HYDROGEN MASERS AND OTHER STANDARDS

by

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This paper is concerned with some of the standards which were discussed in previous papers.

The first picture, Figure 1, is a source of what I really consider to be beauty. It is a hydrogen maser source which is operating and is giving out the Balmer alpha spectrum of hydrogen. This indicates a high percentage of hydrogen atoms in the source. Figure 2 is a photograph of an experimental hydrogen maser which was designed and put together in late 1966 or early 1967. This particular maser has had over four years of continuous operation and has operated most of that time as a basic frequency standard.

Figure 3 shows NP-1, which is a NASA prototype atomic hydrogen standard. It was the first of four models which we built, and was completed in late 1968. It has been oscillating continuously since then, when it was not in shipment, and had required no maintenance other than normal efforts required in transportation and shipping. The hydrogen masers are portable. Our hydrogen masers, to date, have about 100 hours of flight time on them. They can operate in transit; they operate continuously in trucks. It is not absolutely necessary to degauss them when they are put in a station. They seem to continue to function without magnetic field problems as a matter of general operation.

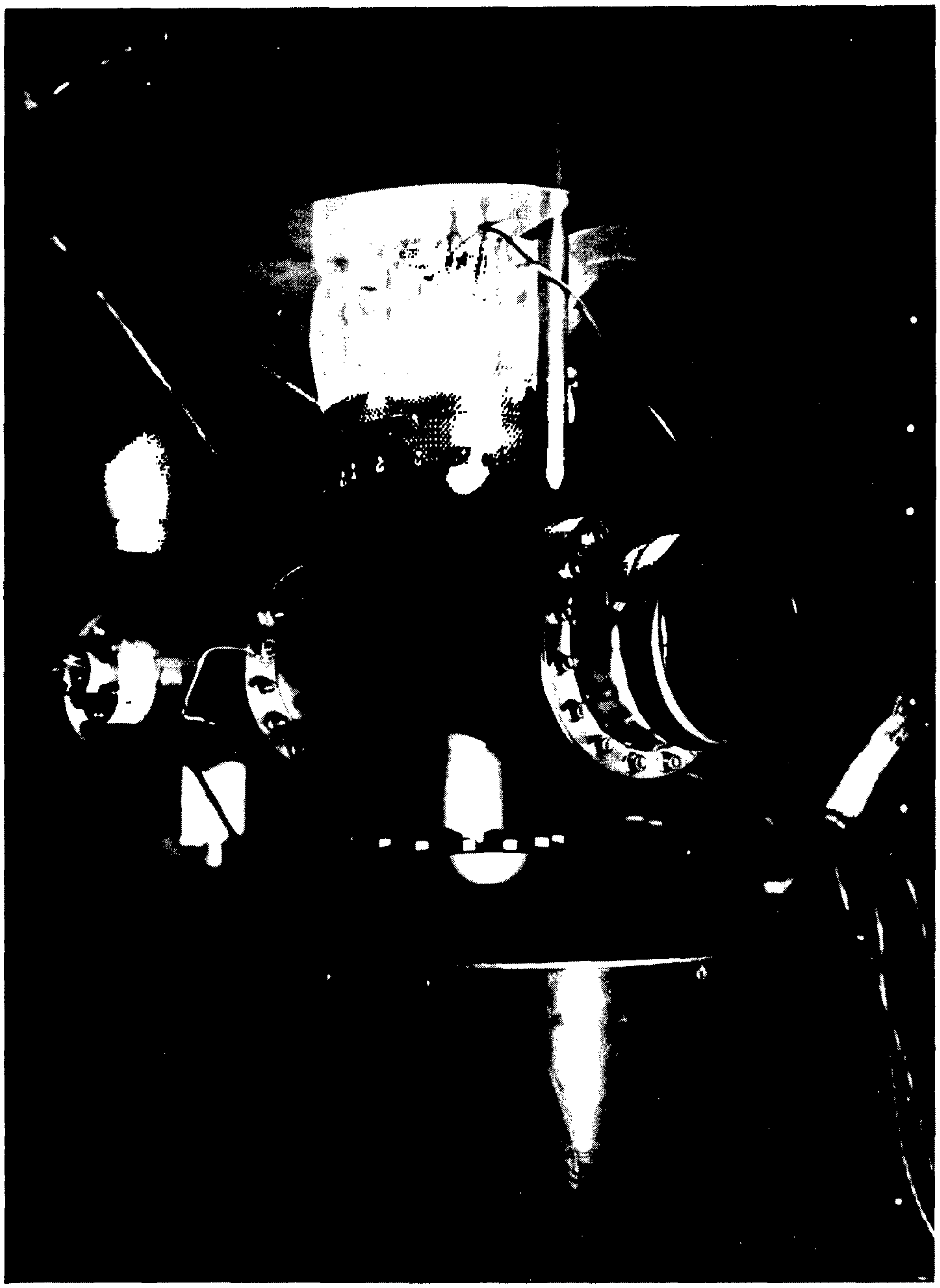

Figure 1. HYDROGEN MASER SOURCE

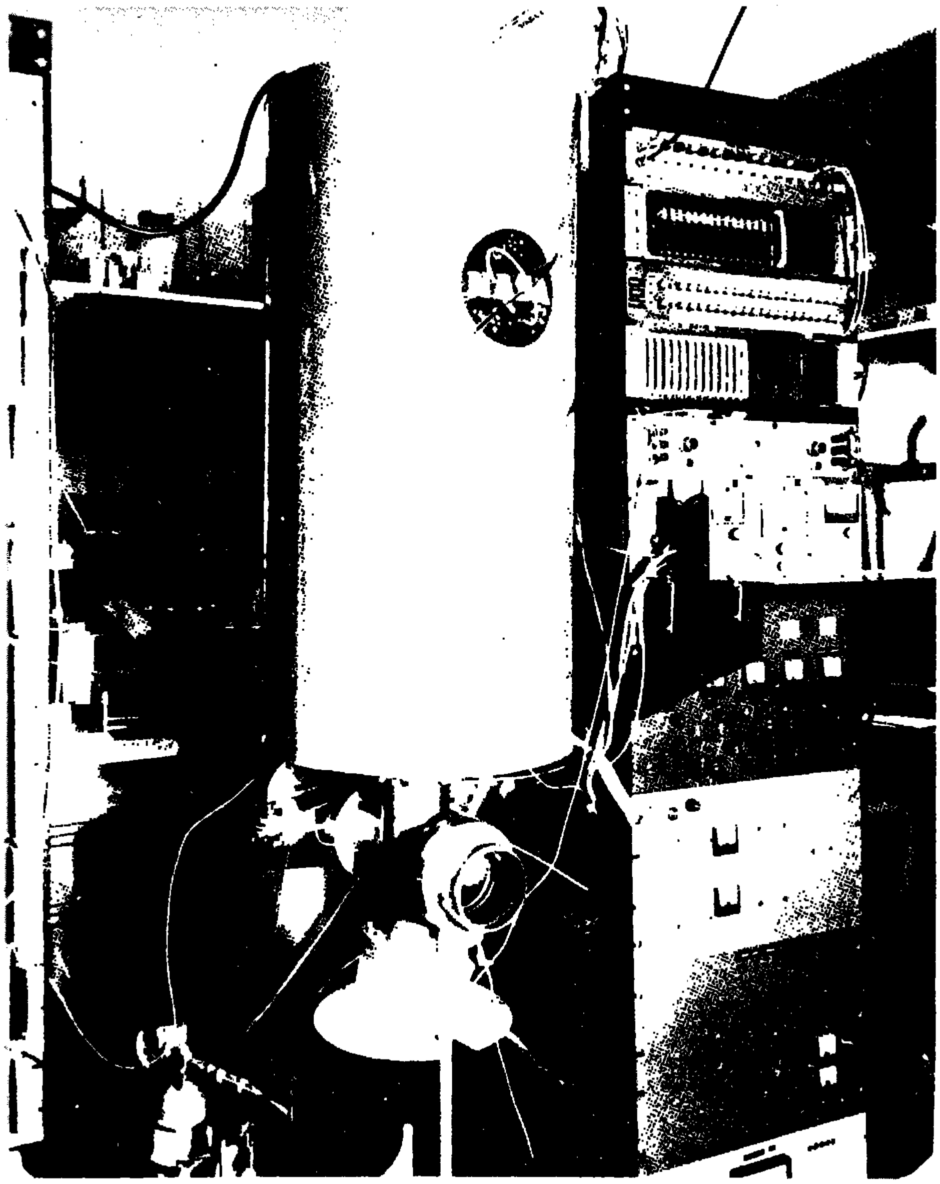

Figure 2. EXPERIMENTAL HYDROGEN MASER

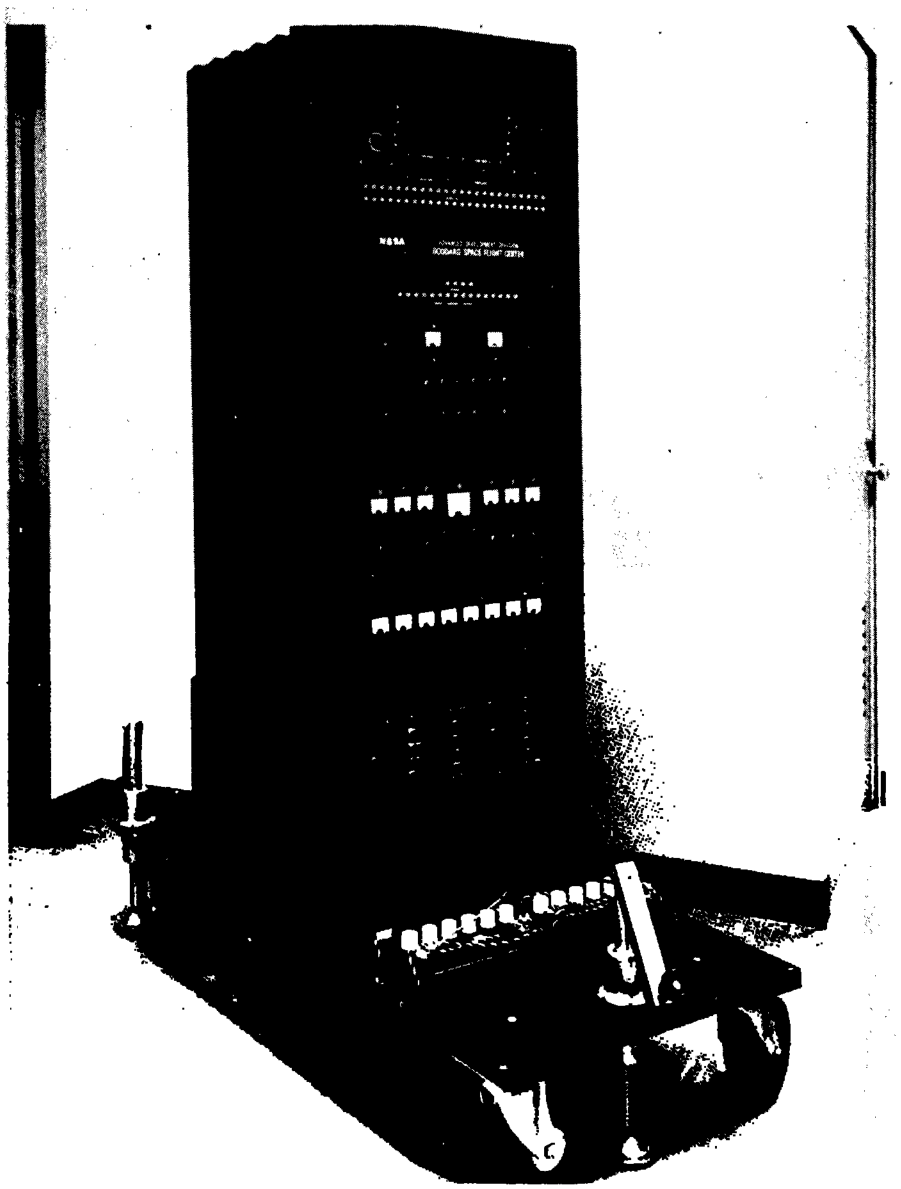

Figure 3. NP-1 -- A NASA PROTOTYPE ATOMIC HYDROGEN STANDARD

Figure 4 gives an idea of the overall Goddard hydrogen maser operation. It gives the continuous operation and experimental use times for NX. In 1969, NP-1 was at Goddard until August and was at MIT Haystack with some VLBI experiments for the rest of that year. It was at MSFN, Bermuda during the Apollo 13 and 14 flights and operated continuously during this period. It is now on-line as the prime frequency standard at the DSN in Johannesburg, South Africa, and it has been there for the past few months looking at Mariner Mars.

