TG

RCC

NINTH MEETING

NINTH TRANSDUCER WORKSHOP

22-24 APRIL 1977

WRIGHT PATTERSON AFB
DAYTON OHIO

TELEMETRY GROUP
INTER-RANGE INSTRUMENTATION GROUP
RANGE COMMANDERS COUNCIL

KWAJALEIN MISSILE RANGE
WHITE SANDS MISSILE RANGE
YUMA PROVING GROUND

NAVAL WEAPONS CENTER
PACIFIC MISSILE TEST CENTER
ATLANTIC FLEET WEAPONS TRAINING FACILITY
NAVAL AIR TEST CENTER

AIR FORCE FLIGHT TEST CENTER
AIR FORCE SATELLITE CONTROL FACILITY
SPACE AND MISSILE TEST CENTER
SPACE AND MISSILE TEST CENTER DET 1
ARMAMENT DEVELOPMENT AND TEST CENTER
AIR FORCE TACTICAL FIGHTER WEAPONS CENTER

NINTH
TRANSDUCER
WORKSHOP

26-28 April 1977
EGLIN AFB
FORT WALTON BEACH, FLORIDA

TRANSDUCER COMMITTEE
Telemetry Group
Range Commanders Council

Edited by
Kenneth D. Cox, General Chairman

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DISCLAIMER

This document has been published for information purposes only. The material contained herein does not necessarily represent the position or conclusions of the Range Commanders Council (RCC).

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INTRODUCTION

The Ninth Transducer Workshop was held in Fort Walton Beach, Florida, from 26 to 28 April 1977. It was sponsored by the Transducer Committee of the Telemetry Group, Range Commanders Council. The General Chairman was Kenneth D. Cox of Naval Weapons Center.

Workshop logistics were executed by a volunteer crew as follows:

William D. Anderson, Chairman Transducer Committee
NATC, Patuxent River

Colonel J. W. Gillette, Welcoming Address, Commander
3246th Test Wing
Eglin AFB

Sid Shelley, Accommodations and Arrangements
Host, Eglin AFB

Martha Cooper, Dayle E. Fitzgerald, Doris A. Cox, Registration

Milton M. Knowles, Clarence A. Tibbetts, Audio-Visual; both of
Eglin AFB

Workshop program duties were also performed by a volunteer crew as follows:

Kenneth D. Cox, General Chairman, Naval Weapons Center

Dayle E. Fitzgerald, Technical Secretary, Lawrence Livermore Laboratory

Session Chairmen:

Pierre F. Fuselier, Lawrence Livermore Laboratory

Steve Rogero, Jet Propulsion Laboratory, Edwards, California

John S. Hilten, National Bureau of Standards

Peter K. Stein, Stein Engineering Services, Inc.

Lawrence Sires, Naval Weapons Center

William D. Anderson, Naval Air Test Center

The traditional discussion format was observed. Workshops are just what the name says; everyone should come prepared to contribute something from his knowledge and experience. In a workshop the attendees become the program in the sense that the extent and enthusiasm of their participation determines the success of the workshop.

Participants had the opportunity to hear what their colleagues have been doing and how it went; to explore areas of common interest and common problems and to offer ideas and suggestions about what's new and what's needed in transducers, techniques, and applications.

GOALS OF THE WORKSHOP

To bring together people who use transducers; to air out problems and maybe come up with some solutions; to identify areas of common interest, and to provide a communication channel among the community of transducer users. Some examples are:

1. Improve coordination of information regarding transducer standards, test techniques, evaluations, and application practices among the national test ranges, range users, range contractors, other transducer users, and transducer manufacturers.

2. Set up special sessions so that people with measurement problems in specific areas can form subgroups and stay on to discuss them after the workshop conclusion.

3. Solicit suggestions and comments on past, present and future Transducer Committee efforts, and towards providing some standardization in the area of transducer signal conditioning amplifiers.

4. Provide task definition for the NBS Inter-Agency Transducer Project for transducer R&D, supported through NAVAIR and national test range funding.

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Data Multiplex

Recorders and Reproducers

Transducers
  Transducer Subcommittee

Transmitter/Receiver

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TRANSDUCER COMMITTEE OBJECTIVES

OBJECTIVES: This committee will inform the Telemetry Group (TG) of significant progress in the field of telemetry transducers; maintain any necessary liaison between the TG and the National Bureau of Standards and their transducer program or any other related telemetry transducer efforts; coordinate TG activities with other professional technical groups; collect and pass on information on techniques of measurement, evaluation, reliability, calibration, reporting and manufacturing and recommend uniform practices for calibration, test and evaluation of telemetry transducers.

RECOGNIZATION OF SPECIAL EFFORT

1. To Aaron Waldman for presenting the paper on Parachute Tests for Joe Doerr (who was unable to attend). He was asked and accepted only 4 days before the Workshop.

2. To Paul Lederer who presented Dale Rockwell's paper on rather short notice.

3. To Walt Kistler of Kistler Morse who replaced Strainsert on the Manufacturers Panel. (On very short notice, the night of the Panel)

4. To Pierre Fuselier for extra effort beyond the call of duty - in helping to organize the Manufacturers Panel and for giving freely of his time and experience to help edit this document.

CHANGES IN AGENDA

SESSION ONE: Jon Inskeep was unable to author or present his paper due to work scheduling beyond his control.

SESSION TWO: Went as planned.

SESSION THREE: Joseph Doerr's paper was presented by Aaron Waldman.

SESSION FOUR: (Manufacturers Panel) Walter P. Kistler of Kistler Morse Corp. replaced Strainsert.

SESSION FIVE: Paul Lederer, NBS, presented Dale Rockwell's paper.

Philip M. Aronson's paper (The Effect of Applied Pressure Step Rise Time and Shape on the Observed Rise Time of a Pressure Transducer) was presented in 5th place. This paper arrived too late to meet original deadline for printing of program, however, he accepted an invitation to participate.

Between the FIFTH and SIXTH SESSION: Although it does not appear in the agenda we had the pleasure of hearing Dr. David Goldman of Basic Standards Institute, NBS, address the attendees concerning the present and future conditions that exist at NBS. A question and answer session followed.

SESSION SIX: Went as planned with the exception of Joe Haden - who was unable to attend.

Corrections of printing errors in agenda:

1. Cover sheet should read Ninth Transducer Workshop.
2. Social hour was 2000 Monday, 25 April.
3. Registration was 0730 Tuesday, 26 April.
4. Sid Shelley's name was spelled incorrectly.
5. The introduction started at 0830 Tuesday, 26 April

AGENDA

NINTH
WORKSHOP
TRANSDUCER

SPONSORED BY:

Transducer Committee of
the Telemetry Group,
Range Commanders Council

26-28 April 1977

Definition of the Transducer Workshop

History:

The Workshop is sponsored by the Transducer Committee of the Telemetry Group of the Range Commanders Council. The eight previous meetings, beginning in 1960, were held at 1- to 3-year intervals at various U.S. Government installations around the country.

People:

Attendees are working-level hardware people who must solve real-life problems and are strongly oriented to the practical approach. Their field is making measurements of physical parameters using transducers. Test and project engineers should attend for education on the true complexity of transducer selection.

Subjects:

These include practical applications of transducers, conditioners and readouts, considered separately and in systems. Engineering tests, laboratory calibrations, development and evaluation all are potential applications involving present problems. Test controls and experimental methods used to assure valid data are essential elements in these applications. Measurands include force, pressure, flow, acceleration, velocity, displacement, temperature and others.

Emphasis:

1. The practical approach.

2. Strongly focused on transducers and related instrumentation used in measurements engineering.

3. Ratio of discussion to presentation of papers is high.

4. Open and universal discussion; problem solving through knowledge sharing. Session chairmen use speakers as a panel to stimulate discussion.

Goals:

To bring together people who use transducers; to air out problems and maybe come up with some solutions; to identify areas of common interest; and to provide a communication channel among the community of transducer users. Some examples are:

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4. Provide task definition for the NBS Inter-Agency Transducer Project for transducer R&D, supported through NAVAIR and national test range funding.

General Chairman:

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PRELIMINARY PROGRAM

Monday, 25 April 1977

Social Hour, courtesy of the Transducer Committee. All attendees welcome.

Tuesday, 26 April 1977

2000 Registration.

Welcome: Col. J. W. Gillette, Commander, 3246th Test Wing, Eglin AFB

0730 Introductions:

0830 Sid Schelly, ADTC (TSGGS) Eglin AFB

Bill Anderson, Chairman, Transducer Committee, RCC/TG

Kenny Cox, Chairman 9th Transducer Workshop

0900 Session 1: Pressure and Flow

Chairman: Pierre Fuselier, Lawrence Livermore Laboratory

Panel Members and Papers' Briefs. (10 minutes each):

Richard Hasbrouck, Lawrence Livermore Laboratory, "A Portable Transfer Standard for Telemetry-System Pressure-Transducer Calibration."

W. P. Brandt, Boeing Aircraft, "Dynamic Differential Pressure Measurements using Miniature Pressure Transducers."

M. J. Burger; D. C. Holten, Lawrence Livermore Laboratory, "Stress Analysis and Material Certification for Mechanically Critical Pressure Transducers."

Jon Inskeep, Jet Propulsion Laboratory, "Surface Pressure Measurement Systems for Aerodynamic Testing."

Thomas Crosby, NATC, "Low Airspeed Systems."

L. J. Mertaugh, NATC, "Aircraft Equipment Cooling Flow Measurements."

1010 Break

1020 Session 1 open discussion, with speakers sitting as a panel.

1200 Lunch

1330 Session 2: Transducer Signal Conditioning and General Transducer Topics.

Chairman: Steve Rogero, Jet Propulsion Laboratory, Edwards, California

Panel Members and Papers' Briefs (10 minutes each):

Captain David J. Ray, Kirtland AFB, "Close Coupled Strain Gage Signal Conditioners."

James Rieger, Naval Weapons Center, "Use of AGC and Compression for Conditioning of Wide Dynamic Range Signals."

Peter Stein, Stein Engineering Services, "Spurious Strain-Induced Voltages in Thermocouples, Strain Gages and Lead Wires—A Survey."

Vern Bean, NBS, "NBS Pressure Transducer Characterization Service."

Lawrence Sires, Naval Weapons Center, "Use of Holography for Fuel Droplet Characterization in Fuel-Air Explosive Clouds."

W. T. Escue, NASA Marshall, "Transducer and Signal Conditioner Philosophy for Large Programs."

1440 Break

1450 Session 2 open discussion, with speakers sitting as a panel.

Wednesday, 27 April 1977

0830 Session 3: Stress-Force and Temperature

Chairman: John S. Hilten, National Bureau of Standards

Panel Members and Papers' Briefs (10 minutes each):

Joseph Renick, Kirtland AFB, "Development of Piezoelectric Soil Stress Gage."

R. P. Reed, Sandia Laboratories, "A System for the Measurement of Free-Field Stress Waves using Lithium Niobate Piezoelectric Transducers."

Ping Tcheng; Kuo-yen Szema, Old Dominion University, "The Investigation of Strain Gage Force Transducer Behavior in a Cryogenic Environment."

Joseph Doerr, National Parachute Test Range, "Parachute Opening Shock Instrumentation."

Alan Holmes; Michael Duggan, Lockheed Palo Alto Research Laboratory, "The Quadraflexure as a Strain Element in Extensometry and Load Sensing Transducers."

Bert Dennis; Evon Stepani and Billy Todd, University of California, New Mexico, "A Thermopile Probe to Measure Temperature Anomalies in Geothermal Boreholes."

