# INTRODUCTION TO

DISTINGUISHED SCIENTIST PANEL

DR. R. L. SYDNOR: I am very pleased to introduce our panel here. I think they actually probably don't need any introduction. Professor Rabi, of course besides his innumerable prizes and awards, positions in and out of the Government and academia, is well-known as the man that probably started the whole development of the atomic standards we have today.

We have collected these people at great expense to come and discuss some of their views with you. I will let Professor Rabi introduce the panel and lead the discussion. Professor Rabi?
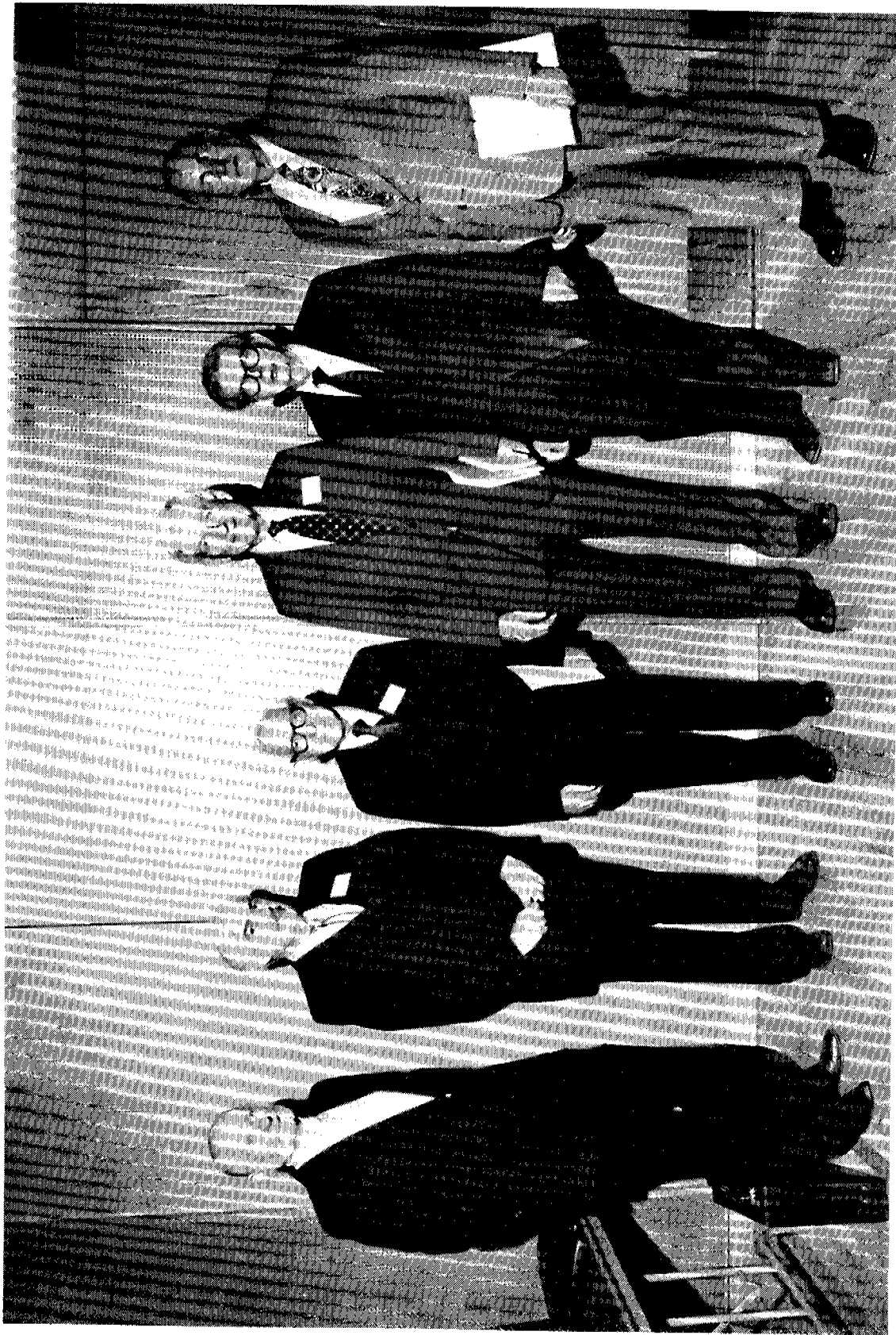
Distinguished Scientist Panel (from left to right): H. G. Dehmelt, R. V. Pound, I. I. Rabi, N. F. Ramsey, R. H. Dicke, and C. H. Townes
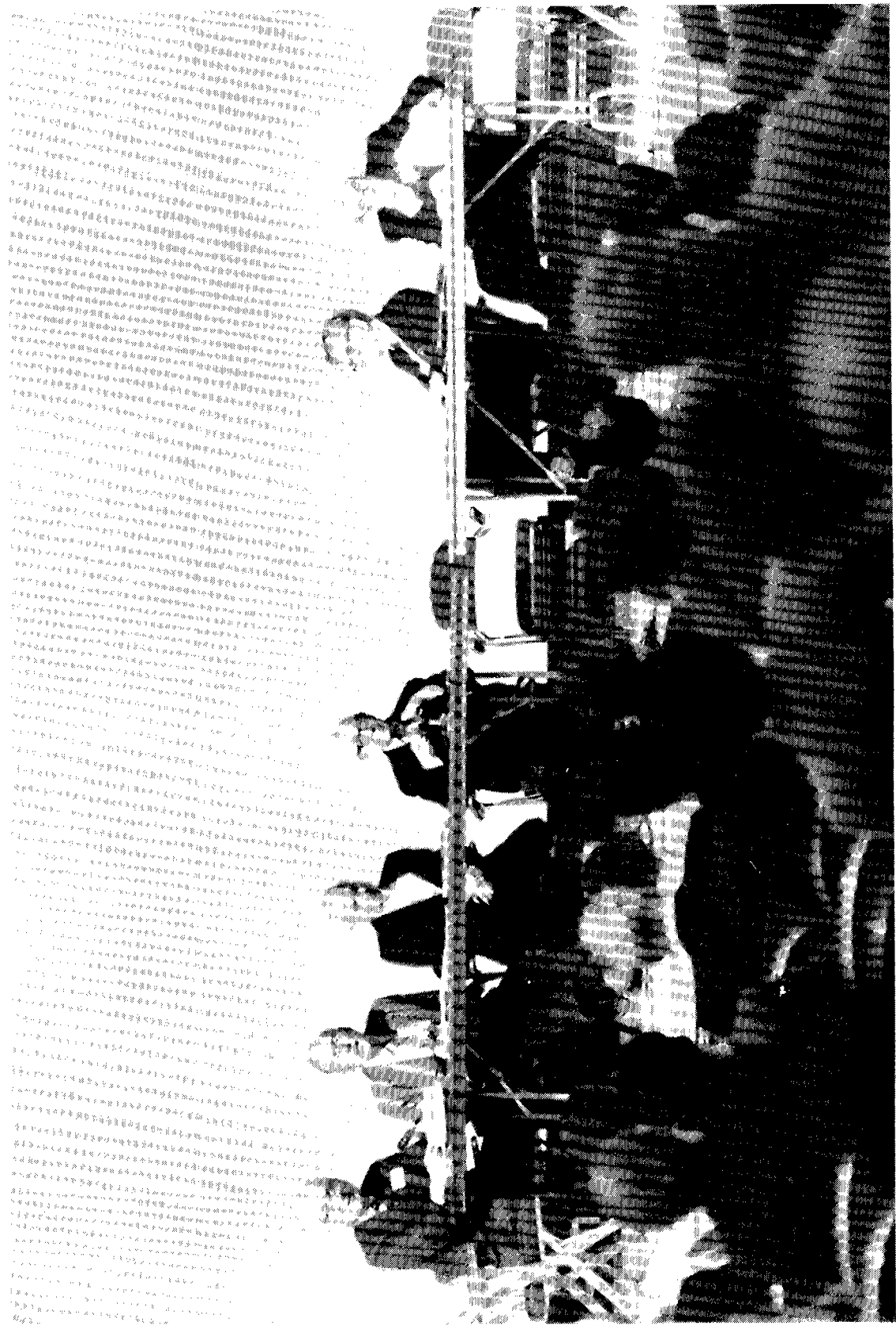
Distinguished Scientist Panel (from left to right): N. F. Ramsey, C. H. Townes, H. G. Dehmelt, I. I. Rabi, R. H. Dicke, and R. V. Pound

