Caldon, Inc.
1070 Banksville Avenue
Pittsburgh, PA 15216
412-341-9920 Tel
412-341-9951 Fax
www.caldon.net

March 20, 2000

Mr. John S. Cushing
MS: 4D7
U.S. Nuclear Regulatory Commission
11555 Rockville Pike
Rockville, MD 20852

Subject: Transcript of Public Meeting of March 8, 2000 Between Caldon and the NRC

Dear Mr. Cushing:

Enclosed is a corrected copy of the subject transcript. Word corrections were made not to change the content but to correct the errors in the transcription.

Please note the corrections of participants' company affiliation.

Sincerely,

CR Hastings

Calvin R. Hastings
President & CEO

CRH:jt

Enclosure

DF03

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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PUBLIC MEETING

Nuclear Regulatory Commission

Two White Flint North

Room T2-B1

11545 Rockville Pike

Rockville, Maryland 20852

Wednesday, March 8, 2000

The above-entitled meeting commenced, pursuant to notice, at 3:02 p.m.

PARTICIPANTS:

STUART RICHARDS

PROJECT DIRECTOR

PD4 AND DECOMMISSIONING, NRR

CAL HASTINGS, CALDON, INC.

HERB ESTRADA, CALDON, INC.

ERNIE HAUSER, CALDON, INC

JENNIFER REGAN, ~~Caldon, Inc.~~ KEY TECHNOLOGIES

ROGER HORN, ~~Key Technologies~~ ProDesCon

CHARLIE WAITE, ~~Key Technologies~~ ProDesCon

GEORGE MATTINGLY, NIST

PARTICIPANTS: [CONTINUED]

JACK CUSHING

BILL HORIN, WINSTON & STRAWN

MALCOLM PHILIPS, WINSTON & STRAWN

LAWRENCE C. LYNNWORTH, Panametrics, Inc.

SETH G. FISHER, Fisher Precision Systems

TOM MAGINNIS,Professor, U-Mass, Lowell

IAN RICKARD, ABB

CHIP FRENCH, ABB

YURI GUREVICH, AMAG

STEVE DEMBEK, NRR/DLPM

JOE RUTBERG

CATHY MARCO, OGC

MIKE NEAL, NUSIS

I. AHMED, NRR/DE

JOE DONOGHUE

RALPH CARUSO

JARED WERMIEL, NRR/DSSA/SRXB

JOSE CALVO

HUKAM GARG,NRR/DE/EEIB

TERRY JACKSON

STEVEN ARNDT, RES/DET/ERAB

FAROUK ELTAWILA

NORMA LAUBEN, NRC/RES

JOHN ZWOLINSKI, NRR/DLPM

PARTICIPANTS: [CONTINUED]

ALEX ZARUHNAK, MPR Associates

PROCEEDINGS

[3:02 p.m.]

RICHARDS: Good afternoon, I am Stu Richards. I am the Project Director with PD4 and Decommissioning in NRR. This is a public meeting between the Caldon company and the Nuclear Regulatory Commission.

By letter dated February 15th, 2000, Caldon submitted to the NRC information related to the measurement of feedwater flow at commercial nuclear power plants. In this letter Caldon specifically expressed concern that instruments measuring flow by means of cross-correlating ultrasonic signals affected by eddies in the flow system may not support a significant reduction in the 2 percent power margin of 10 CFR 50, Appendix K.

Caldon further expressed a willingness to meet with the staff to discuss their submittal, hence, today's meeting is an opportunity for Caldon to present directly to the staff their concerns with cross-correlation flowmeters.

Portions of Caldon's February 15th, 2000 submittal are proprietary, however, there will be no discussion of proprietary information in today's meeting. Because this meeting was noticed less than 10 days prior to the meeting, the staff has elected to have the meeting transcribed in order that members of the public who could not be here today can have access to specifically what discussed. So, please identify yourself before speaking in order to aid the transcription, and usually the transcriber will ask if, you know, we get too many people talking at one time, so help out on that.

There is a sign-up sheet going around the room. Please ensure that you sign the sheet so that we will have a record of meeting attendance.

Today's meeting is scheduled to last until 4:30. I would ask Caldon to keep this in mind in making your presentation. The staff has reviewed the material you submitted and is somewhat familiar with its content already.

Members of the public in attendance here today are here to observe only. If members of the public have questions or comments, I would ask you to hold them until the end of the meeting. The NRC staff will be available to speak with people who have questions and comments then.

Mr. Hastings, could we please start by your introducing yourself and the members of your contingent joining you today.

HASTINGS: Thanks. I am Cal Hastings, I am the President and CEO of Caldon. I have with me some people who -- some of whom are employees of Caldon, some of whom are people that I have asked to be here on our behalf. They are people who I regard highly as experts who may be able to speak more to the point on some of the issues than even we can.

I would like to introduce Herb Estrada, who is seated here at the table. Herb is our Chief Engineer and a number of you at the NRC have met him during previous meetings.

Next is Jennifer Regan. Jennifer is with a company called Key Technologies, and I will use the "key" again to say she is one of the key consultants to Caldon and has been involved for some years in our work in feedwater flow measurement.

Next is Dr. George Mattingly. George is probably well known by people here for his work at NIST in Gaithersburg.

Seated closer to me from George is Seth Fisher. Seth is President of Fisher Precision Systems. This is a company that manufactures transit time ultrasonic flowmeters and I didn't intend to make reference to Seth's age, but Seth has got to be more familiar for more years with ultrasonic flowmeters than anyone in this room.

FISHER: Since 1960.

HASTINGS: Seated to Seth's left and George Mattingly's right is Larry Lynnworth. Larry is the Vice President of Panametrics. And both Larry and Seth are competitors of Caldon. I don't know that I have ever asked a competitor to come and speak to make a point to support my own before, but I will say that I did it in this case because of the high regard that I have for their work and for what they have published, and for what they know about flow measurement.

Next is Dr. Tom Maginnis who is seated here more closely to my left. Tom was formerly in research and product development at Foxboro, a company well known in the flow measurement field, where he worked on transit time and cross-correlation flowmeters. He is now a professor at the University of Massachusetts in Lowell.

Over here by the -- well, let's see, there is Dr. Roger Horn, I am sorry I passed over Roger. Roger accepted an assignment from Caldon last year to do an in-depth study on cross-correlation flowmeters for us, to help us to identify and to bound their uncertainties.

And then last, close to the post here, is Malcolm Philips and Bill Horin. They are well known here at the NRC. They are attorneys, and they have been advising us from the beginning as we have tried to do work in support of this 1 percent uprate, and they have been advising us on nuclear regulatory matters. They also are pretty good engineers.

I believe we have copies of the resumes of all of us here with us, and they will be available for anyone who wants them at the end of this meeting.

RICHARDS: Before you start, Mr. Hastings, if I could ask the rest of the audience to introduce themselves, just so we know who is here.

HASTINGS: I think I would like that very much.

RICHARDS: Jerry.

WERMIEL: My name is Jared Wermiel, I am the Chief of the Reactor Systems Branch at NRR.

CALVO: Jose Calvo, NRR.

DEMBEK: I am Steve Dembek, I am Section Chief in charge of Vendors and Owners Group, and I am going to be making -- we ran out of copies of the handout. I am going to be making more copies. Can I get a show of hands of who would like a copy? I will make about 15. A few more.

ZWOLINSKI: I am John Zwolinski, I am the Division Director, Division of Licensing and Project Management.

MARCO: Catherine Marco, OGC.

RUTBERG: Joe Rutberg, OGC.

~~COE~~ CAVE: Christie ~~Coe~~ Cave, McGraw-Hill.

NEAL: Mike Neal, NUSIS.

WAITE: Charles Waite, ProDesCon.

DONOGHUE: Joe Donoghue, Reactor Systems Branch, NRR.

