PANEL DISCUSSION

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MR. CHI:

In a discussion period, usually there's not enough discussion unless there is some stimulation. What I have tried to do is to cover the three themes of the papers given today. The three themes are: the frequency sources; the distribution of frequency and time; and the application of frequency and time.

Since there are five panel members and we have about one hour's time, I would like to limit the time to about five minutes per panel member, after which there will be a short discussion. After we go through the series of presentations and discussions, if we have time we can go into other areas.

The sequence which I would like to follow is to give those panel members who did not have a chance to speak today the first opportunity. So I will first call Dr. Knowles. His will be in the area of characteristics of radio sources and how to select them for applications.

DR. KNOWLES:

I actually don't have any prepared discussion, so my remarks will be quite brief.

I think the means of selecting radio sources for VLBI applications is rather simple and obvious. You want a source which is a point source and which does not vary. There has been considerable work done in the field of radio astronomy toward selecting these sources. We have pretty well listed the sources by now: a list of quasars, which are known to be point sources over baselines of the diameter of the earth; and also lists of the spectral line sources.

It's interesting to note that it turns out that the earth is a very good interferometer for natural sources for both spectral line sources and for quasars. Most of these sources are, quite clearly, heavily resolved by the time you get to a baseline the length of the earth.

Now the problem with this is that for possible applications, such as navigation systems, you have a fairly small percentage of sources to work with. For example, with the water vapor sources that Dr. Johnson spoke of, we expect that only between 10 and 25 percent of them will be visible over baselines of the length of the earth. And this definitely limits the utility of such sources and is a factor which hasn't quite been brought out in the literature on station locations, navigation systems, and related topics.

I might mention that, in the case of water-vapor sources, we discovered this resolution problem during one experiment involving three sources.

The other thing I'd like to mention is for purposes of using these sources on any routine basis for either navigation systems, which the Navy has asked our group to look into, or for station location or geodesy, it is quite important to have some program of systematic observations of the positions of all these sources concurrently and any observations of proper motion. I think this is being done by a couple of groups with regard to the quasars. We are doing it to some extent with the water vapor sources. But I think more long-term systematic program study of the source characteristics is a necessary part of any attempt to use these on a routine basis for measuring station position, for uses in navigation systems, or for anything like that.

MR. CHI:

The next topic will be in the area of VLBI applications for position determination and the comparison of this technique with other techniques such as lasar tracking and so on. Our panel member is Dr. Jayaram Ramasastry.

DR. RAMASASTRY:

What I would like to talk about is this: We all have been doing VLBI experiments up to now, fighting at centimeter levels, and submeter levels. But I'd like to focus attention on what kind of frequency stabilities or fractions frequency deviations we have been observing, what geodynamic problems we will be trying to investigate, what kind of philosophical approach we should adopt in the future, no matter who does the experiment.

First of all, I think we should thank Harry Peters of GSFC and Dr. Vessot of SAO and several others in the precise time and frequency field for supporting all the major VLBI experiments up to now, and pray God to give them long life and good health so that they can be of use to us all the time.

Coming to the frequency deviations, most of us doing credible work have been seeing one part in 10^{13} frequency stabilities, sometimes better, sometimes lower.

It is now understood that it is no use doing a VLBI experiment without reliable and well-performing primary-frequency sources. In the end, I will be talking about what I consider to be five commandments of good conduct for geodetic investigators. I think that's what we should keep in mind in planning future experiments.

The four major areas of geodynamic investigations that we are concentrating on in our Earth Physics Program are UT-1, polar motion, solid earth tides, and plate motion. I will go in that order in my discussion. In UT-1, seasonal variations of randomness have been observed, and the conventional technique like the PZT (photographic zenith tubes) and the transit circles have not been able to give anything better than about 5 milliseconds each. It seems that VLBI can provide about a half a millisecond or better. We have demonstrated this in our latest VLBI experiment between Agassiz and Owens Valley. And that's one area where I think that VLBI can contribute signficantly to geodynamic research.

The second area is polar motion, which is really the motion of the earth's mass around the rotation axis of the earth. In this area, VLBI, as has been shown, can really provide very significant data, even though no direct measurements have come up to date. However, I would like to point out that Doppler tracking data of Anderle and others have provided polar motion components at the one-to-two-meter level. As far as lasers are concerned, Dr. David Smith of GSFC is, I think, the only person who has come up with what I consider to be credible polar motion determination from a few hours (six) of laser ranging data.