DISTINGUISHED SCIENTIST PANEL

Panel Chairman: Professor I. I. Rabi, Columbia University

Thank you. I am delighted to see what happened to a suggestion that I happened to make years ago at a Richtmeyer lecture, it was either in 1944 or 1945. It is something which I didn't publish. I was asked to give this Rickmeyer lecture at the annual meeting of the American Physical Society and also the American Physics Teachers.

This was during the war, and as you might guess, we were very busy. The question was what to talk about, and so I began to think of what we were doing, not in the war but what had happened before on the state-of-the-art. Now as an admirer of Einstein through high school, college, and graduate school, I was very familiar with his notion of an atomic clock. When he spoke of a clock in relativity and so on, he meant that somehow in nature there was such a thing as a clock, which is working according to the laws of nature, such as they are.

So that seemed to be the ideal thing. We always thought it was, and then I realized that, to give this talk, there was an atomic clock, and there was the atom, there were those frequencies. And I suggested this, not in any detail, but enough: This would open a rather basic field of using the basic clock of Einstein for other scientific or practical purposes. I didn't write this out but I gave this at a lecture.

Present at the lecture was the science writer for the New York Times, William Lawrence, and he published it in the New York Times, in a column, about the atomic clock. That is the way this thing was born, came to public attention, as far as I know. I have never investigated it historically but there is a fine paper here, which I have not yet read, by Norman Ramsey which goes much deeper into the subject. My points are personal reminiscences, not historical in the sense of a historian's footnotes.

This is about as far as I went. Of course, this received quite a bit of publicity from the New York Times, absolutely free, unintended publicity, and I was approached, I think it was by the Bulova Watch Company, who wanted to give me $100,000 to make this clock. My interests were otherwise and I really had never worked for a private concern, and I turned it down. But Gerald Zacharias took it up, and other people in England, the Bureau of Standards, and so on.

And cesium, I think I mentioned cesium, I'm not sure, cesium seemed to be an ideal substance for this thing. I don't think it can be patented because it was announced. My name wasn't on the publication but a famous reporter was on it, so that's about the historical basis, the only real connection I have had with the atomic clock.

I did feel then, and certainly now, that when you have a new device, you open up a new avenue in physics, great things are bound to happen in time, because I don't think, much as it may look right now when we have a unified field

theory and all that sort of thing, that physics by its very nature will not be a closed system; that nature will hold surprises for us all the time.

A story which I have told often is my visit to Max Born in 1928 with Professor Otto Stern. When we came in, I stood in great awe of Born for all his works, in a certain sense the founder of the use of matrices in quantum mechanics, Max Born announced as we came in that "Physics will be over in 6 months."

It was quite a blow. I was a fresh-caught Ph.D., I was a post-doc, and there it was, over in 6 months. He said, "Of course, there will be a lot to do but physics as we know it will be over because the shock of the DIRAC equation and what it did for the electron was really traumatic." It came right out of the relativistic equation, right out of relativistic wave mechanics.

And he said, "Of course, in 6 months we will have the proton. Of course, there will be a lot to do." And I thought to myself, "This is a great man but he has limitations."

To use Einstein's models, "der lieb gott" dear God is not so limited in physics with the DIRAC equation. And this has happened: As the advances in instrumentation have been made, more and more new phenomena have been discovered, unexpected phenomena.

To me the most remarkable thing, apart from the electron spin, was the polarization of electrons, beta decay the same as mu mesons. If you are a simpleminded experimental physicist and think of an electron or a mu meson, it is a marvelous thing. It has a spin, it has a magnetic moment, it has mass, all sorts of charge. I don't think that we will know all about it or do know all about it, despite the fact that we have excellent equations which will describe a lot of the phenomena we know.

So my feeling was and is that if we drive our experimental techniques to the limit to which we can, it has always turned out in the past that a new phenomenon will be discovered, a new aspect of nature. One of them now that is just being talked about, that people all over the world are working on it, is the possible decay of the proton. We're not accustomed to the proton decaying but we have not lived 10^{31} years and we can't say too much about it.

But almost everything -- and go to the history of physics -- almost everything which was deeply believed as an article of faith has usually been, turned out to be wrong, possible irrelevant. So I am delighted that this measurement of time, particularly time intervals, is moving ahead as rapidly as it has been. I am sure in some respects, either in atomic physics or nuclear physics or astronomy, and maybe even geology -- although one doesn't think of precision measurements of time with respect to geology, I think it will come in -- that you are opening a new field for greater and greater understanding of time.

I just wanted to add that we do have senses which give us a direct feeling for and understanding of space but not of time. Time is really an artificial thing, and dependent very much on the invention of a clock. We have not yet, to my mind, decided what a good clock is, a sensitive clock to measure divisions. What a real good clock is, I don't know, but what time means in quantum mechanics, basically when you get down to it I know how you put it in an equation but I don't know how to talk about it, let's say, to my grandson who is not a scientist.

So I am just quite inspired to be with a group of people who are so interested in pressing on with this problem of time and this measurement, particularly the interval measurements.

Now we are supposed to have a discussion amongst the people here. We have gotten together beforehand and have come to no conclusion on what to present to you, both for your entertainment and possibly edification, and perhaps a deeper look into how these things were and these eminent scientists we have assembled here.

I have no way of starting, except I think I will start with my right, with Norman, especially since Norman has a paper here on the whole business.