CARUSO: Ralph Caruso, Reactor Systems Branch at NRR.

CUSHING: Jack Cushing, Project Manager, Division of Licensing and Project Management, NRR.

GARG: Hukam Garg, NRR.

GUREVICH: Yuri Gurevich, AMAG.

FRENCH: Chip French, ABB.

RICKARD: Ian Rickard, ABB.

ELTAWILA: Farouk Eltawila, NRC, Research.

LAUBEN: Norm Lauben, NRC, Research.

JACKSON: Terry Jackson, NRC, Research.

RICHARDS: All right. Mr. Hastings, with that, we will turn the floor over to you.

HASTINGS: Okay, Stu. I have prepared a list of topics. It is almost an agenda. Let me run through it and explain. We have, of course, already identified the participants.

I want to tell you a little bit about Caldon and it will be brief, it is merely to give you a feel for where we are coming from. I will then try to explain to you why we submitted the February 15th information package.

Jenny Regan and Herb Estrada will then discuss the technical information that we submitted.

I am going to look to the experts here with me to make comments, to offer clarification, offer their opinions as we go through our material. My feeling is they know what they know, and they should react accordingly. If they feel what we are presenting is inappropriate or wrong, I am sure they are going to tell me that. And if they want to raise questions of their own, we will welcome them.

We would expect and welcome questions from those of you from the NRC as well. They can be at any time during the discussion, and I think the only time you would expect me to not answer is if I felt my answer to your question was going to come along in the next point or two, or that it would be better to let me continue with my thought before we get around to it.

At the end I would expect to have some concluding remarks based upon the interaction here, and I have made the assumption that you would as well. And it seems to me we just have to wait until we get to the end to see what those will be.

Caldon is a privately held company located in Pittsburgh, Pennsylvania. I founded the company in 1987. We are a high technology company with significant know-how in ultrasonics, electronic circuits, computer systems, and flow phenomena within fluid systems.

We have two principal product lines at Caldon. One we call LEFM, these are ultrasonic flowmeters. LEFM is a trademark that stands for leading edge flowmeter. The other product line is called LineWatch. LineWatch is the trade name of a leak detection system that we apply normally to petroleum pipelines, but other pipelines as well.

These products are used in important and technically demanding applications, including ballast control on the Seawolf Class submarines; leak detection on the trans-Alaska pipeline; turbine monitoring at New Zealand's hydro-electric stations; and feedwater flow measurement in 33 nuclear power plants.

Now, I want to get at the reasons for our submittal. Last month I was given a copy of the Special Edition of ABB's CROSSFLOW Currents, the one that you see projected on the screen. This newsletter provides information on the status of, at that time, the NRC's review of ABB's Topical Report on CROSSFLOW UFM technology.

I was surprised by this statement, and I just extracted it, I didn't change any of the words here, just highlighted. What surprised me, that "the SER will find acceptable the CROSSFLOW UFM System's measurement accuracy less than or equal to a half a percent."

I had fully expected the NRC would find that a bounding VALUE for the overall uncertainty of this instrument would be significantly larger than that, and I have my reasons for feeling that way. I am going to attempt to let you understand what they are.

I was accused, I guess, of cutting to the chase by letting you see some numbers which quickly give you a feel for why I was surprised. This is our assessment of uncertainties for ultrasonic flowmeters for a typical 2-loop nuclear plant installation.

On the left, the left column represents uncertainties. Under the heading of chordal transit-time I list uncertainties that are associated with a 4-path transit time like Caldon's LEFM(check). In the middle column I list uncertainties that we find apply to an externally mounted transit-time system like the one manufactured by Caldon. In the righthand column we list our assessment of the uncertainties for a cross-correlation type flowmeter externally mounted to hot pipes on feedwater lines.

At the extreme left I listed the method I have used for categorizing these uncertainties. What I mean by the top one are those uncertainties that are from acoustical effects like scattering, like reverberation of sound waves. What I mean is the errors that can be induced in any of these instruments by such acoustical phenomena.

Next, it is our assessment of those uncertainties are associated with less than perfect knowledge of dimensions. In this case, dimensions, I mean inside pipe diameter, how well do we know that? The pipe wall thickness, the spacing between ultrasonic sensors.

The next category of uncertainties are those from errors in measuring transmit time or cross-correlation time.

At the bottom I used the term "velocity profile." One more appropriately might have been flow distortions. And what I mean is the uncertainties in flow reading from an ultrasonic instrument that derive from the distortions inside -- the distortions of the flow field inside the pipe, and these are often referred to as velocity profile, and I used that shorthand notation here.

You can see why I was surprised, given my beliefs about the uncertainties, and our own assessment, supported by a fair amount of literature and analysis, which we will discuss here momentarily. We believe that the uncertainties of that device for this application -- we are speaking now of one in particular, feedwater lines in a nuclear power plant -- we believe that is more like 1.45 or 1-1/2 percent.

I felt I had to, feeling this way, and I hope I don't come across as too fanatical about it, but believing that, I felt I had to do something, and I didn't know exactly what to do in a situation like this, and I felt I had to come to the NRC, because you have reviewed our work, and I knew you were reviewing ABB's work, and the ABB Newsletter told me that I should expect an SER to soon hit the street, and that SER would -- they already changed the slide -- but that SER would state that you agreed that the accuracy of their instrument was about three times better than I thought it was.

I think all of us in this room agree that this is an issue that is important to safety. I will illustrate that point a little bit more clearly in a moment. But I also think that everyone can lose if an error in feedwater flow measurement causes a plant to go overpower. The good work that has gone into the Appendix K Rule Change and the intended positive outcome would surely suffer. And I guess I have to tell you that after putting an awful lot of good work into this thing on our part, if the industry would stop buying ultrasonic flowmeters because of an overpower incident, my company is going to lose enormously. And as the CEO of the company, again, I can't sit idly and not try to bring to you my concern before it leads to a significant problem for me.

I said I would come back to the issue of safety. It is not the easiest thing to deal with, so I tried here in my own way. I have attempted to show the safety implications by relating uncertainty in the thermal power measurement to the probability of a plant overpower incident. The case identified as #1, and I can see it is very small, on the left, unfortunately, but this case represents the use of the LEFM(check) for a 1 percent uprate in power. In this case the thermal power measurement uncertainty is six-tenths of a percent. Also, for this case, the probability of an overpower incident in a plant using that device with a 1 percent uprate is .04 percent.

That number has been discussed with you all previously, it ties directly to information that was in our Topical Report.

This is contrasted with Case #2 that represents the situation in most power plants today. Except for Comanche Peak, all other plants are still operating under the 2 percent Appendix rule. Caldon's analysis, previously given to the NRC, concludes that the uncertainty in thermal power measurements is typically 1.4 percent. And I might say that some of the staff felt that it is greater than that and expressed that in our SER. For these plants, the probability of exceeding their design basis power level is .21 percent. And, so, you can see that under Case #1, we have actually made an improvement in safety through the LEFM(check), and that ~~is~~ was appreciated by those of you here at the NRC who were involved in reviewing our Topical Report.

Case #3 represents a situation in which a plant uprates its power level by 1 percent and that it employs a flowmeter that produces a thermal power uncertainty of 1 percent. For this plant, the probability of an overpower incident is 2.28 percent. And, likewise, the next case is very similar except the measurement uncertainty is 2 percent.

~~And~~ One way to look at this is, if you assume that an instrument has an uncertainty as low as, and this is on a thermal power basis, I think the SER, ~~and I~~

mention one-half a percent, that refers to, I believe, mass flow measurement. So there is some slight difference. But if you make this assumption, or I make the assumption and I am wrong, if I am wrong a little bit, or if I am wrong a little bit more, or if I am wrong a lot, it can have significant safety consequences.