0940 Break

0950 Session 3 open discussion, with speakers sitting as a panel.

1130 Lunch

1300 Tour of Eglin AFB ADTC Climatic Laboratory and Transducer Evaluation Facility.

1800 No-host social hour at hotel

1830 Dinner at hotel

1930 Session 4: Manufacturers' Panel

Chairman: Peter K. Stein, Stein Engineering Services Inc.

Panel Members:

PCB PIEZOELECTRONICS INC.
BELL AND HOWELL
SENSING SYSTEMS AND
MEASUREMENTS
ENDEVCO
STRAINSERT
SCHAEVITZ ENGINEERING
SENSOTEC INC.
RdF CORP.
FLOW TECHNOLOGY INC.

0830 **Thursday, 28 April 1977**

Session 5: Displacement, Vibration and Standards

Chairman: Lawrence Sires, NWC

David Erlich, Stanford Research Institute, "Ultra-Sensitive, High-Frequency Surface Motion Transducer."

P. Wayne Whaley and Michael Obal, Wright-Patterson AFB, "Measurement of Angular Vibration using Conventional Accelerometers."

Rudy White, NATC, "Airborne Environmental and Electrical Measuring System."

Dale Rockwell, Navy Metrology Engineering Center, "Update on Transducer Standards Prepared Since the Eighth Transducer Workshop."

0900 Session 5 open discussion, with speakers sitting as a panel.

0940 Break

0950 Session 6: Definition of RCC/TG Transducer Committee Efforts to Implement Workshop Goals and Conclusions.

1000 Chairman: Bill Anderson, Chairman, Transducer Committee, RCC/TG

Panel: Transducer Committee Members

Charles Thomas, Wright-Patterson AFB

Kenny Cox, Naval Weapons Center

Joe Haden, Holloman AFB

Pierre Fuselier, Lawrence Livermore Laboratory

John Hilten, NBS

Paul Lederer, NBS

END OF WORKSHOP

GENERAL INFORMATION

The Ninth Transducer Workshop will be held 26-28 April 1977 at the Holiday Inn, Fort Walton Beach, Florida. The hosting agency is ADTC Eglin Air Force Base.

Registration

The registration fee is $17.50, by check, money order or cash (no purchase orders can be accepted).

Advance registration is desirable. Please use the enclosed registration form, include a check or money order for $17.50, payable to the Ninth Transducer Workshop, and mail by 4 April 1977. The registration fee covers coffee or soda water and doughnuts, the Wednesday evening fixed-menu dinner at the hotel, and a copy of the minutes of the workshop. Late registration will be provided at the Workshop registration desk in the hotel.

Hotel Accommodations

The official hotel for the Workshop is the Holiday Inn, Highway 98 & Santa Rosa Blvd., Fort Walton Beach, Florida, 32548. The telephone number is (904) 243-9181.

Special rates have been set up for Workshop attendees and will apply if you state on the enclosed reservation card, or state at time of registration in person, that you are attending the Ninth Transducer Workshop. Send in your reservation card early to be sure of getting a room; the special rates are $15.00 single or $20.00 double. Requests for room reservations should be mailed two weeks in advance, include first nights lodging fee.

All sessions will be held in the Ball Room of the Holiday Inn.

No formal program will be provided for wives; however, they will be most welcome at the Social Hour on Monday and the dinner on Wednesday.

Format and Background

The traditional discussion format will be observed. Workshops are just what the name says; everyone should come prepared to contribute something from his knowledge and experience. In a workshop the attendees become the program in the sense that the extent and enthusiasm of their participation determine the success of the workshop.

Participants will have the opportunity to hear what their collegues have been doing and how it went; to explore areas of common interest and common problems; to offer ideas and suggestions about what's new and what's needed in transducers, techniques, and applications. A few manufacturers, selected to represent a fair sampling of transduction methods and measurands, have been invited to the Ninth Transducer Workshop. Give some thought (and write it down!) to the questions, comments and topics you want to present to them, and make a copy to give the girls at the registration desk on Tuesday morning.

Our final session of Thursday morning is quite as important to the attendees as to the panel members. Please come prepared to contribute your opinions, ideas and recommendations as to the past, present and future tasks that these groups have accomplished or should undertake.

We will act as intermediaries to help individuals and small groups to get together on Thursday afternoon to discuss specific problems in measurement areas uniquely interesting to them.

Additional Information

May be obtained from the General Chairman or:

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NINTH TRANSDUCER WORKSHOP

OPENING REMARKS

Sid Shelley of Eglin Air Force Base:

My name is Sid Shelley and I work in the 3246th Test Wing at the Armament Development and Test Center, Eglin AFB. The Test Wing is the responsible agency at Eglin for all testing and for test facilities. My particular involvement relates to the test facilities. I provide an assortment of ground based range instrumentation and transducers used to obtain performance profiles on experimental munitions. Typical measurements required include blast pressure, acceleration, temperature, strain, fragment velocity and dispersion. I'm pleased to serve as project officer for hosting this event. To welcome you to Fort Walton Beach and to kick off the 9th Transducer Workshop we are fortunate to have with us the Commander of the 3246th Test Wing, Colonel Jack W. Gillette. Colonel Gillette took over the command of the 3246th Test Wing in June of 1976. Prior to that he was Chief of Test Operations Division. He is a graduate of the US Military Academy and has over 27 years of Air Force experience, 13 years of which have been associated with flight testing and test management. He is a command pilot with almost 7,000 hours flying time.

Ladies and Gentlemen, Colonel Jack W. Gillette

Colonel Jack W. Gillette, Commander, 3246th Test Wing, Eglin AFB

I'd like to welcome you to Eglin and to Fort Walton Beach. I hope you will find your accommodations to your likeing. Most of all I think you will find this, which I understand is the 9th Transducer Workshop, to be one of the most productive ever. Sid tells me that these sessions started 18 years ago, so I think it is particularly significant that they have come along this far. Everyone who has attended these workshops has found them extremely rewarding and has commented on the good ideas that seem to come from these sessions. We have the opportunity here to bring together working level people who must provide the hardware instrumentation and design, develop and use this instrumentation on a day-to-day basis. Direct, significant and rewarding benefits are derived because the emphasis is upon frank and open discussion and mutual understanding of each other's problems. I think sharing your experience of successes and failures among other members of the instrumentation community, in a test community, is vital to advancing the state of the art of measurements engineering. Those outside the test community often view testing as merely establishing the success or failure of a test item, but this is only part of the instrumentation problem. It is necessary to evaluate the item and know why it failed. That's really the substance of testing. Obviously the transducers used have a tremendous bearing on answering the question "why?". This Workshop couldn't have come at a better time. I've been involved in the test business off and on since about 1959, and I don't know of a time period when we were in deeper trouble financially in the R&D business. Since the early 60's tests of weapons systems have grown more and more complex, and the instrumentation needed to evaluate these systems has grown equally as complex. Also, testing and the cost of conducting tests have risen steadily over the past few years; and now the slope of the cost curve as a function of time is increasing even more. We now require devices that have never been built for test systems. Money to buy this instrumentation is in short supply. The man hours and technology associated with testing are going to be substantially reduced in FY 78, next 1 Oct. To live within these financial constraints, the test engineers and test designers are going to have to weed out all but the most hard-core objectives in developing a test plan. Then working with the instrumentation engineers, they are going to have to figure out the most economical way to conduct the test and still get sufficient data to render an accurate evaluation of the item that is being tested. Tough questions will have to be asked and answered; what type of data is needed; do I really need to get this particular parameter; is there a cheaper way to get the information; how accurate must the measurements be? It's possible to "gold-plate" an item to get data of much higher quality than is really needed. When we're under the cost constraints that we are under today, we have to get just what is needed and not pay for anymore. I noted from the agenda that you have a very challenging array of topics to discuss today, and I know that when you get through discussing these topics you will each find some new ideas to take back with you. I hope that some of your discussions will center on ways to reduce the cost of instrumentation. Unless people like you can figure out ways to decrease these costs and still have the physical parameters that we need to prevail, we're in deep trouble. I foresee no curtailment of items to be tested, but I foresee a strong curtailment of funds with which to do a test. We of the 3246th Test Wing are delighted to have the opportunity to act as host for this Workshop and I hope you will enjoy your visit here. If you have a very successful Workshop, pass on all the good words to Sid Shelley of my organization, but if it sort of falls flat, tell Ken Cox about it.

William D. Anderson, Transducer Committee Chairman, NATC

Good morning. I'm Bill Anderson, Chairman of the Transducer Committee of the Telemetry Group, Range Commanders Council. I and members of the Transducer Committee welcome everyone here to the 9th Transducer Workshop. I want to thank Colonel Gillette for his opening remarks and also Sid Shelley of the Test Center for hosting this 9th Transducer Workshop. I work in the Naval Air Test Center, and we at the test center are actively involved in the use of transducers in measuring parameters of aircraft flying qualities. I want to cover some of the background of the Transducer Committee of the Telemetry Group. The Range Commanders Council is an association of 13 national test ranges formed in 1951. One of its principal objectives is to get some standards established for making measurements on the test ranges. The Telemetry Group is one of several working groups within the Range Commanders Council. The Telemetry Group has been responsible for developing telemetry documents such as RCC 106 "Telemetry Standards", and RCC 118 "Test Procedures for Telemetry Systems or Subsystems". These items have become widely known as "IRIG Standards". The Transducer Committee is one of five committees in the Telemetry Group. The objectives of the Transducer Committee are, first, to inform the Telemetry Group of any new advances in transducers; especially telemetry-type transducers. Secondly, the objective of the Transducer Committee is to be a liaison between the NBS Inter-Agency Transducer Projects Group and the Telemetry Group. The final objective of the Transducer Committee is to look at all types of transducers and determine what's available, what our techniques are for calibrating and developing these transducers, and what's involved in manufacturing them. Also to develop standard methods for the calibration, evaluation and development of these transducers. One of our methods for accomplishing these objectives is to hold Workshops. This is the 9th Workshop that we are sponsoring. The first was in 1960, and generally the Workshops are held every two years. Session 6 is devoted to the Transducer Committee, who will report what has happened since the last Workshop and report to this Workshop the Committee's recent efforts. Also in that session we would like to get the attendees' viewpoints on where we should be going and what areas of work are necessary in the transducer field, both for the Telemetry Transducer Committee and also for the NBS Transducer Group. We hope you will all contribute to that session, so that we can get your ideas as experts in the area of transducers. We also hope that you will contribute in each of the discussion sessions. I now want to introduce Ken Cox, he is a member of the Transducer Committee and Chairman of this Ninth Transducer Workshop.

Kenneth D. Cox, Workshop Chairman, Naval Weapons Center.

Thank you Sid, Colonel Gillette and Bill. I would like to add my welcome to all the attendees at the 9th Transducer Workshop. Now lets get down to business. First, some announcements and copies of the Directory (the result of a task assigned to the Transducer Committee at the 7th Workshop) are available. If you wish to be a part of this directory please fill out the form and give it to me or one of the committee before you leave, or mail it to us. Also, Paul Lederer has 40 copies of the proceedings from the 8th Transducer Workshop, if you are interested contact Paul. I would now like to introduce Steve Rogero from the Jet Propulsion Laboratory, Edwards.