DISTINGUISHED SCIENTIST PANEL

Panel Member: Professor Norman F. Ramsey, Mt. Holyoke College

After Dr. Rabi's remarks, I shall add an additional reference to the paper on the History of Atomic Clocks which many of you received as a preprint at this meeting. This reference will be to Rabi's Richtmeyer Lecture and to the New York Times.

I think Dr. Rabi was unduly modest in listing his contributions to the precision measurement of time. This whole subject, in my opinion, really goes back to his earliest work on the molecular beam magnetic resonance method; the invention of magnetic resonance method for molecular beams is where the precision clock started even though the work at that time was initially directed towards the measuring of magnetic moments. The magnetic resonance method was a great invention, enabling magnetic moments to be measured to parts in 10^5 or better instead of 5% or worse.

For my remarks I have chosen to review the history of three important atomic clock developments subsequent to Rabi's initial invention of the molecular beam magnetic reasonance method. These developments are ones with which I was particularly concerned. One development was the transition from the period of the resonance method as primarily a method of measuring magnetic moments to essentially molecular beam spectroscopy or radio frequency spectroscopy, as we eventually called it in an earlier paper. My second topic will be the early history of the separated oscillatory field methods and the final topic will be the early history of the hydrogen maser.

When Dr. Rabi first proposed the molecular beam magnetic resonance method, he had the courage to start two different projects on it at the same time. Kusch, Rabi and Zacharias developed a technique for measuring magnetic moments of alkalis while Kellogg, Rabi, Ramsey and Zacharias did the same thing for the proton and the deuteron. We knew the latter would be more difficult. On the other hand the proton and deuteron were particularly important nuclei to measure.

Starting from the point of view of trying to measure the magnetic moment, the first results for the proton were in some respects pleasing and some respects terribly disappointing. What we expected to get was a nice, sharp resonance from which we could determine the peak of the curve. Instead we got something much even worse than the top figure of my transparencies [Fig. 7 of Phys. Rev. 56, 737 (1939)]. Unfortunately, it was so bad I don't know anywhere that that is preserved. There was a somewhat bigger peak in the middle but just a bunch of fuzz in the background which we couldn't interpret. Nevertheless we found that that central peak shifted with the magnetic field the right way for a magnetic moment, and then we discovered that with deuterium we got a good peak. In this way we could get the magnetic moments, but it was clear that we ought to get better resolution to see how much of the peculiar

resonance shape was apparatus effect and how much of it was perhaps something intrinsic. There was also the problem at that stage though, since I was a Ph.D. student, what does the Ph.D. student do for his Ph.D?

It was decided that I should study this fuzzy background for my Ph.D. thesis while Kellogg and Zacharias were constructing the next round of equipment which was going to have higher resolution. Basically, what I found during the summer -- this was during the summer of 1938 -- was that if one lowered the amplitude of the oscillating magnetic field the resonance shape got steadily better. Unfortunately I dropped the amplitude in too-small steps. I would drop it a factor of two and when I found it was much better, another factor of two. I should have done it in factors of 10 and it would have gone a little faster. It took about 2 days to get one of those curves, because each point was taken very slowly. At low enough oscillating field amplitude it became apparent that there were several resonances, as in a spectrum.

At this point it began to become clear that we were studying spectroscopy, not just measuring magnetic moments. There really did seem to be at least five resonances and perhaps six, if you wanted to consider one as a double resonance (Fig. 8 ibid.). At the end of that summer, the newer modifications of the apparatus being made by Zacharias and Kellogg were completed, and then we obtained the bottom curve (Fig. 6 ibid.).

It was essentially this work that marked the transition of the magnetic resonance method from just the measuring of magnetic moments to spectroscopy. Also, a key thing was that the distance in frequency between the left most peak and the right most peak was independent of the strength of the magnetic field and therefore did provide something that was field-independent as is required for a clock. Other than that, the device was really more of a magnetometer.

In the summer of 1938, I also did similar studies with deuterium. Initially there appeared to be a single, nice isolated peak, but when I studied it with a little more care over that summer it became apparent that there were really subsidiary resonances in the background there, too. [Fig. 1 of Phys. Rev. 57, 681 (1940)]

Again with the improved apparatus that was being made during the same summer, we later obtained a resonance curve similar to the bottom curve where there was clearly one very marked resonance and several subsidiary ones. At this point it was quite clear that we were studying radio-frequency spectroscopy, the first step to something that would be a good basis for an atomic clock. Again, the separations of the subsidiary curves from each other were approximately magnetic field independent.

A year or so later Kusch, Millman, and Rabi found a resonance with lithium 7 in zero magnetic field, by looking at a transition for which there was a relative reorientation of the electron and nuclear spins. Such a fixed frequency resonance provided the bases for an atomic clock.

I think these comments essentially cover the period of transition from magnetic moment measurement to the potential, at least, of a clock. Subsequently many developments led to the later clocks.

Now the second thing I would like to say a little bit about is the development of the separated oscillatory field method which is used in almost all of the cesium clocks.

In the case of the invention of the separated oscillatory field I had just gotten a new job at Harvard and I was trying to build up an experiment. One of the experiments that I wanted to do was to do a more precise job in measuring the interactions that we had discovered with H_2 and D_2. In one sense the obvious way to do so was to make the apparatus longer because by the Heisenberg uncertainty principle, the longer the atoms are in the apparatus, the longer the time Δt and hence the smaller the ΔE, the uncertainty in the width of the resonance.

But I also knew from our Columbia experiences that this didn't work. It didn't work for the following reason: if one increased the apparatus much longer than we had at Columbia, the inhomogenieties in the magnetic field were so great that the line became broader rather than narrower because the field was lower at say the final end of the oscillatory field region than it was at the beginning. The question was, how do you overcome this. While I was still building the new apparatus and not quite sure how to overcome this difficulty, I was giving a course on physical optics at Harvard. The time came to talk about the Michelson stellar interferometer. One of my friends in Cambridge, England, describes the Michelson stellar interferometer as follows: If you have two stars that you want to distinguish, if you have a big telescope that doesn't quite separate the two, and if you borrow a can of black paint and paint over all the middle of the telescope except for leaving two little slits at the edge, you double the resolution of the apparatus and can thereby resolve the stars. This procedure has the added advantage that it doesn't make any difference how bad the glass is in between because, clearly, what is under the black paint won't make any difference.

Well, I was in the midst of describing this in these terms to my class, when I thought, "Wouldn't it be nice if we could do the same thing with our magnetic field, some how wipe out the middle of it." Eventually I realized that the analog in that case would be to put an oscillatory field at the beginning and an oscillatory field at the end, whence indeed one obtains a resonance that is twice as sharp as it was before, and in addition it doesn't make any difference if one has moderate fluctuations of the magnetic field in-between, because they average out correctly. That is essentially the origin of the idea from which, the separated oscillatory field resonance method came. Subsequently many modifications and further extensions were added.