At this time I am going to ask Jennifer Regan to go through the materials. And what Jennifer will try to do first is acquaint you with the book and, apparently, since you have looked at it, she won't have to spend too much time at that. But if you have questions about the book itself, I would say now would be the time to raise them.

REGAN: I am Jenny Regan, and this is the book. I wanted to say a few words about myself so you become familiar with who I am. I am the President of Key Technologies, that is an engineering company in Baltimore. I have been an engineering consultant in the nuclear industry serving electric utility clients for about 17 years. I have been working generally in the area of instrumentations and controls for this time.

I have been working in particular with Caldon, as Cal mentioned, on feedwater flow measurement and ultrasonic technology since 1991.

Back to the book. We submitted a large book to the NRC on the 15th of February. This book contains 19 documents on ultrasonic flow measurement. Each document in the book is relevant to Caldon's concerns. There are three documents proprietary to Caldon and the rest are available in the open literature.

We are unaware of any published documents in the open literature, any outside these, that refute the contents of this book. This represents a selected set that we have chosen to help with the review.

We included entire documents in this book, rather ~~that~~ than sections of those documents, in order to avoid citing sections out of context. The documents are separated by tabs. Some of you are probably familiar with ~~this~~ these. And there is a summary in the front of the book that is intended as a road map, tying Caldon's concerns to specific references within the book.

These documents span years of engineering research. Some are as recent as this year, while others go back as far as the 1970s. We included documents because they contain information about inherent fluid flow and acoustic effects for each of the flow measurement technologies.

For example,, the hydraulic testing report in this book, by Seth Fisher, who is here today, and Paul Spink, for chordal transit time meters is just as valid today as when it was published in 1971.

There are three types of information in this book. There is cross-correlation technical research and field experience. There is transit time research and field experience for both chordal and external mount meters. And then there is a comparative analysis of these technologies.

In the first category there are quite a few documents on cross-correlation technology. I am just going to choose a couple of particularly pertinent examples of this type of information. For example, Tabs 2, 3 and 4 in this book contain publications supporting the designer's accuracy statements of between 1 percent and 3 percent for the cross-correlation meter, published in 1992, 1996 and 1998. These papers also describe field applications of these meters.

Another example, Tabs 5, 6 and 17 describe experimental data that illustrates the profile sensitivities of the cross-correlation technology. Here I am speaking about hydraulic profile.

Next slide. Another pertinent example is Tab 8. This is Dr. Roger Horn's independent analysis of the cross-correlation technology uncertainties. This document is proprietary to Caldon.

The next category of information in the book is transit time technology information. This transit time technology information is provided as perspective. Tabs 10, 11, 12, 13, 14, 16, 18 and 19 support Caldon's accuracy statements about Caldon's chordal and external mount transit time flowmeters, using analytical and experimental data, and referring to field experience with these meters.

Finally, there is comparative information in this book. The introduction to the book, which I mentioned earlier (the summary that is in the front) and Tab 15 compare the three ultrasonic technologies in order to provide a frame of reference for the reviewer of the material in this book.

Looking at the information in this book, we can conclude that data in the open literature indicate that the hydraulic and acoustic uncertainties inherent in the cross-correlation technology would support an accuracy of between 1 and 3 percent rather than the half percent that we have seen published. In fact, the 1 to 3 percent numbers are nowhere near the half percent number we have seen published.

These same data in this book raise fundamental issues that cannot be addressed without extensive testing in a variety of piping configurations and wall roughnesses.

We will now address the hydraulic and acoustic uncertainties in greater detail, referring to the book of references as appropriate as we go along. Herb Estrada is going to be doing that for you.

I am going to put the book over here, Herb.

ESTRADA: Okay. And I will need the viewgraphs.

I should mention before I start that a number of the pictorials in this part of the presentation are excerpted from Tab 8 of the compendium, which is a proprietary document, however, the pictorials that are included in this presentation should not be treated as proprietary.

As Jenny mentioned, our own investigations of the cross-correlation flow measurement system indicated to us that there are significant and variable sensitivities of such technology to both the shape of the velocity profile within the ~~tank~~ pipe and to the acoustics of the installation, that is to say, the installation -- the interaction between the ultrasound and the hydraulics themselves, as well as the piping environment in which the flow is being measured.

RICHARDS: Could I ask you to face this way because the acoustics of this room aren't very good.

ESTRADA: Very good.

RICHARDS: I have been back there before and it is hard to hear in that end of the room. So we apologize for that.

ESTRADA: Part of the reason for being concerned about the sensitivity to velocity profile is that the relationship between what a cross-correlation instrument measures and the bulk average flow in a duct is not obvious. There is no first principles theory that provides that relationship.

Now, we have, in fact, in the book, provided you with two references. One, a reference that is roughly 15 years old, Beck, which is a good text and describes approximately what the cross-correlation meter is reading. That doesn't constitute a first principles analysis. It provides an explanation of what it is you correlate when you do a cross-correlation of the two ultrasonic beams.

Tab 8, I guess it is, is Roger Horn's analysis. That also endeavors to describe analytically and mathematically what it is the cross-correlation flow measurement system reads. Neither of those, however, constitutes anything approaching a first principles type of instrument. Let me illustrate.

A Venturi nozzle fundamentally changes the momentum flux between two adjacent areas of a pipe, and one can relate the differential pressure that a Venturi measures to the mass flow rate, providing you make some estimate of the density of the flow rate. You can relate those using first principles, you can derive what the meaning of that relationship is.

If he does this, without ever calibrating the Venturi nozzle, the chances are he will be within 1 or 2 percent of the true answer. The reason is that the Venturi behaves, in the absence of fouling, and if its configuration doesn't include bypass flow, it behaves like the physics that you use to describe it. One can analyze then the Venturi. One can describe the errors in the Venturi by methodically looking at the mathematics that describe it and assessing what the uncertainties are in each of those elements. That is, in fact, what the ASME did 30 years ago when they developed flow measurement standards for the Venturi.

Likewise for chordal ultrasonic meters. The principles of a chordal ultrasonic meter are clear. The transit times of the pulses can be related directly by first principles to the sound velocity and the fluid velocity along a line of the acoustic path. It is a clear, unequivocal type of relationship and the uncertainty in relating those velocities to the volumetric flow, because the chordal paths are spaced such as to perform a numerical integration, again, are typically within a few tenths of a percent of what a strictly analytical approach would tell you that the volumetric flow is.

So, again, one can make a rigorous mathematical analysis and bound the uncertainties associated by a flow measurement using a chordal ultrasonic system by simply writing the algorithm and then quizzically and rigorously, and skeptically analyzing each of the elements in that analysis. No such rigor exists, to our knowledge, in the open literature on cross-correlation flow measurements.

Now, absent that, absent the underlying theory, the danger that you miss something, that there is an error that you simply haven't thought about, that later comes back to bite you, that is the key. It is the nasty surprise that the absence of underlying theory makes you vulnerable to.

Let me amplify a little bit. As I mentioned, a chordal ultrasonic flowmeter makes measurements along each of four acoustic paths, in effect, determining what the axial velocity is on each of these four chords. And because of the placement of the chords and the weighting functions that are applied to them, in effect, it integrates the velocity profile across this area. And one can calculate, as I mentioned, the flow from the velocities, from the numerical integration rules that you get out of a handbook, and you can calculate the volumetric flow, and, typically, with this instrument, you will be between one-tenth of a percent to four-tenths of a percent of the true answer, depending on whose numerical integration rules you use.

The situation is a little bit different with an externally mounted time of flight instrument. And I bring this up because it is our experience with such instruments that led us first to question the fundamental accuracy being claimed for the cross-correlation flow measurement.

