PANEL A

Relative Merits of Different Hydrogen Maser Designs and Their Evaluation

Panel Members

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PANEL A DISCUSSION

DR. RAMSEY: That brings us to the end of this portion of the Session. The next, I believe, is to be sort of a panel, or long table discussion. I think the tables are being set up now. I might make a couple of comments while we are getting this set up. In fact, while we are setting up why don't we start since we are running a little behind time. I would suggest for the first perhaps 10 or 15 minutes that the various panel members ask questions amongst themselves, then we will throw it open to the floor for questions.

Will the speakers then please come forward.

To start this, since I think the first initial discussion will be amongst the panel members I think I will take advantage of my position as chairman to ask two questions. Since I probably will be busy, I'll think of other things as we go along.

The first is perhaps more of a comment and even a suggestion to Bob Vessot in his somewhat rounded shields which he pointed out he had to make the inner shield cylindrical and use some of the advantage of this for the purpose of being sure he gets a uniform magnetic field.

In fact, you can still do it with a spherically shaped shell provided you put windings on the upper portion of the spherical shape and with a constant pitch. So, if there is any great advantage it seems you could benefit from that.

DR. VESSOT: We have a torusspherical end so that might complicate the winding a little further and are uncertain whether the net permeability of the shield is uniform.

DR. RAMSEY: Independent of what the shape of the end or cavity may be, provided the pitch is uniform as you go up you get a uniform magnetic field.

The second question or comment, Dr. Desaintfuscien, on the, your version of how you measure the wall shift, namely, being able to put a plug in with a different one as opposed to the distorted shape.

Seems to me there you lost one of the big advantages of the distorted shape in that when you put the plug in, you have different portions of the wall exposed and consequently, you are not as free of the properties of the wall, one of the attributes of the others is that it is independent of the same wall coating.

DR. RAMSEY: Yes

DR. DESAINTFUSCIEN: But we can verify that the wall has the same properties by measuring the ratio of the wall – the two configurations and verify the ratio is the same as the one from the geometric - - from geometrics.

DR. RAMSEY: But I think at least what we have found in some of the earlier ones that let us be interested in the variable shape has been the fact that, well, if you do it, yours is an intermediate kind of wall. We have done it with separately coated bottles, when you do that one time and another we find there is a variation from one time to another.

I think it is a suspicion, not a confirmation, that you get some degree of variation even within different portions of a single wall, so it is clearly better in that regard than totally separate decoding processes.

DR. VESSOT: Perhaps the first thing might be to say, why the torusspherical shield in the first place. We found, it was an accident, most good things are. We had to make a torusspherical pressure vessel which in our search for light weight we elected to combine with the magnetic fields.

All I can say about that idea is, don't do it.

But we had torusspherical magnetic fields and wanted to find out what they did. What we simply did was measure against equivalent sized flattened shield and found we had about 40 percent less inhomogeneity in the longitudinal shielding factor combined with the transverse shielding factor.

Transversely where you are looking at the field lines going sideways through a cylinder you have very good, closed and continuous path the lap joints don't bother and one has, I think, intrinsically a better shield.

The sharp corners on the covers and reluctance of the gaps of the joints themselves is the main problem. Generally you have almost 20 to 1 less shielding factor in the axial direction than in the transverse direction.

The other improvement, I feel, is that the flat plane of an oilcan-like end is subject to much strain and this degrades the shield very much.

So, by eliminating the sharp corners and keeping a radius of curvature, you have a far more structurally acceptable configuration. Just handling these things, it is obvious that the thing is really quite strong. So that was why we tried it.

Of course, the next game is how do you make the field uniform inside, and this is the real question.

DR. RAMSEY: In fact, one of the things I found very pleasing and impressive were the actual large number of new improvements that have come within, or even new ideas for improvements that have been coming in the whole field within roughly the last couple of years.

You might think it would be by now saturated. I think it is apparent from the discussion there are really quite a number of these.

My own belief is that this gives a lot of mutual stimulation to each other. In many cases I think they can be adopted.

For example, in the case of the passive hydrogen maser which, I think, is a very valuable development to have, I think many of the advantages you get from that can indeed be taken over in the other and likewise in the vice versa direction.

There are, however, still some very real differences, I think time will tell which is the best. But I think whichever proves to be best in the long run, I think that the development of each will be extremely valuable because of the particular features pushed one system versus another and then recognizing the value of that. Frequently you can do the equivalent thing, a good example being, I think, the much greater planning of use of various automatic tuning techniques.

You get some advantages doing it with this, if you do it you can get many of these advantages back in the passive, in the active maser.

DR. WALLS: Well, in the passive maser it is clear it will not be a good device from, say, ten to a thousand seconds because it comes down as square root of Tau because it is incoherent.

So, I don't think it will be useful in the short term as phase comparisons, for example, as the active masers have done exceptionally well. But in the long term for a stand-alone system where you are not able to do auto tuning against another hydrogen maser, and with the present limitations we have on quartz crystals, I think that the passive system will do much better because of the active control, and you can make the storage time very, very long because you don't have to oscillate.