The third thing, I would say just a word on is the invention of the hydrogen maser. I have an analogous history in that case. For this history I am especially sorry that Professor Zacharias is not here. I was hoping that he would be. One of the things that stimulated this invention was the disappointment I felt when Zacharias proposed the so-called fountain experiment. This

was a brilliant idea. Namely, he was going to do molecular beam experiments much sharper than people had done before, increasing the time and getting the benefit of the Heisenberg uncertainty principle by having the beam shoot up and then fall down under gravity. It is well known that anything, whether it is baseballs or atoms, take around a second to go up 16 feet or so and come back down again, and therefore we would get an immensely fine resolution.

Unfortunately, this experiment failed to succeed because there weren't enough very slow molecules. They just weren't there, presumably due to scattering. The Maxwellian velocity distribution in practice dropped off to zero, so although the experiment was a failure the idea was a good one. I was thinking, "Why didn't I think of that?" Well, at the same time I was also, in my split personality, helping with the design of a big accelerator being built at Cambridge with a 236-foot diameter ring, and we were proposing this to be constructed. This machine was eventually constructed but the time of concern was in the early phases of it.

There is always the worry when you are proposing a multimillion dollar machine: "What do you do when it fails?" Well, one thing would be to convert the tunnel to a great molecular beam. You would have the molecular beam go around in a circle and keep it there a long time, and hence get a very high precision. I realized you could do this with an inhomogeneous magnetic field to confine the beam to a ring. Then I also realized, alas, that this is just the wrong thing to do because if you have an inhomogeneous magnetic field and are trying to look at the spectrum with high precision, you of course broaden all the lines. If you have an inhomogeneous field and some of the atoms are in one field and some in another, it would be fine for trapping the atoms but it would be no good from the point of view of spectroscopy. Incidentally, something like that is currently done now by Wofgang Paul with neutrons, but for measuring the neutron half-life.

Well, having realized that problem, then I also realized what was really needed. It was much better to have an intense interaction for a very short period of time rather than an inhomogeneous field essentially present all the time. The limiting case to that was to put a box around the ring, essentially a long tube and have the confined atoms there. Well, then I realized that you could do even better than that if you closed up the ends of the tube. If the atoms are going to hit the walls of the tube, you might as well let them hit the ends as well. Eventually this led to an experiment which was done by Dan Kleppner working with me for his Ph.D. thesis, the so-called broken atomic beam experiment where we made a box and a beam of atoms came in, as in a molecular beam resonance experiment. We let them undergo four or five collisions and come out in a different direction, and measured their transition and did indeed get a resonance, but it then became apparent to both Dan and myself that we would do very much better with atomic hydrogen than with cesium, which was being used for the first run. Cesium tended to stick on the surfaces, but then hydrogen has the disadvantage of being very hard to detect. So we thought maybe we could detect the radiation, and then eventually realized we could in fact detect the radiation very well. Then we also recognized that the system could be a self-oscillator or an atomic hydrogen maser, analogous to the molecular ammonia maser that had been developed earlier by Charles Townes.

It is from this that the idea for the atomic hydrogen maser came. We realized that atoms could be used in such a confined box. This also differed from what had been done, say in optical pumping, where at that time extensive amounts of buffer gases were used to narrow the lines and retain the atoms.

In this case we realized we could do it with aligned beams from using the molecular beam technique, and then have a box initially with paraffin-coated walls. Then we discovered experimentally that it was better to do it with Teflon, which has been essentially the standard thing in hydrogen masers since.

Well, so much for past history. I'll conclude with a brief remark about the future, I fully echo Dr. Rabi's views that there is much to be said for precision measurements. Out of them come unexpected things. At one time we thought that there would be no interest in clocks if they were more accurate than a part in 10^7, and now it is quite clear that each time we make a step forward, there is a small revolution. Now most highly accurate measurements are reduced to the measurement of a frequency because frequencies can be measured so well. Precision measurements of almost all quantities now remind me of my mother's story of a doctor who could cure only one disease -- stomach ache -- but he became a great doctor because he also knew how to reduce all other diseases to a stomach ache.

Well, this is basically the procedure for almost all precision measurements at the present time. We know how to measure one thing very accurately -- time and frequency -- and therefore the big advances in all other measurements have come from figuring out how to reduce those measurements to measurements of time or frequency. As a result, precision time and time interval measurements are at the heart of nearly all precision measurements at the present time, and I believe that in the future this will be more true rather than less true.

Thank you.

DISTINGUISHED SCIENTIST PANEL

Panel Chairman: Professor I. I. Rabi, Columbia University

Thank you very much. We now, I suppose, have done the pre-War effort which
resulted in the measurement of time with atomic clocks, we will go on to
Professor Townes. I don't know whether he got into this act before the War
but he certainly did after the War. That will perhaps conclude the Columbia
contribution.

DISTINGUISHED SCIENTIST PANEL

Panel Member: Professor Charles A. Townes, Berkeley University

Thank you very much. It is a great pleasure to be with this group of people again, whom I used to see frequently and from whom I have received a great deal of instruction and inspiration, but I have been out of the field now for some time.

Looking back, one can see in this field, as I think in so many fields, a burst of activity in a field which was considered classical and more or less finished. I think that is very typical of how science has developed. I was with Bell Telephone Laboratories during the early forties, and I was doing microwave spectroscopy, one very precise measurement of the frequency of microwave lines. Bell Laboratories was one of the most expert places in time measurement. Quartz crystal oscillators were the thing, and they were fantastically good, at least as good as a part in 10^8, and if one had a collection of them then perhaps you could approach a part in 10^9, and that seemed just awfully good. Of course, we do a million times better now but there were people who had been working on it a good fraction of a lifetime. They knew all the answers. They had worked on it very hard. Quartz crystals were supreme, and they had just reached the limit of the technique.

What was needed, of course, was a completely different approach which by now has changed the situation, and furthermore, in changing the situation has made the quartz crystal people look again and find in fact they could do a lot better with quartz crystals than they ever thought they could before. As I say, I think that is fairly typical of how scientific fields develop. Currently the quartz crystal people are doing remarkably well and considerably better than what was felt to be the limit.

And I might quote the field of amplifiers, where people have worked on sensitive amplifiers for many years, derived theories showing what the limits were, and the limits were good but not as good as we have today. When maser amplifiers came along, which improved the sensitivity by a couple of orders of magnitude, then people began to think again about some of the classical techniques and new techniques. Now many of the classical techniques do almost as well as masers, and the whole field has proliferated into a wide variety of techniques for very fine amplifiers, simply because people were forced to challenge, to look rather more thoroughly than they had before.

In each case you see new ideas have come in from elsewhere to stimulate the field to get going again.