Externally mounted time of flight measurements typically measure the velocity along one or, in our case, two diametral paths, as is shown in the middle circle here. Now you can make, if you are very careful about your acoustic uncertainties and your time measurements, and your geometry, you can make a pretty precise measurement of the velocities, the axial velocities that intersect those paths. But that doesn't tell you what the volumetric flow is, and, typically, in one of these instruments, we find that the relationship between the velocity that we measure along these paths, and the true volumetric flow velocity, may range from anything from .94 to 1. That is to say the calibration coefficient typically ranges from .94 for this instrument, where I take the velocity of that measure and multiply it by .94, or, in another hydraulic installation, it might be as high as 1.

Fairly heavy sensitivity, one can -- that doesn't mean that the answer is uncertain over that range, but it does mean you are sensitive to hydraulic geometry, and you have got to work like hell to get accuracy out of that instrument. And, as Cal showed you in his earlier chart, our claim is that such instruments can be made to read within about a percent but no better, and we would not propose to use such an instrument for a thermal power uprate.

Now, as we will discuss later, a cross-correlation flow measurement system, if it uses two stations separated from each other by, say, one or to three diameters on a pipe, and measures the and correlates the turbulent eddies at each of those two stations against each other, and determines a time delay, and, from that, determines a velocity, that is really measuring information along a chord which corresponds, depending on the specifics of the profile, to something like 80 percent of the total diametral chord. It doesn't look at the whole profile, it looks at only a fraction of that, and that information can be found in Beck, the reference I cited earlier, that is 9, Tab 9, and also in Roger Horn's work, Tab 8, and I believe also -- is it 4, reference 4?

REGAN: Tell me where Horn is. (Which tab?)

ESTRADA: Hitchcock.

REGAN: There is not a specific reference to the percentage of the diameter.

ESTRADA: No, but it says --

REGAN: But it is the facts that are referred to.

ESTRADA: Yes, it is well known. What I am trying to establish here is that it is well known that a cross-correlation meter, for reasons that we will get into, does not give you information about an annular region which may vary from, say, 10 or 15 percent to as much as 20 or 25 percent of the radius, and, therefore, can constitute roughly 35 to 40 percent of the flow area. You simply don't get information about that part of the flow field.

MAGINNIS: Excuse me. Before you put that --

ESTRADA: Sure.

MAGINNIS: I have a question. In the clamp-on case, -- this is Tom Maginnis. In the clamp-on case, you have drawn two paths connecting those transducers. Those transducers are connected on the outside of the ~~plant~~ PIPE. How do you even know ~~what~~ that the sound ~~power of~~ follows those paths?

RICHARDS: Mr. Hastings, before we continue, may we ask the members of the public to hold their questions till the end of the meeting. So if he is with your group --

HASTINGS: He is.

RICHARDS: -- and you want to answer the question, that is fine.

HASTINGS: We do.

MAGINNIS: Who will answer it?

HASTINGS: I tried to explain --

RICHARDS: I couldn't keep track of, you know, who is with who.

HASTINGS: All right.

ESTRADA: Normally, with an externally mounted flow measurement, there is a substantial uncertainty about the exact angles that the acoustic paths make through the fluid flow and, therefore, a substantial uncertainty in the actual velocity. In our cases, we are applying this to precision flow measurement. We do experiments in which we make displacements of the transducers and measure the change in certain acoustic properties with the displacement of the transducer to establish, essentially, what the angle of the acoustic path is with respect to the flow. And, thereby, we reduce the uncertainty.

Now, you may not have noticed it in passing, but there was a substantial acoustic uncertainty associated with the externally mounted Caldon meter in the chart that Cal put up earlier, it is roughly four-tenths of a percent. At best it is not very good.

All right. Larry Lynnworth.

LYNNWORTH: Just one question. I just want to verify, in a cross-correlation, the righthand circle, if there were a second diameter, top to bottom, path which would also sample a segment, is it correct that there is still the same annular region that is --

ESTRADA: That's right. A second path, as with an external mount, adding a second path enhances some of the measurements, such as the time measurements, but it really provides you no more information about what is going on in this annular region toward the outside of the pipe.

MAGINNIS: Would it be correct to say that, because of the measurements that ~~we~~ ARE made external to the pipe, transit time, pipe ~~X~~ diameter, comparing time measurements to theory and that BECAUSE you ~~only~~ take measurements, you have high confidence that it is -- (the correct ~~hydraulic profile~~ ACOUSTIC PATH IN THE FLUID IS WHAT IS DEPICTED IN THE TRANSPARENCY)

ESTRADA: Yes. Among other things, we have made numerous pitot rate measurements to find out what the real profile is, according to a pitot rate; and then checked what our integration says versus that. Typically, they are within a fraction of a tenth of a percent, less than a tenth of a percent.

MAGINNIS: Do you know of any measurements that support the path that you have drawn of the plant as far as correlation?

ESTRADA: I do not.

Next slide. This is a -- excuse the cursor. Roger was not able to make that disappear. This is a 3D representation of Roger Horn's simplified model of how a cross-correlation meter works. This is, I want to emphasize, a cross-correlation depiction. What is shown -- excuse me for turning, but I need to point at something. Let me show you a point here. What is shown here, first of all, is the pipe diameter, zero corresponds to the center line of the pipe, plus a half and minus a half corresponds to the outer walls of the pipe. On the vertical axis is the evaluation of the correlation coefficient itself. And now along this axis here, this third axis, is a time delay.

So what we are doing here is showing you, if I were able, obviously, a cross-correlation takes some aggregate result, but if I were able to reach inside the pipe and take slices of the flow, here at roughly 20 percent of the radius from the wall, I get a correlation whose mean time is roughly .085, and which is, in fact, approximately normally distributed about that, but if I waited long enough, that guy would correlate at .085. As I move in, of course, the eddies correlate at faster and faster times. This particular curve is drawn for nominally fully developed flow with, I believe, Roger, a friction coefficient of .00015 corresponding to --

HORN: .001.

ESTRADA: Roughly, fairly smooth pipe, as I recall.

Now, as I said, what a cross-correlation meter does is it takes the aggregate result of that. Before I leave this curve, I want to point out something to you. This is the -- here is the outer 20 percent. There is no significant correlation in this outer 20 percent because the eddies that are traveling out there are traveling too slow -- too slowly to be included in the correlation function. You can see that, I think, better on the next slide.

This is for the same graph that we just showed you, what the correlation function looks like, aggregate, using, again, Roger's Model.

I want to emphasize that this is a simple model. It's not intended to be rigorous in the sense that one can use it to bound all the uncertainties associated with this instrument.

It's a single line-of-sight piece of sound that's traveling across there, and you're looking at the eddies and how they modulate the received signal, how they accelerate or decelerate the received signal along that single line, and correlating.

It doesn't account for the breadth of the wave, and it is not, except in one instance in the book, it does not deal with distorted profiles; it deals strictly with mathematically describable, fully-developed profiles with various viscosities and roughnesses.

Now, those slow guys on the outside of the pipe, they're in there all right, but they're out here somewhere. I think there's a little lump out there. On one of the others you can see a little lump.

They don't contribute, really, to the peak, and they don't weight the peak strongly. You pick a number, and, in effect, you're representing something that's strongly representative of the middle of the pipe.

This is a slice through a time that corresponds to the peak time in that last graph that we showed you. In other words, this is showing you which eddies, and, therefore, which velocities are actually being correlated by the meter.

We're really just picking up the average velocity along that middle 80 percent, roughly, of the pipe diameter. We're picking up preponderantly eddies that are roughly a quarter of the diameter out.

Now, fortuitously in this case, that corresponds to something not too far away from the average velocity. But that doesn't always occur, as we will describe.

What I read out of this thing then is that it's sensitive to what the velocities that the eddies are traveling at. And from the graphs that we just showed you, those velocities were traveling along a relatively well-defined, developed profile.