And it is not really well known now whether laser ranging is going to be a better technique, or VLBI is the one for geodynamic applications. It doesn't matter, as long as we are going to solve the problems confronting us.

I'm just reporting here that lasers are providing these types of polar-motion data, and VLBI has not yet come up with it, but we hope we will soon be able to do so.

The third area concerns solid earth tides, where measurement of the tidal oscillations with gravimeters has yielded sufficiently good-quality data. There is another area where VLBI can also contribute.

Plate motion and fault motion are the areas where VLBI can make very significant contributions. We have been planning short-baseline experiments with combinations of a transportable dish and a main dish for fault-motion studies, and at the same time we are working on transcontinental baselines with the idea of measuring the relative motions between the transcontinental plates. In this area VLBI can give much less than ten centimeters resolution, and it seems as though in the next four years we should be able to settle down to what I consider to be the observatory type of experiment. At present, we have measured transcontinental baselines with an accuracy on the order of 40 to 100 centimeters. Within the next two years, we should be able to improve this to the five-centimeter level. That means we will not be experimenting anymore with VLBI; but will be investigating geodynamic phenomena.

Let me now summarize all the problems that are involved. In this context, I would like to discuss the five commandments of good conduct which I mentioned earlier. They are:

1. Never conduct experiments using stations of no geodynamic value.
2. Never conduct experiments without stable frequency sources and wideband recording systems.
3. Never, under any circumstances, improvise any hardware and software in any experiment.
4. Know the difference between precision and accuracy.
5. Tell the truth always. It pays to be honest in the long run.

VLBI should receive its share of material support if all the major error sources, including atmospheric effects, are to be resolved in order to realize the five-centimeter level absolute accuracy. Otherwise, VLBI will simply remain a radio astronomy tool.

DR. KNOWLES:

I might just add a word seconding Dr. Ramasastry on the importance of frequency standards and adding to his comments, thanking those who have made them for us. I might also add that it's clear that the whole field of frequency standards is changing rapidly. We are now getting improved rubidium and cesium instruments which may be more competitive with hydrogen masers at a greatly reduced cost, and this is well worth following up. It's really important to make these experiments economical and feasible.

And one more comment in that regard, while we are talking of the tools of the trade, let us not forget the important expense and difficulty of making a good wideband recording system for use in the separated interferometer technique. The so-called Mark-1 technique, and other narrowband systems are now pretty well under control. But there are at least several groups – I know the spectral line people are one of them – to whom it is very important to have this wideband and long recording-time capability, which is only available on one system now, the Mark-2 system, which has been developed by NRAO, but it's still in the process of development. They have many more engineering hours to go. It's possible that yet a third system, which would be very noticeably better than the others, could be developed by somebody with enough time and effort.

DR. RAMASASTRY:

I have one more comment to make. I have been working on a system which can provide the instantaneous 28-megahertz bandwidth using the 2-megahertz instrumentation tape recorders that are presently available at the STDN stations. One other person who has been working on the same problem is Dr. Hans Henteregger of MIT. The instrumentation tape recorders already exist at NASA/STDN stations, and they have 14 channels. They could be used directly. No frequency switching is involved: the recorder consists of 14 channels of 2 MHz each, which gives you 28 megahertz instantaneous bandwidth.

DR. ALLEY:

I just happen to know, from experiments being conducted at the University of Maryland by Dr. Currie, that they are leading towards long baseline interferometry in the optical region. And I wonder, since the discussion has centered a good bit on applications of VLBI to obtaining geodetic and geophysical information, whether he might like to make some comment at this point on the advantages of optical VLBI techniques.

DR. CURRIE:

I think the implication for geodesy is--it has the same level of the VLBI in the radio, but there are a number of advantages. I think that as far as this conference is concerned, it is perhaps less significant because of the lack of a need of precision clocks. It has the advantage that one can use rather ordinary clocks in the comparison. It does permit determination (in the initial straightforward form) of positions of the pole with respect to the station longitude, certainly less than the meter level, probably down to the 10- to 20-centimeter level.

In a later version, it will also permit the study of UT. But it is something that test measurements have been made at the baselines of five million wavelengths, and these results have been used to resolve objects that have a diameter of about 20-thousands of a second of arc. But at present those measurements are made on a large optical telescope, and the use of separate aperatures has not been initiated yet.

MR. CHI:

This is the first time that I see we have the people who are in the frequency and time field, and their users. And the requirements start to catch up with the capability. Now, I'd like to call Dr. Hurd, who covered more or less the same area of applications, the signal processing requirement and approach for the VLBI applications.