Now when I was trying to measure the frequency of lines, there was some very remarkable work that Bob Pound had done, stabilizing microwave oscillators on cavities, and that was spectacular in its day, giving fantastic stability. You've got the stability of a cavity rather the stability of an electron tube as an oscillator, and we used some of those to stabilize oscillators

but it became apparent to me that while actually microwave lines and microwave spectroscopy was just developing, they were essentially as good as quartz crystals and probably better than cavities.

As everybody is pretty well aware now, I think, there are two aspects to getting high precision. One is getting good enough signal-to-noise, and that could easily be done in microwave lines or, for that matter, in stabilization on a cavity. The other was a high enough Q, and one which is stable and not affected by the surroundings. Microwave lines had a higher Q than normal cavities by an order of magnitude or so, and so pretty quickly I was examining the theory of how well one could do by stabilizing on molecular lines.

Now I have never really built an atomic clock myself, at least not purposely. I was interested in the theory, I was interested in using them, and I built them somewhat more accidentally. Harold Lyons built a clock based on stabilizing a microwave oscillator on the ammonia line, in much the kind of general technique that Bob Pound had introduced, and I remember very well his wanting to call it an atomic clock, and I kept saying, "Well, it really is a molecular clock." But this was 1948 and he said, "Well, you know, everybody pays much more attention to the atom these days. It's got to be an atomic clock and there are atoms in it after all."

Now the trouble with molecular lines for a random gas was that the Q was still not terribly high, so a frequency could be pulled by a variety of external influences, and of course this is where Dr. Rabi's suggestion of using molecular beams and a variety of other techniques for longer-lived transitions obviously in the long run would be more stable.

Nevertheless, when the maser came along the maser added one thing, in giving a spectacularly pure frequency. Its long-term stability at that time was not terribly good. It was better than gas lines because we were using a molecular beam. It had a longer transit time so it had an improved Q by, again, another order of magnitude or so, and it was a pretty good clock but its real contribution, I think, was the purity of the spectrum over short lengths of time.

And then Norman Ramsey's invention of the hydrogen maser added that to a really very high Q system and made us a beautiful clock.

Now I think another interesting aspect of the developments of fields when there are breaktrhoughs and new ideas introduced, is their proliferation into other fields. Norman has mentioned some of that, the use of these new ideas in a wide variety of ways, and of course time has been very important in a variety of aspects. But when I had to face giving this talk and read what we were supposed to do, to talk about the historical context of development, I decided that maybe I had better go back and look at some of my papers and see what I was saying at that time, in order not to kid myself as to, in retrospect, how things looked.

I am going to put you through the reading of a paragraph of one paper of 1961, which was one of the applications of precise time or precise frequency measurements. Now in 1961 there were a few lasers in existence, just a few types, and this was a meeting of the Second International Quantum Electronics Conference. It happened to be meeting in Berkeley, and I had been in Washington for a couple of years and really not doing very much physics but I was supposed to give an introductory talk, so I had to say, "Well, let's look at the future, what kinds of things might be done." You may be surprised at what I said at that time, simply because it all seems so obvious now, but because I was saying it at that time to a sophisticated group of people, we'll have to assume it wasn't completely obvious. Here is what I talked about, one of the things, one of about five experiments I talked about:

"Maser techniques will probably make possible frequency multiplication from a radio frequency of microwave range into the visible and ultraviolet regions." Now I said "maser" which I was using at that time in a general sense; the maser, of course, the principle is operated at any frequency.

"It should be remembered that physicists have never directly measured the frequencies of infrared and optical radiation but rather they measure wavelengths in this region and then compute frequency from a knowledge of the velocity of light, accurate to about a part in a million which was the accuracy of the velocity of light at that time. Frequency multiplication up to visible regions should allow these frequencies to be directly counted, and hence measured in terms of our standard of time. Our standard of length is now defined in terms of the wavelength of visible light; hence, a measurement of its frequency should allow immediately a determination of the velocity of light to a precision as great as that to which length is defined. The measurement of frequencies in the optical region to precisions greater than about 1 part in 10^8, to which length is now defined, will in fact connect length and time together through the velocity of light so firmly that perhaps separate standards of length and time would no longer be appropriate. Time, which can be measured and defined more precisely than can length, might be taken as a fundamental defined unit and length then derived from it by taking the velocity of light," and so on.

Well, as I say, all that seems fairly obvious at this time. I had to go and describe how one could hope to get frequency multiplication and beats and so on in the optical region in order to get that, and I don't think it was taken awfully seriously at the time but of course people have done very beautiful work in this range now, and we in fact, now use time as a standard of length.

A couple more things I want to mention as a way in which these things come back on themselves, spread out into other fields, and interact: I haven't worked in this field for a long time. As I say, I never really built any good clocks because I was mainly interested in using them and in the theory, but much of my work depends on that and quite recently I have been interested in interferometry, spatial interferometry, Michaelson type spatial interferometry, and

I want to remind you of what time really was at some time in the past, and that is a measurement of the rate of the rotation of the earth. That was the definition of a second. We no longer use that as a standard anymore because that rate can't be measured accurately enough, and anyhow it's not all that constant.

Nevertheless, the rate of rotation of the earth is still of some importance to various things, such as navigation and the study of the earth itself, and interferometers built primarily from astronomical interest, spatial interferometers which measure the position of stars, are now going to give us a substantial improvement, I think, in the measurements of the rates of rotation of the earth.

There are a number of kinds: radio interferometers, and you know the connection between radio interferometers and timekeeping, radio interferometers which will measure positions of quasi-stellar objects, for example. There are infrared and optical interferometers, I happen to be working on infrared but optical interferometers are bing built of good quality, and all of these will measure stellar positions considerably more accurately than we have been able to do in the past. It seems really quite likely we will be able to approach a precision of about a part in 10^9 in the rate of rotation of the earth in one day, and of course in longer times the precision can build up.

Now the measurement of the rotation of the earth is a kind of a dirty problem, though. It's not exactly fundamental physics. The real limitations in the long run are the behavior of the atmosphere, because we have to propagate waves through the atmosphere in order to measure the position of stars, and the motion of bedrock. Those are going to be the primary limitations. As we improve techniques, we will find out a good deal more about just what those limitations are, and of course already the motions of bedrock, continental drift, and tidal motions and so on are being determined, and presumably all of this will contribute to some extent to atmospheric and earth physics.

I think that is all the comments I will try to make at this point.

DISTINGUISHED SCIENTIST PANEL

<u>Panel Chairman</u>: Professor I. I. Rabi, Columbia University

Thank you.

We did not get together and tell one another stories of what we were going to tell you, and this has been fascinating to me and perhaps the rest of you as the story comes out from Norman Ramsey and Charlie Townes.

I did say somewhat humorously that these frequency measurements that you are doing, time measurements, will ultimately be related to geology. There you are. Townes has just closed this, so there is no field in which this time measurement, especially intervals and perhaps in very short intervals, will not yield a great deal of basic knowledge. Of course, I spoke a little earlier more in fundamental knowledge, the use of the whole concept of time and its relation to the structure of physics.