NP-2 was a little longer in coming out of the cocoon. It was at our network test and training center for some period of time as well as under test at our Goddard Labs. It was at MSFN in Madrid, Spain during the Apollo 14 flight and is now on-line at DSN Woomera, Australia. These masers are not just being used for Mariner Mars. Several VLBI intercontinental base line experiments have been based upon their use and other experiments in tracking, geodesy and so forth are anticipated.

The NP-3 was at Cal Tech for VLBI experiments in August and September, 1969. It then went to the NBS in Boulder, Colorado. We had some very nice stability curves from it at that time, which indicates that at least for three months operation we were well below 1 part in 10^{13} in comparison with their cesium ensemble. It was at MSFN Goldstone during the Apollo shots. It is still at the DSN Goldstone Pioneer Station on-line for Mariner.

Our experience has not been as good with NP-4; however, it has been used for over a year and a half. It was at MSFN in Madrid during Apollo 13. We replaced it with NP-2 in September of that year. It has been undergoing repair and modification since then. Some new and improved parts have also been put into this hydrogen maser. As you realize, these represent a state-of-the-art circa 1968. We know quite a few things now which can improve our stability curves but, unfortunately, we do not have any data. We have not put any of these changes in these masers but we will work more on NP-4 and I think it will represent, if future masers don't, some of the improved stability characteristics.

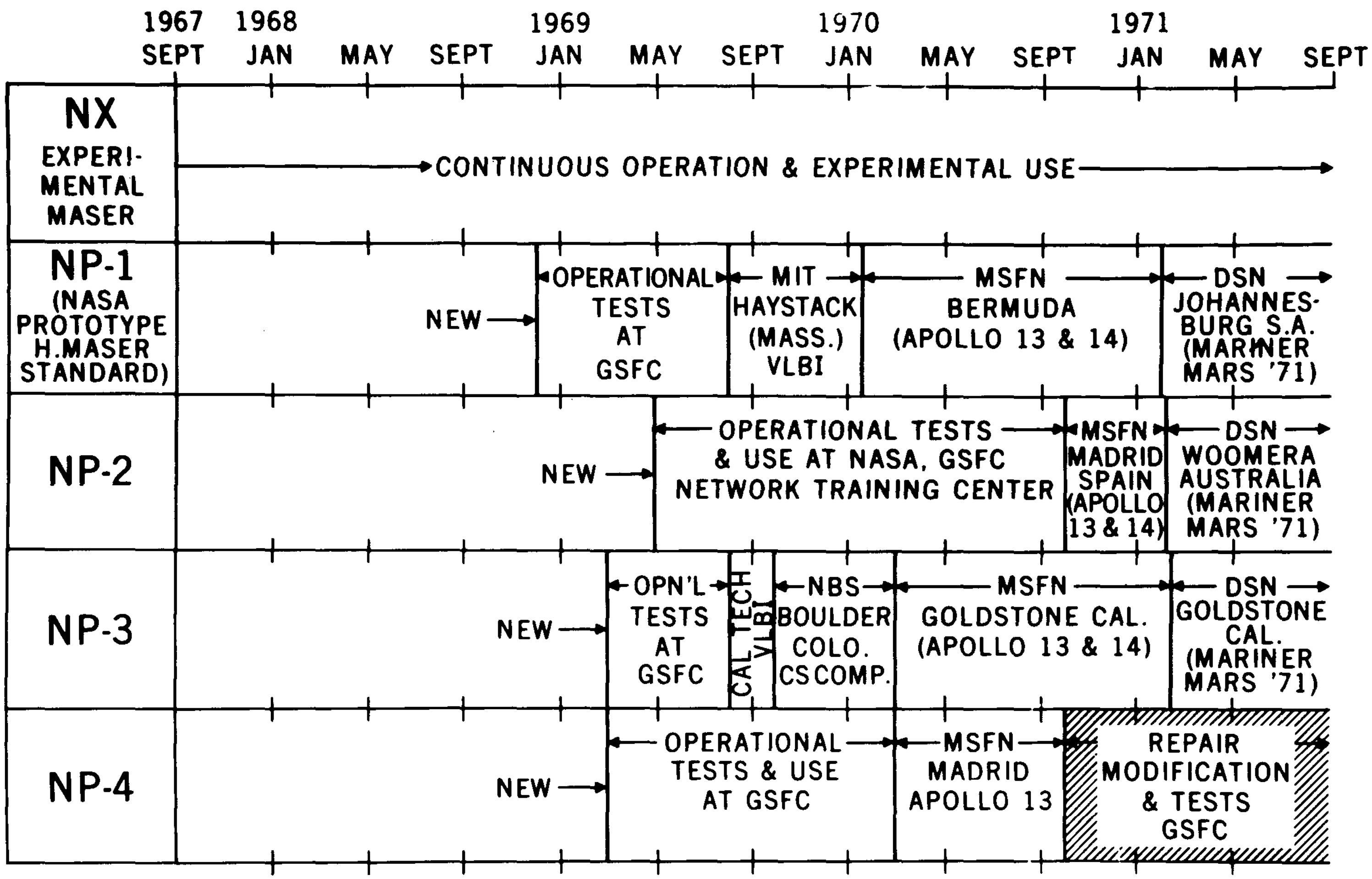

Figure 4. GSFC HYDROGEN MASER OPERATION

Figure 5 shows the stability comparison of instability contributions involved with the USB system at some of the NASA tracking stations and illustrates the effect of the delay in light transmission times. This is a very important parameter when using a standard at the stations. As you can see, this figure relates very much to the useful stability characteristics, not only of the cesium standards and the exciter synthesizers, buffers, etc., but also to the hydrogen masers.

One of the more important points brought up in previous papers was the sensitivity of a hydrogen maser or of similar devices to magnetic field perturbations. Figure 6 illustrates the homogeneity that can be achieved in magnetic fields. In this case, we are not using a hydrogen maser, we are looking at the magnetic field using a beam. Although this resembles cesium beam resonance it is atomic hydrogen beam resonance and it is rather unique. The top curve is a single state transition in hydrogen and is occurring at 8.8 milligauss; the next one is 0.88 milligauss; and the lower one is "0" milligauss. This is with an error of approximately 30 microgauss. If there were a cesium atom going through these shields, the upper curve would show other resonances which would try to crowd into this picture. The nearby transitions would be only seven divisions away. In the lower curves, however, the pattern would be completely washed out by overlapping transitions. This is one of the most fundamental limitations in accuracy and stability with the cesium atom.