DR. HURD:

I don't really have any prepared remarks at this point. I think I would like to comment a little more on getting wider bandwidths. People seem to be synthesizing wider bandwidths up to the receiver bandwidth by taking two narrowband segments, one at each end of the spectrum. But for signal processing the important problem is to minimize the amount of data you need, because this is the big problem: storing and processing data, and the computation time is going to be proportional to the number of bits of data required.

Now, to get wide bandwidths, core memories and even semiconductor memories are becoming very, very cheap, like a penny a bit. Anyone -- almost anyone – can afford a million bits of memory and can make this memory as high-speed as you want just by paralleling the bits.

Take a batch of data at twice the bandwidth of your system, according to the Niquist rate. And if you need more than a million bits of data or so, or more data than the amount of memory, well, write this out onto magnetic tape at your leisure and then take another batch of data. And this is the natural way to achieve wide bandwidth utilization in the system. I think it's becoming competitive these days with the cheaper memories. And I think it's really the natural way to do it, and that it will minimize processing costs and system complexities.

Most people seem to have been processing VLBI data, even extracting the fundamental fringe rate and phase and the time delay by the least-squares technique. And I think maybe the maximum-likelihoood approach that I've taken might be somewhat beneficial in this regard. I'm not sure exactly what the computation-time rate also would be, but it seems like a very natural approach to me.

It leads very naturally into the interpolation between the sampling times. In my system, the samples were taken four microseconds apart, but with the minimum signal-to-noise ratio required for reliable detection, this naturally led to an interpolation down to an rms accuracy of one-tenth of the time between samples. So if you can sample every ten nanoseconds or so, you are down to the one nanosecond level, just at the minimum signal-to-noise ratio.

DR. CLARK:

Just in regard to the previous comments, as Dr. Knowles pointed out, the problems that you are going to face in time synchronization is one of available, relatively strong sources, especially when you start synchronizing over longer baselines.

The sensitivity you can achieve is a function directly of the number of bits and only the number of bits you can record phase coherently. Therefore, your end bits can be distributed in any way in the frequency domain to achieve the same detectability of sources. Your idea of distributing them over as wide a frequency range as possible is very fine. I would point out that with our relatively conventional cross-correlation techniques we are routinely doing time synchronization in the same sense that you described time synchronization, to the tenth of the sampling period interval which, for the Mark-I recording system, is in the 100- to 200-nanosecond time range. That's kind of our instantaneous level on a per-tape basis time accuracy, although we have demonstrated we can actually do it on what we call a record of data, which is two-tenths of a second of data, that is 140,000 bits; that we can achieve the same kind of accuracies if the sources are strong enough.

We also find that very frequently we can work down to things more like four or five times the rms noise level, still achieving a tenth of the sampling interval, providing that we integrate to get to those kinds of sensitivities, to take as much data as we need to do that. But we find that we can get those accuracies at the 5 sigma level rather than the 10 sigma level.

MR. MACDORAN:

There is, perhaps, a point that is not well understood by some members of the audience with regard to what it takes in the way of time synchronization.

For instance, in the results which I have sort of previewed and which are going to be given at the AGU with regard to the four centimeters in three dimensions. It is not fair to conclude that time synchronization per se was achieved at the 150 picosecond level.

The way that we have achieved the three-dimensional measurement is by the scheme that Dr. Hurd alluded to, which is a kind of synthesis process that I suppose is due mainly to Dr. Rogers at MIT. What we're doing in the JPL work at least is taking two windows that are separated by 40 megahertz, deriving the fringe phase at each of those two windows, and allowing those two phase patterns to beat against one another.

What one comes out with is a measure of the difference between the geometric time delay and the one that you have a priori modeled in the computer, so that you can adjust the model parameters to get the baseline parameters, for instance.

Now, each of the individual channels themselves – at least in the JPL work has been recorded at a 48-kilobit-per-second rate. So we must be able to extract that phase from each of those two channels, which are rather narrow. So what we had to do was to get the bits aligned to the sample time, which is about 21 microseconds.

So if you asked what was the time synchronization requirement for that experiment, the answer is something like ten microseconds. And on that basis of that ten microseconds we take it down and beat the two signals together and deduce a parameter which we can interpret in a geophysical sense equivalent to about one tenth of a nanosecond. And it's kind of a trick, it has a certain convenience, and it has the homely virtue that it can be done.