One of the most fascinating series of experiments, which fascinated me through the years, were the experiments which were done in Heidelberg and the use of this technique developed by Kastler of optical pumping, which later on found use in Kastler's laboratory in Paris. These methods and these techniques were used to demonstrate mostly quantum mechanical properties of light and the analogies and very brilliant theory, and beautiful experiments, but I was very much impressed that in Heidelberg these methods were applied to molecular beams yielded new results of a fundamental nature.

I hope to hear and learn more of the thinking of our next speaker, of those experiments and the fascinating things he is doing now on a man-made artificial atom, namely having a single electron or positron floating around under his control at his time. Won't you please --

PROFESSOR DEHMELT: Mr. Chairman, as the youngest member of this group, may I pass for the moment?

PROFESSOR RABI: Very good. To whom do you wish to pass the torch?

PROFESSOR DEHMELT: Well, the next man in line.

PROFESSOR RABI: We have two. Which one?

PROFESSOR DEHMELT: Dicke.

PROFESSOR RABI: Alright, you are nominated.

PROFESSOR DICKE: I don't know whether I should pass or not.

PROFESSOR POUND: We'll have to get out the birth certificates.

PROFESSOR RABI: Well, the rules are such you don't have to give a reason for passing.

DISTINGUISHED SCIENTIST PANEL

Panel Chairman: Professor I. I. Rabi, Columbia University

I think rubidium had another role, wasn't there, Charlie? Wasn't rubidium what you used for the maser?

PROFESSOR TOWNES: No, we used ammonia.

PROFESSOR RABI: Ammonia, yes. I confused Zieger, who worked with you on ammonia, who previosuly had worked with me on rubidium.

PROFESSOR TOWNES: Oh, yes. Well, we needed Zieger's help, certainly. We also needed the big nitrous elements which ammonia had.

PROFESSOR RABI: Those are interesting questions. Since we are reminiscing about old times and how things came to be, as you know, Charlie Townes was at Columbia while we were doing the molecular beam work. Of course, to understand what was happening in these experiments with molecular beams and selection of states, with all due respect to Professor Ramsey, we knew about atomic spectroscopy and the spectroscopy of the waves which we used. They were not exactly microwaves then, but the important thing was the Einstein -- I come back to Einstein all the time -- induced transitions in an excited state in the molecular beams at those levels, at those frequencies which were being used, would live a very long time and we wanted, then, the atoms to move from one state to another, from a lower state to a higher state and also from a higher energy state to a lower energy state, and there you used Einstein's induced transitions, the idea which he introduced back in 1916 for the famous paper. He had arrived at the Planck formula but really was the basis for what happened later in the whole quantum theory, and leading to the Heisenberg dispersion principle.

It seemed that we might use something of this sort but when we calculated, the intensity was low. The question was, how could this radiation be detected? In preparing this famous lecture -- "famous" in the sense that I talked about it first -- the Rickmeyer lecture in 1924, I thought about this matter, and this was in 1924. Great, great improvements had been made in electronics. I said earlier about the detection of these transitions which led to the measurement of moments, that they could not be detected by electromagnetic means -- it was too weak -- but that you had to see the effect on the atom, and that's what the atomic method was about, but then in writing this I said, "Is this really true now?"

I went and spoke to buddy, Ed Purcell, and spoke to Henry Tari about this. They felt yes, the method was sensitive enough that they could detect those transitions, and induced radiation from those atoms. I also had my doubts about this and talked to Felix Wacht. This was during the War, and East and West were rather concentrated in Cambridge. Purcell was at the Radiation Lab

Various things indirectly came out of this, and one is the atomic clock and the other is the nuclear magnetic induction and other things like that. Something which was impossible, seemed to be impossible in 1940 -- as I was talking last night -- as the result of a war, a lot of people working in their field, it became possible four years later.

And now we have Professor Pound. We have two Harvard people now.

PROFESSOR POUND: Two what?

PROFESSOR RABI: Two Harvard people.

PROFESSOR POUND: Two Harvard people? Well, why not?

PROFESSOR RAMSEY: I am now at Mt. Holyoke, Professor Rabi.

DISTINGUISHED SCIENTIST PANEL

Panel Member: Professor Robert V. Pound, Harvard University

I thank Charlie Townes for perhaps, at least in part, giving some explanation of what I am doing here because I was asking myself that and I thought probably you were all asking that even more, because not many of you probably have coupled my role with atomic clock development.

It does happen that this project which in fact I got into by strict accident in the Radiation Lab -- it wasn't on the route of Radiation Lab's commitments, in fact -- the business of stabilizing microwave oscillators grew out of some customer of mine. I was in the business of making mixers and things like that for microwaves at the time, and Bob Dicke and I used to talk a good deal and Bob had introduced the concept of the "magic T" and a customer of mine asked me, had I any good ideas as to how to make a microwave discriminator. I said surely that has to be based on a "magic T" and after a few scribblings on paper, I came up with a thing that would play the role at microwaves, the thing that was used as a frequency discriminator in ordinary, low-frequency radio.

Then I said, "Gee, I think that could be used for purposes of stabilizing microwave oscillators, and so on the side I gave that a try and I was rather surprised at how much it did. Previous to that, if you took a couple of microwave oscillators like Klystrons and you tuned them, say, within 30 MHz of one another so that you used a 30 MHz receiver, communications receiver to listen to the beat note between them, you didn't hear anything much at all but a lot of hash and trash and so forth. I discovered that by making two of these things you could get a pair of 10 GHz oscillators so that they beat together and produced an audio tone. You used to be able to go up and down the corridors where you could hear my audio tone going, sort of wiggling around, but you know, it was wandering on the order of 100 cycles or so with a 10 GHz base, and I was sort of impressed that you could do this.

Actually, it has been mentioned that obviously -- Charlie said that the obvious next direction was to use, instead of a cavity -- which after all isn't a fundamental phenomenon and isn't the highest Q in the world, but to use instead an atomic or a molecular spectral line as the reference. In fact, you will find in my paper of 1946 that I so suggested, first of all using the cavity that was used to stabilize the oscillator as an absorption cell for studying microwave absorption, and then mentioned the use of what now would be called the Q-dip method, I believe, to lock the oscillator ultimately to the absorption line that might be so contained.

In fact, in the spring of 1945 some of my colleagues had the kindness, including Professor Rabi, of recommending me to the Society of Fellows at Harvard. Rabi claims he never would do that again.

But that is a special organization which gives one a 3-year term as a fellow of the Society of Fellows, and in order to get that way you had to go to an interview. I went to an interview in March of 1945, in which interview there were such persons as Alfred North Whitehead, and Paul Buck, Dean of the Faculty at Harvard, Faculty of Arts and Sciences, and a few other well-known people. Arthur Darby Knox, who was a professor of history of religion, who told me in his Cambridge, England accent that I would have to speak up because Professor Whitehead was quite hard of hearing, and here I was a 25-year-old who was to explain what he had in mind for the future to this committee.