So, you can run down the storage times, you get so little power out it doesn't oscillate so you have a narrow line, cavity pulling goes down.

DR. CRAMPTON: Just a minor point, as a diagnostic tool since you can make measurements now with very much greater atom confinement, then you have a chance to look at surface properties, relaxation on the surfaces.

DR. REINHARDT: One comment I would like to make is that I think the real power, the passive technique, is that it frees you from geometry limitations. I think where it will really pay off is in terms of variable volume masers and experimental devices which, since you have the stability, you have demonstrated, you can build it any size or shape you want and not worry about oscillation.

You don't need two cavities, you don't need the amplifier in between. For any of these masers, for example the Concertina maser, the external bulb maser, where I showed we were pushing the limit of filling factor we can use the same technique here.

I think it should always be kept in mind it is sort of an ace-in-the-hole that you have, if our external bulb maser doesn't oscillate, we can always use it as a passive maser.

DR. VESSOT: We shouldn't be too mesmerized by the concept of a single stand-alone maser. I think anyone who relies on one clock is on terribly dangerous ground. In fact, even the British Navy in its early days would never sail without three chronometers on board if they were serious about getting there, and they were.

But to acquire more than one maser, I realize, may be a terrible economic problem.

DR. RAMSEY: How many masers did you have in your shot that went up in the air that you used for testing relativity?
(Laughter.)

DR. VESSOT: That was a budget constraint. We had more than one on the ground, however. That was a constraint we were not in favor of, but nonetheless had to live with.

DR. RAMSEY: I think, though, there are many instances when you will, in fact, have that constraint for one reason or another, so I think there are real advantages to having them both do well alone and do well the other way.

DR. REINHARDT: One comment, there has been some comments about the reliability of masers but the masers proved more reliable than the tracking stations in this case.

DR. VESSOT: I am glad you said that because you guys at GSFC ran the tracking stations. I would like to investigate the past history of some equipment including the individual characteristics of a certain circuit breaker. But I must say the people who ran the station were able to find that circuit breaker in one minute, eight seconds from the time it tripped, which I think is a track record considering the number of circuit breakers involved in the very complicated station.

MR. PETERS: I wanted to comment first on the magnetic shielding.

One of the things everyone has observed in early hydrogen masers, those with very large holes in the shields. is that they have very large inhomogeneity and require trim coils and so forth.

As we have experimented with masers. smaller and smaller holes in them, it appears most of the inhomogeneity is due to the large holes in the shields.

In the later masers which we have we do not even need trim coils. They have no effect upon the ability to go to low fields, and, of course, this also implies a terrific improvement in the inhomogeneity when you have small holes in your magnetic shields.

It goes as a very strong power as the distance from the bulb to the shields and I think this is probably, in the practical world. assuming you have enough shields outside, inhomogeneity is more important than any fact of life, in magnetic shielding.

I did have one other comment. but it is not directly related to magnetic shielding. Several people have mentioned film used in hydrogen maser. Teflon film, it's been used in the big box maser, it's used in several conceptions of the TE11 mode maser and used in the variable volume maser.

I think one of the most exciting things and it hasn't been really documented or published, but it's becoming so well known, I would like to mention at this time, and because it is being used in a hydrogen maser design which hasn't been published I would like to mention it because what is so exciting in conjunction with the measurements on wall shift which Victor Reinhardt has made is that the film material can be made into a bulb, a cylindrical bulb. This has been actually measured at Goddard Space Flight Center originally right after the variable volume, Concertina maser was first designed, what has been measured is that the pulling. due to a one-mil Teflon cylindrical bulb is about 300 kilocycles.

This is a factor of 200 less than the pulling due to a quartz bulb. If one designs a maser where the thin bulb is at the peak of the Bessel function for the electromagnetic field in the cavity, you can reduce further any geometrical variations due to a film by at least a factor of a hundred if you calculate the slopes.

So one can get on the order of ten to the fourth less pulling and less perturbation due to a film bulb in a maser than you can with a quartz bulb.

Of course, this is a very good solution to dialetric pulling, and thermal effects and things like that. There was one other point in conjunction with a factor of four or less wall shift as measured by Victor recently and this is quite exciting as it bears upon one aspect of wall shifts which has seemed possible to me. There is a good chance that much of the wall shift is not due to the Teflon molecule chain or due to interactions itself as such, but due to impurities in the material or exposure of the wall through the Teflon film.

This tends to support this conjecture, also lends credence to the possibility of achieving much longer storage times with a film bulb. Hopefully a homogeneous bulb might have much more uniform properties, certainly — than a sintered on material.

So, I think this is an exciting development and can possibly contribute to a good factor of improvement in hydrogen masers whether they be passive, TE111 or TE011 and I am still somewhat in favor of a TE011 maser at the moment but that may change.

DR. REINHARDT: I would like to make one comment on the film bulb.

