QUESTIONS AND ANSWERS

Paper 5: "The Role of the International Radio Consultative Committee – Its Functions and Influence" was presented by Hugh S. Fosque, NASA Headquarters, Washington, D.C.

QUESTION: I notice on your draft that you didn't show the 60 kilohertz authorization. Is it a true, guarded frequency?

MR. FOSQUE: This is an experimental frequency, I believe. I will be glad to stand corrected by anybody here, but I don't believe that is an allocation. It is an experimental use of a particular frequency.

QUESTION: Could you say a few words about the methods by which you hope to get down to a one nanosecond time reference distribution?

MR. FOSQUE: With great difficult. I can't say too much about it, except that there is obviously a need, or at least there seems to be a need within the next 20 years for synchronizations of that sort. And especially so among users, but also among the principal laboratories involved in keeping time and frequency. The hope is that one will be able to go to a very broad band satellite and achieve this.

Now, the details of that technique are being looked at at the moment, and were the concern of a study group meeting that we held yesterday. I just don't think they are far enough along for me to provide you with the kind of assurance that you are looking for from the nature of your question. I don't know if we will be able to do that or not, but we will try.

QUESTION: Peter Gorham, JPL, with a comment on an implementation.

In very long baseline interferometry right now we solve for a term, which is related to clock synchronization, and I qualify that by saying related to, because we have some instrumental factors to pull out. But we are getting consistency in our solutions, now, operating independent stations with hydrogen masers. But the offset terms that we get out are good to 10 to the minus 10 seconds, 1/10 of a nanosecond.

Recently we have incorporated a phase and cable calibration system that has been put together by Goddard and Haystack Observatory; and we expect to be able to take out the instrumental terms so that, via the technique of very long baseline interferometry, I think, it is going to be a possibility to come up with nanosecond synchronization at intercontinental distances, and one could operate it in near real time via a satellite communication implementation, if you wanted to go that route.

MR. FOSQUE: Thank you.

I am somewhat of a coward in speaking to this group because we have had some meetings, and there are certain requirements that people think are emerging that would require one to take an even broader look than we have talked about here. In fact, there are a fair number of individuals who think we should strive for a 50 picosecond capability. But as I say, I am somewhat of a coward quoting those numbers to this group.

Paper 7: "Definition, Requirement, and the Determination of UT1 by the U.S. Naval Observatory" was presented by Dennis D. McCarthy, U.S. Naval Observatory, Washington, D.C.

QUESTION: My name is D. Antonio with the Navy Department.

Are there any efforts underway to do any predictive work on the variation of UT1 with UTC?

As I understand it, UT1 is for most people at the moment not very predictable and you have to keep relatively current with the work you do to find out what the current correction factors are.

Are there efforts underway to see if this can be predicted better than it is now?

MR. MC CARTHY: There are always efforts being made to predict time. However, when we are talking about accuracy. We can probably predict UT1 minus UTC to 1/10 of a second with some degree of accuracy for maybe a year. But that doesn't do anyone very much good; to a millisecond accuracy, it just is an impossible thing.

Paper 8: "The Determination of UT1 by the BIH," was presented by Ms. Marine Feissel, Bureau International de l'Heure, Paris, France.

QUESTION: George Milburn, Army, West.

In your prediction technique you mentioned that you had a tectonic factor in there, prediction for smoothing of individual stations. I am wondering if you have a, more or less, regular continental drift term that goes into that for any particular stations.

MS. FEISSEL: No. We have a variation of the drift between Europe and Asia. We found a result which is of the same size as the errors are. So, as the present continental drift is not very well known, we do not use continental drift material in our prediction. This lack of knowledge is one of the reasons why we take only four years of the past to predict.

QUESTION: Rueger, Johns Hopkins.

Did I understand you to say you are using Doppler tracking of transfer satellites as part of your data?

MS. FEISSEL: Yes, we received the final results of the DMA and we used the X and Y results.

QUESTION: Is there any plan in the near future about making comparisons, using satellites exclusively for the UT1 measurement?

MS. FEISSEL: Our methods are devised so that they can include any new method, as long as this method gives regular results, and as long as there has been a longer period of time in the past when systematic deviations from the present system of UT1 can be known and predicted.

QUESTION: Would you be in a position to say whether the errors by the satellite methods are lower or greater than you are realizing by the star observations then?

MS. FEISSEL: For the pole coordinates, the Doppler system has precision which is about the same as the astronomical system and, of course, it is increasing faster as time goes on.

QUESTION: To be able to fall back on?

MS. FEISSEL: Yes, and there is a project to set Doppler net of stations observing the satellites but these stations will be international. It will be, it is expected to give its first results in 1978.