Steve Rogero, Jet Propulsion Laboratory, Edwards, California

I'm the chairman of an American National Standards Institute Subcommittee on pressure. This committee has been functioning for several years. My purpose this morning is to make you familiar with a guide for static calibration of pressure tranducers. It is now in preliminary form. We're planning on coming out with a standard probably within a year. If you would like to make comments and send them to me by the 15th of June we will consider them for inclusion in the report.

xxx

NOTES ON THE SESSIONS

The traditional format of the Transducer Workshop was observed. Each paper presentation was limited to 10 minutes and all papers in a session were given sequentially.

Authors then sat as a panel to spark the discussions and to answer questions and receive comments on their work. Participation in the discussions was extensive and productive.

The discussion summary reproduced here at the end of each session was taken from shorthand notes and tape recordings in an effort to capture the interactive spirit of the Workshop. Transcription and editing were done by Dayle Fitzgerald, Technical Secretary; Pierre Fuselier, Past General Chairman of the 8th Transducer Workshop, both of Lawrence Livermore Laboratory, and Kenneth D. Cox, General Chairman of the 9th Workshop, of the Naval Weapons Center.

SESSION I

PRESSURE AND FLOW

Pierre Fuselier, Chairman

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Lawrence Livermore Laboratory

EPMT: A Portable Transfer Standard for Telemetry System Pressure-Transducer Calibration

Richard T. Hasbrouck

April 5, 1977

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EPMT: A Portable Transfer Standard for Telemetry System Pressure-Transducer Calibration*

By

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Lawrence Livermore Laboratory, University of California
Livermore, California

Abstract

The LLL developed electronic pressure meter (EPMT) is a portable static-pressure calibration instrument for use with the LLL telemetry transducer system at the Nevada Test Site (NTS). It is significantly more accurate and rugged than the bourdon-tube pressure gauge it replaces, and can be incorporated into a field-use, semi-automatic, pressure calibration system.

This paper discusses the process by which a transducer is selected for EPMT use from our inventory of field-service-certified transducers and subjected to an extensive preconditioning and calibration procedure. By combining this unusual calibration procedure with a unique, statistically based data-reduction routine, the total uncertainty of the measuring process at each calibration point can be determined with high accuracy.

Work performed under the auspices of the US Energy Research & Development Administration, Contract No. W-7405-Eng-48.

1.0 INTRODUCTION

In many of the nuclear events conducted by LLL, at USERDA's Nevada Test Site (NTS), pressure measurement must be made on experimental systems which are located in a "downhole" environment. These measurements are used to verify actual system performance and provide diagnostic information if there is a system failure.

The pressures of interest range from atmospheric to the thousands of PSI and are measured using aerospace quality, gauged diaphragm, integrally signal conditioned, high-level-output transducers. The high dollar value of each experiment requires that great care be taken to insure that transducers committed to a system will not fail mechanically (an absolute "no-no") or electrically (very undesirable). This is accomplished through stringent specifications, close LLL/vendor liaison, and an extensive in-house certification program.[1]

Once in the field, output signals from the transducers are hardwired from their downhole location to the surface, a distance which can range from 500 to 6,000 feet. After being digitized and encoded, the signals are multiplexed onto the LLL PCM microwave telemetry link for transmission to the remote control-room facility. There they are decoded, recorded, and displayed, as required, in appropriate engineering units.

2.0 BACKGROUND

The key to reliable accurate transducer performance is good design and quality assurance, coupled with a program of controlled mechanical and electrical "past history".

Prior to being qualified, each candidate transducer is subjected to an LLL design evaluation of both the pressure cavity[2] and signal conditioning electronics. In order to guard against the undesirable effects of free hydrogen which may be present in the system, the LLL Specification[3] requires that the pressure body and diaphragm be an integral weld-free part, machined from an H_2 compatible material. Since the associated systems are ultra-clean, the cavity design, Figure 1, must be free from any voids or spaces which could entrap particulate matter.

When a design appears promising two evaluation units are procured and tested mechanically and electrically. If successful, the transducer design is then considered as qualified. The vendor is not to make any modifications to this design without LLL approval.

Unfortunately, qualification of a design and vendor are not enough to preclude transducer failures. These failures fall into two main categories; shelf-life and infant-mortality. With the former a previously good unit, when removed from bonded storage, is found to no longer meet the required specifications. In the latter case a failure occurs after a limited number of operating hours.

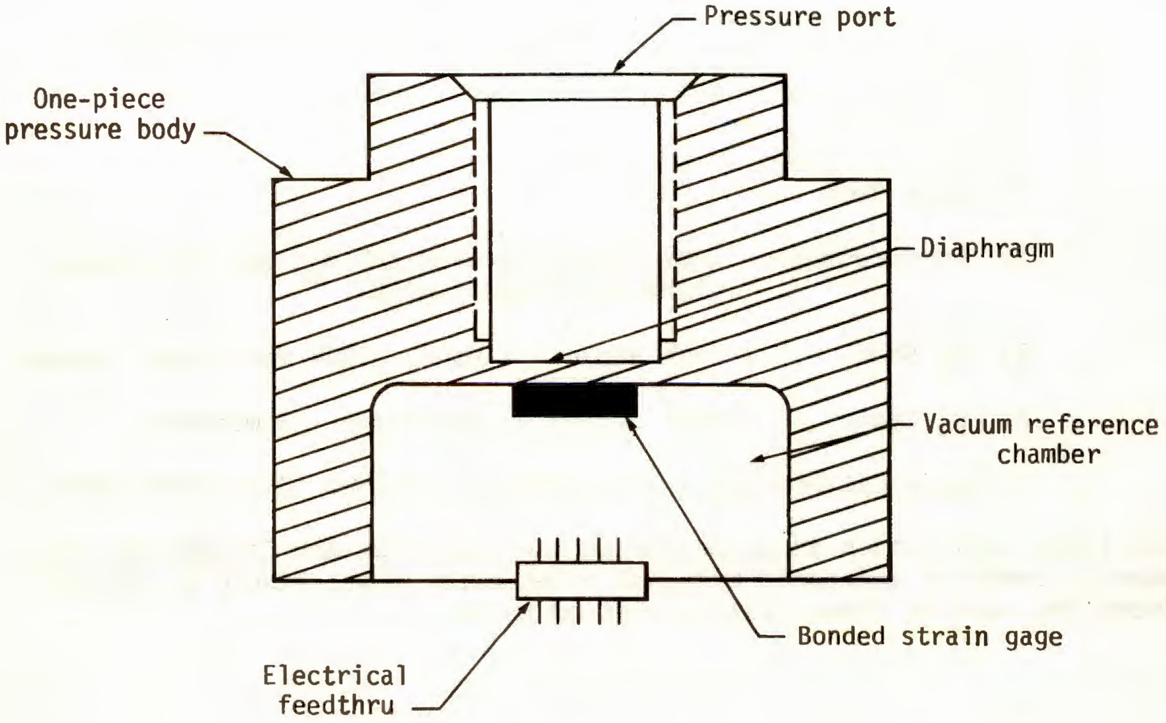


Fig. 1 Pressure Body Cross Section

Such problems are not detected by the vendor since prior to shipment the units generally experience less than ten pressure cycles and eight hours of electrical operation.

Thus, during actual use, the zero can vary as a function of strain gauge creep due to aging of the bonding adhesive, diaphragm characteristics can change due to working of the material, a gradual leak can destroy the reference vacuum, and electronic components can age.

To ameliorate the latter problem, we require that all transducers be burned-in for 168-hours at 150°F, with the maximum excitation voltage applied. This is done either at the vendor's plant or in LLL's Reliability Test Lab. Most infant-mortality failures are culled out and the subsequent overall reliability and stability are enhanced.

Once a shipment of transducers arrives at LLL, the pressure cavities are thoroughly cleaned of oil and particulate matter. Then they are routed through the following sequences:

a. Initial calibration check (gas) 2 cycles; 0/50/100%-Range
b. Burn-in (if not performed by vendor)[5]
c. Post burn-in calibration check (see a above)
d. Vibration and shock[6]
e. Calibration check (see a above)
f. Pressure Test

    1) Overpressure - 150%-Range (He plus 10% O_2) for 30-minutes (110% for 20-KSI units)

    2) H_2 Soak - 140%-Range - 2 hours (100% for 20-KSI units)

    3) H_2 Cycle - four cycles to 90%-Range - 5 minutes

g. Pressure Calibration - two cycles, 21 points (0-to-100%-Range)

The final calibration is performed in the LLL Force and Calibration Lab, where a computer generated terminal point error plot, Figure 2, is produced for each of these field-certified units.

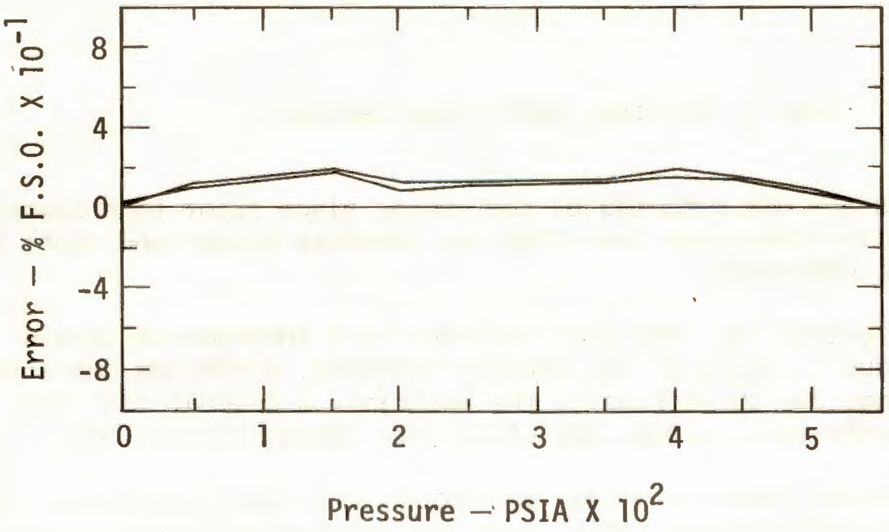


Fig. 2 Terminal Point Error Plot

When a transducer is installed in the final-use system, an end-to-end static pressure calibration is performed involving all portions of the event system. Ultra-pure inert gas, from a high pressure bottle, is manually introduced into the system until the desired calibration pressures are obtained, as observed on a bourdon-tube gauge. It is these gauges which we are replacing with the EPMT.

Consider that, following their calibration at LLL's Livermore Calibration Laboratory, the gauges are subjected to a multitude of "mechanical history" altering inputs: temperature excursions from 68°F in the Cal Lab, to 120°F in an NTS pick-up truck, to an air conditioned forward area building; ambient pressure changes from 14.7-PSIA at Livermore to subatmospheric during the plane ride to NTS with its 12.6-PSIA; physical impacts coincident with their transport. It is a wonder that they possess any accuracy at all, mirror scales notwithstanding.

3.0 ELECTRONIC PRESSURE METER WITH TRANSDUCER (EPMT)

To obtain a field calibration standard whose accuracy is in keeping with that of the transducers being checked, we have developed a rugged, portable, easy to use, transfer standard known as the EPMT.

A transducer for the required range is selected from our inventory of "well exercised" field-certified transducers. The criteria are: better than average linearity and repeatability; minimal hysteresis.

After being mounted within a blast case (see Figure 3), the transducer is installed in a small, portable, electronics enclosure (see Figure 4), with the pressure fitting brought out to the front panel. Also contained within the box are a power supply, buffer amplifier, digital voltmeter (DVM), analog voltmeter, power and shunt-calibrate switches, and power and output signal connectors. In addition, a small mechanical pressure gauge is included to provide a rough indication of the pressure. This gauge becomes very important if an electrical power or electronics failure occurs while the system is pressurized.