DR. WINKLER:

I just would like to repeat my request that if there is any operational capability anywhere nearby, that we would like to know about that. It would be extremely useful, for instance, to have traveling clocks in laboratories which have been synchronized to, let's say, 100 nanoseconds. It would be really useful in studying a number of things which are quite puzzling at the present time. That is one remark in regards to the clock synchronization.

I have another remark on the question of polar motion and UT-1 determination. And that is that it seems to me that one should look for some agreement in the scientific community about reference observatories, similar to what we actually use today as a definition of the conventional pole, the OCIs defined by location of five latitude stations. The latitude has once more been defined, and that is the origin of the international convention origin. Something like that is lacking in these more recent experiments and the consequence of that, what we are all referring to, is changes or differences of latitude, or changes or differences in UT-1.

I feel, scientifically speaking, that it would be desirable to propose and agree to a reference system for these other purposes. That would seem to me the only way that one can really study longer-period phenomena extending, let's say, over 10 years or 25 years.

DR. JOHNSTON:

Still going to the VLBI's application to the earth's rotation, interferometry has a sensitivity to absolute declination. But the parameters involved in the earth's rotation are sort of free-floating. There are three sensitive parameters: the longitude of the baseline, the right ascension of the radio source, and the universal time.

So if you want to start it, it must be a start by fiat, and I would suspect that one cannot fiat such a start until there is some degree of consistency, or an agreement on the part of how the data is going to be taken and how it is going to be reduced. It's kind of an arbitrary thing and I would suppose one could back up, you know, and if we discover later that current applications were good enough in principle we could just back it up. Things have been backed up to 1900 for origins in the past, so I guess we could do it again.

DR. VON BUN:

I just want to make some comments on Dr. Winkler's thoughts of just a minute ago. First of all, I think we just started out, as I said before. To my knowledge, nobody had determined the polar motion with one station in six hours, and it was just a preliminary experiment to see if it worked and if so how well.

I agree with his statement but at the present time I think it's just too early to discuss this. As soon as we get some additional results from the VLBI and the laser stations, I think these questions will be resolved.

For instance, we just finished the San Andreas Fault experiment. We operated two laser stations over the last three months to determine ultimately the motion between the plates, but at the same time these measurements will be used to determine the pole motion. At the present time only relative motions are measured of course, and absolute motion will come with time, I think, maybe in two or three years, I would guess.

MR. MACDORAN:

Can you give us some inclination or some early information on how the "SAFE" experiment on the San Andreas went?

DR. VON BUN:

No, because we just finished the experiment and are collecting the data at this time. It looks like, very preliminarily, that we probably could determine the distance between the two stations to maybe 10 or 15 centimeters. It is better than we hoped for. Again, this is just the first trial run that we did this year, and we expected it would be in the 20-centimeter range but it looks like the data are better than expected. The lasers we are using right now are 20-centimeter lasers. We are bringing these instruments back -- as a matter of fact they are just on their way now to be modified to a 5- or 10-centimeter range. Next year we'll send these back again, to continue the experiment. Right now I would say we are in the 15- to 20-centimeter range, safely.

MR. MACDORAN:

How well will you be able to independently confirm the accuracy of the measurements? I'd guess this is about an 800-kilometer baseline.

DR. VON BUN:

We will try to determine the distance in two or three batches and then see how they compare relative to each other. I really can't give you an exact answer to this because we haven't evaluated the data yet.

From the polar motion experiment we did, where we had lasers in the 30-centimeter range, we are coming down to the 30-centimeter accuracy in the baseline. So, assuming a linearity, I would assume if we have a 10-centimeter laser, we may come down to the 10-centimeter distance, and with a 5-centimeter laser we may come to a 5- or 10-centimeter distance.

But at the present time we just finished the experiment taking the data and no analysis has been made yet. As a matter of fact, we don't have the data here yet; we still have it at the station.

MR. MACDORAN:

Do you see an opportunity any time soon for collocation with the VLBI?

DR. VON BUN:

I hope so, because I consider this as an important experiment, because if we do something either with VLBI or with laser you will really never know what the absolute accuracy is; and I think a very good independent test will have to be made, and I hope we can soon make it, putting two lasers side by side with the VLBI system and really determining the distance between the two stations independently with both systems. We have both the laser and the VLBI capability at GSFC, so such an experiment is not a problem to us.