These data, which are excerpted from Tab 6 of your book, which describes some Korean experiments with cross-correlation flow measurements, downstream of various complex hydraulic geometries, these data are taken from Tab 6 and show the change -- we didn't get that on here, I see.

Let me just tell you what they are: The vertical axis is the deviation from the straight line calibration coefficient of the instrument. In other words, this point here deviates from the straight line, from the fully-developed coefficient for the instrument pipe, roughly seven percent.

This point, downstream of a bend, 30 diameters, says that the calibration coefficient for this location varies from the straight line coefficient by about one and a half to two percent.

What this shows is that sure enough, hydraulic distance is important from hydraulic features like this. It's important as to what this instrument reads.

One doesn't simply put this instrument ten diameters downstream of a bend, and expect to read it the same as it would read if it was 30 diameters downstream from the bend. If you did, you'd make an error of about three or four percent, by the way I read that curve.

So, one has to model the hydraulic geometry for this instrument, the inertial effects, particularly, before you can use it.

Now, here's the point: These are big variations, six, seven percent, numbers like that. We talked about the Venturi and the transit time portal meter.

We said that if we stuck those things in a pipe without calibration, we would expect them to read within two or three tenths of a percent of what you would expect to read purely on the basis of theory in most hydraulic locations.

This instrument -- and, oh, by the way, an external mount instrument is not like that. You must model the geometry. The result is sensitive to the geometry, and intrinsically then, you don't get protection. If you don't get the geometry right, your potential for error is intrinsically larger, and therefore your uncertainty is intrinsically larger than it is for an instrument whose first principles are well understood and whose sensitivity to outside effects are small.

Now, this does not describe the uncertainty of these measurements themselves. It simply describes the sensitivity of these measurements to where this instrument is in a hydraulic location.

The next slide, though, does give you some insight into that. These are the data that take -- that the author of Tab 6 took for various hydraulic locations as a function of Reynold's Number.

So what you should look at is clusters, in this case, the little bats or the little triangles or the squares, and you can see that, for example, regardless of Reynold's Number, the bats, for example, correspond to a location of five diameters downstream of a bend, and a spread of one and a half to two percent.

And that spread corresponds to the spread in each of these data sets, and, in fact, in some of them, they're larger.

What that means is that for this particular calibration experiment, the observational uncertainty which I would have to carry as part of the calibration coefficient for this instrument, is in the order of one and a half to two percent, or perhaps half of that, depending on if you're talking about spans, and you'd split the difference and take the mean.

It's not a trivial number. That same kind of effect is why we carry uncertainty coefficients for our external amount that are in the order of 7/10ths of a percent for hydraulic effects, because of that same kind of sensitivity.

Obviously, you have to carry the uncertainty of the calibration lab and the instrument they used to make the measurements, as well, but this one is a biggie in this case.

There's another point I want to make: To get precision with cross-correlation requires lots and lots and lots of samples. And the reason is that turbulence is inherently a noisy process, so you don't get very much signal for noise. The practice is in many instances, to average for extremely long periods of time to get a nice sharp correlation peak so as to define the time with low uncertainty.

It's hard to do that in a calibration lab. A weigh tank fills in 40 seconds to a minute. So you get one set of data in 40 seconds, and that's not much data, not enough to make a calibration coefficient.

And five runs that takes you maybe three or four minutes to empty the tank and get ready to run again, so yo run five times and that still doesn't get you enough data. If you have to get 12 hours, it's going to take two weeks worth of data at one specific location to get a precise measurement of the transit time.

Yes?

HASTINGS: Maybe you could clarify why you mentioned 12 hours.

ESTRADA: Yes, the 12 hours is based on two things: It's based on some numbers that we've made, but it's also based on data that we see in the literature. The reference at Tab 4 shows 12-hours, or times varying from nine to 12 hours, ~~as~~ are used to get precision.

And we agree with that, that that's the kind of number that it takes to get the precision of the transit time measurement for this instrument.

HASTINGS: If I just might add a point, as I understand it --

REGAN: Say your name.

HASTINGS: Cal Hastings, sorry. One characteristic of this instrument that Herb is describing, it has what we would call a low, poor repeatability, short-term repeatability. In the short term, if you didn't average data very long, you would get a lot of scatter.

The way that one deals with that, if he wishes to use such an instrument, is, he averages the data for a long time, maybe ten or 12 hours.

And so unlike other instruments that are used where they have quick time response where you ~~can~~ encounter change and pick up transients, this one is not useful. I think it's one reason, probably the foremost, of why this technology is not used in mainstream flow measurement, because you ~~should~~ could not use it for process control.

There are not many applications where you can get away with an instrument that has to average for 10 hours or 12 hours.

ESTRADA: The punchline from this set of slides is that without putting a precise number on it, the calibration coefficient will carry a substantial uncertainty; we believe, in the neighborhood of a percent.

Now, I have measured a calibration coefficient in the lab. I now have to take that under my arm and go out in to the field and make a flow measurement in the field.

And here is where that outer part of the pipe comes back to bite me, because things change between the lab and the field. There are differences between the lab and the field which are intrinsically hard to bound, although I can make the effort.

I don't know what the roughness is going to be for the pipe I walk up to in the field. I can, however, choose a very smooth pipe in the lab, and perhaps a pipe that I believe to be representatively rough and bound -- at least establish a bound of the uncertainty.

I don't believe that if I did that with a cross-correlation meter, the range of coefficients I would find to be acceptable; that is to say, that would force me to take too large of an uncertainty, and we're going to talk about that.

There is another factor as well; the viscosity changes from the lab to the field. If I say, gee, I've got very smooth pipe in the lab, and that's simulates the low viscosity that I will see in the field. A viscosity of 450 is maybe one-fifth of what it is in a typical calibration lab.

And I still have a problem because I make smooth pipe in the lab, but I can make low viscosity and smooth pipe in the lab, so I have the twin variables of viscosity and wall roughness to deal with, and I have to methodically put bounds on what I see in the lab, so that I can carry these numbers to the field.

The notes, by the way, in the viewgraphs, refer to where these subjects are developed further in the book that we've sent you.

Let me go back to the technicolor marvel here. This is, again, the base case, same case we saw before, and you'll recall that I think the average correlation time was something like .075 seconds for that case.

Now, if I might, here is the correlation time that we had for the first curve. If I now increase roughness by -- relative roughness by what is not absurd, a factor of 10, this corresponding to commercial steel pipe, new; this corresponding to relatively rough but not inconceivable -- we've encountered this kind of pipe in the field -- I see a change in transit time, correlated transit time of 3.2 percent.

If I say, okay, I'm going to handle that by taking it as an uncertainty, I'll take the coefficient that's halfway in between those two, and I have an uncertainty of 1.6 percent, clearly not an acceptable procedure for a half-percent flow instrument.

And this simply shows the two correlation peaks. The slower time with the higher wall roughness drags these guys forward, and winds up correlating eddies that are closer to the center line of the pipe.

Now, wall roughness in nuclear power plants is not a constant. But we have data from one of our customers who has a chordal meter. We get profile data from the chordal meter.

We have data that indicates changes in service on the order of 25 percent of the amount of -- the equivalent amount of wall roughness. That is to say, the change in the profile that we see, based on the chordal flow measurement corresponds to a change in wall roughness of about 20 percent.

If such a change occurred in service with a cross-correlation meter, that would result in a 6/10ths of a percent shift in its calibration coefficient.

Incidentally, with the chordal meter, one can prove that the change is less than 500ths of a percent. It's trivial because it's making a careful integration across the entire pipe.

Incidentally, we can make that available to you. That is not in the book but we can make it available to you if you'd like, and our analysis of it.