The high level (0-5 V DC) transducer output signal is applied, via a unity gain buffer amplifier and resistive attenuation network, to the precision, 40,000 count, auto-zeroing, DVM which displays pressure directly in PSI (refer to Figure 5). Zero adjustments are included to correct for unavoidable transducer and buffer amplifier zero shifts. The attenuator is adjusted to provide an output voltage which represents the pressure in PSI. Output from a second buffer amplifier is applied to an analog meter to permit observation of trends during periods of rapid pressure change.

Binary coded decimal (BCD) and 0-5 V DC output signals are available for use with digital and analog recording equipment.

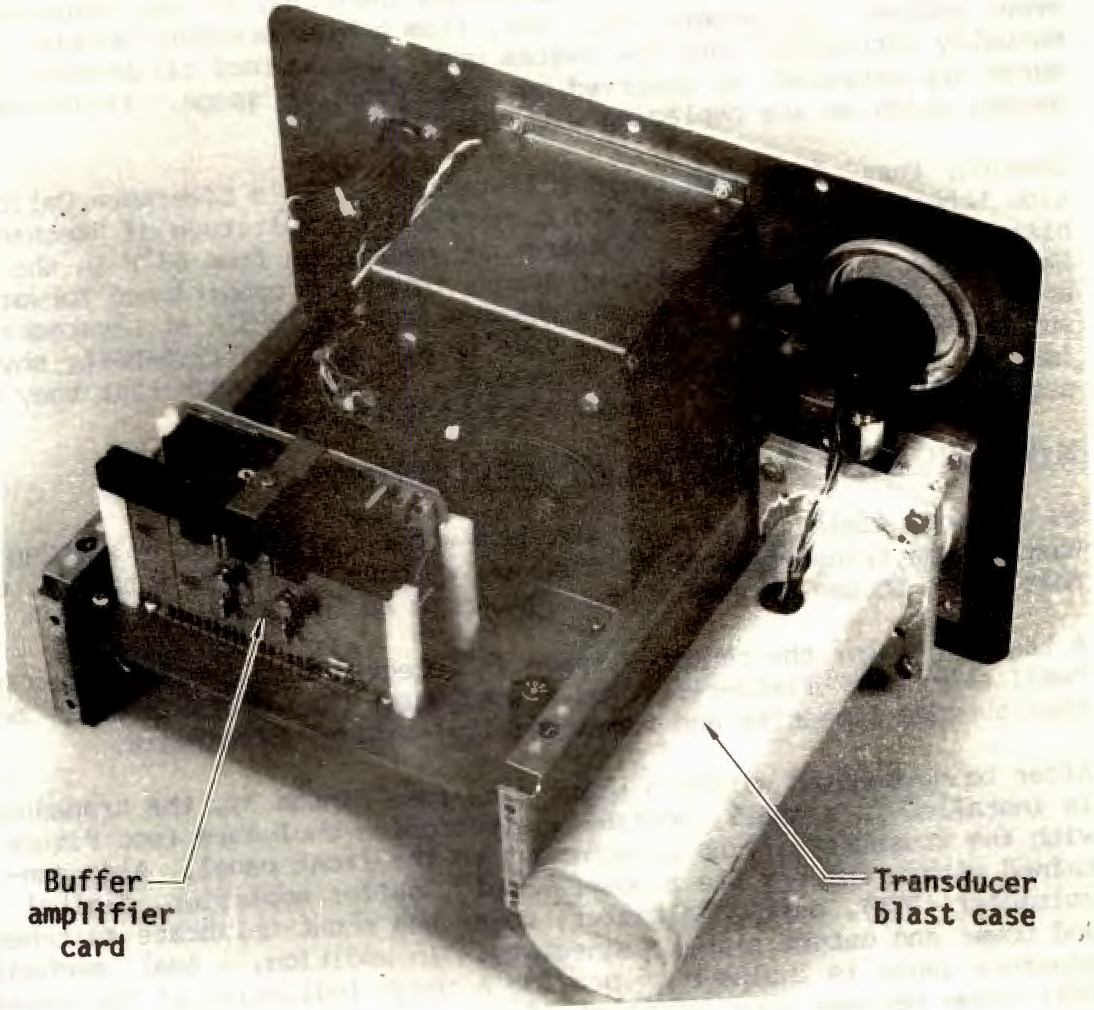


Fig. 3 EPMT - Inside Rear View

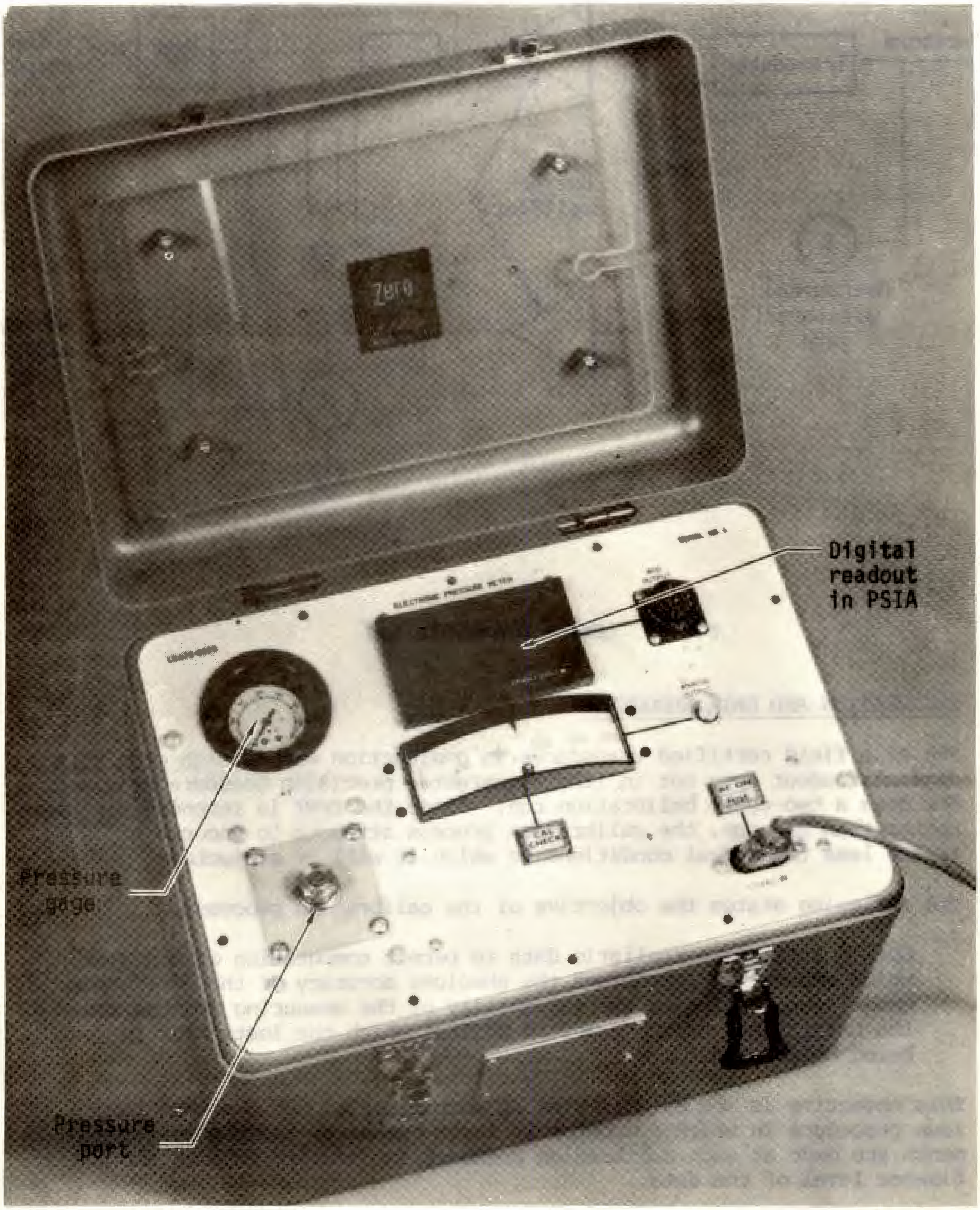


Fig. 4 EPMT - Front Panel

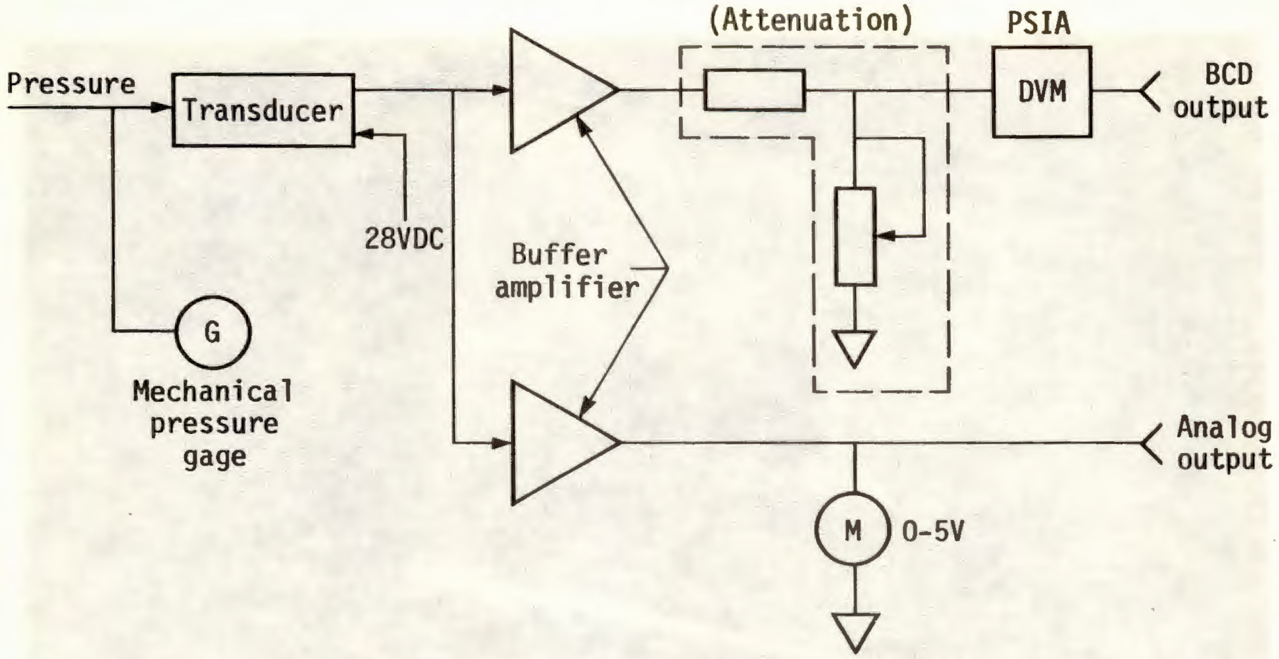


Fig. 5 EPMT - Block Diagram

4.0 CALIBRATION AND DATA REDUCTION

Use of a field certified transducer in conjunction with a high resolution digital readout does not in itself guarantee precision pressure readings. Nor does a two-cycle calibration run. Since the EPMT is intended for field rather than Lab use, the calibration process attempts to incorporate some of the less than ideal conditions to which it will be subjected.

The following states the objective of the calibration process:

> Obtain sufficient realistic data to permit computation of a number which reasonably represents the absolute accuracy of the instrument under test, including the variability of the measuring process and the uncertainty of the standard against which the instrument is being calibrated.