MR. MACDORAN:

In the Goldstone experiment we have a design featuring that experiment to compare against the National Geodetic Survey over that 16-kilometer baseline, and the National Geodetic Survey guarantees their work at the 20-centimeter level, and we have a comparison to within 12 centimeters of that. So I guess I take a little bit of issue with the strict interpretation.

DR. VON BUN:

Well, let me give you an example on the polar-motion experiment. We determine not only the polar motion but also the distance between Goddard and the station in Seneca; when we evaluated the distance, it fell exactly within the range we got from a survey, which I think is a coincidence because I just can't believe we determined the position of the two stations to 30 centimeters, and this is – in three dimensions – exactly what we got from a survey which was taken a year before the instrument was started.

DR. MCCOUBREY:

Andy, I just have a question about the very long baseline interferometry. In the discussion today I didn't hear any discussion of the possiblility that the presence of the earth between these sensors, between the antennas, may lead to effects, and I wonder if it does. The advancing plane wave must certainly be disturbed by the presence of the earth between the sensors, and I wonder if this is taken into account or if any large effects actually do occur?

MR. MACDORAN:

Well, on my bar chart, there were three separate bars that addressed that. There's something labeled ionosphere and the wet and dry troposphere. And those were phase delays, and that's the concern here rather than, say, a ray-bending type of thing.

DR. MCCOUBREY:

My question really relates more to the ray bending. Every part of the surface of the earth becomes a scattering point, and signals from those points must interfere with the advancing plane wave before it comes. . . .

MR. MACDORAN:

You mean scattering from the ground around?

DR. MCCOUBREY:

Right.

MR. MACDORAN:

Oh, well, the antennas themselves reject that to a very high degree. I mean just the natural beamwidth of the antenna itself given by the received wavelength in the aperture of the antenna will, you know, cancel out the things in the background.

You get such effects as spillover. We have a microwave receiver looking down into the optics, and there's a diffraction pattern that tends to see the ground around it and raise the system temperature, but the attenuation of the scattered signals is very far down, 30, 40, 50, 60 dB.

DR. MCCOUBREY:

These other effects you mentioned – the troposphere and the wet and dry atmosphere and so on – really lead to refractive effects, don't they?

MR. MACDORAN:

We tend to interpret it more conveniently in how the wave is slowed down rather than how it is bent, but one could make an interpretation in bending; it's just not as useful.

DR. RAMASASTRY:

I just want to comment a bit. The parabolic reflectors have a strong rejection capability as compared to the hemispheric antennas. There is definite antenna pattern, and sidelobes are a smaller fraction of the main lobe. Diffraction patterns due to ground are mainly rejected. So this is not a serious problem.

MR. CHI:

I'd like to go on to the next area, that is in the relativistic time correction. There is much talk about the measurement and the theory to prove the theory. Once the theory is proven to be correct, when or where is the correction of time needed? I'll ask Dr. Alley to discuss this.

DR. ALLEY:

I'd like to supplement some of the remarks I made this morning about the way in which one could use a traveling package of accurate clocks to distribute accurate time once one has convinced oneself that the relativistic corrections that are normally calculated do indeed describe the situation.

We have undertaken the development of a small compact package of clocks to serve not only for this possible space relativity experiment, but also to serve the USNO in actual clock trips. That is, the ability to have a very small package with the infer comparison of relative phases quite frequently as one does in the master clock system there, and have this all done automatically, would let one transmit the time with an accuracy of perhaps 100 nanoseconds over a trip of reasonable length.

Now, when you come to trying things in satellites, I point out that for Skylab the combined velocity and potential effects lead to a rate change of -3.3 parts in 10^{10} which is a change in epoch of the recording clocks of about 1.01 microsecond per hour for the orbit of Skylab (~400 km).

Now, one important aspect of the proposed space relativity experiment is that there are communication links on these manned spacecraft, namely, the frequency modulated TV link which is also used for high bit rate telemetry dumps, which has a bandwidth of about 2 megacycles. And one can readily transmit seconds ticks down. We have designed into the package a composite output which consists of 3 seconds ticks, one from each clock, clock A being 1 microsecond wide; clock B, 2 microseconds; clock C, 3 microseconds; and the rise on these pulses is very sharp. It's determined by opening a gate to let through a shape portion of the 10-megacycle rise sine wave, and we have something like a 5-nanosecond rise.

Now, the 2-megacycle bandwidth of this transmitter will limit you to something like half a microsecond rise, but receiving this pulse repeatedly, you can do a certain amount of averaging, and I think it's quite reasonable to expect 100 nanoseconds or so with that kind of a bandwidth.