What is also very exciting about this is that because the pulling is so much smaller you eliminate one of the big production problems with hydrogen masers, that is, you can't get the quartz bulbs reproduced well enough so you have to do some last minute trimming on the cavities.

From Harry's calculations it looks like if you could hold the tolerances on machining for these cavities, there is no reason why you couldn't spit these cavities out one after another and just put the bulbs in and not worry about trimming them up after the fact.

MR. PETERS: It is also very light, whereas quartz weighs 800 grams, this weighs less than a gram; it is very applicable to lightweight hydrogen masers.

DR. RAMSEY: Other comments or questions from one panel member to another? Including to himself.

DR. DESAINTFUSCIEN: I would like to make a comment about, "The warped shape is not due to Teflon." I'm not sure you are right. I found that Teflon on wall shift – closed area at peak temperature range. Perhaps it is due to impurity, but it is a property of the Teflon.

DR. WALLS: I think that is fairly clear from measurements that have been made that show that phase transitions in the Teflon are related to changes in slope, in relaxation, in other things. So I think that is fairly clear, that Teflon has to play a role with – perhaps a major portion of it, but not all of it.

DR. VESSOT: I would like to strengthen Dr. Desaintfuscien's argument, by saying that every measurement we have made of wall relaxation seems to come out to be about 6 x 10 to the minus 5 probability of loss per collision at the temperature we operate, which is dangerously close to the data you get. It's quite independent taken on many separate instances.

So, I believe the properties of Teflon intrinsically are what are governing. This doesn't say the end groups of Teflon cannot be considered a property of the Teflon, but if you consider that to be impurity, I think you have to define pretty well what the Teflon really is in the first place. I wouldn't be surprised if the end group did contribute, and the thought now is to polymerize the material in place.

There is even the remote possibility – and this is really very remote – that the Teflon could be made without end groups, by simply joining the things into hoops so they would be like Spaghetti-O's and lie on the surface in some random manner like a pile of rubber inner tubes all over the place on a garage floor.

DR. REINHARDT: One comment I would like to add to that is that in the type L film we use to make these measurements – the principal difference between it and FEP film is the higher molecular weight, so there are fewer end groups. The correlation with the change of state, if it is due in fact to the end, or impurities, could correlate with dimensional changes in the latter that occur.

You could expose more or less of the chains or spread the ends out as you change the density of the Teflon, rather than a direct effect of the Teflon itself.

DR. CRAMPTON: Looking ahead a little bit to things that don't work yet but might work in the future in that line, it seems to me possible, with variable temperature of hydrogen masers like the one developed at Orsay, to look at quite different kinds of wall coating materials. After all, the length of the Teflon chain doesn't do you any good except to help the thing lie down on the wall.

There are some advantages to going to other kinds of materials which you can't form into walls unless you go to lower temperatures. We are working on this ourselves, and I'm sure there may be some work like that at Orsay where they have already a low-temperature hydrogen maser.

DR. RAMSEY: Are there any other questions that members of the panel want to address to each other?

MR. PETERS: It might be worth mentioning, since all the masers which have been described, I think, today, and mostly in the past, have used hexapolar state selectors, which are ideal when you have maximum efficiency of the state selector is far from the bulb, that the latest designs at Goddard have used quadrupole state selector.

Under certain circumstances these have a very great advantage. For example, when you have small holes in your magnetic shields, and you can put the state selector within two or three inches of the bulb itself – (it's five inches in some of the NASA masers that have already been built) without any disturbance you can measure. The atoms captured an a quadripole state selector nearly all enter the bulb due to the geometry.

So you don't need to worry about exact focusing, as happens in a hexapolar state selector.

A quadripolar state selector as designed, has much greater pumping speed sideways from the pole tips because of their design, but most important there is at least a 20-percent improvement in the peak magnetic field.

But more significant than that, particularly for small size hydrogen masers, is not the focusing properties but the defocusing properties. In a hexapolar state selector, the magnetic field and gradient goes to zero as you go to the center of the focusing magnet. In a quadripole it is a constant from the center out.

Therefore, you have an exit angle, you are defocusing the wrong state atoms, very strongly, whereas if you put the source and state selector close to the bulb with a hexapolar system you can get a significant proportion of other state atoms into the bulb with that particular geometry.

So, for small hydrogen masers we do have extremely high efficiency in utilization of the hydrogen due to going close to the bulb, but a quadripole state selector is very desirable under those conditions.

DR. RAMSEY: Well, I think it's becoming apparent that the members of the panel can keep going until 12:30 just questioning and making comments amongst each other. I think maybe I would like to summarize my own personal view in reaction to the meeting this morning and, in fact, in contrast to some meetings that have occurred in the past.

I am just terribly impressed by the sort of amount of fertile developments that are going on in the field from all sides; also, particularly impressed by the extent to which most of these can be taken from one to another. I mean, each of the kinds of masers, or passive and active, each have certain features, each I think can benefit from some of the improvements that have occurred in the other.

I think there is great optimism of considerably and very marked improvement in the period to come and also shows the benefit of the fair number of people working on it. I think most members of the panel probably agree on this, so we can end on a statement with which we can all agree, and I think at this stage we should throw the discussion open.

