dr. Winkler has brought out, there is a problem in comparing hydrogen masers to cesium standards.

One is an oscillator and one is not.

Or one can be an oscillator and hydrogen maser can either be an oscillator or a filter like cesium.

But if you were referring to equal band widths, and as I recall the question there was some equality, equal conditions, you would certainly have to have a very long cesium standard to have the same line width as you have with a hydrogen maser.

That was my only point.

DR. WINKLER: The question refers to equivalent or equal environmental conditions, please?

MR. FOSQUE: Do you want to make a comment?

DR. CLARK: I was going to add another Winkler-type comment, that perhaps instead of one meter tube, we are talking about 10 to the 4th 100-meter tubes.

MR. FOSQUE: I guess I still have the question Dr. Costain wanted to bring up, but so we won't lose the thought Dr. Winkler made I would like Harry Peters to comment on that first.

MR. PETERS: Well, I think it's clear from the performance of hydrogen masers and – and many other standards, that the statistical properties at one second, ten seconds, a thousand seconds typically, at least for H masers, particularly in this range, are important. However, in the long term at about 1000 seconds and beyond, we are subject mostly to the environmental problems, the sensitivities of the device to the environment, magnetic field, and various and sundry things. And that was one of the main efforts of the NASA research at Goddard, was to identify these and as well as to try to illustrate operation of ground-based hydrogen masers. I do feel that by testing and quantifying the systematic sensitivities that we should be able to identify those things which are causing us to, say, flicker out, is the usual word; but I really don't believe in that idea of flickering out because typically I think of this as saddle point, and that is where the systematic things begin to take over and typically so we always see a low point in the curve.

But by changing the temperature or magnetic field of the maser, it's being done much better now than we really did or should have done when we made our hydrogen maser. This type of thing can give coefficients and quantify the systematic sensitivities and perhaps show us how we can get down into the 10 to the minus 16. I don't know if I answered that exactly in the right way, but I think this is a very important area of research.

It's important also for cesium or any other standard I think that we can likely identify the systematic effects, and that such things as just a random measure of frequency instability without identifying what its physical cause is may at least occur at a lower level than it does at present.

MR. FOSQUE: Thank you, Harry.

I would like to take Dr. Costain's question, and then perhaps any comments generated from Harry's.

DR. COSTAIN: I think it's probably, better that I comment on what Harry Peters says, in reference to the National Standards Laboratories on primary cesiums.

We are beginning to think perhaps it's time to take the primary cesiums into the field or at least get online with communications and navigations systems from the Standard Laboratories.

MR. FOSQUE: I want to take Mr. Allen's comment or question first, then go to Dr. Winkler.

DR. ALLEN: The point that Harry Peters makes I think is an extremely valuable one. The long-term flicker floor as it's sometimes called I think really is almost always environmentally induced. If you can characterize this, it's an extremely important thing.

Along those lines the comment I really wanted to make is that if all of these very nice hydrogen masers that have long life, that exist at these various observatories, could be tied into the international time scale, this would be very valuable in understanding their long-term performance, what kind of environmental sensitivities and drifts they have.

I think this would do two things. One, it would help us to understand their long-term performance. It would also help them when they go to decorrelate the data because they are synchronized and they know where they are in time; it takes less computer time to search to know where they are because all of the clocks in the system, as it were, are synchronized to the same scale.

I think there are those two advantages.

One further point that I would like to make is that I think there is a little bit of comparison of apples and oranges, when we talk about the same basis that Dr. Reder is, I think, referring to.

We must keep in mind that most of the cesiums out are commercial cesiums. It's really not quite fair to compare a production model with these hydrogen devices which are put together by experts. The people who are not production line operators.

MR. FOSQUE: Dr. Winkler; Professor Ramsey, Dr. Winkler defers to you.

DR. RAMSEY: This was a minor comment on Allen's comment. It's quite true, this is a very major difference in comparisons; but the argument goes both ways. I think in many respects the commercial ones, those in commercial development, have many advantages. One of the things hydrogen masers have suffered most from has been that their has not been until quite recently any major effort. They have been essentially hand-produced ones.

In our lab at least experts do rather less well than the routine people. Since this is Naval Research Laboratory, this was certainly true when I was involved in radar work at MIT. Navy sailors could do a lot better with production radars than we could ever do in the lab when we were developing them.

MR. FOSQUE: I think Dr. Winkler wishes to speak.

DR. WINKLER: I completely agree with Professor Ramsey about that. It is a two-way affair and there are advantages and disadvantages. But one — going back to Dr. Klepczynski's remarks, if you take a high performance cesium clock to the lab and immediately start making performance checks, you will be immediately disappointed. In the first couple hours there is a temperature shock usually involved and you have shifts in the various circuits imposed upon whatever performance disturbances you may have. So I would discount that completely.

But, Dr. Costain, how much would be a five meter long cesium if we would ask you to produce one, how much would it be?

DR. COSTAIN: We are going the other way, essentially two meter overall.

DR. WINKLER: Maybe you do get into the cesium business and you become a competitive producer. Honestly, do you have any estimate for the amount of money which was necessary to put together a large standard?

DR. COSTAIN: I think a long beam primary standard could be produced at the scale of a dozen or so for $120,000, $130,000.

MR. FOSQUE: Well, this has been very interesting and certainly enlightening to me and I hope to many of you in the audience.

I see that we have already overrun our time by a few minutes, and as much as we might wish to continue for a few more questions, I believe unless there is, as Professor Ramsey said, a really important question out there in the audience, perhaps we better fold up this panel.

I thank you, gentlemen, for participating in the panel discussion.

We will, I think we have a coffee break now; then we will resume in session 5.

(Recess.)