This objective is met by employing an uncomplicated but somewhat laborious procedure in which a moderately large number of independent measurements are made at each calibration pressure in order to enhance the confidence level of the data.

Each EPMT is preconditioned with five overpressurizations to 125%-Range. Zero is set at 0%-Range and, with a very precise 100%-Range pressure applied, the span is adjusted to provide the required readout in PSI. No further zero or span adjustments are made for the duration of the calibration process.

Next, the EPMT is subjected to two calibration cycles in 10%-Range increments. A maximum pressure of approximately 110%-Range is applied, with no data taken at that point, so that ascending and descending data at 100%-Range can be recorded. The EPMT is deenergized, pressure fittings disconnected, and the unit removed to another location where the ambient temperature differs by at least 15 to 20°F from that of the Cal Lab. After a minimum of 1-hour, it is returned to the Cal Lab where, after a 30-minute warm-up, the cycle is repeated. After a total of ten calibration cycles (220 points) have been recorded, the process is complete. Because of Cal Lab workloads, this process may extend over a period of several weeks, which lends to the statistical validity of the data.

Figure 6 shows the applied calibration pressures and the corresponding EPMT readings for a 500-PSIA EPMT. The odd sample numbers (S#) are for ascending pressures while even S#'s are for descending pressures.

	Applied pressure — PSIA										
S#	0	50	100	150	200	250	300	350	400	450	500
1	0.0	50.7	100.9	151.0	200.5	250.6	300.6	350.4	400.5	450.4	500.0
2	0.3	50.6	100.8	150.9	200.4	250.5	300.4	350.2	400.6	450.2	500.0
3	0.0	50.6	100.9	151.1	200.6	250.6	300.6	350.5	400.7	450.4	500.0
4	0.3	50.6	100.7	150.8	200.4	250.4	300.5	350.4	400.6	450.2	500.0
5	0.0	50.5	100.7	150.9	200.5	250.5	300.6	350.6	400.8	450.6	500.2
6	0.1	50.5	100.7	150.9	200.4	250.5	300.5	350.5	400.7	450.5	500.1
7	0.1	50.7	100.9	151.1	200.7	250.7	300.7	350.7	400.9	450.7	500.3
8	0.2	50.6	100.8	151.0	200.5	250.6	300.6	350.6	400.7	450.6	500.2
9	0.4	50.8	101.1	151.3	200.9	250.9	301.0	351.0	401.1	450.9	500.4
10	0.4	50.7	101.1	151.2	200.8	250.9	300.9	350.9	401.1	450.9	500.4
11	0.4	50.9	101.2	151.3	200.9	251.0	301.0	350.9	401.2	451.0	500.6
12	0.4	50.8	101.0	151.1	200.8	250.8	300.9	350.8	401.0	450.8	500.5
13	0.2	50.7	100.9	151.1	200.7	250.8	300.9	350.8	401.0	450.8	500.4
14	0.2	50.6	100.8	151.0	200.6	250.6	300.8	350.7	401.0	450.8	500.3
15	0.2	50.7	101.0	151.2	200.7	250.8	300.8	350.8	401.0	450.8	500.4
16	0.2	50.7	100.9	150.9	200.6	250.7	300.8	350.8	401.0	450.8	500.4
17	0.2	50.7	101.0	151.1	200.8	250.9	300.7	350.8	401.0	450.8	500.4
18	0.2	50.6	100.9	151.0	200.6	250.7	300.8	350.7	400.9	450.7	500.3
19	0.2	50.7	101.9	151.2	200.7	250.8	300.8	350.7	401.0	450.8	500.4
20	0.2	50.6	100.8	151.1	200.6	250.8	300.5	350.6	400.9	450.8	500.4

Fig. 6 Calibration Data
500-PSIA EPMT

Next, the data is reduced using a unique statistically based routine from which an accuracy statement for each cardinal point is obtained. In addition, a conservative overall accuracy statement (total error band) is produced.

Data Reduction Procedure

The arithmetic mean ($\bar{x}$) and standard deviation (s) for all 20 observations are found for each value of applied pressure (P_A).

$$\bar{x} = \frac{\Sigma x_i}{n}$$

$$s = \sqrt{\frac{\Sigma x_i^2 - \frac{1}{n}\left(\Sigma x_i\right)^2}{n - 1}} = \sqrt{\frac{\Sigma \delta^2}{n - 1}}$$

Where x_i = The EPMT reading at a particular applied pressure, P_A

n = The total number of observations (20 for this process)

δ = $(\bar{x} - x_i)$, the deviation of each observation from the mean of the set of data.

Determination of Uncertainty

Since the EPMT is to be used as a working standard, it is desirable to know how closely a particular reading can be trusted. If a random pressure is applied, the resulting reading can be believed, with a stated probability known as the "confidence level", to represent the true absolute value of the applied pressure within a band of uncertainty known as the "confidence interval" (C_s).

The standard deviation, ± s, defines a range about the mean within which it is reasonable to expect 68% of the observed data to lie, assuming normal distribution.

To obtain the confidence interval, the standard deviation is multiplied by a coefficient which takes into account the number of observations and the desired confidence level. For 20 observations and a confidence level of 95%, the coefficient extrapolated from the referenced table[7] is equal to 2. Thus, $C_s = 2s$, and may have a different value for each value of P_A.

Next, the uncertainty of the mean must be considered. For the confidence level stated above the confidence interval, $C_{\bar{x}}$, is

$$C_{\bar{x}} = \frac{C_S}{\sqrt{n}} = 0.224\ C_S$$

Likewise, the value of applied pressure, P_A, produced by the primary calibration standard, possesses some uncertainty. This value, U_S, (expressed in PSI) is the stated accuracy of the standard. Note that $U_S = 0$ for P_A = 0-PSIA if 100%-Range of the test transducer is much greater than vacuum.

The total uncertainty, $\pm U_T$, of the EPMT, at each value of P_A, is

$$\pm U_T = \pm (C_S + C_{\bar{x}} + U_S)$$

A substitution of terms yields

$$\pm U_T = \pm (2.448s + U_S)$$

Zero Corrected Mean

During the period of the full calibration cycle, the EPMT readings at 0%-Range also vary. Thus, the mean of those readings, $\bar{x}_O$, should be subtracted from the mean pressure reading at each observation point to obtain a zero corrected mean, $\bar{x}_C$.

$$\bar{x}_C = \bar{x} - \bar{x}_O$$

Correction Factor

Taking the difference between the zero corrected mean and the applied pressure provides a correction factor, C_F.

$$C_F = \bar{x}_C - P_A$$

When obtaining a precise pressure (P), the correction factor is added to the value of P to determine what EPMT reading (P_M) should be observed.

$$P_M = P + C_F$$

Note that for the above to be valid, the EPMT zero must be adjusted to provide a reading of 0-PSIA at 0%-Range.

It can be stated:

> when the applied pressure produces an EPMT reading equal to P_M, the actual pressure present equals the desired pressure P, within the stated uncertainty, $\pm U_T$.

When the greatest accuracy is required of the EPMT, it should be used at the specific calibration pressure points, with readings appropriately corrected.

Determination of the Error Band

For more general use, an overall accuracy statement can be used. At each value of P_A, calculate the sum and difference of the correction factor and total uncertainty to obtain an error term, $\varepsilon(\pm)$.

$$\varepsilon(\pm) = C_F \pm U_T$$

Using the above equations the calibration data has been reduced to the final form shown in Figure 7. Note that all values are in units of PSIA.

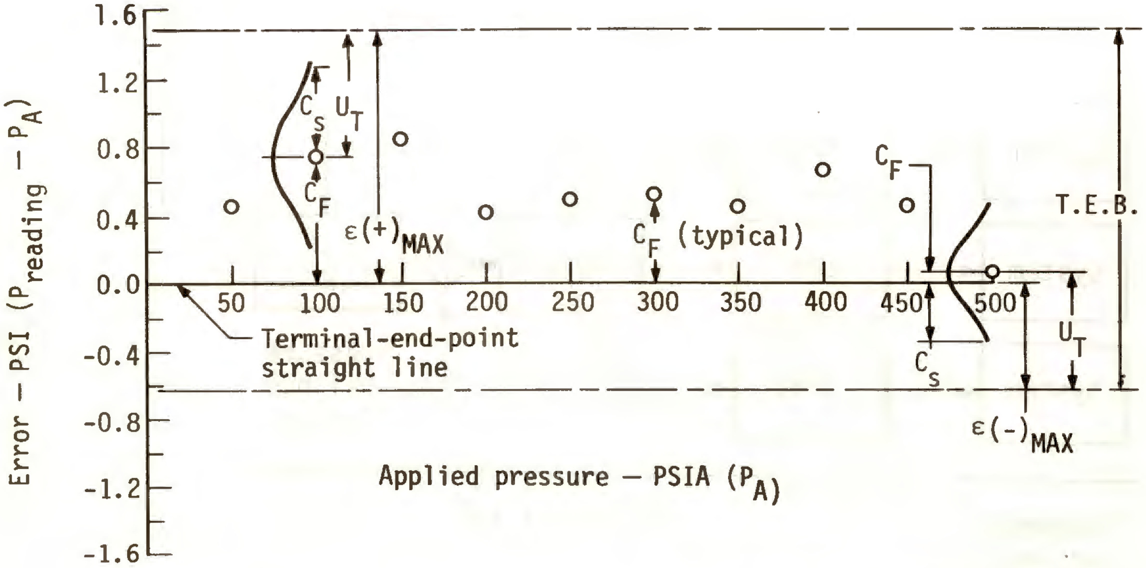
P_A	$\overline{x}$	s	C_s	$C_{\overline{x}}$	U_S	U_T	$\overline{x}_c$	C_F	$C_F + U_T$	$C_F - U_T$
0.00	0.21	0.129	0.259	0.058	0.00	0.32	0.00	0.000	0.317	-0.317
50.00	50.67	0.099	0.198	0.044	0.10	0.34	50.46	0.455	0.797	0.113
100.00	100.95	0.263	0.525	0.117	0.10	0.74	100.74	0.740	1.483	-0.003
150.00	151.06	0.139	0.278	0.062	0.10	0.44	150.85	0.850	1.291	0.409
200.00	200.64	0.157	0.313	0.070	0.10	0.48	200.43	0.425	0.908	-0.058
250.00	250.71	0.164	0.328	0.073	0.13	0.53	250.50	0.495	1.021	-0.031
300.00	300.72	0.177	0.353	0.079	0.15	0.58	300.51	0.510	1.092	-0.072
350.00	350.67	0.198	0.395	0.088	0.18	0.66	350.46	0.460	1.119	-0.199
400.00	400.89	0.193	0.385	0.086	0.20	0.67	400.68	0.675	1.347	0.003
450.00	450.68	0.227	0.454	0.101	0.23	0.78	450.47	0.465	1.245	-0.315
500.00	500,29	0.181	0.363	0.081	0.25	0.69	500.08	0.075	0.769	-0.619

Fig. 7 Analysis of Calibration Data
500-PSIA EPMT

Select the maximum positive and negative value of ε (+ 1.48 and - 0.62 PSIA for the data shown). Next, using the terminal-end-point straight line (passing through 0%, 0% and 100%, 100%-Range) as a reference, the pair of parallel lines passing through $\varepsilon(+)_{max}$ and $\varepsilon(-)_{max}$ define the limits of the total error band (T.E.B.).