Figure 7 shows the transitions in hydrogen and cesium energy levels, with hydrogen shown on the left and cesium on the right. With cesium, there is a total of 35 transitions and there are 7 sigma transitions; with hydrogen, there is a total of 5 transitions with only 1 sigma transition and things can be arranged so that only this 1 hydrogen transition is seen.

Two important points which have not been adequately discussed at this meeting are accuracy and reproducibility. As everyone who has used standards realizes these are very important factors in the capabilities of the standards. Figure 8 illustrates the accuracy capability of the various standards. It also shows our potential stability for hydrogen beams. It is unfair, of course,

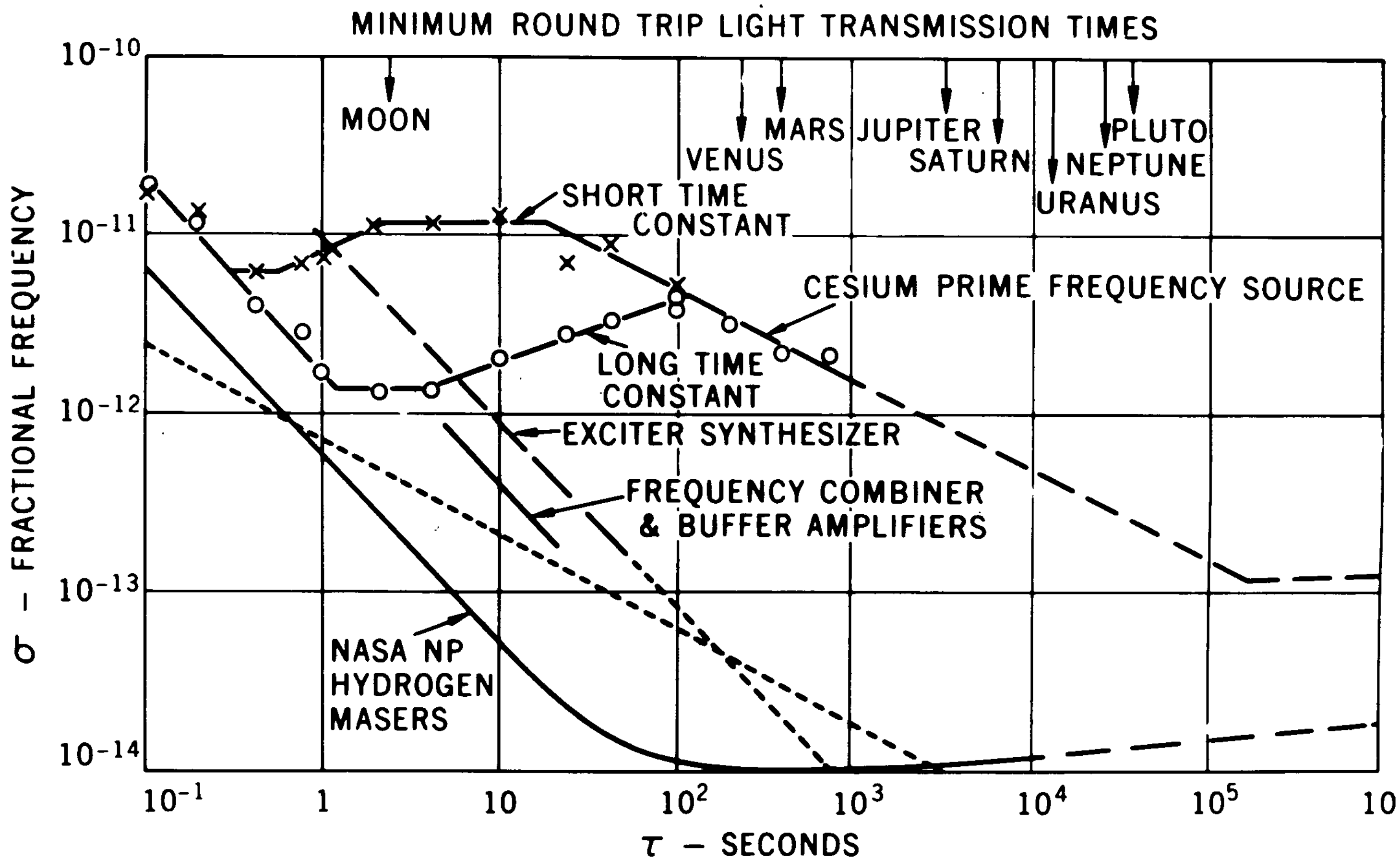

Figure 5. NASA USB SYSTEM INSTABILITY CONTRIBUTIONS

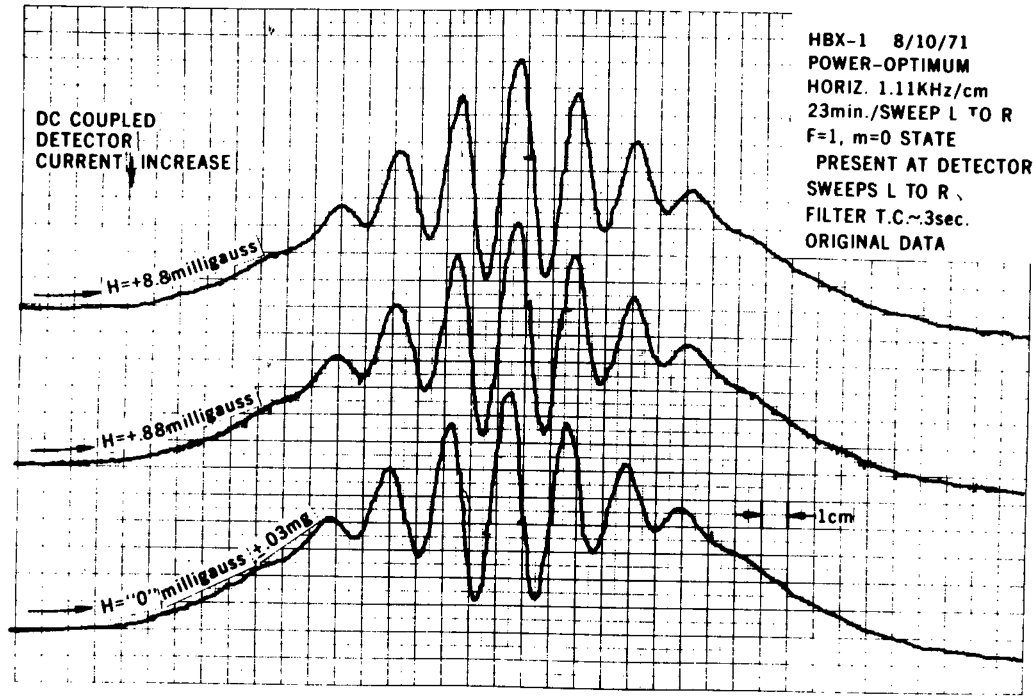

Figure 6. HYDROGEN TRANSITION (F=1, m=0) → (F=0, m=0) at VARIOUS MAGNETIC FIELDS

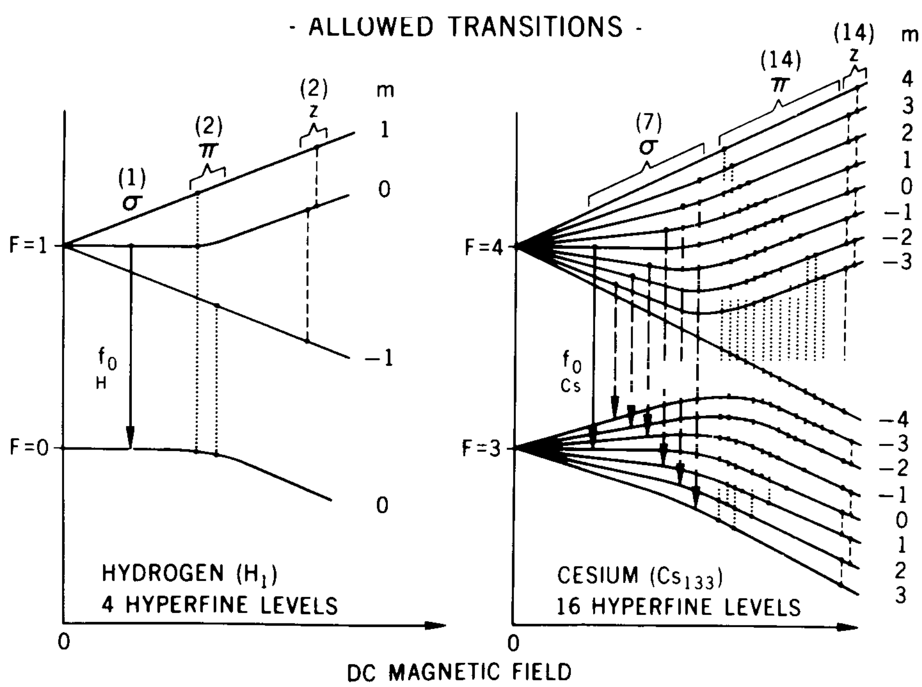

Figure 7. HYDROGEN AND CESIUM HYPERFINE ENERGY LEVELS (Allowed Transitions)

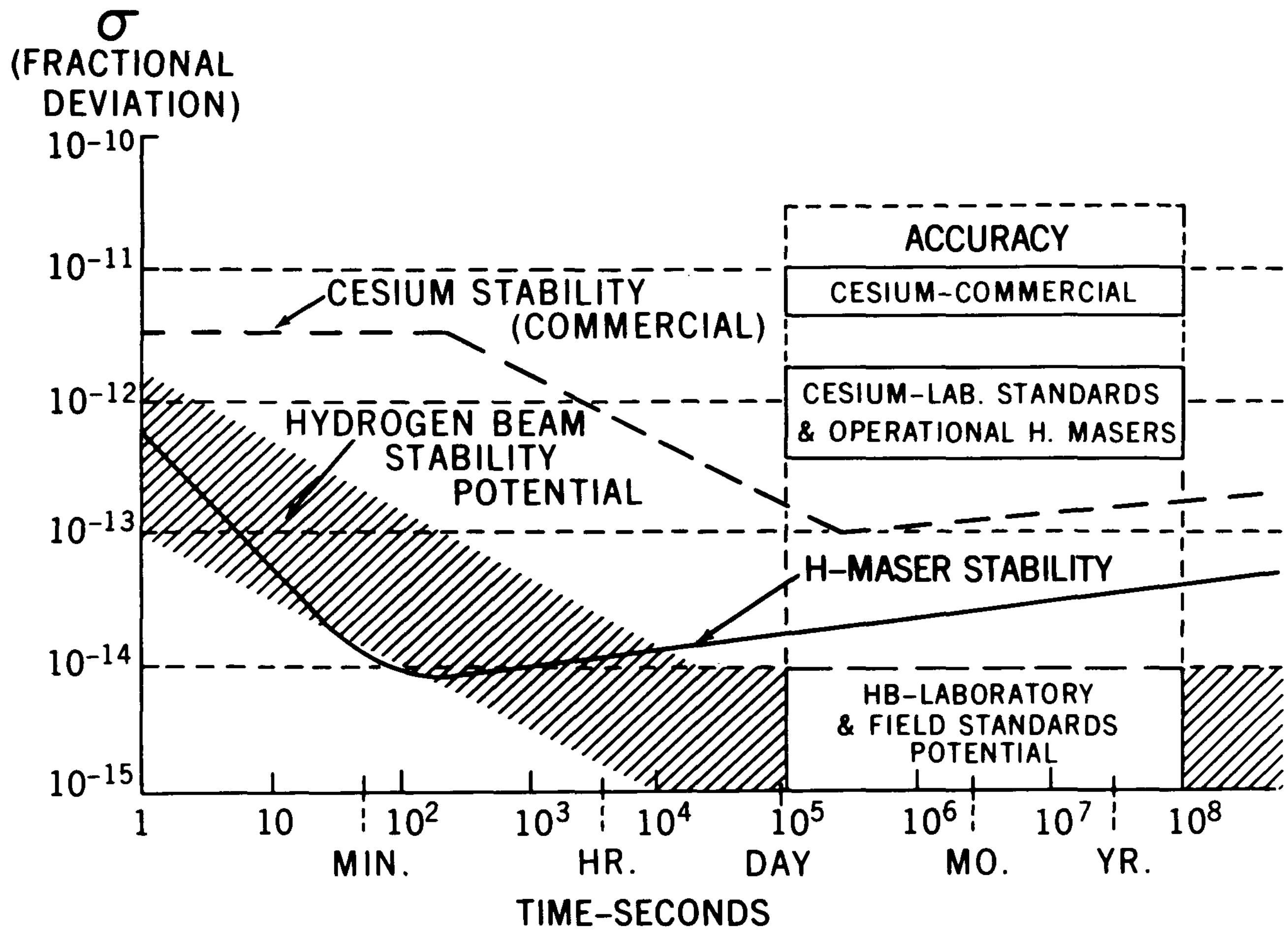

Figure 8. STABILITY AND ACCURACY (Hydrogen Beam Standard vs Hydrogen Maser and Cesium Beam)

to compare a potential stability of a hydrogen beam, an experimental possibility, with state-of-the-art of other equipment. The hydrogen maser, we expect, will be improved. We expect cesium will be improved, too.