Actually, one question was submitted in advance. Dr. Reder would like to ask a question, so to make sure he has not forgotten, I would like to call on him as the first question.

"Considering expected 'mean time before failure' cost, status of cesium techniques, environmental effects and requirements, what is the justification for large H maser effort?"

DR. REINHARDT: I will stick my neck out a little on this one. As Dr. Reder knows, we did present some data on mean time between failure with the NASA NP masers which looked quite favorable and compared quite favorably with the cesium standards.

I think the justification for expenditure is really that H masers, in certain ranges of time intervals, can outperform cesium masers in terms of stability. Obviously, cesium standards are smaller. Right now they are cheaper. You can buy them off the shelf as production items; hydrogen masers you still can't. Price might get lower if produced on a production level.

But in terms of short-term stability, I think the VLBI people show that they just can't do with anything but H masers. I think for long-term stability, too, because of the smaller line width, that they will outperform cesium.

I have heard a lot of comments about hydrogen masers with poor long-term stability, but JPL has demonstrated 1×10^{-14} stability, out to 10^6 seconds.

I have not seen commercial cesium standard which will do this. That competes favorably with the best laboratory cesiums at NBS or the other national labs, and on full ensembles where the cost is quite comparable. So I think in terms of a cost-to-performance ratio, that justifies the expenditure.

Does that somewhat answer your question?

DR. REDER: Well, I wonder, is there anyone here from the cesium manufacturers who would like to say something?

DR. RAMSEY: There is a question there.

DR. C. COSTAIN: This is not exactly in the line of Dr. Reder's comments, and I also would like to question the fact that cesium standards are always smaller.

But one of the things that worried me in the presentations, with rare exceptions was, it seemed to me, a complete lack of indexing in the measurements. In fact, a stability measurement of parts in 10 to the 15, I question what is it against. We have measured a pair of hydrogen masers independent with autotuner will keep time to a nanosecond a week, but we know in comparison with our primary standards that they were both drifting several nanoseconds a day.

We have measured it, it doesn't say it happens in all H masers, but we have on one of ours very carefully indexed data over 18 months giving six parts in 10 to the 13, decrease in frequency per year, about two parts in 10 to the 15 per day, monatomic drop. We don't really know why.

Over five years it has been three parts in 10 to the 12.

DR. VESSOT: I understand, Cecil, what you are saying. We agree the hydrogen maser in fact has some intrinsic long-term effect with the bulb. You have seen it, I think, very vividly, and I think the causes that give yours the magnitude you have are probably well understood by you.

I think it's a question of how they are built and how they are outgassed to avoid contaminations that lead to whatever is going on in the bulb. This long-term effect, I believe, is real and I understand that Dr. Winkler has also observed it.

So, as far as long-term stability, I, too, question the accuracy over a period of time in the order of years. I believe we will have a systematic change. No amount of autotuning or massaging is going to change that. That is a property of the interaction of the bulb and hydrogen atom.

As far as the other question of having the masers in different environments, I believe there have been enough experiments between masers to say that the systematics between one environment and another, I believe, have been removed.

I don't want to steal Alan Rogers' thunder, but there are enough tests, I think, that say it's unlikely that in the 10^3 to the 10^4 second domain we are dealing with correlated phenomena. In the very long term, yes, I think definitely that the wall coating is going to have some effect, and we'd better learn to either cope with it, eliminate it, or live with it.

DR. REINHARDT: I would just like to comment on the last statement before we move on to a question.

I think the wall shift drift or change is a limitation, but I think that is where the importance of the variable volume maser comes in. All the other parameters are evaluatable in the hydrogen maser as in primary cesium. The wall shift was the last one.

In fact, at NASA we are planning to do three- or four-year experiments where we will continuously evaluate our field masers with the Concertina maser on the part in 10 to the 14 level.

But one other thing I would like to comment about this wall shift drifting with time is that Harry Peters did do a three-year experiment in which he did measure several masers against each other and TAI and found they reproduced over three years to within two parts in 10 to the 13. The resolution there is limited by what happened to TAI in two years. I think that is probably a question that has to be answered.

It's the same problem you get when you evaluate a primary standard. At a certain point, you have to use theory to make evaluations. You cannot just build two identical standards and put them at separate parts of the world. You should, as a check, but you must rely on theory at a certain level.

The problem that you always encounter when you build the most stable standard: What are you going to measure it against?

MR. PETERS: It seems to me that the wall shift drift with time is a problem of atomic contamination, which is not necessarily inherent. It's a function of the design.

DR. RAMSEY: May I make one comment, too? I think some of these new proposed designs, for example, being able to use less beam intensity – can affect this problem, which has been a relatively new one.

DR. WALLS: I think that is very clear. We, in the past, have not had a chance to measure hydrogen masers against the uniform time scale that was sufficiently smooth that we could evaluate these things down to a part in 10^{14}. As we get better time scales, and have more experience, and look at aging (perhaps of the wall shift if it is real as a function of temperature) as a function of cleanliness and other things, we will have a chance to evaluate it.