Figure 8 depicts the T.E.B. for the example 500-PSIA EPMT. In order to obtain adequate resolution, the abscissa represents the error, in PSIA, between the zero-corrected EPMT reading and the corresponding applied pressure. The terminal-end-point straight line corresponds to zero error.

Also shown are the normal distribution curves associated with $\varepsilon(\pm)_{max}$.



- OVERALL ACCURACY (Conservative) - TOTAL ERROR BAND
  - There is 95% probability that any EPMT reading will represent the true pressure within the uncertainty expressed by the T.E.B.

Fig. 8 Total Error Band Plot - Partial 500-PSIA EPMT

The result of this process can be stated as follows:

> There is a 95% probability that any EPMT reading will represent the true applied pressure within the uncertainty expressed by the T.E.B.

The preceding calculational procedure is presented in the Appendix for use with a programmable pocket calculator (HP55). A programmable desk-top calculator (HP9830) was used to obtain the tabulated data and data analysis shown in Figs. 6 and 7. The latter program is available from the author upon request.

5.0 THE OVERALL CALIBRATION SYSTEM

To achieve and maintain the accuracy of the EPMT requires the availability of a hierarchy of field and laboratory pressure standards. (Refer to Figure 9.)

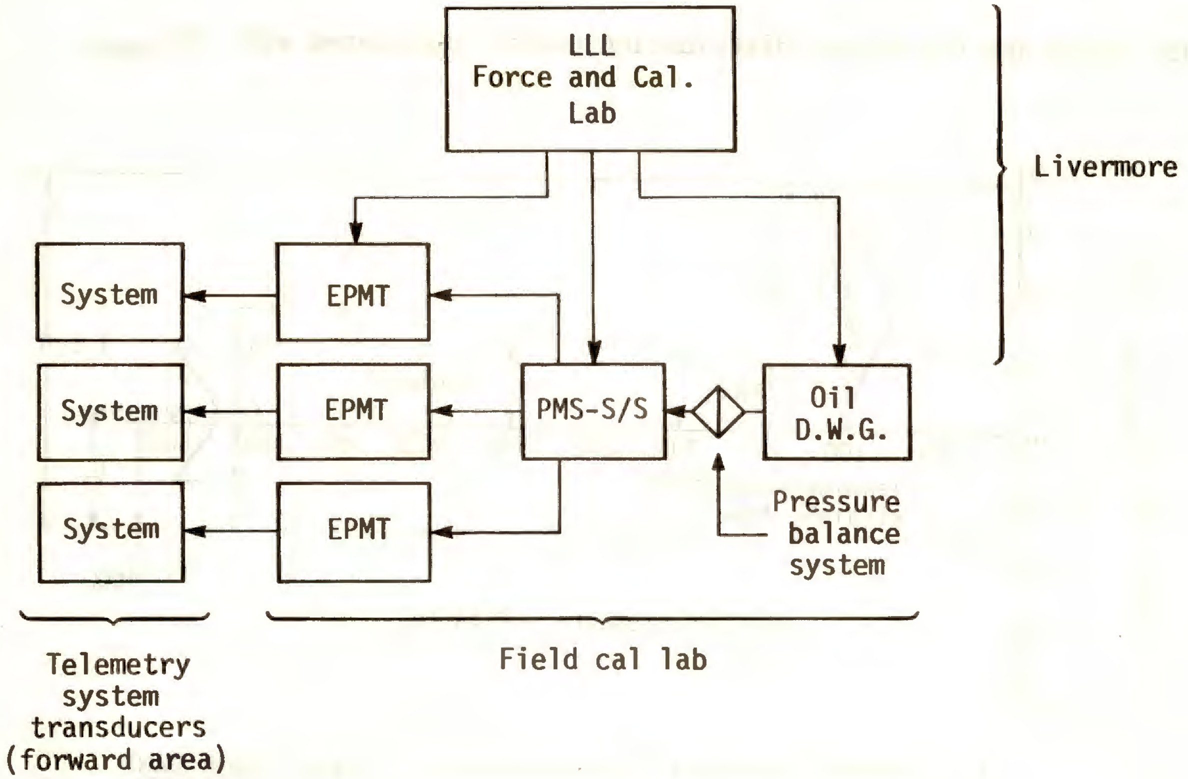


Fig. 9 Block Diagram - Overall Calibration System

The LLL Force and Calibration Lab utilizes a Gilmore 10-KSI automatic gas dead weight system and a Ruska 40-KSI oil dead weight gauge, (DWG). A field calibration facility has been established, at NTS, which incorporates a Ruska 12-KSI oil DWG as a local "primary" standard, plus a working standard, the Pressure Measurement System - Secondary Standard (PMS-S/S). The latter system incorporates oven-mounted, precision, diaphragm-type, pressure transducers, in conjunction with linearization electronics. The pressure is displayed digitally in engineering units.

The EPMT functions as the transfer standard used for calibration of the "downhole" telemetry-system transducers.

All three field instruments were initially calibrated at Livermore, with both the EPMT and PMS-S/S receiving the 220 point calibration described above.

The PMS-S/S is used to perform regular calibration checks of EPMT's, transducers, and gauges. Periodically, the PMS-S/S is checked against the DWG and adjusted, as required, to correct for zero and span errors.

Note that the oil DWG is isolated from PMS-S/S by a differential pressure cell operated in conjunction with an electronic pressure-balance readout. This is done to avoid the introduction of oil into the clean systems which are to be calibrated. Extensive testing has been done in order to determine the null uncertainty introduced by ΔP-cell null shift at high line pressures. The resulting term must be added to the PMS-S/S uncertainty in order to define the total uncertainty, or variability, of the measuring process.

6.0 A PROPOSED SEMI-AUTOMATIC CALIBRATION SYSTEM

Currently, when an end-to-end static pressure calibration is performed, pressure is metered into the system via a hand-operated valve. The EPMT can be easily incorporated into a semi-automatic system as seen in Figure 10.

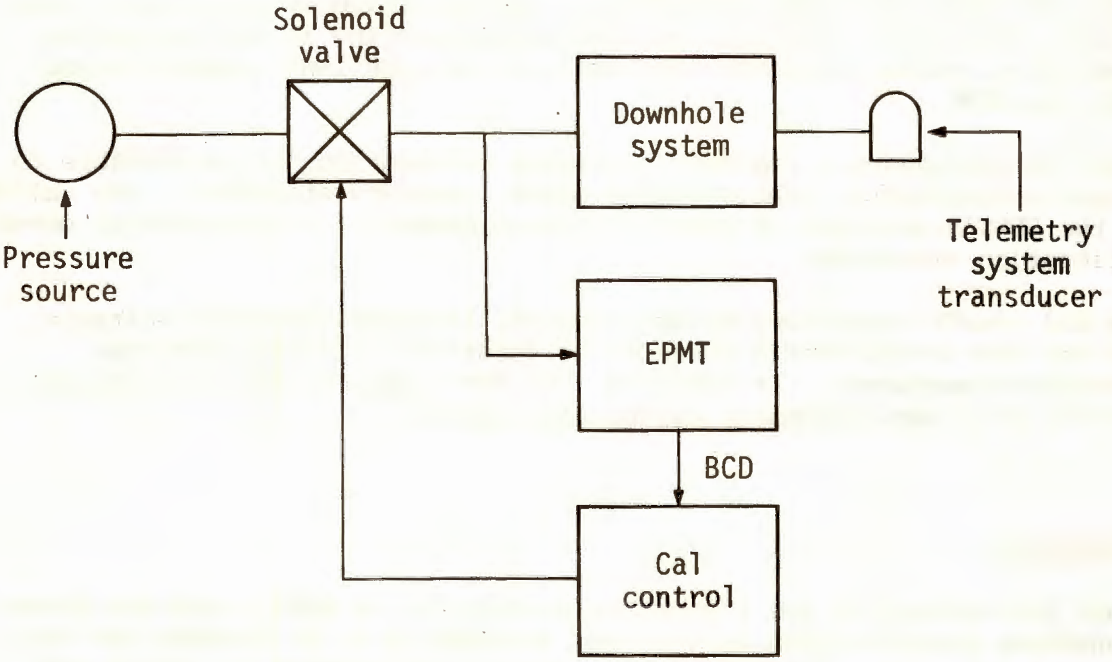


Fig. 10 Proposed Semi-automatic Calibration System

Gas from the high pressure source passes through a fast acting, normally-closed, solenoid actuated valve.

The pressure is sensed by the EPMT and the resulting BCD output signal is fed into the portable calibration control box. Having selected the number of calibration points desired and the appropriate EPMT pressure range, the START switch is activated. The solenoid valve opens and the pressure rises until the EPMT BCD output equals the value of the first calibration point set into the control box digital counter. The valve closes and following a suitable delay, to permit temperature equilibration, the time-of-day and EPMT pressure readings are printed out. The telemetry system transducer output can also be recorded locally, on a second printer, as well as at the remote control-room facility. Again, the valve opens and the calibration run continues. A preliminary study indicates that all of the necessary control can be accomplished using either a few discrete logic components or a microprocessor.

7.0 SUMMARY

The key to reliable accurate pressure transducer performance is careful control over both the design and accumulated "mechanical and electronic past history". From an inventory of such transducers it is possible to select units which exhibit better than specified linearity, repeatability, and hysteresis characteristics. Incorporation of such a unit into a safe, convenient, portable enclosure and coupling it with a digital electronic readout provides the basis for an excellent transfer standard, the EPMT.

Next, by performing a rigorous precision calibration, it is possible to obtain a meaningful, statistically based, accuracy statement. The validity of the EPMT's accuracy is based on the existence of a hierarchy of pressure calibration standards.

The end result is an easy to use, rugged, transfer standard, suitable for the end-to-end static pressure calibration of telemetry-system pressure-transducers. In addition, the EPMT can provide a significant portion of a semi-automatic calibration system.

Acknowledgement

Design and fabrication of the transducer portion of the EPMT, plus the extensive transducer qualification testing was provided by A. W. Hampton and his High Pressure Lab crew. The laborious precision calibration procedure was done by J. Kimberling and J. Phair of the Force and Calibration Lab. Finally, special thanks go to C. Miks of the Ruska Instrument Corporation, who introduced me to the concept of the "variability of the measuring process", and P. Stein who acquainted me with the significance of "past mechanical history".