A hydrogen beam device is shown in Figure 9. It is primarily designed to test velocity distribution, detectors, and various other problems that used to be considered a real problem with hydrogen beams. Figure 10 is a diagram of this device. We have a source of atoms, transition field region, a detector, and resonances in the laboratory at this time. It is not necessary to have such a large hydrogen beam device. However, it will not be as small as a hydrogen maser and it will not replace commercial standards, such as rubidiums or cesiums, which are much more portable. These will be important in the future, if we can achieve the stability and the accuracy illustrated on Figure 8. We have one operating experimental beam apparatus and have parts for another in the lab. We have just completed, on the drawing boards, a unit which is much smaller, has a higher velocity atomic beam than the one I illustrated, and could be something for the future which is field operable and has all of the advantages of the beam technique. If we are successful with this, we will have two good ways of looking at the hyperfine transition of atomic hydrogen. I have very great hope for it in all of the applications which are coming up in the future.

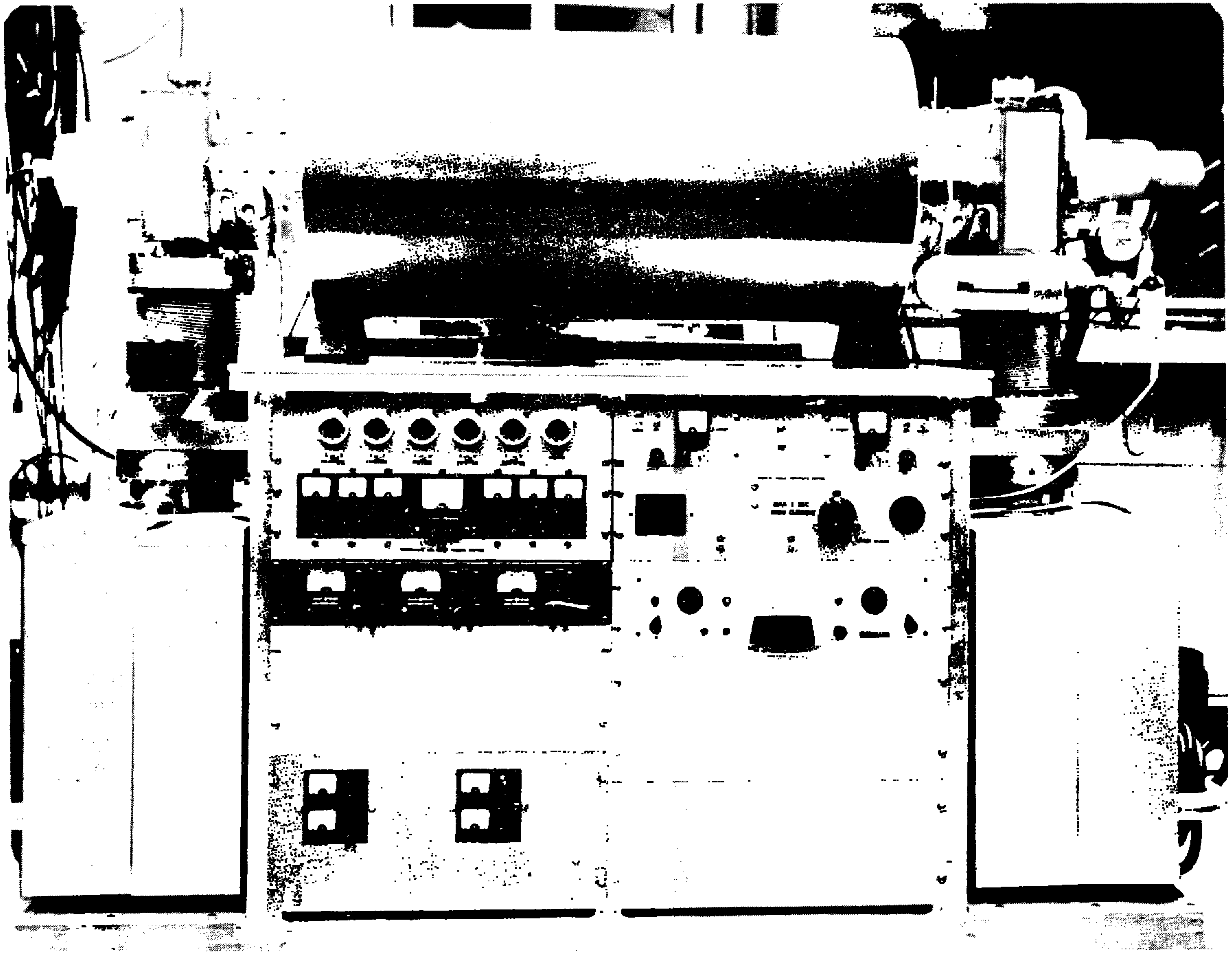

Figure 9. HYDROGEN BEAM DEVICE

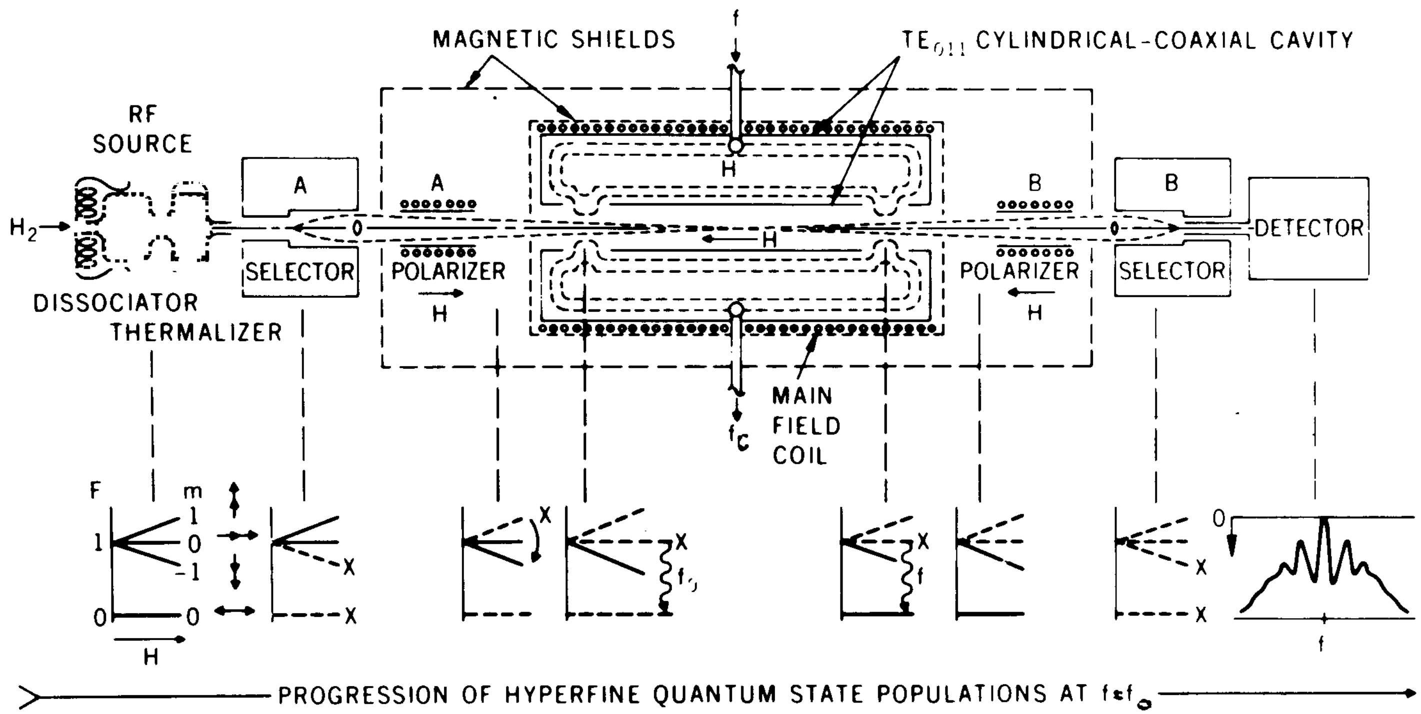

Figure 10. HYDROGEN ATOMIC BEAM STANDARD SCHEMATIC

DISCUSSION

DR. WINKLER: Mr. Peters, I am delighted to see your illustrations and performances, but there are two items to which I would have to take exception. First, is your statement that the first machine is portable. I don't think it is portable in the Navy sense, because I didn't see the crane hook. The second item to which I would like to take exception is your claim on accuracy. It has been the unanimous opinion of the members of the consortitive committee, which was formed to provide recommendations to the International Conference of Weights and Measures that it would be premature to even consider changing the definition of the second from the present reference (the cesium atom) to anything else, including hydrogen. This is because there is no clear understanding as to what way any frequency extracted from the hydrogen atom will be directly traceable, by means of a theory, to a fundamental natural constant. Therefore, I would think that the claim of accuracy is confusing. If you would restrict yourself to precision, I would be in complete agreement.

MR. PETERS: Again, I think that accuracy analysis of the hydrogen beam shows some rather startling results. There is not a unanimity of opinion throughout the scientific community on what we might have in the future, although I certainly am in agreement with the committee, of which I am not a member, that for practical reasons they have the best choice at the moment. Returning to your first question, we have never used a hook for moving masers. However, we have used airplanes and they are easy to transport. We use elevators, we don't carry them on first class, I'm afraid.

DR. VESSOT: I thought I'd add that we are building four small standards at a comparatively modest cost. These are being built for radio astronomers; in particular for NRL (Mr. Easton and Dr. Meyer each will be using one), for the Haystack Observatory, and for the NRAO Observatory. We have an inquiry, I think a serious one, from the Swedish National Science Foundation for the observatory at Onsula, also. These masers are substantially smaller and weigh between 500 to 600 pounds, depending on how many pounds of batteries you try to jam into them. They travel very nicely in the belly of the 747 and we have found that four able-bodied technicians with good stomach muscles can indeed lift them, although they have no handles or hooks on them. Generally, we slide a pallet underneath them. They will travel in the back of a station wagon. So, although it isn't going to be a small rack-mounted item as we've seen for some other standards, there is hope one can wander around the country at modest speeds and with some degree of flexibility with the hydrogen maser, as long as you have four strong people who are willing to lift it.