What did I have in mind? I had in mind the development of an atomic clock for the purpose of comparing atomic to gravitational time, because I had been recently reading an article -- actually it was written by an Englishman named J. B. S. Haldane, who was interpreting and promoting the theories of E. A. Milne which suggested that there were two different time scales, kinematical and dynamical time, one of which being atomic and the other being orbital, and that his theory suggested that these two scales drifted with respect to one another by the fraction which represents the time of observation over the age of the universe.

Well, I looked at three years and the Hubble constant of that era, and that was a ratio of about 10^{-9}, and I knew that I could probably make an atomic clock that was good enough but I couldn't see how to measure gravitational time to that precision because I knew that the earth's rotation fluctuated by the order of a second per year if you measured it by looking at the star transits and so forth.

But I happily, or perhaps unhappily -- well, I guess it is happily -- a big diversion or deviation from that route occurred because of my collaboration with Purcell and Tari in the project that Rabi mentioned, in the first experiment which successfully detected magnetic resonance of nucleii in solids, which in fact weren't solid as we thought they were. That diversion took me away from that direction, although I must say that the concept of finding a reference system that would be useful for ultimate atomic clocks was always somewhere in the back of my mind. One of the reasons I embarked in the trade of pure electric quadrupole resonance, a field which Professor Dehmelt here scooped me on in a certain sense, I thought that there would be a case where there would be a high Q available. If one went to a pure electric resonance at a few thousand MHz and had dipole-to-dipole linewidths only, there should have been a Q of 10 million or so and those were big numbers in those days.

Well, I didn't get back into an active role with respect to the use of ultimate high-precision devices until suddenly the Mossbauer effect appeared on the scene, and we moved rather rapidly, and some of you may know about our employment of that method to measure the effect of the gravity on effective frequency of photons. There we measured shifts of 2 times 10^{-15} fractionally to 1 percent accuracy. We have been upstaged in the conclusions from those

651

measurements in more recent times by Bob Vessot and his colleagues at the Smithsonian and NASA, using the hydrogen maser in the rocket probe where the gravitation red shift was measured to better than a part in 10^{14}. However, they were measuring a much better effect than what we measured, so that the fractional precision of the measurement still belongs, the extreme in that area still belongs to the Mossbauer effect, and I thought I would just mention that in more recent times we have been playing with Mossbauer resonance whose Q is 10^{15}, 2 times 10^{15}.

Here is an absorption line -- I could show you a transparency that shows such an absorption line but I think you can imagine it -- in the Mossbauer effect one has an emitter radiating gamma rays through an absorber, and if the two are stationary with respect to one another and if a lot of other conditions are satisfied, there is an absorption of the gamma rays by a resonance process in the absorber, but if you move the one slowly with respect to the other, the doppler effect of that motion is sufficient to reduce the amount of absorption by putting things out of resonance.

The speed that is required to reduce the absorption for the case of zinc 67 is .15 microns per second, which is a rather slow motion. It is the kind of motion that results from about a meter per year, I guess, and you have to compare that with the velocity of light -- in other words, it is a meter over a light year -- for the fractional effect.

Now Professor Townes also mentioned that ultimately the use of time in the optical domain should be available by successful multiplication or a derivative of some device that is a coherent source in the optical domain, which can then be controlled or locked to some reference device, and I say that with this new Mossbauer effect we are able -- if someone will kindly develop a coherent oscillating source -- one can use the Mossbauer absorption as an absolute reference with a stability -- well, the resonance full width is 10^{-15}, so I think following the enhancement over the natural resonance widths of most other atomic clock references, this should get us into the domain of 10^{-21}, which is sort of another domain of interest.

Uhhappily, the attack time or the feedback rate that you could use for these things is rather slow because the signal-to-noise ratio is relatively poor, and it takes maybe an hour to develop a decent picture of that line shape at the present time.

One last remark, I think -- oh, I might have mentioned that in addition to the role of the original the ideas of stabilizing microwave oscillators, in 1949-50 Ed Purcell and I collaborated on an experiment which became published under the title, "A Spin System at a Negative Temperature." We introduced the concept of negative temperatures, which particularly pointed to the phenomenon of induced emission as a contributor to radiospectroscopy under the conditions of having inverted population states. I think that also played a role in the development and understanding of the techniques that led to the maser, at least so the Smithsonian people told me when they asked for the crystal we used in that as a part of their laser exhibit a few years ago.

But finally I was going to remark that there is also the question as to whether quantum mechanics is really at issue in things, say, like the problem with the Mossbauer effect, in addition to the fact that it is unlikely that it is going to end up being a maser in its own right. The case that I mentioned, for example, is a wavelength of .13 angstroms or a frequency of 2.5 times 10^{19}Hz, which is sort of high. It has a line width, therefore, of 25 kHz.

Now in some very narrow resonances, like in NMR, you find there are transient effects. For example, if you do magnetic sweeping in an NMR experiment, instead of seeing a nice absorption line, you see a thing which Purcell and I called the "wiggles" back in 1946 when we first had this annoying effect. When you sweep through a resonance, you do start to get an absorption but then there is a beat that appears. You can develop exactly the same picture by a combination of single-tuned circuits, tuning one and shock-exciting the other, and the after-ring of the shock excitation beats with the one that the frequency is moving away and you see the thing as a wiggle.

It turns out that in the Mossbauer effect you have precisely the same effect: Namely, if you look at this ultra-narrow resonance line of zinc 67, which has a dip in the ordinary way if you sweep through it quite slowly, you start sweeping through a little faster, the lines start to become a little unsymmetric in shape, and if you sweep through it in a time which is short compared with the coherence time, which is 10 microseconds, of this resonance, what do you get? You get the wiggles, exactly the same as in this quasi-classical case of the NMR line.

One of the curious things that is a pedagogical problem is that the wiggle, if you go with an absorber, then in the first wiggle the intensity transmitted through the absorber is greater than this intensity would have been at those times without an absorber, and maybe I will just leave you with the challenge of figuring out how that can possibly be. Who made the energy that appears in this overshoot, as it might be called? But it does tell one that the correspondence principle, and the fact that it is no problem to answer this problem if you think of the whole system as classical.

DISTINGUISHED SCIENTIST PANEL

Panel Chairman: Professor I. I. Rabi, Columbia University

Well, before we turn to the next speaker, Professor Dehmelt, I want to make a confession of which I am very proud and which has had an important part in the history of microwave spectroscopy.

During the war, I happened to be more or less in charge of magnetrons, and finding radar for higher and higher frequencies and better definition. One of the problems which we set ourselves was 1-centimeter radar. For that we had to make a 1-centimeter magnetron which would give power, and of course the Klystron that would go with it.