In order to do so, it's imperative that we have a smooth time scale that we all agree on that we can make measurements against.

So, I would like to encourage everyone here and in the audience that hydrogen masers that are run for a long time be reported and referenced against TAI so we can try and start to make a time scale which is quite smooth.

The other comment is that the measurements in long-term have been made with the active masers, and Victor Reinhardt, I think showed rather nicely that even with autotuning there is an offset which is proportional to the drift of the maser over the attack time of the autotuning loop, whereas with the passive maser we can, in principal, make the attack time not 1000 seconds, as it often is in the active masers, but a millisecond.

Currently, we are running at 10 seconds. And across 8 days or 14 days one has only a limited amount of data and one shouldn't stick his neck out. But I think we can still say the drift is exceptionally small in our present prototype, and I think we will find it even smaller.

DR. RAMSEY: I think Dr. Winkler has been trying to get the floor back there.

DR. WINKLER: I would like to continue the discussion with Mr. Reinhardt.

You gave as justification for the massive effort and support for H maser work that it outperforms cesium standards in a large range of talents, and I think that is a very valid argument, and I completely accept it.

But how about the superconducting cavity masers which outperform the H masers in the same region? Are we giving commensurate support to that effort?

DR. REINHARDT: One statement I want to make: I agree with you, and I think superconducting cavities should be supported.

First of all, I would like to say I don't think their is massive support for hydrogen masers. If you look at the support for cesium over the years, it would be comparable. How much is the support in rubidium in dollars all commercial labs produce? There are still only a few labs in the country doing work on hydrogen. You have got them all here.

Another comment on the superconducting cavities: Yes, I think a lot of support should go into that, but the superconducting cavity has a basic limitation, that it's no good as a primary standard. It's still just basically like a crystal. It relies on dimensional stability and you cannot use it as a primary standard.

I think with the variable volume techniques you have the possibility of getting a primary standard with part in 10 to the 14 level accuracy.

DR. VESSOT: The point about aging in the case of walls is that it is quite likely they will age of themselves in that if we put a material down that is basically amorphous, I see no reason why, in time, it couldn't crystalize.

The question is what happens to the wallshift during this process. I don't think we know. We do know, however, that there is a difference between amorphous and crystalized teflon and that it is substantial.

So maybe the thing we ought to do to get out the aging is to have the Teflon in the least energy condition which is to have it in crystalized form.

The other thing I believe is quite important is that ultraviolet light, however it gets into the bulb, can be quite dangerous to the teflon; and we all know that the molecular binding energy is well below 1216 angstrom the principal U.V. component from the hydrogen dissociator.

So I suggest maybe we should use the stopping disc as an ultraviolet dodger.

MR. PETERS: If I may just say one sentence, it is not as interesting as relativity which I am also interested in; but I think there is a misconception when you speak of ultraviolet light interacting with the bulb from the source, because the area which it hits is less than a part in 10 to the 4th.

If you change the entire wall shift by that fraction, the total effect could be no more than a part in 10 to the 15 so I don't believe it is really a serious consideration whatsoever.

DR. VESSOT: It can be reflected.however, and the deterioration occurs at the place where the atoms first bounce.

MR. RUEGER: Rueger from Johns Hopkins.

I would like to pose a question to the general panel of how we can make intercomparative measurements of the various designs so we will know where we stand with who is doing the best job in the various environmental effects and in the overall performance.

I wanted to pose the question to the panel of how we might intercompare these various standards insofar as their sensitivities to environmental effects and other parameters that might let us understand better how each is working and where it is working best.

DR. RAMSEY: I will make an initial comment on that one which is I think indeed these kinds of comparisons are coming forward and should be coming forward fairly soon. I think one of the reflections of the fairly small amount of support that has been available up until the recent period of time is that here haven't been many masers for which there could be these intercomparisons.

It is only in the last period of time they were there.

We at Harvard have indeed had some, but also had to do everything with graduate students who were also looking at thesis and certainly weren't primarily directing themselves towards intercomparisons of this nature.

I think now with the various organizations that are developing, I think there will be many such standards on a high quality and you can really find out what this is.

I guess the answer is they should be indeed intercomparing and getting together with the intercomparison people.

DR. REINHARDT: At NASA we are going to be getting a frequency standard and test facility so we can make this kind of long-term test comparison.

I think one of the problems in the past is that since BIH needs long-term data without interruption and we are limited in the number of masers, we just can't leave them alone for two or three years; we haven't been able to do this.

But at NASA we hope we are going to have a three-year or more experiment on long-term stability in conjunction with the Concertina masers and hope to contact you sometime in the future about reporting our data to the TAI.

DR. VESSOT: That is a role for the Bureau of Standards and these people ought to be the arbitrators of whatever happens. They have done so in the past, and should continue.

DR. WALLS: We hope to get two of the passive design hydrogen masers on our time scale within the next year or a little more. And so we fully intend to do that. But it has been really a limit in terms of funds.