Finally, in traveling from the lab to the field, I have to deal with the acoustics. The wavelength of the sound beams that are being transmitted across the ~~lab, not across~~ the pipe, is the quotient of the sound velocity and the frequency, and the sound velocity in water at 450 degrees Fahrenheit is in the neighborhood of 4,000 feet a second, while it's around 5,000 feet a second in the typical calibration lab.

So the wavelength is shorter in the field than it is in the lab. Now, the way this thing works is, the eddies whose dimensions are in the order of the wavelength are those that are most strongly correlated by the ultrasound as it passes through the beam.

So it's fair to say that the eddies that I correlated in the lab may be a different set than those that I correlated in the field, and since the eddies, the dimensions of the eddies that are traveling through the pipe tend to vary as a function of radius, small eddies start at the wall, and the biggest eddies tend to gravitate toward the middle, we may be looking at a different segment of the velocity in the field than we were in the pipe, so that the calibration coefficient that we have to deal with has to account in some way for that uncertainty.

I think I've said most of this, but we do note -- and this is not in any sense a proof -- but there were experiments done at Pt. LePreau where transmit frequency was varied, and there was a change of around two percent in the calibration coefficient.

It's possible, though, as I emphasized, not proved, that that sensitivity was, in fact, a wavelength sensitivity.

I know this in that regard: We do see different amplitude modulation effects in hot water than we see in cold. That's the way it affects ~~it~~ us, but that doesn't change the transit times any, but the ultrasound fluctuates, owing to the turbulence, and we see different amplitudes in hot water than we do in cold water. The amplitude fluctuations tend to be much larger in the field than they are in the lab.

Now, at this point, I wanted to invite Dr. Maginnis and others of the folks to present a separate and different experience from us in this regard, about which we did not know ~~till~~ until yesterday. I would like them to speak to that, because Foxboro, as I said, did a fair amount of work in cross-correlation instruments, and Tom will share some of that with us.

MAGINNIS: I have to apologize to everyone because I didn't have a whole lot of time to prepare for this, but I did however clear what I am going to say with Jay Morris, who is the attorney at Foxboro who was a patent attorney at the time I was there.

RICHARDS: Would you identify yourself for the transcript?

MAGINNIS: Yes. My name is Tom Maginnis. I am here as an independent flow consultant. ~~I was good enough to be invited by Cal Hastings~~. CAL HASTING WAS GOOD ENOUGH TO INVITE ME.

Now they found out that I had some knowledge that might be relevant to this question and I will try to make this very brief and show you some experimental data.

I would like to say a few words first. There were four or five people at least who worked at Foxboro. We did a high level effort. We did it from 1979 to about 1985. The company did not proceed to commercialize the product, which they often did not. I also worked on transit time ultrasonic flow meters. I steered them away from Doppler because I felt that was not a viable technology and everything that I have heard today confirms me on that. It doesn't even ~~prove~~ come up as one of the possibilities.

Introduced 1st transparency →

Now this is very old but it makes a point. When you are measuring something with ultrasound you have to generate the sound, get it into the process, and there will be some interaction in there that has to take place, and then you have to get the ultrasound out, detect the change, and extract the useful information. You have to know what you are doing to extract the useful information, so you need a good theoretical basis.

2nd transparency Pipe Ultr Trans Mesmt →

Now when it comes to putting acoustical energy through a pipe, how do you know that you are getting the sound through ~~t~~here? It is not the situation like the medical ultrasound where the human body is very close to water in its acoustic properties. The sound goes right through except for a little scattering ~~of~~ BY the bones. It is not like x-rays going through the human body. There is only one kind of pipe where sound goes through like that, in a direct straight path, and that is PVC pipe.

PVC plastic is very close in its acoustic impedance to water and so it essentially makes this inside boundary disappear and the sound can go right through, a straight path, no problem.

When you use steel you might have an acoustic mismatch between the water and the steel which causes an 80 percent or thereabouts energy reflection to occur when a beam or a pulse hits that interface. The result is that you can send sound in. It's going to hit this interface and then bounce -- and of course this transducer on the far side will pick up sound. It's to that frequency, but there is no way to tell from out here what path it's going to follow getting from one transducer to another.

In fact, acoustic short circuit has been occasionally a problem even with clamp-on transit time meters.

So the question comes up what path does the sound follow and also what characteristics should that path through there have?

Now I am going to show you -- this describes a measurement, all right? If you want to find out how well your transmission process works to go through here, you can do it this way. You have a transducer now in the pipe, whatever transduction mechanism you use, piezoelectric -- crystals. If you have a frequency synthesizer, which is an instrument that will put out a very precisely defined frequency and vary it in a controlled manner, in this case I believe we rank it in time, that would send out a signal which electrically might be characterized this way -- sine wave, electrical sine wave, which will cause this transducer to put out an acoustical sound wave or an ultrasonic sound wave.

That will then make its way somewhere into the other crystal where it will be detected and if there is any kind of wave this wave may differ in magnitude from the other wave and also in phase, so you can define a transfer function, which is a function of frequency, which is basically a complex number.

There is a meter that will pick that up and I have left the line. Out here.

(referring to HP 3575A gain/phase meter)

There should be a line coming over here to reference this phase to the frequency synthesizer, so this instrument will compare the sound coming out, the signal coming out to the signal going in, and compares them --

That could be transferred over to a strip chart recorder which will display pictures. I will show you the phase on one chart and the energy transmission on the other chart, on a logarithm scale, decibel scale.

So first I am going to show you a PVC pipe -- oh, first, what would you like to have? What is the ideal you would like to have coming out of here? If you had a straight pass across there the only difference -- we'd like to have the same magnitude come out. We probably won't get that. There will probably be some reduction in magnitude. What about the phase? We would like the phase to be linear with frequency. You want a pure delay so as you go up in frequency you get more wavelengths in that and it turns out that that will correspond to a phase characteristic that is proportional to frequency with a negative slope and the slope will give you the delay time.

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PVC -- this is what you get PVC, plastic. Now the frequency range on this is from about 900 kilohertz on the left to about 1.2 megahertz on the right. This is basically the magnitude of the transmission on a logarithm scale. It is not absolutely flat. It does fall off some. There is a peak, but you expect something like that because the crystal has a peak frequency,

The phase is nice and linear. The jags here are resets when it gets to minus 180th -- it jumps up to plus 180 again, but in between those you get nice linear phase. That is perfect -- see that?

HASTINGS: Speed it up.

MAGINNIS: I'm sorry. All right, well, very quickly if you do it with steel, this is stainless steel, you see all of these resonances and you see the phase, this kind of stuff, reverse phase. You don't have a nice transmission channel and in particular if you have a little different problem upstream and down stream that can foul up your correlation measurement. The correlation measurement requires identical properties on both channels.

[Introduced new transparency — Fig. 8]

Now I have some photos to show you, one other measurement which I did, which actually visualizes the sound flow inside the pipe and we use a system like this called a Schlieren system. I can provide more details on that later for those interested.

What it does, it creates a plane wave of light which goes through a sound field. On the far side almost all the energy is blocked a little dot at the focus, so only scattered light gets through and picked up by the camera and viewed on a monitor. You can do this for the pipe.

[—Next transparency →]

The optical path is this way. ~~It gives us a~~ glass put on the end of the pipe, the sound is put in here and you can tune this manually and you view the interior wave pattern on the monitor.

[Next—Showed photos →]

I haven't found a good way to reproduce these but I am just going to pass them around and let you look at these. These are recorded in that manner. These are photos of the sound field. Bright is high pressure; black is low pressure; these are standing waves inside the pipe which have a great variety of different patterns. Apparently this repeated 21 times in a row, cycles as you went through frequency.

ESTRADA: This is a way of visualizing?

MAGINNIS: This is a way of visualizing where the sound actually is inside the pipe, so what this shows is there is a great deal of variability.