Paper 9: "Precise Worldwide Station Synchronization Via the NAVSTAR GPS, Navigation Technology Satellite (NTS-1)," was presented by Thomas McCaskill, Naval Research Laboratory, Washington, D.C.

QUESTION: Samuel Ward, Jet Propulsion Lab.
How long will that receiver remain in Australia?

MR. MC CASKILL: The site in Australia is now considered a permanent tracking station, part of the navigation and technology satellite tracking network. So, it will remain there for an indefinite amount of time.

Paper 28: "Special Purpose Atomic (Molecular) Standard," was presented by Mr. David J. Wineland.

QUESTION: Tom English.
The real question I wanted to ask, where your stability turns up at the end, is there any possibility that might be due to the changes in pressure, pressure fluctuations over a period of a couple minutes or so?

MR. WINELAND: No, in fact, that was not. I could correctly monitor the pressure and rule out that. I am sure to a high degree that was due to our drifts, for example, in the servo offsets. There was a direct correlation with that. As of yet, we haven't implemented the digital servos, which we hope to do. So I am confident we can beat that down.

QUESTION: Mr. Rueger, Johns Hopkins.
What is the size of the cavity and what is the shape of it?

MR. WINELAND: We haven't at all finalized it yet. I should have pointed out on the last graph, that was for just a piece of K band wave guide, 50 centimeters long. Essentially, terminated at both ends.

That was also at a fairly higher pressure than we would want to work at. In other words, we would like to reduce the pressure, reduce the temperature sensitivity. So we envisage going to a cell of maybe about a liter volume, but the configuration yet is undecided. It may either be a box, or maybe loops of wave guide, sort of folded up on itself. But that remains to be seen.

Paper 31: "OMEGA Synchronization: Current Operations and Future Plans," was presented by Mr. Howard J. Santamore.

VOICE: I notice your data there. Could these unmodeled forces be due to atmospheric tides?

MR. SANTAMORE: I really don't know. I think Dr. Reder has considerable experience with that. I don't know of any atmospheric tide effects in OMEGA propagation.

DR. REDER: Actually, I am quite glad because I notice some of the variations against our cesium standards, and as a good citizen, a good neighbor, let's say, I blamed our cesium standard for it. But apparently it was not.

MR. SANTAMORE: Thank you.

VOICE: Perhaps I could explain to you why the atmosphere, the tides should affect this, because since the mode of propagation is from the E or D layer, underside, and the surface of the earth, the bulges caused by the atmosphere cause the radiating sources, the sun, would be up closer and the angle of incidence will change, for instance, at new moon when the solar forces and lunar tides are lining up, if you look at those periods, for instance, the diurnal effect, you find that the jump is much bigger during those times.

And in timing when this effect occurs, I have noticed that it occurs approximately 6-1/2 hours after moon passing over head, or even – the effect doesn't quite line up with the instantaneous position of the moon and sun. There is a delay.

MR. SANTAMORE: How large an effect are you talking about?

VOICE: Distances from Fort Collins to Canberra, Australia, the effect was something like as much as 7 to 10 microseconds. And in comparing the same sort of data with the Fort Collins to South Africa, the effect was nearly the same, but of the opposite phase.

MR. SANTAMORE: Thank you.

Paper 32: "A Brief Review of Frequency Stability Measures," was presented by Dr. Knowles.

DR. WINKLER: You have only a one-way transfer, not a two-way?

DR. KNOWLES: Yes, at the moment it is only a one-way transfer. This is essentially because of the combination of the economics of how many link transmitters we could afford and the satellite characteristics.

A possible future extension is a two-way transfer. In order to make that technically worthwhile, one would want to have a fully coherent satellite link whereby you could transmit the phase of your local oscillator from one station to the other as well as simply a time reference signal.

This wouldn't be possible with CTS because it doesn't have coherent oscillators, a future satellite which was equipped for a coherent slave mode, that would be worthwhile.

Paper 38: "NTS-2 Cesium Beam Frequency Standard for GPS,"
was presented by J. White.

MR. RUEGER: Are there any questions?
Al Bates.

MR. BATES: Bates, APL.
I am curious why all that isn't done within the loop, the relativistic synthesizer.

MR. WHITE: As you know, it could all have been, but the time sequence of things was not such that we could do it that way. We found out the offset was a hard and fast requirement.

The design of the cesium was such we could no longer go back and make the changes within the loop, and it was too late to do it.

MR. BATES: You paid a terrible price.

MR. WHITE: Yes, we did. There is a plug for what is coming up. We hope to report in Atlantic City this spring on the next family of these standards which are now under construction for the Air Force satellites. They will have all of this built in.

The output is 10.23 and it will be offset as it comes out of the standard.