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MR. PETERS: Thank you. Could I comment on portability, and operation in transit? This is interesting in regard to the special relativity experiment. It is known to be possible to make time and frequency comparisons between aircraft flying overhead and ground stations which have very excellent standards. This can be done by looking at a plane going overhead; first in one direction and then in the other, and integrating it over many, many samples with only one moving standard involved. It is also possible that a hydrogen maser could contribute to this experiment, since they can operate under these environments very well. They are not subject to vibration or acceleration to an undue degree. Their mass helps stabilize them a bit. They will operate in an aircraft and supposedly there aren't many magnetic field variations, not many cars going by up there, so that we might get extremely good accuracy on such an experiment. I think that portability is illustrated in these possibilities.

MR. FOSQUE: I would like to ask Dr. Winkler a question regarding the comments he made. Dr. Winkler, you made the point that the Committee for the Definition of the Second, had concluded that the adoption of a hydrogen standard was inappropriate at this time. I wish to make sure that I understand completely your comments regarding the accuracy concerned that the oscillators would not oscillate repeatedly on the same frequency or just that not enough evidence had been accumulated over a period of time.

DR. WINKLER: The principle involved is that you would need to be able to relate the output frequency from the device to an inner atomic condition under conditions of controlled environmental influences. For instance, if you take a cesium atom in an atomic beam, the only force which acts on it while it is in a transition region is a magnetic field, which can be controlled. There exists a formula to give the effect of the magnetic field. It is true as Harry Peters said, in a case of cesium, one cannot reduce that field to the same lower levels as you operate in hydrogen, but at the same time, I would point out, that even in hydrogen, you don't go to 0 field. So, the idea is to require that a clear connection, by means of a physical theory, exists between what you measure at the output and between what goes on inside the atom. An atom, which during the observation is bounced around, is perturbed mechanically by a close contact or proximity to other atom's teflon. For instance, teflon which is bouncing with other hydrogen atoms, is not conducive to a clear understanding and that is the reason why the cesium atom has been recommended, and of course, there are other reasons. It is very easy to detect in a beam apparatus. The accuracy requirements, although not the very highest of all possible atoms, are certainly sufficient for all present practical applications and for these reasons it is extremely unlikely that any change in the definition will be considered for the next couple of years, possibly ten years or so. A fundamental change in the situation would have to exist. But let me clear up another misconception once and for all. Relativity experiments are not proving a special theory of relativity. They are trying to prove, or to be in conformance, with the consequences of the general theory of relativity, because the clock effects are due to the distortion of the matrix of space and time. In the case of the special theory of relativity, you have no such distortion. You have transformation formulae which bring you from one system to the other, but there is no absolute frame of reference. In the case of the general theory of relativity and the application which Professors Hafele and Keating have made of the clocks, you do have absolute frames of reference; any inertial frame of reference can be used for that. Space exhibits a frame of reference, when you talk about accelerations, but it does not exhibit any absolute reference when you talk about uniform motion. So, a clock effect exists only in the concept of the general theory of relativity, because it is a consequence of the distortion of the space time matrix.