If you look at the frequencies, these are very close in frequency. There's something like 7 kilohertz separating the whole family. If you work out the Bessel functions all of these would be degenerate for perfectly circular pipe. When you go to an elliptical pipe they get split and so you go through these patterns one after the other in a regular sequence as you raise the frequency, so these are very sensitive to temperature because as temperature changes the sound velocity in the wall, the sheer, and longitudinal velocity will change with a different temperature coefficient and the sound velocity in the water, so a small change in temperature can throw these patterns from one to the other.

They were able to sit in the lab and just the energy coming in to create the sound field which slowly warm the water and if we left the frequency fixed we could watch the system walk through these different patterns as the temperature changed.

PHILIPS: It's very helpful -- Malcolm Philips by the way -- very helpful to us but we have until 4:30.

MAGINNIS: Okay, I'll stop.

PHILIPS: Fifteen minutes left --

MAGINNIS: The final thing is we decided at Foxboro after investigating this for six years, and very high level modeling, that you would be between one and two percent with this technique and to get better than that you really have to have a good physical understand of that little correlation function, which is a single channel -- to be able to predict it.

Finally, I was at Foxboro when the Three Mile Island incident occurred, and so I know about the importance of reliability and the fact that instruments often or let's say occasionally may be needed to be used in non-nominal situations.

If this correlation meter works with the accuracy that's claimed at very high flow, which I sincerely doubt, what would happen if an emergency occurred and it was called upon to operate in a lower frequency? Now at Foxboro we had one of the few functioning sensors after the event at Three Mile Island was inside the containment vessel. It was a pressure vessel. It wasn't a primary pressure vessel of pressure sensor. It was monitoring oil pressure and some kind of hydraulic thing for a pump, but it continued to function and it was one of the key sources of information about what was the situation inside that vessel after the failure, so with that, I will stop and let someone else talk.

RICHARDS: Thank you. Do you think we could get copies or make copies of the slides that you presented today?

MAGINNIS: Certainly.

ESTRADA: This slide presents information which is included in a 1999 report prepared I believe by MPR Associates on nuclear feedwater measurement and cited -- that report included the claims made for cross-correlation flow measurement at that time, and I don't claim that this represents what is in the topical report because I don't know.

In this case the acoustical and the calibration coefficient uncertainties, which we have talked about, or which Tom has talked about, have got to be wrapped up in this quarter percent number and there is no way in my opinion that they can approach that or even the bottom line uncertainty of a half percent.

Our conclusion is that the uncertainties associated with acoustics and profile have got to be greater than 1 percent. That is the conclusion of my section, but I would like to invite Larry, Seth, George and others to comment.

MATTINGLY: My name is George Mattingly. I have a document I would like to read to the records here if I could --

RICHARDS: I just want to make sure -- if someone is going to speak, Mr. Hastings, as we have discussed before, they need to be part of your presentation.

HASTINGS: They all are. If they aren't I will tell you.

RICHARDS: The floor is not open to the public.

HASTINGS: Yes, I appreciate that.

MATTINGLY: I just want to mention that from the standpoint of NIST there is a lot of emphasis on NIST's part in ultrasonic flow metering techniques. We think it's a technique that actually can be extended, echoing the comments of Herb Estrada that we think it is essentially a well-understood technology that could evolve into a primary standard status and that is how highly we think at NIST of the technology.

What I want to say, extremely briefly, the full story is in the handout, is we have made tests at NIST of a couple of different kinds of clamp-on flow meters and one particular kind of a multichordal, an eight chordal path called "U" in this thing here. It happens to be a unit that was manufactured by Mr. Seth Fisher, a four chordal meter with eight paths.

What we wanted to do was we wanted at NIST to take a look at the existing clamp-on technology as it is in the industry now and what we did is we made, we invited a number of participants to participate in this. We had for example Controlotron and Panametrics, we had AMAG and Krohne and Mesa Labs -- I think that is the lot.

What I am showing you here is data that we have taken at NIST where we have clamped on these meters and we have measured the flow with the national standards for flow measurement, .1 percent accuracy facilities, and what we have gotten is this kind of a range of performances for each of the meters, all anonymous, listed here A through F, and what I am finding here is the actual data -- that is the actual datapoint you get when you calibrate that meter against the bucket.

What you have done is you have averaged the meter response over the 30 or 40 second collection time and the error bars on this chart represent the temporal excursions over the 40-second interval, and the dot at the center of these points represents the mean.

That is not how we recommend accuracy be looked at in a flow meter. What we recommend is that repeated tests, as Herb was saying, be done and you look at, for example, repeatability. What this means is that we have done separate tests five times each condition and we have averaged those five and gotten the standard deviation of those, and you can see here the scale is zero to 5 is about an inch on the real plot and what I applying here are the means and standard deviations of those, so that is repeatability. That is the best precision you will ever see, but we don't call that accuracy either.

Accuracy that we define is called reproducability and that is when you have done that same test under "turn off the pipe and turn it back on again." So the kind of things that we get out of that performance is shown here. What we do before we declare an accuracy -- by the way, on this plot this is a percent here. There is no manufacturer on that plot that's better than a percent, none, and we have not even added in the NIST contribution from my .1 percent to this kind of situation, so the there is nothing on this collection that is one percent.

The point I would drive home is that when you take multichord meters like the one that Mr. Fisher manufactures and you do the same kind of test for his meter and here is the reproducability for his, notice there is a significant scale change here, about 5 to 1, so this is a tenth -- this is two-tenths of a percent here.

The dark plots are a low velocity, which is bordering on the edge of the performance of this device. The high flows are the blue and the red. What you can see here is that across ~~the~~ them both, he's two-tenths or better and my facility is only a tenth, so the bottom line is here, and this is in ideal conditions when you have measured the flow. We know exactly what the Reynolds numbers are and we have done a lot of work to characterize the facility and this is the kind of performance we get with a multipath, four chordal unit. With the clamp-ons the ranges go up to about 3 percent, so 1 to 3 percent is what we get for the clamp-ons.

The conclusion I would like to leave you with is that the profile really does matter. I am telling you about a pristine, ideal test situation. Once we get into a power plant, where we get into an elbow situation or the roughness vagaries that Herb just talked about, I don't think you are going to see performance anything like the ones I have just showed you here.

I will stop there. If there are questions, I'd be happy to try to answer them. I think the conclusion is profile really matters. Even in a flow lab the performance we get for clamp-ons at this stage is not 1 percent. It is more like 3 percent. In a real power plant, I just shudder to think what the excursions could be from those levels.

RICHARDS: Dr. Mattingly, could we get a copy of your slides also, please?

MATTINGLY: Yes, they are in what I gave you.

RICHARDS: Thank you very much.

REGAN: ~~Jane~~ Jenny Regan. I would like to make a comment. Dr. Mattingly's data are also in the book in Tab 16.

FISHER: I am Seth Fisher and as he told you, I have been involved in ultrasonic flow measurement since 1960.

I have done a lot of testing and I certainly can confirm what is being said today about profiles and how they don't necessarily show the classical expectation based on the mathematics.

I have done a lot of testing at NIST. I have done a lot of testing at Alden Labs. I have done testing at Cap Line and I have done testing at Smith Meter -- all with flow facilities, and none of those facilities have produced the velocity distribution that one would predict based on the Reynolds number of operation.

If you take the Reynolds number that the meter shows versus the Reynolds number that is true for the viscous and inertial forces, I find that it varies from a ratio of .2 to one and this means then that any meter that is sensitive to that is going to have errors.