MR. RUEGER: They have the relativistic synthesizer involved.

MR. WHITE: It is all programmed into the synthesizer of the cesium standard.

MR. BATES: You said the resolution synthesizer was 7 parts, 10^{10}.

MR. WHITE: That is the range. The resolution is 3 in 10^{12}. The tuning resolution on the cesium is 1-1/2 in 10^{13}. We have a range of about plus or minus 1 and 10^{11}.

MR. ENGLISH: Tom English.
I would like to point out with regard to the rubidium standards that were used, they were basically modified conversion.

MR. WHITE: At the time we originally prepared, there was a great deal of trouble, but you're quite right, they were commercial units, not designed for space flight. We did modify them to the extent necessary for our purposes.

Paper 39: "A New Rugged Low Noise High Precision Oscillator,"
was presented by D. A. Emmons.

MR. RUEGER: Are there any questions to this paper?
I have one, you mentioned you have designed this with hardening in mind. Would you care to say what characteristics you were trying to preserve with the hardening, transient performance or survivability or continuous service or what?

MR. EMMONS: I believe that I would prefer not to go into details, but we are using swept quartz and treating the quartz to attempt to get the radiation induced component of aging, if you will, down to acceptable limits.

MR. RUEGER: Thank you.
Are there any other questions? Back there at the microphone.

MR. BARGER: Chuck Barger, Hewlett-Packard.
I have sort of a two-fold question. Number one, you said you had a two and three part 10^{10} shift on the pyrotechnic shock. Was that a permanent shift or did the unit return after shock?

Number two, you made a point of also saying that that particular test was done operationally. Did you also by chance do the vibration tests operationally? And if so, would you be willing to say anything about the sideband performance there?

MR. EMMONS: I am afraid I did not look at sidebands during operational random vibration tests. We did random vibration tests to 10 GRMS and watched the frequency excursions and transients and so on, and watched the oscillator operate during the test, but did not have spectral analyzer equipment there.

The other part of your question could be amplified a little bit by saying that in a sequence of 12 shock pulses, we saw a final frequency offset of about five parts in 10^{11}. So that what happens is that the frequency jumps back and forth. But I don't think that that is very predictable and certainly couldn't be predicted by me.

But it was an interesting result that those shock pulses tended to give an average frequency shift which was not as large as some of the individual shifts.

QUESTION: What kind of thermal insulation system did you use in the oscillator?

MR. EMMONS: It is a dewar single proportional oven.

QUESTION: Glass?

MR. EMMONS: Yes, glass.

MR. RUEGER: You remind me of another question. How did you meet the shock requirement?

MR. EMMONS: Well, by careful attention to details.

Paper 40: "Performance of a Dual Beam High Performance Cesium Beam Tube," was presented by Gary Seavey.

MR. RUEGER: This paper is now open for questions.

QUESTION: Would you care to comment at all on the amount that that package weighs?

MR. SEAVEY: That the package weighs or the tube?

QUESTION: No, the entire electronics, the 5061A.

MR. SEAVEY: I really don't know. Maybe Mike Fischer would have an answer to that.

MR. RUEGER: I have another question. You mentioned that you worked to close tolerance on these tubes. Would you care to say what kind of a tolerance you were talking about? A few mils or something a lot better?

MR. SEAVEY: We are usually talking thousands of an inch on this tube.

MR. RUEGER: Thank you.
Are there any other questions? Here is one.

MR. WARD: Sam Ward, JPL.
After calibrating one of those cesiums with the 004 option and then turning it off and turning it on again, how close will it turn to the calibrated frequency?

MR. SEAVEY: Well, if you go through the correct procedure, you will be well within specification which usually means that it also should be de gaussed anytime the instrument has had a power interruption.

VOICE: How close are the oven temperatures controlled from unit to unit, the temperature setting of the oven controller?

MR. SEAVEY: Within about plus or minus 3 degrees Celsius.

DR. WINKLER: How well do you stabilize the oven temperature to evaluate wherever you set it in the first place?

MR. SEAVEY: I don't think I can very well answer that question right off the top of my head. It is pretty much determined by the electronics in the instrument.

DR. WINKLER: It appears to us some of the pathological behavior of clocks similar to 877, at least in part may have been caused by oven temperature changes.
Such a change of oven temperature will affect the velocity distribution in your tube. And any phase error will produce a frequency shift. That is in conjunction with magnetic field variations and with microwave power level which is another very critical thing.
These are, by far, the most important sources for systematic changes of frequency as we have seen them today. Therefore, I really wonder how well the electronics control is keeping that oven temperature constant.

MR. SEAVEY: I agree with you. I am not sure that I know exactly how well the electronics is doing as far as controlling the oven temperature.