Now, if you could have a wider bandwidth–which is very practical it seems for low orbit satellites–I mean, at the distances of the moon it becomes very expensive to transmit anything more, but at a few hundred kilometers very wide bandwidths are certainly possible. In fact, one could even go to optical transmitting systems–for example, semiconductor junction lasers with pulse rise times of a few hundred picoseconds – and be able to use these transmitters to synchronize ground clocks to this accuracy as the satellite goes around. Such a technique obviously requires knowledge of the satellite distance to high accuracy so that the propagation delay can be corrected for. This is readily accomplished with laser ranging techniques. Subnanosecond-range time measurements are being routinely made in the lunar laser ranging experiment.

Now, one of the uncertainties in all of this is how the clocks perform in a zero-gravity environment, when they go into free fall, and this is practically impossible to simulate on the earth. The best you can do is turn the clocks upside down in the earth's gravitational field, do that about various axes, and we have been doing some of that, and we'll be doing more. The proposed space-flight relativity test has aroused some considerable interest at high levels in the Department of Defense for possible future systems where one might want to fly lightweight, low-power, small-volume clocks and where performance in an O-g environment is a very important aspect of such systems.

So, just to summarize, not only is it important to carry clocks out into space, get large relativistic effects, and bring them back to convince all the doubters that these effects are really there to high accuracy, but one could leave a package of clocks in orbit, and we would like to see some clocks left on-board Skylab and have some minimal power maintained, if this could be done, if the experiment could be flown, so that once the relativity measurement has been made, this kind of system can be used for distribution of time, as has been done with crystal oscillators on the Timation satellites that Roger Easton and Al Bartholomew and others have been working on.

Well, let me just stop there to see if there are questions.

DR. WINKLER:

I would like to second the comments by Professor Alley. In fact, I agree 100 percent with his first statement, the utility of knowing and being able to trust corrections to be applied for relativity effects. This capability could really be very useful. As a matter of fact, during the recent general assembly in Warsaw, the URSI assembly, just to get the discussion going I made a provocative statement which I want to repeat here, maybe to see what you have to say to that.

I believe that we will always have to have as a last calibration reference a portable atomic clock or clocks, and after the experiments done by Hefele and Keating, I want to remind you that in their analyses, as published in *Science*, the larger effect was not the performance of the clocks; it was in our ability with presently available navigation methods to exactly assess the relativity effects, even assuming that they are accurate.

In that extrapolation of the effect, or the prediction of the effect, the precision was in the order of 20 nanoseconds. Whereas the one sigma value of the agreement of these four clocks using the correlated rate-change method, as Hefele and Keating call it, there the sigmas were in the order of seven to ten nanoseconds.

So we have to keep that in mind, and I want to repeat that maybe a portable set of clocks will remain the most precise way to synchronize, to certify synchronization between any two points provided that they can really trust the theoretical relativity corrections, and that has to be established with high precision.

DR. ALLEY:

Could I just add – it is obviously essential to know the velocity and gravitational potential in which the satellite finds itself. Now, one of the very attractive features of these manned space flights is that this is all done routinely with the unified S-band system. Repeatedly one gets the range to 15 meters or so with the pseudorandom noise code, and one has the Doppler tracking to millimeters per second so limited by the atomic clocks at the tracking stations. One would have to continue keeping that information. For example, on the transfer of the seconds ticks you have got to take into account the propagation delay. That is, you've got to have a combination of radar plus clocks on board the spacecraft, and it's this combination of the two that I think lends great strength to the accuracy of time dissemination by means of satellites.

And, again, one can get the range exceedingly accurately by passive means, with ground-based lasers. It's so striking how the developments in quantum electronics keep coming up over and over again at this meeting, not only atomic clocks, but in other areas.

DR. WINKLER:

Can I add to these comments again? My only point of slight hesitancy in endorsing everything you said down to the last comma is in regards to the utility of systems. When we look for operational distribution of time, we certainly want to use a system where forever, or for the duration of the system, the bookkeeping and orbit calculations are being done routinely as they are being done in the Transit system or as they are being done even with the Timation navigation system. It is for these reasons that I feel that for operational dissemination of time to passive users, a navigational satellite system is the one to concentrate on.

Now, this does not mean that I do not agree that it would be a good idea to leave such a clock maybe on board of an experimental satellite to make these measurements, but I just want to be sure that we understand when we, as part of our conference intends to, look into the future for the design of systems and specifications of how do we want to bring time to the user, to a large number of users; I think the navigation satellite will play a very important role.