I would like to talk a little bit about the technique of tagging a section of flow and then measuring how long that section takes to travel a certain distance down the pipe. That technology is incorporated in the Allen salt method of measuring the flow and the dye dilution method of measuring flow and the isotope method of measuring flow as well as the correlation technique that's presented today. The difference is that this method of defining the section of flow is ultrasonic as opposed to salt. I am quite familiar with the Allen salt velocity method of measuring flow. They take great pains to distribute the salt slug that defines a section of flow in the pipe throughout the pipe, so they measure the whole wall-to-wall circumference, the circumference of the flow, and then they take that and then measure how long it takes that slug to move down to another set of sensors that then define the volume that the flow has passed through and measure the time. You can then calculate the flow.

That technique has never produced an accuracy better than 1 percent, and the experts in the field that have spent years trying to perfect that technology never quote accuracies any better than 1 percent, and that technology has been around for 100 years or so.

The other thing that I'm impressed with what's been said to day is that the cross-correlation method doesn't measure from wall-to-wall; it measures a certain portion of the velocity in the center of the pipe, the typical number being 80 percent.

That 80 percent means that one is not looking at 36 percent of the flow in that pipe, and that's a huge amount of flow not being accounted for, and that leads me to the conclusion that I don't believe that this technology, as I understand it, is capable of achieving a half a percent accuracy.

RICHARDS: Thank you, Mr. Fisher.

LYNNWORTH: Can we stop in five minutes?

RICHARDS: We're not going to run off.

[Discussion off the record.]

LYNNWORTH: I will talk fast.

HASTINGS: Larry Lynnworth.

LYNNWORTH: I'll try to save some time here. I have concerns in three areas, and I'll cover these in two minutes, I think: Theory, experiments, and independent verification. These are the three areas that I'm troubled by as an outside observer and competitor of Caldon.

I might say I'm here at my own expense, other than traveling.

The problem before us is to measure the true mean flow velocity, and what that means is to measure the average velocity of all the molecules going down the pipe. And the question I have is, how many of those molecules can we afford to disregard?

Yesterday was Super Tuesday. I'll draw a brief analogy to that. If anybody tried to predict the outcome of the November election by using a one-~~quart~~ chord sample right down parallel to the Mississippi, the results would not be likely to be accurate. So, say, I'll do better, I'll also go from New York to Los Angeles or Honolulu, and maybe stop at Anchorage.

It still would not be an adequate sample and give me a good way to predict the outcome of the election. And I think there is an analogous problem here in looking at one or two diameters, as opposed to the quad chords.

I'll do one quick arithmetic exercise for you. I'm familiar with the 3, 4, 5 triangle and most of you are, in terms of an RMS. If you recall, earlier on page 8, there's an error budget that's roughly .3 percent associated with dimensions.

If the hypotenuse of the RMS triangle is going to be.5, that leaves .4 for all errors associated with acoustic fluid interactions.

The last point is on independent verification, including looking at installation effects and disturbances that change or drift over time, like roughness.

So it seems -- you know, this is not a criminal situation where one is innocent until proven guilty. It seems to me, as a manufacturer, that the burden is on a manufacturer to prove the validity of his claims, his or her claims, because I don't know who drafted them.

My final comment, my final concern is the proposed one- or two- path cross-correlation tag method. Is this like a wonder drug, a cure for a very complicated disease, or is it something where it looks like a cure, and side effects are yet to be observed?

RICHARDS: Thank you.

HORN: I'm Roger Horn. I'm with ProDesCon.

I just wanted to amplify on what Herb said about this. Some of the analysis that I did was based upon a fully-developed flow, and that's what this diagram represents, which has a classic falloff, and it's very smooth.

The relative roughness for fully developed flow, as that varies over the range from .00015 to the .0015, has a larger effect upon the accuracy of the -- the uncertainty of the cross-correlation meter than it does as you get closer to the pipe bend.

And that was one difficulty I had in the analysis, is that as you get closer to a pipe bend and you have the disturbances, there is, as far as I could find in the literature, no analytical expression for how you develop that flow profile. And it's pretty much based upon experiment.

One of the things we had to do, but we don't want to short-change the cross-correlation meter, as you get closer to the band, the uncertainty is going to be less.

One of the things we were able to do, though, was related to some of the external mount meter and how its uncertainty is affected as you get closer to a bend. We actually did just a simple linear extrapolation.

When you're at fully developed flow, it's around 1.6 percent, and the using the same linear extrapolation you do for an external mount LEFM meter, we extrapolated that down to about one, maybe 1.1 percent for cross-correlation flow meter.

So what we did in the analysis was try to give the cross-correlation meter as much of a leeway as possible, and put it in the same situation that you would put an LEFM meter in, in order to come up with some reasonable uncertainty bounds.

So there are the numbers that are in the table, a proprietary document, Tab 8, where there is worst-case and best-case. Those were cases where we tried to fit it in with what we knew how the velocity profile was affected in an LEFM meter, fitted to a cross-correlation meter, because we do not have that in an analytical form.

Thank you.

RICHARDS: Thank you very much.

PHILIPS: Malcolm Philips. And this was kind of a very unusual meeting. In the preparation for the meeting, a number of experts came down here, and these experts didn't come together till last night, basically.

And last night and this morning, we all sat around the table talking. Our presentation was totally different until last night when we started asking each other the question of, is the cross-correlation flow meter capable of achieving the accuracy claimed?

And to a person, there was a resounding no. So we wanted to try to get something akin to a blue ribbon panel and bring them before you and say there's something wrong here.

And that's what these people tried to do. I tried to monitor time, and I apologize for rushing them, because they each had a piece to say, and they each wanted to get it out in their convincing way. But all of them, at bottom, had that one significant concern, which is, can the cross-correlation flow meter such as proposed by ABB, achieve the accuracy claimed?

And to the person, it was, in their professional opinion -- and we're talking about years and years and years of professional opinion here -- was no. And that's what they've said here today, and I just wanted to stand up and apologize for rushing things through.

I was trying to assure that we got it out on time, and not like it was a mistake on my part. But I wanted to make sure that you understood that.

RICHARDS: All right, thank you.

HASTINGS: With all that's been said, it seems that I could add very little. I'd just like to summarize the way I see it.

All the information that we provided in our submittal, I think that the points that were made here today tell me that you cannot conclude that this instrument can achieve an accuracy of a half of a percent; you can't do it.

I would say that it tells me that we cannot write an SER that says this meter has a half percent accuracy. The risks of that being wrong are just too great.

RICHARDS: Do you have any other presentations to make?

HASTINGS: I have none. I would say that I might even -- because I spoke to you by phone, I had thought we could make a recommendation. I always like, when I'm involved in something like this, to make a positive recommendation.

But as I thought about it, it's inappropriate for me to tell the NRC how to go about making their decisions. It just seems to me that I cannot do that.

All I can do is present to you, why I was concerned. I hope that I was able to convey how strongly I feel about it.

RICHARDS: What I was going to ask is, if there are no other presentations, if you would bear with us for a few minutes, I think we'd like to ask the members of the staff who were involved in the review to step outside so we can caucus separately.

HASTINGS: Surely.

RICHARDS: We'll join you again in a minute.

[Recess.]

RICHARDS: If I could everyone to have a seat, please?

Mr. Hastings, we very much appreciate you and your colleagues coming into today. It was a very well done presentation.

We've talked separately as the staff, and I can say that most of the material that you presented today, the staff is familiar with. We have done some review of the material that you presented us.

You have mentioned the review of the ABB submittal that we are presently looking at. Of course, we're not here today to debate that with you.

So at this point, we've talked, and we really don't have any questions. If we do, if you don't mind, we'll give you a call, but unless you have any other comments, we, again, appreciate your coming in, and we'll take the information you provided us under consideration.

Any other comments? Mr. Hastings?

HASTINGS: No, I have no more. I would thank you very much for the opportunity to come here and clarify.

RICHARDS: All right, thank you very much. That concludes the meeting.

[Whereupon, at 4:41 p.m., the meeting was concluded.]