We haven't had funds in the past to make hydrogen masers, but developmental work had to be done on some new concepts.

We would like very much to have masers from other laboratories come to our place and sit in a quiet corner and let us make measurements, but they haven't been offered because they weren't available. And we haven't had the money to purchase them.

Perhaps that is something that ought to be taken care of.

DR. CLARK: A comment to Gernot's question earlier and in part extend that to another question that may impact a little on our thinking of the user's standpoint this afternoon.

Much of the dollars that have gone into H maser work is because various users have required H masers now in order to be able to do their program. Tracking networks, the astronomics community, they need these things now. It is not we need the boxes five or ten years from now after additional research goes on and another technique to find out which one of those two ends up being the best.

Much of the driving force, because of that other funding, has been to get some of these masers out to certain of the critical stations to do these various semioperational or R&D programs that are being done at those stations. Because that is the place where most of these masers are in fact now located, and techniques like very long baseline interferometry, offer good intercomparison techniques for comparing frequency standards which may be located around the world, there exists the possibility of using the VLBI technique as part of the intercomparison which should be then fed into the BIH.

So this is a place where I think this morning's discussion and this afternoon's discussion really overlap. As one of the users, to be armed for this afternoon, I would like to ask one question, though.

We find even hydrogen masers aren't good enough for many of the things we are doing. What are the ultimate limitations on frequency standards? Even using combination techniques of masers and the cryogenic cavities or some of the additional development work that is going on in the laboratory now, what stability levels from the one second out to hundred thousand second levels can we expect of frequency standard performance in the next five years?

DR. RAMSEY: I can make some comments on it. Others might want to make some also.

First place, I think as far as the stability, certainly one limit you eventually run into in most devices is second order broadening from the second order Doppler shift.

This sort of comes in at regions of parts in 10 to the 15 or so depending on how accurately you make that determination. There are a couple of bright ideas that may, in principle overcome this by trapping techniques with ions, and so-called resonant cooling.

Maybe Dave would want to speak to that a bit or someone else here. I think these devices are down to that limit now. I think there are, actually, with the kinds of things that are currently going on including the passive maser, I could conceive it getting down there.

I think for the shorter periods of time, it is quite clear that it is hard to beat power for getting stability. The ideal device for that is the superconducting cavity. I think this is an absolutely superb technique for the short period of time.

At one time I had thought about ways of making the H maser also function down there by having beams coming in from all sides, huge quantities of hydrogen since it is a matter of power. It is clear that is not the best way and I think I would agree with the comment that you want to be a little specialized with what you want to need.

On the other hand, for longer periods of time it is my impression, and it is reenforced by the discussions and papers presented today. that there are quite a large number of opportunities for major improvements in the basic active and passive hydrogen maser techniques over the next year or two up to where you start to get really into serious problems with this rather fundamental limitation that affects all things, lasers and everything else, of the second order Doppler shift.

DR. WALLS: Let me amplify the comments of Professor Ramsey slightly.

In short term it really is a matter of power. If we compare the H maser with, say, a line width of one hertz and a power of maybe a 100 minus 85 dbm or so, 10 to the minus 11 watts, with a superconducting cavity which at X band has also a line width of about 1 hertz, but its power is a milliwatt or can be as large as a milliwatt.

So you can beat KT a whole lot easier and one might expect stabilities in the 10 to the minus 16 level to be routine from perhaps a few seconds out to. I don't know, a thousand seconds or more.

MR. D. WINELAND: A couple questions, on hydrogen.

One was on surface. As I recall, Professor Ramsey 10 years ago talked about using different surfaces, for instance, lithium fluoride. Maybe some comments on different surfaces besides Teflon can be made.

The other question was to solicit some opinions on absolute accuracy of hydrogen masers. I presume the limitation is the wall shift. And, whatever it is, maybe some comments on what future accuracies of hydrogen masers could be.

DR. VESSOT: Dave, I think the limitation is less likely to be the wall shift than it is to be the knowledge of the absolute temperature of the atoms, and the determination of the second order Doppler shift.

I think the wall shift problem can be beat into the ground by many, many techniques, some of which we have seen.

With relation to the question of ultimate stability, I think it is entirely a question of signal to noise. I just made myself a note, you could couple up Niagara Falls to a monstrous magnetron and you would get superb stability, very short term to be sure.

These are questions I don't think have an answer and should only be responded to in the context of the use to which the device is being put. In my opinion, right now VLBI, as Professor Clark mentioned, has a clear and pressing need, and I don't think we could have done a redshift experiment over two hours without having a device that would develop stability at a time substantially shorter than two hours.

I really think if you should look at the application and then decide what kind of an animal you are going to need in order to cope with the problem.

DR. ROGERS: I am Allen Rogers from Haystack Observatory.

I would like to ask the panel a question about having a high flux mode in the masers which would not necessarily be used a high fraction of the time. For many, many applications you only need very good short-term stability for a very short period, like maybe a few hours experiment, even, which might be carried out, say, once every two months, or maybe a couple of days, say, every two months. Maybe we could afford to have a high flux mode that we could use for special experiments without impacting the lifetime of the maser.