DR. ALLEY:

Yes. I think leaving a clock package on Skylab is not likely. I think it's going to be very difficult to maintain that. But I think a satellite dedicated to that purpose, perhaps along with related purposes, and continuous replacement of such satellites is going to be needed in the future.

MR. CHI:

I'd like to call on Dr. McCoubrey. In 1965 he had a very good review article on the status of frequency standards and their capability. At this time, I would like to ask him to review again and perhaps project into the future the capabilities of precise frequency sources.

DR. MCCOUBREY:

Well, I think it is useful to look backwards and see what has happened in the past several years to precise frequency sources and then perhaps to try to look ahead and see at least what the driving forces are that are going to cause the changes in the next several years and perhaps be sure that we recognize these guiding forces and know what they are.

I'm not sure I could really do a very good job of this now, but I'm sure that there are others here who will have ideas to add, and I think they should do, but in just looking back at the paper which is several years ago now, and looking at what happened, there have been some changes, of course. Many of the changes have been caused by the applications which have during that time become important in the case of all of the atomic oscillators. They've become much more reliable generally through the experience that people have had in using them and in refining the details of their operation, and I think this happens -- this applies to the hydrogen masers, the rubidium oscillators and the cesium oscillators and standards.

In the case of the cesium, of course, there have been substantial advances in the direction of high performance -- high performance particularly with regard to short-term stability which makes it possible to use them in relatively short periods of time, shorter periods of time.

Also, there have been advances in making them more compact, compatible with systems, such as collision avoidance systems, and a good deal of experience in using them in the environments of such systems.

I think in the past several years there have been questions; in the case of the cesium, pushing the sigma tau plots out into longer period of time, times where the parts in 10^{14} are becoming important, and I think there's a lot still to understand there – what is the true nature of the statistics, statistical performance of these standards out in these areas of a part 10^{13} out towards a part in 10^{14}; what is the physics of the operation of the system. I think there are some frontiers still to be advanced into.

In the case of the rubidium just looking at the figures we talked about this morning and where it was 5 or 6 years ago is a little easier to do. Five or six years ago we didn't have the heart of the rubidium standard separated from the rest of the electronics that make it into a useful frequency standard–it's power supply and things of that sort. So, one can't make a direct comparison, but it is possible to compare a rubidium frequency standard of that day with what's available now, and at that time 1000 cubic inches was roughly what was available in the smallest configuration that you could plug into the wall and use as a frequency standard.

Now, you can do the same thing with something that's about 750 cubic inches. I'm talking about a little more than the modular oscillator, but that has a standby battery in it which will operate for a couple of hours, and so I think probably the volume has come down effectively by a factor of two or so. The weight should certainly come down by a factor of two on the same basis.

Cost in the rubidium case has come down more than it has in the other cases – to perhaps three-quarters of what it was five or six years ago.

In the case of hydrogen, it's harder for me to comment. I think Harry Peters has done this very well. Certainly there have been a lot of refinements that relate to the convenience of using them and the confidence in what they do. Refinements involving the automatic turning of the cavity to correspond to the frequency of the maser oscillations, nominal frequency of the hydrogen resonance; there's been a good deal of advance in their reliability, and I think generally this has come out of the experience in working with them and in making the changes dictated by this experience – generally, engineering changes.

I think there remains a good deal to do as far as the wall shift is concerned in gaining more confidence in the stability of this wall shift. There is good evidence now to the effect that it has remained constant for a number of years, but a larger volume of measurements is certainly necessary.

One other matter comes up in looking backwards, and that's the question of what new inventions have occurred in this area, and I'm not sure, but I don't think any of them have been mentioned during the day yet. The one that occurs to me involves the methane stabilized laser, and I think in a few minutes it might be interesting to ask the Bureau of Standards people – either Dave Allen or Roger Beeler to comment on the status of that.

But I'd just like to comment on the importance of it. Certainly this is an important step in linking – coherently linking the radio spectrum to the infrared spectrum, and it holds out the prospect for linking the radio spectrum right into the optical spectrum. I'm sure this has many important consequences, only one of which is the possibility for a single length and time standard.

I think I'm going to leave those comments now and try to look ahead a little bit and ask – or raise the question of: Just what are the driving forces which are going to cause the changes in the future? I feel that there's no pressing need for new advances or new inventions in any case. There's much to do in terms of application and the refinements of the present technology which is available. I think the emphasis is certainly going to be on the applications and the choice of a right type of a precision oscillator, the right configuration of that type of oscillator to do a particular job with particular applications.