Appendix

Data Reduction Routine for HP-55 Calculator

STEP	INSTRUCTIONS	INPUT DATA/UNITS	KEYS				OUTPUT DATA/UNITS
1	ENTER PROGRAM						
2	STORE Us FOR 0%-RANGE	0	STO	0			
3	STORE $1/\sqrt{m}$	0.224	STO	1			
4	CALC. $\overline{X}$, s @ PA = 0% RNG	P_0	Σ+				
5	FETCH $\overline{X}_0$		f	$\overline{x}$			$\overline{X}_0$
6	STORE $\overline{X}_0$	$\overline{X}_0$	STO	2			
7	FETCH $\overline{X}$		f	$\overline{x}$			
8	STORE $\overline{X}$	$\overline{X}$	STO	3			$\overline{X}$
9	FETCH s		g	s			
10	STORE	s	STO	4			s
11	STORE PA & COMP.	PA	STO	5	BST	R/S	
12	FETCH $\overline{X}_c$		RCL	6			$\overline{X}_c$
13	FETCH CF		RCL	7			CF
14	FETCH UT		RCL	8			UT
15	FETCH ε(+)		RCL	9			ε(+)
16	FETCH ε(−)		RCL	•	0		ε(−)
—	END OF RUN —						
17	CLEAR Σ+ REG'S		g	CL.R			
18	STORE Us IF ≠ STEP 2 VALUE	Us	STO	0			
19	CALC. $\overline{X}$ s FOR NEXT CAL. RANGE	P_N	Σ+				
20	GO TO STEP 7 &						
	CONTINUE THRU STEP 19 UNTIL DONE						

INSTRUCTIONS

Press **BST** in RUN m

DISPLAY		KEY ENTRY					
LINE	CODE						
00.							
01.	34	RCL					
02.	03	3	$\bar{X}$				
03.	34	RCL	$\bar{X}$				
04.	02	2	$\bar{X}_0$	$\bar{X}$			
05.	51	−	$\bar{X}_c$				$\bar{X}_c = \bar{X} - \bar{X}_0$
06.	33	STO	$\bar{X}_c$				CORRECTED MEAN)
07.	06	6	$\bar{X}_c$				
08.	34	RCL	$\bar{X}_c$				
09.	05	5	P_A	X_c			
10.	51	−	CF				$CF = \bar{X}_c - P_A$
11.	33	STO	CF				
12.	07	7	CF				
13.	34	RCL	CF				
14.	04	4	s	CF			
15.	02	2	2	s	CF		
16.	71	×	C_s	CF			$C_s = 2s$
17.	41	ENT	C_s	C_s	CF		
18.	41	ENT	C_s	C_s	C_s	CF	
19.	34	RCL	C_s	C_s	C_s	CF	
20.	01	1	.224	C_s	C_s	C_s	
21.	71	×	$C_{\bar{X}}$	C_s	C_s	C_s	$C_{\bar{X}} = .224\,C_s$
22.	61	+	$C_{\bar{X}}+C_s$	C_s	C_s	C_s	
23.	34	RCL	$C_{\bar{X}}+C_s$	C_s	C_s	C_s	
24.	00	0	U_S	$C_{\bar{X}}+C_s$	C_s	C_s	
25.	61	+	U_T	C_s	C_s	C_s	$U_T = U_S + C_{\bar{X}} + C_s$
26.	33	STO	U_T	C_s	C_s	C_s	
27.	08	8	U_T	C_s	C_s	C_s	
28.	34	RCL	U_T	C_s	C_s	C_s	
29.	07	7	CF	U_T	C_s	C_s	
30.	61	+	$\varepsilon(+)$	C_s	C_s	C_s	$\varepsilon(+) = CF + U_T$
31.	33	STO	$\varepsilon(+)$	C_s	C_s	C_s	
32.	09	9	$\varepsilon(+)$	C_s	C_s	C_s	
33.	34	RCL	$\varepsilon(+)$	C_s	C_s	C_s	
34.	08	8	U_T	$\varepsilon(+)$	C_s	C_s	
35.	02	2	2	U_T	$\varepsilon(+)$	C_s	
36.	71	×	$2 \cdot U_T$	$\varepsilon(+)$	C_s	C_s	
37.	51	−	$\varepsilon(-)$	C_s	C_s	C_s	$\varepsilon(+) - 2U_T$
38.	33	STO	$\varepsilon(-)$	C_s	C_s	C_s	$= (CF + U_T) - 2U_T$
39.	83	•	$\varepsilon(-)$	C_s	C_s	C_s	$= CF - U_T$
40.	00	0	$\varepsilon(-)$	C_s	C_s	C_s	$= \varepsilon(-)$
41.	84	GTO 0					
42.	-00	R/S					
43.							◇ STORE FOR
44.							EACH RUN
45.							
46.							⬡ RESULTS
47.							OF RUN

REGISTERS	
◇ R0	U_S
R1	0.224
R2	$\bar{X}_0$
◇ R3	$\bar{X}$
◇ R4	s
◇ R5	P_A
⬡ R6	$\bar{X}_c$
⬡ R7	CF
⬡ R8	U_T
⬡ R9	$\varepsilon(+)$
⬡ R.0	$\varepsilon(-)$
R.1	
R.2	
R.3	
R.4	
R.5	
R.6	
R.7	
R.8	

PROGRAM

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DYNAMIC DIFFERENTIAL PRESSURE MEASUREMENTS

BY
WILLIAM P. BRANDT
SENIOR SPECIALIST ENGINEER
THE BOEING COMPANY
SEATTLE, WASHINGTON

INTRODUCTION

Advances in Technology have resulted in new measurement capabilities. Dynamic differential pressure measurement is one of the techniques recently incorporated in wind tunnel testing at The Boeing Commercial Aircraft Company.

A dynamic differential pressure measurement, as applied to a wind tunnel airfoil model, is that fluctuating pressure difference which occurs between the upper and lower surfaces at the same airfoil chord location. These pressure fluctuations are usually random in nature and can have a bandwidth up to 3 KHz. Their amplitude can vary from a few thousandths of a psi to several psi, depending on the test conditions imposed on the model.

The conditions often encountered during a transonic wind tunnel test cover the range of velocities from 100 ft/sec to near the speed of sound. At pre-selected velocities, the model is pitched through an angle-of-attack series, and dynamic data recorded at each angle.

Dynamic differential pressure measurements obtained from various locations over an airfoil surface can be combined for use in estimating dynamic loading characteristics. The primary use of this technique is in support of dynamic loads studies, although buffet measurements are also an important use of this measurement technique.

The 747/Space Shuttle Program is an example of a familiar project which made extensive use of this measurement technique during its wind tunnel testing phase. An ambitious study of the dynamic loads and buffet levels was conducted to help determine the compatibility of the Space Shuttle Vehicle with the 747 Transport Aircraft. The dynamic information obtained resulted in a significant impact on the Program.

Transducer Selection

Miniature diffused silicon pressure transducers were selected to make this measurement for a number of reasons:

1. Wind tunnel testing, almost without exception, requires a transducer which is rugged, small in size, low in sensitivity to vibration, high in sensitivity to pressure, and stable with temperature.
2. Flat frequency response over the measurement spectrum, as well as the usual high quality transducer specifications, are also required.

Diffused silicon pressure sensors, supplied by at least two transducer manufacturers, fill these requirements very nicely.

Thanks to the cooperation of the transducer manufacturers, the most critical item, output sensitivity, has been increased by a factor of five over the nominal published sensitivities. The increase in sensitivity required the modifying of two of the transducer specifications: first, the linear operating range was reduced by a factor of three and, second, the zero shift specification was substantially relaxed.

These specification reductions have negligible effect on dynamic pressure measurements, due to the fact that the pressure fluctuations rarely exceed ± 3 psi, and the transducer output is A.C. coupled to its recording system. This increase in sensitivity, however, has dramatically enlarged the scope of dynamic pressure measurements.

The reliability and ruggedness of the present day miniature diffused silicon pressure transducers are greatly improved. Fitted with a wind screen, they are as durable as most larger steady-state pressure transducers. In recent years, most of our miniature transducers have been used on test after test without deterioration of measurement characteristics. This is an important factor when considering the cost of wind tunnel down time.

Calibrations

Steady-state pressure calibrations are performed (pre and post test) on all transducers to determine their sensitivity, linearity, hysteresis, and temperature characteristics. Dynamic calibrations are usually not conducted on all transducers, as the dynamic performance changes little from transducer to transducer. Frequency response characteristics are determined from a representative sample of each transducer type. Infinite tube comparison techniques, as well as acoustic standards, are employed for stimulation of the transducer during this determination.

Laboratory calibrations are transferred to the field using the transducers' excitation voltage as a transfer standard. This requires an accurate knowledge of the excitation voltage at the transducer and is usually accomplished by remote sensing techniques. To assure the predicted millivolt sensitivity, the excitation voltage of each pressure transducer is carefully checked and readjusted if necessary.

Test Installations

Two different techniques have been successfully used for this measurement. The most popular method takes the output difference from two flush-mounted pressure transducers to produce the dynamic Δ P measurement. The other method produces a direct measurement by connecting a miniature Δ P transducer between the two measurement orifices.

When choosing a transducer configuration for this measurement, a number of potential obstacles must be considered:

1. If the transducer is not flush-mounted, any additional tubing will alter the system frequency response.
2. Any electrical or pneumatic phase distortion between transducers may jeopardize the ΔP measurement and make time correlation studies between transducers impossible.
3. The transducer must also be structurally isolated in areas of high stress; otherwise, the transducer will react to the local stresses transmitted from the model.

The usual configuration which best satisfies these restrictions, if room permits, is the difference of two flush-mounted pressure transducers. Restricted areas, such as wing-trailing edges, often require the use of the double tube ΔP transducer, with flexible tubing attached to the upper and lower measurement orifices. This configuration may be frequency-limited below the desired corner frequency.

Data Applications

The measurement of buffet and flow separation by recording the dynamic ΔP RMS level is the most straightforward use of this parameter. Some of the more exotic uses of this measurement include the determination of disturbance size through auto and cross correlation techniques and, of course, the determination of dynamic loads estimates.

CONCLUSION

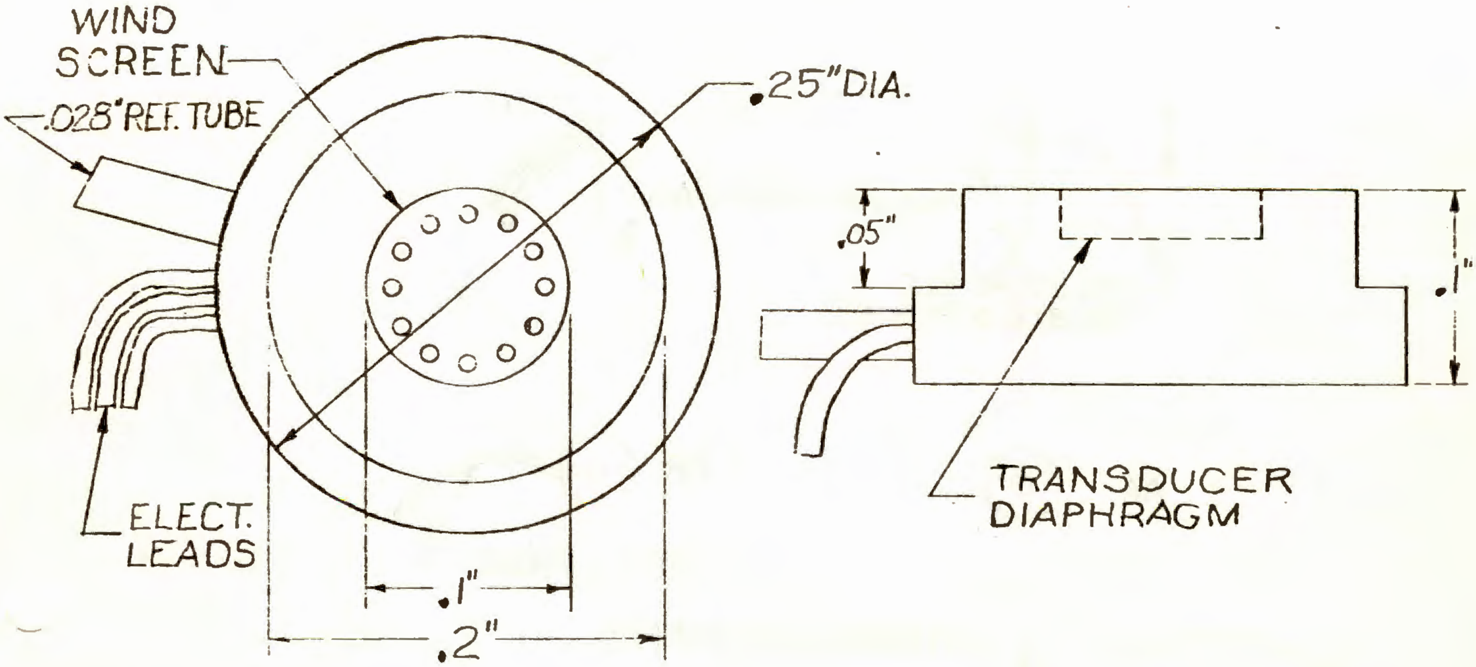
Although there are still some problems remaining with dynamic ΔP measurements, meaningful information can be obtained for most wind tunnel test configurations, and indications show that future application for this type of measurement is on the increase.