We worked at this, and it's a hit-and-miss affair but we finally got a magnetron that worked with high efficiency and a lot of power and got a Klystron made in that region and set up a wonderful radar. The radar was tried and we got some wonderful pictures of New York harbor with fine definition of all the streets and the piers and so on. Of course, when the rains came it turned out that we were precisely on the water absorption line. I knew we were in great danger of being there but once you've made a magnetron, scaling is easy for the circuits once you have it. So this is fine. We'll get this made, see how it works and then scale to the appropriate frequency.

Fortunately, the war ended before we could get this scaling done, and as a result we had those Klystrons, surplus, cheap -- and the Government sold them -- easily available for experimenters, and therefore microwave spectroscopy at that time. I'm not sorry the war ended and we were not able to fix the radar. It was a 1 centimeter radar but not an all-weather radar.

But it shows that if you are in a new field, even mistakes can be of positive value.

PROFESSOR TOWNES: A great contribution to science. It has to be opportunistic.

PROFESSOR RABI: It has to be opportunistic, yes, and use everything just as Chicago did. You use the squeal in the hog.

Now I will turn to Professor Dehmelt.

DISTINGUISHED SCIENTIST PANEL

<u>Panel Member</u>: Professor Hans Dehmelt, University of Washington

I have a paper which hopefully will address itself to the points which the chairman has raised, and also to entertain you to some degree.

In these days of cost-benefit, the case history of non-mission oriented work leading nevertheless to very useful contributions to development of new frequency standards may be of some interest. Having been a radio amateur since age 10, in my work in Kaufferman Institute on radio frequency spectroscopy research for nuclear quadrupole resonants in solids was a natural choice. However, solids are complex and the electrons seemed to be a much more interesting particle than the closing nucleus for which I had just found the search for resonance in 1950.

Also, my teacher, Richard Becker, one day in his electricity and magnetism lecture, had drawn a dot on the blackboard, declaring, "Here we have an electron." Minding Heisenberg's admission that physics should concern itself with absorbers, I have wondered ever since how one might go about in the laboratory to localize an electron in free space.

For the time being, then, however, I made do with my nuclear quadrupole resonance spectrometer in a search for the 400 MHz hyperfine line in potassium vapor, but I couldn't find it. Having been invited to work in Walter Gordy's laboratory in this country, I had more luck with the atomic resonance spectrum of atomic phosphorus in a high-pressure buffer gas. That was about in 1953-54. Now at last I had measured something on the free atom, namely its hyperfine structure. However, signal-to-noise was poor so I turned to the trigger techniques pioneered by our Chairman, Rabi, and by Block, Lamb, and my fellow colleagues Dicke and others. Adding a new wrinkle, I hoped -- as it turned out it was previously proposed by Kastler -- namely, alignment by electron impact -- I built a planar plasma diode filled with mercury vapor, and searched for a permanent resonant signal in the DC diode current but I couldn't find it.

However, shining filtered light from a mercury rectifier on it, I found the resonance in the transmitted light, and this was the origin of the transmission monitoring technique now used in the rubidium clock.

In the mercury experiment I also learned about spin exchange between electrons and atoms. The easy availability of polarized alkali atoms gave me the idea for my free electron spin resonance experiment, based on the back and forth spin exchange between electrons and optically pumped sodium atoms. To realize the long interaction and relaxation times necessary here for this kind of a spin-exchange polarization experiment, I fell back on my high-pressure buffer gas work on the atomic phosphorus.

Having very little money, I first demonstrated in a simple optical pumping experiment using adiabatic field and spin reverser, relaxation times approaching a second for sodium in a 30 torr buffer gas of argon. The correspondingly sharp $\Delta f = 0,1$ hyperfine resonances suggested by this had considerable application potential in magnetometers and frequency standards. Nevertheless, my brethren at the University of Washington, where I then was a visiting Assistant Professor, were not overly impressed by my efforts and in fact had invited me to shop around for another job.

So I contacted Martin Packard at Varian Associates, and also demonstrated my experiments to Felix Bloch, whose nuclear induction technique formed one of the cornerstones of this young company, Varian Associates. This led to a very successful collaboration in which Bell and Bloom at Varian quickly repeated my optical pumping experiments and also detected the 0-0 hyperfine transition in sodium in the transmitted light. No isotropic filter was used then. This simple approach has recently been revived for the least expensive rubidium clocks.

In my paper on the mercury work, I also had proposed radio frequency spectroscopy on trapped ions. This line of work in my lab at the University of Washington now has culminated in the magnesium monoion oscillator, an individual laser-cooled magnesium ion in a small ion trap. Well, I should mention that after our chairman, George Menley, got around to have a look in my laboratory, he did invite me to stay on as an Assistant Professor in 1956, so I am still with the University of Washington.

My collaborators, Roland Nagoni and Gary Jennings, just have measured the width of this trapped magnesium ion of the optical resonance line, which is just like a sodium-D line. The result: namely, within our limits the natural line establishes an ion temperature of about 5 milli-Kelvin.

The ultimate accuracy of a monoion oscillator laser frequency standard might approach -- I mean accuracy -- 1 part in 10^{18}. I should say "resolution" in order not to get in any big claims about how much one can split the line.

Again, the sideband cooling technique was first searched for by David Vineland and myself, not on an atomic ion for frequency standard purposes, but in our monoelectron oscillator work at the University of Washington. The goal of this work is the study of the finer structure of this most important elementary particle, namely, the elctron and the positron.

I do not presume to know what frequency standards with 1 part in 10^{18} accuracy will be good for. However, for example, nobody also seems to know very well if two atoms of the same element will actually prove to be identical to that accuracy. And harking back on points raised before, maybe sequels of the Hubble expansion of the universe will become detectable in the laboratory. The important thing, again, seems to be to build one, and of course this is what you have also dedicated your efforts to.

I thank you for your attention.

DISTINGUISHED SCIENTIST PANEL

<u>Panel Chairman</u>: Professor I. I. Rabi, Columbia University

Well, I should perhaps mention that Professor Dicke really also tried to construct an atom.

PROFESSOR DICKE: That was an experiment that failed.

PROFESSOR RABI: Well, an experiment that failed. I have always thought about that because it was a brilliant experiment, and I know that in my case, and I collaborated with people like Norman Ramsey and Jerald Zacharias and Kusch -- that if the experiment was such that I could do it, it always succeeded, because I am not an experimenter and it had to be very simple, and others who are brilliant will always add wonderful things to it, which one deeply appreciates.

Well, I think it has been a good afternoon in the sense that it is almost 2 hours and we have lost very few customers, and the doors were open.

So I think it has been and I hope it has been entertaining, but I hope sort of inspirational of various attempts which have been made along this direction. Some have succeeded. Some did not succeed so well and haven't been pursued, but there is a very wide field and I think the richness of the field has been demonstrated by the last few speakers. Some of the new fields suggested are very intriguing, like geophysics and astrophysics. In other words, it's a great and wonderful field. I congratulate you for being in it, and I hope this has been a useful meeting. We thank you for your attention.

In the name of my colleagues, I thank you, there will be no encore and we will go on.