MR. FOSQUE: Excuse me, Dr. Winkler, I'd like to again make sure I understood the impact of your comment. My own limited knowledge would lead me to conclude that if a sufficient number of these hydrogen oscillators were constructed and if it was found that over a wide variety of circumstances they oscillated on pretty much the same frequency, although it might take five years for the CCDS to change the definition, it is not fair to say that these won't prove to be a better standard than the cesium could.

DR. WINKLER: That is not what I wanted to say. There may be a better frequency standard, but the question is, do we understand the disturbances. If you build a hydrogen maser without looking at any other standard, where will that output frequency be in reference to one which you have built according to different principles. There is a tremendous amount of detailed information. I think that one of the prime concerns of the Standard Laboratories is to investigate the effects of the observation of these standards. In the case of cesium, you have a more transparent situation than in the case of any confined observation space where you bounce atoms around and expose them to additional perturbations. It is not a question of better or worse, it is a question of theoretical understanding and transparency in that process of explaining an output frequency in terms of an inner atomic situation or constant.

MR. FOSQUE: Could we perhaps have your opinion as to what might happen if we had a well proven, more stable, center frequency for the oscillations without suitable theory for this explanation. Would you care to comment on what the CCDS might do under those circumstances?

DR. WINKLER: I don't think that would, at least under the present philosophies of operations, be considered a possible candidate.

MR. CHI: Dr. Winkler, would you wish to distinguish the difference between the hydrogen maser and the hydrogen beam? For instance, if you're discussing in terms of hydrogen beam, it would not be the same as cesium beam; rather than trying to consider hydrogen maser.

DR. WINKLER: I agree.

DR. VESSOT: I cannot resist. There is a light at the end of this tunnel of accuracy consideration on the hydrogen maser. Dr. Winkler is certainly very correct that there has been a great deal of controversy and some very substantial differences of opinion on how one should represent the hydrogen frequency in relation to the cesium frequency. Not long ago this was done with a set of measurements between Harvard, the observatory at the Smithsonian, and the National Bureau of Standards. The agreement there was within two millicycles at L Band. In regard to the wall shift, this is the effect of confining the atom and having it relatively pounded by the walls as it collides many, many times. The situation is indeed more complex than that for cesium, but it is not without a solution and I think that we are beginning to understand it well and that there are means now to eliminate it. I'd like to point out to this group something that they may not realize and that is the resonance transitions which you've seen in Mr. Peter's illustrations are the very first of their kind in the world. Secondly, they are of a resonance which is in the same nature as Dr. Winkler has described as applying to cesium, namely, that of a beam, where a particle flies and doesn't hit anything. It's in completely free fall. Those resonances, when they are properly explored, will provide us with a very good basis for comparing the corrections that we must apply in the maser to obtain the right frequency. We have both an experimental weapon and a beginning of an understanding of how to correct. I won't go into the details of how it's done, but, believe me, it's beginning to look a lot better.

DR. REDER: I wonder why no one mentioned that there are also some possibilities of improving the cesium standard.

MR. PETERS: Dr. Cutler would be best qualified to discuss that at the moment, I think.

DR. CUTLER: One comment I'd like to make concerning the magnetic field in either hydrogen masers or hydrogen beam devices, is that there are the $\Delta M = 1$ transitions, which will be excited unless the RF magnetic field and the static fields are absolutely parallel. So these do represent some source of pulling that must be taken into account and may prevent, in the last word, the reduction of the magnetic field to the really small values that you would like to use. Concerning cesium, there are indeed improvements possible there. We've been doing some work in our laboratories and will have available shortly, an improved 16-inch tube to replace, in a retrofitable manner, tubes that are in existing H.P. cesium standards. This tube has a cesium flux considerably larger than the previous tubes; hence, has considerably greater short-term stability. In addition, it has greatly improved magnetic shielding and magnetic shield structures to improve the immunity to external magnetic fields and changes in external magnetic fields and has improved the homogeneity of the field inside the interaction regions, so that the accuracy should be improved. In addition, we have included a new cavity structure that is very precisely machined and can be tested for symmetry to very good precision, so that we expect the reproducibility of the intrinsic cesium frequency with regard to its perturbation by phase shift effects, to be considerably reduced. We might expect reproducibility to be better than 1 part in 10^{12}. It's not a guarantee, but that's the sort of thing that we expect. I think that covers my comments.

DR. REDER: One more question for Dr. Cutler. Is there any possibility or has any thought been given to the possibility of eliminating states which contribute to the $\Delta M = 1$ transitions before the beam enters the cavity so that you could then use a smaller C-field in cesium?

DR. CUTLER: You would still have the possibility of $\Delta M = 1$ transitions, even if you had atoms in an absolutely pure state going in. The atoms still have the possibility of making a transition to that state while they're in the transition region due to small inhomogeneities in the magnetic field and just the latent potentiality of making such transitions. So I don't think you can do very much there.

DR. REDER: But, wouldn't it still help if you use ionic excited states in the cavity which really wouldn't make any difference, right?

DR. CUTLER: I didn't understand that remark.

DR. REDER: You can either go from excited to unexcited or from unexcited to excited.

DR. CUTLER: That makes no difference.

DR. REDER: Right. So, Suppose you would have a very good homogeneity, but have eliminated those higher states. Let's say you use f=4 states and you want to use M-0.

DR. CUTLER: That's right.

DR. REDER: If you eliminate M = 1, 2, 3 and 4 states, and f = 4, and you have a good homogeneity, couldn't you stand to reduce the magnetic field, without being bothered by these additional transitions?

DR. CUTLER: Well, you could if you had sufficient homogeneity to guarantee that you could not make a transition to any of the other lower states.