Certainly it will become more important for systems, particularly those that will require a large number of precision oscillators. It will become more important for attention to be given to this question of how much does it cost to run one for an hour. And, or run one for a year, or whatever length of time that is important in this application. In the case of satellites, for the lifetime of the satellites.

As far as cost is concerned, it seems to me that it's really a question of: What is the scale of the application? How many are going to be used? Actually, if you consider a frequency standard or precision oscillator, there's really less technology in it than there is in a television set, and the only difference I think really is in the scale of the application, and that scale is cetainly going to affect the advances towards lower costs, smaller sizes, and so on.

Other questions of advances which may be desirable or necessary: In the case of rubidium, I think it would be desirable to see a better understanding of the aging effects. It's my own opinion there are several physical effects involved there, some depending on the physics of the situation, others depending on the chemistry of the situation. And, while I think advances would be desirable, I don't think the application of the rubidium standards is in any way contingent upon these advances, and the applications will occur even with the technology as it stands insofar as aging is concerned.

I think the rubidium standard is going to be a contender for many of the applications of quartz crystal oscillators in the past. I mentioned one figure this morning I think which may be a little bit in error that I learned later, namely, this question of warm-up time, the rubidium oscillator can warm up very rapidly now, and I think this morning I suggested perhaps the state-of-the-art is such that a rubidium oscillator can be warmed up, up to 1000 times faster than a quartz oscillator. Since this morning, I have discovered that Hewlett-Packard has quartz oscillators that warm up in 30 minutes to within a part in 10^9.

In the case of the hydrogen maser, I've already mentioned the wall shift. I think more understanding of the wall shift is going to be more important and those resources that can be applied to this question should lead to a clarification.

I'd like to suggest that we ask Roger Beehler or Dave Allen to say something about the status of that methane-stabilized laser.

DAVID ALLEN:

Helman Helwig at the Bureau of Standards has published some results on that and his results at that time which were superior to any stability results of any oscillator. And that was essentially stability to one part 10^{13} in a sample time of ten seconds.

Since then, some better stability results have been achieved, but interestingly enough these have been in nonquantum electronic standards. Stein and Ewer at Stanford, using a superconducting niobium cavity have achieved stabilities better than this with an X-band source. In the future. this appears to be a real competitor.

The methane device, of course, has a very impressive stability, and one has to ask the question, "How can you use it?" At this date, doing synthesis in the optical or the infrared is very difficult. This undoubtedly will improve as time goes on and is improving right now. But currently you can't really utilize it.

I would like to, if I may, backtrack a little on Dr. Winkler's comment regarding portable clocks and whether or not you always need to rely upon this system. If you look at systems in general, the optical region of the spectrum is the one with the greatest bandwidth and has some extremely impressive promise in my mind. If you look at a portable clock, all it is, is a portable transmitter, and you use a coaxial cable to propagate the signal. The stabilities in the optical are very impressive, say, from satellite distances. The corner reflected signal from the moon is at such a large distance that using it decreases your signal-to-noise quite significantly, but, if you had a corner reflector on a satellite; and if you fully utilized, say, synthesis using methane-stabilized lasers in the optical region, to me the assumptions of reciprocity in this region might lead to future time-synchronization methods in the subnanosecond region. Theoretically this seems achievable, so this is kind of a projection on your comment that perhaps the ultimate system will be a portable clock on a satellite.

MR. BEEHLER:

NBS is about ready, we hope, to go into an operational phase on our new NBS five-frequency standard. Now, of course, this is far different from anything we've been talking about here today, because this is a sub-20-foot-long ultimate cesium standard which is designed strictly for laboratory standards use.

But, in terms of performance, it is also perhaps the kind of ultimate in that we envision an absolute accuracy for this device of one part in 10^{13} and a 1-second stability of about two parts in 10^{13}. The standard is put together, it is under vacuum; we are, we think, within a few days of observing the first beam in the standard.

MR. CHI:

I'd like to take this opportunity to thank the panel members, the authors of the papers, and the audience. I also want to remind you that one reason we have these discussions is in the hope that we can exchange knowledge and viewpoints between the users and the people who actually generate frequency and time. I'd now like to turn the meeting over to Mr. Wardrip.

MR. WARDRIP:

My thanks to you, Andy, and also to the speakers and panel members of today's session, and, of course, the audience.