Could you comment on how much flux would be needed to make the maser as good as the superconducting cavity oscillator at a thousand seconds? And is that technically feasible?

DR. VESSOT: I can't answer the second question but I believe it would be difficult to get at 1-10 seconds.

In most of the equipment that is in the field, certainly in the case of the ones we have produced, there is in fact a switch with two settings for the hydrogen flux level. If you need the stability you turn up the wick, and, get more power out.

I don't know how much more power you would need in a maser in order to compete with a cavity. If one increases power output, one automatically diminishes line width so you're really trading off short term stability against the long term stability.

It is really a question of how long do you want to integrate the correlations that you're seeking, and what level of stability do you expect. And it is clearly going to be some kind of optimum solution for each kind of oscillator.

DR. RAMSEY: Also, I think it is an excellent suggestion, and typical of the fact that the sort of what masers that have been handed out have usually been incidental to some other purpose.

I think you certainly could do even more explicitly than was done, maybe by accident in some of your masers, by making an adaptation by deliberate design. That you could, indeed, adapt them to have a mode which could be pushed to the most favorable in that direction.

My impression is that with that, and for periods of the orders of thousands of seconds, this could be reasonably comparable to the best obtainable with the others. If you want it on the other hand for periods of a tenth of a second, it's got to be a pretty formidable switch.

I think there, there is no question that for periods of time on the order of tenths of second or even a second, this high stability ought to be achievable from the superconducting cavity or in certain cases even from a copper cavity, that has a lot of power too and for a short enough period of time, can be a very good one.

But I think your point is excellent, I think people who are particularly planning things, especially for some of the uses such as Haystack where I understand full well the desirability sometimes for quite long-term stability, and sometimes for sort of medium term stability. I think you could do a great deal by making the design bear that in mind, and I think it could be a more multipurpose device without sacrifice.

DR. REINHARDT: I would just like to add my comments to this.

You mention the 100 to 1000 seconds as short term. But I don't think any of the masers are limited by that kind of short-term noise that could be decreased by increasing the power at 100 to 1000 seconds.

The JPL maser which runs at tremendous power compared to ours you can say has comparable results.

I think you're limited by your multipliers chains, and other things in that range.

You wouldn't get any advantage except, as Professor Ramsey pointed out, for about a tenth to one second.

DR. RAMSEY: In passing, I think there could indeed be a bit of extra work on the electronics.

In many cases the electronics has had to be rigged up with certain limitations, costs and otherwise, in mind.

DR. CRAMPTON: I would like to comment on these questions of development on the one hand and ultimate obtainable accuracy on the other. And whether it will be possible to get these devices together to see who is doing how well.

It seems to me that the development effort has been, at some places that have been playing with them for the physics, namely Harvard, Williams, and a few other places.

Beyond that, development has been done primarily by clock people. It seems to me that where the development is needed is in between there where people are willing to go back and work more with the basic physics of how you make a better standard. I think the best job of that has been done at Orsay. But I think the effort is needed to go back, and Teflon is terrible stuff. I think more work needs to be done on a basic level. Frankly, my personal view is that that kind of development work and cross-comparison of how well you are doing ought to be done at the Bureau of Standards.

DR. RAMSEY: Again, I would like very much to emphasize this.

I am really a very great believer and have been for a long time, that Teflon isn't necessarily the best substance, particularly the kind of Teflon we normally put on.

On the other hand, for a long period of time there was simply no one to work on it and it's not easy to persuade a graduate student even such as Stuart Crampton to delay his Ph.D. experiment a couple of years while he investigates various forms of Teflon and other materials. I think now there are places such as Orsay and now I think the Bureau of Standards and other places working on it where I think this kind of development - I think it has a great future.

It's to be remembered, that Teflon was essentially the first thing ever tried. So there is no reason to believe that we are all that clever just because frying pans are made that way.

DR. REINHARDT: I think something we can all agree on is that no matter what field we are in, whether hydrogen maser or cesium or crystals, we all need more money.

MR. ENGLISH: Tom English, from EFRATOM, California.

One of the obvious requirements for military applications of frequency-type standards is nuclear radiation hardening.

I would like to ask the panel perhaps to comment on what they think might happen for example to the wall shift if you had nuclear radiations present.

I don't know if anything has been done on this or not, but it's certainly a problem all standards have to look at least in some point of the development.

DR. WALLS: I won't comment on the Teflon itself because I don't know that much about it.

But nuclear radiation clearly is going to cause major structural changes, perhaps. So active cavity control I think would be essential. If it still worked, then you could worry about what happened to the Teflon on the wall.

MR. PETERS: I don't have the data, but I think there have been some discussions on the effect of nuclear radiation on the wall material. I am thinking primarily of the wall shift.

But I believe the flux rates you anticipate, we don't think it's a large effect, but it certainly needs to be measured.