DR. CUTLER: I can probably give a little answer on this. Under ordinary ambient conditions or laboratory conditions it is probably better than a few tenths of a degree centigrade.
The source of variations that were seen during this problem with the oven were not caused by temperature variations. They acted like temperature variations, but it was really a cesium vapor pressurization caused either by a chemical contamination or obstructions within the oven. It was not a true temperature variation.

MR. RUEGER: Thank you, Dr. Cutler.

MR. GARGYI: Gyula Gargyi, Tel Line System.

Are the tubes that failed the test for 004 options the tubes being used for 5061 standard oscillators?

MR. SEAVEY: I am not sure I understand.

MR. GARGYI: I understand your figure of merit was an average of 24 and some tubes don't meet that test apparently. Are those tubes the ones that are being used for standard 5061 oscillators?

MR. SEAVEY: No, the typical realized figure was 24. Our specification is 10.

DR. KERN: Bob Kern, F.T.S., Inc., Danvers, Mass.

Gary, you mentioned two tubes came back after four years of service. Were these the modified ones with the higher figure of merit or the initial ones?

MR. SEAVEY: These were the high performance tubes of the old oven design. They were unmodified.

Paper 41: "Application of High Performance Cesium Beam Frequency Standards to VLBI," was presented by W. J. Klepczynski and K. J. Johnston.

MR. RUEGER: I feel compelled to defend the working of these standards. I am sure he must have been referring to the way they operate and are moved around. The ones we had to look at were rarely in place very long at a time.

Mr. Winkler.

DR. WINKLER: First, I must say I am amazed. We can see a new phenomenon, and that is a strong dipole moment in a pair of speakers. They must have been exposed to field gradients of considerable proportion.

Seriously, I think what you have seen on the cesium standard is, of course, what one would expect if you just take in the numbers of an 004 standard and see the random walk in phase. That is typically a random walk in phase.

And you cannot hope to improve things by putting a straight line through as long as you are in it. You're in a walk in phase and not random noise limited.

Now, on the other hand, in regard to your questions, it is my feeling that these standards unless you come and put them down for an extended period, will have a degraded performance.

Dr. Cutler mentioned before the problem of temperature gradients or initial differences. If you expose such a clock to a temperature difference of 10 degrees and along with banging around in a car; they simply do not perform as well as if they can rest in a place for a couple of weeks. One has to consider it as a system which is excited into some kind of an excited state. And that it will relax afterwards at an unpredictable rate. I think I mentioned in the Observatory that we have never been able to take any one of our normal clocks as a standard into the system. They seem to suffer. The very same standards 1025 which you have in vain tried to use for radio experiment performed exceedingly well in our experiment.

MR. JOHNSTON: Let me say there, I guess I wasn't perfectly clear. We didn't really get VLBI proceedings on the 1025 that were very successful. What I did say was that when I took the 1025 down to Maryland Point, it matched the specs. We did check and see that it matched the specs, and have matched specs on that experiment. Unfortunately, we didn't get fringes on that experiment.

The main point was we used three of these cesiums, one very good, probably as good as rubidium. The other two weren't as good. It makes one wonder when one buys one off the shelf what you are going to get.

DR. WINKLER: What is the difference when you go to higher oven temperature?

MR. JOHNSTON: The higher oven does give you about a square root of two better performance. We beat the signal against the maser signal at Maryland Point and looked at them in a strip chart.

When you turn the oven on, you see the nosing down by a square root of two. It is a very graphical demonstration. I could have showed that on a slide, but the actual calibration didn't come out. There was a mistake made in the calibration.

So absolutely, I can't tell you exactly what the number was. But I can see clearly that it was much better.

MR. RUEGER: I believe Bill Lindsey made a measure of that directly against the hydrogen maser and it was the square root of two.

MR. WARD: How much of the phase drift was due to frequency offset between the two standards? And it seems to me when you move one of them, the short-term noise will limit your ability even to measure frequency. It could until it settles down.

MR. JOHNSTON: That is possible. The frequency offset between the different standards, the hydrogen maser and cesium and rubidium, is taken out of these observations. We assumed frequency offsets between the different oscillators, and we do take those out as an experiment frequency offset.

What I am really concerned with from my observation is spectral purity. The frequency could be almost anywhere, but it is the purity of the signal I am concerned with because that indicates how much noise I am going to get on my fringe phase and it limits my integration time.

But I do think, from the observations that we have made, that for low frequencies, I think this cesium standard can be used. And if I was provided with cesium standards by the Naval Observatory and going into the VLBI business, I would not run out and buy rubidium standards because I could select one of the cesium standards they have.

And I think that would prove perfectly adequate under almost every application they would have.