DR. VESSOT: Well, there was a quantitative estimate made, I think, some time ago.

I think Dr. Winkler was responsible for its inception, to determine levels of nuclear radiation from the normal environment would be expected inside the maser. I believe the Naval Research Labs were involved in this, too.

They came to a conclusion that the radiation over five years, using what they saw as shielding materials, namely the molypermalloy shields and quartz or whatever the bulbs are made of and the cavities, that they felt confident that this would not cause significant drift at the five year level.

DR. ALLEN: Dave Allen, National Bureau of Standards.

Good accuracy ultimately translates to good long-term stability, looking futuristically at hydrogen, if in fact Bob Vessot is right and you can beat the wall shift down into the dirt, would it then be good to beat the second order Doppler down by looking at the low temperature cavity type materials?

DR. RAMSEY: My first comment on that is yes, by all means.

In fact, there are several advantages you could have with low temperatures.

You just have to be sure that you aren't getting the atoms sticking to the walls too much.

But I think there is a great deal of research to be done in that.

One graduate student of mine, Bob – Paul Zitzewitz, that Paul Zitzewitz, did some studies of temperature effect on wall effects.

Actually, one of the things we wanted to do was go to really low temperatures, but there simply wasn't the funds for the work.

DR. CRAMPTON: There has been some work done at lower temperatures at Orsay and it shows that as you go down some to liquid nitrogen temperature, for example, there are some real advantages, things work okay.

If you try to go very below that you get into trouble. But liquid nitrogen is a very attractive temperature, it's very good to stay at. I think more work needs to be done on that.

DR. DESAINTFUSCIEN: Teflon becomes a real solid at temperature below 200 K. Perhaps it is possible to create another kind of Teflon whose properties would be different.

DR. RAMSEY: In this connection, as soon as you can afford to do anything in the way of going to very low temperatures you open up many possibilities of totally different surfaces.

In fact, practically from the beginning even almost before we tried Teflon, my definition of the ideal surface for many purposes was a solid helium surface at appropriately low temperature because this is something in which there would be very little sticking characteristics, and you would get it in a very pure form. I think the solid helium might be a little hard to achieve. I would be very optimistic about things like argon as a possible surface material. Are there any other questions from the floor?

MR. CHI: I wonder before you close the panel discussion, could you leave a clear and optimistic prediction of the performance of hydrogen maser, assuming all the problems which have been identified, that have been highlighted, what would be the performance and who are the people who might be interested in doing those kinds of activities?

DR. RAMSEY: In the first place, I think you should give realistic estimates and optimistic estimates.

DR. VESSOT: We are going out to a big limb, but we are betting we will see data consistently below 1 part in 10 to the 15 for average in times beyond 10^3 seconds. We have been tantalized with data that has been at the 1 in 10 to the 15 levels occasionally.

DR. WALLS: How long?

DR. VESSOT: That challenging voice was Walls saying for how long and the answer is I don't know how long but I suspect you would have to go to about 2000 to 4000 seconds to get it. It will probably go roaring up right afterwards, too.

MR. PETERS: I would like to make another independent estimate of this lower limit.

I really feel that parts in 10 to the 16 which may be another way of saying better than a part in 10 to the 15, but I think we will be closer to part in 10 to the 16, possibly better than that. I don't see why. But it's the long-term systematic phenomena we're being limited by, of course, this is all a function of what averaging time we are talking about. And this is right where the limit is set now. But a continued study of these I feel should get us lower than Bob feels he will get.

DR. WALLS: We are counting on the Bureau of Standards doing for ten days and beyond in the very low parts in 10 to the 15. I expect to do a part in 10 to the 14 per year. We may not quite make it or we may be better. I think a lot of it's going to depend on what is really going on with the wall shift. I am not quite so worried about the second order Doppler effect, but I am worried about the long-term stability of the wall shift. If we have troubles, it's just more research. You use a different coating, use a different temperature, whatever. I don't see it's fundamental, but it takes time and money and people.

DR. RAMSEY: Does anyone else want to make a comment?

DR. REINHARDT: I think one question that has sort of been missed a little, one part of it that has been missed a little in talking about the Doppler shift, we have ignored some of the magnetic shifts and other problems we face. The real way to get better stability is narrower lines.

I think until you get some narrower lines that you might have some problem with parts in 10 to the 16.

It's the same problem with cesium and with all the standards. When you start to split these lines by 100,000 or so, you run into all kinds of systematic problems. If we can get a factor of 10 or more improvement in lines, then I think we can get parts in 10 to the 16.

MR. PETERS: I think part of my optimism arises because I think we may get storage times which are much longer, possibly with new materials or different size bulbs and we possibly can improve the line Q significantly.

DR. RAMSEY: Is that an answer to your question? You sort of have to average over these numbers, but certainly part in 10 to the 15 and maybe beyond that point.

Is there any other very important question?

More important than lunch?

I guess lunch wins in which case I would like to thank the panel members and the audience both.

(Whereupon, at 12:38 p.m., the meeting was recessed.)