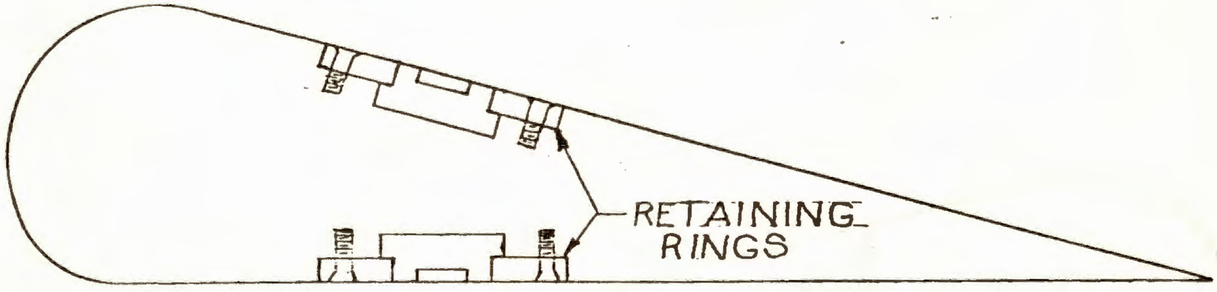
ACKNOWLEDGEMENT

The author wishes to thank P. E. Connell and A. E. Davis for their able assistance in evaluation of the dynamic ΔP pressure transducers.

Particular thanks, also, are due to T. D. Cohen and D.P.B. Larsen for their technical consultation in support of this Paper.

HAT SECTION DYNAMIC PRESSURE TRANSDUCER

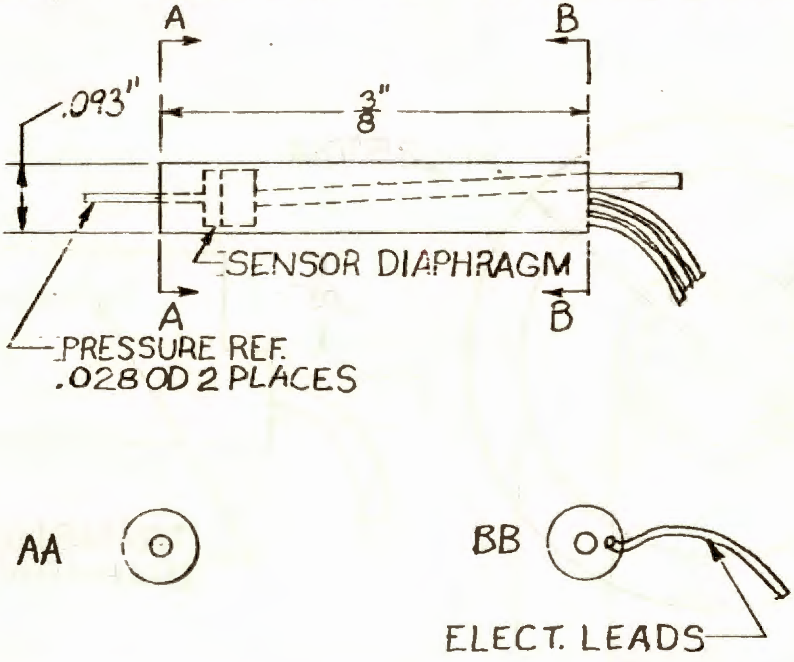


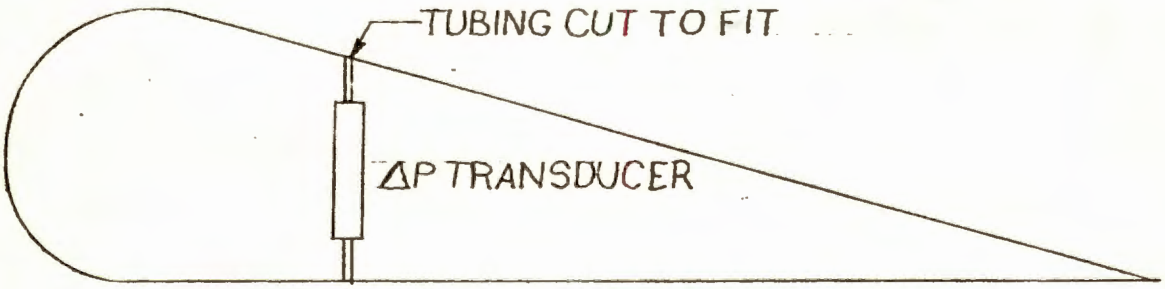


AIRFOIL SECTION

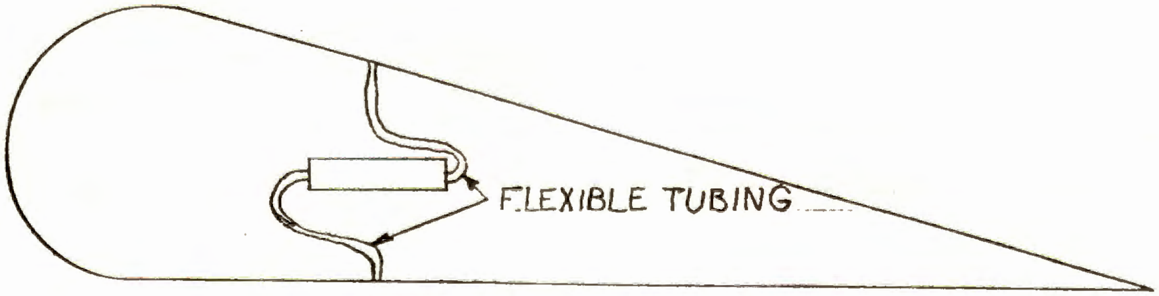
FIGURE 1

DOUBLE TUBE DYNAMIC DIFFERENTIAL (ΔP) PRESSURE TRANSDUCER





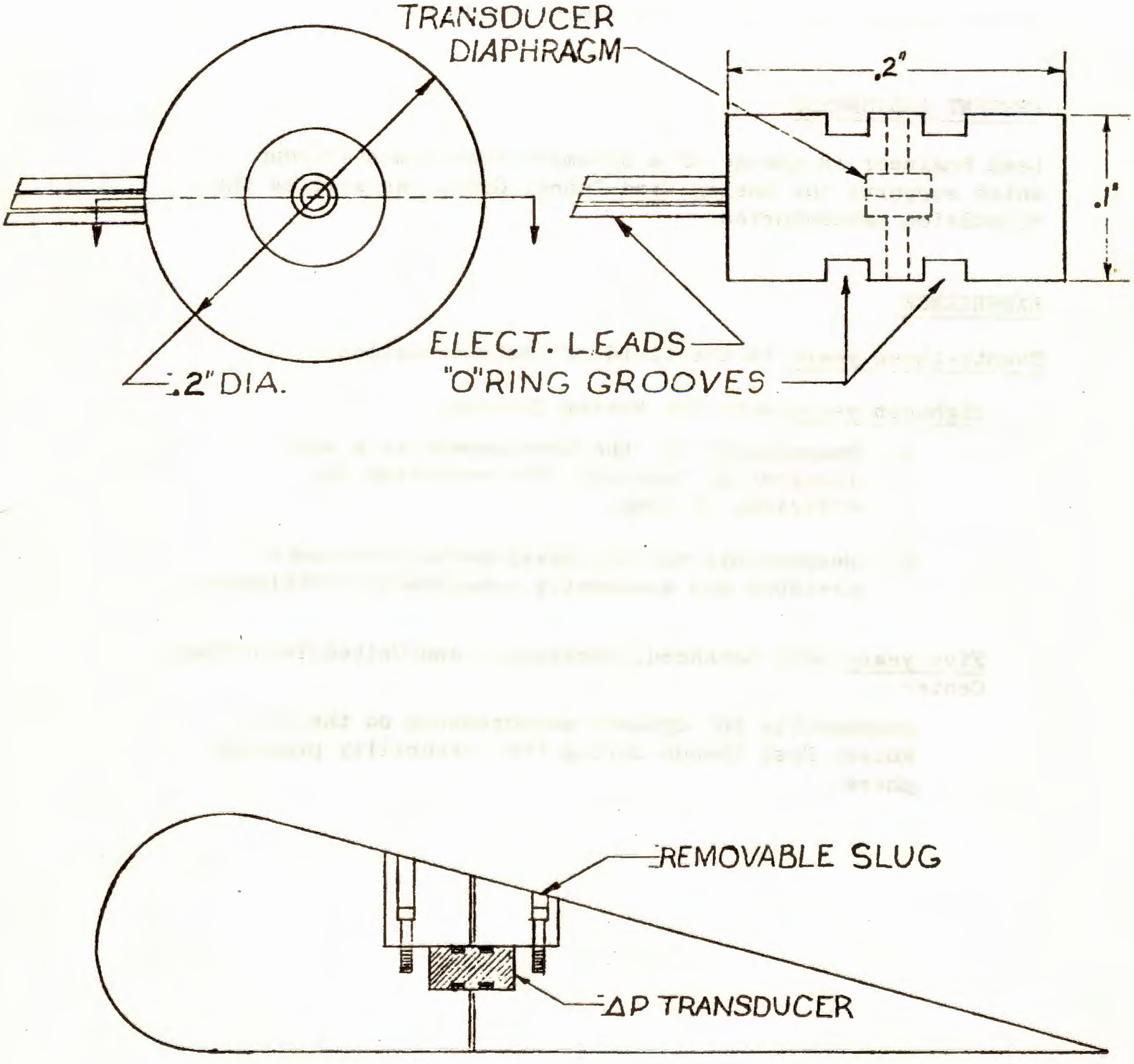
AIRFOIL SECTION



AIRFOIL SECTION

FIGURE 2

DYNAMIC DIFFERENTIAL (ΔP) PRESSURE TRANSDUCER



AIRFOIL SECTION

FIGURE 3

BIOGRAPHICAL DATA ON W. P. BRANDT

PRESENT TITLE

Senior Design Specialist (Instrumentation)

PRESENT ASSIGNMENT

Lead Engineer in charge of a Dynamics Measurements Group which supports the Boeing Wind Tunnel Group, as well as the Propulsion Laboratories.

EXPERIENCE

Twenty-three years in the field of instrumentation:

Eighteen years with The Boeing Company -

1. Responsible for the development of a wake integration technique for measuring coefficient of drag
2. Responsible for the development of dynamic pressure and anemometry measurement techniques.

Five years with Lockheed, Rockedyne, and United Technology Center -

Responsible for dynamic measurements on the F-1 Rocket Test Stands during the instability problem phase.

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THEY KNOW IT ALL
IRRITATE THOSE OF US WHO DO

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Lawrence Livermore Laboratory

STRESS ANALYSIS AND MATERIAL CERTIFICATION FOR MECHANICALLY CRITICAL PRESSURE TRANSDUCERS

M. J. Burger and D. C. Holten

MARCH 1977

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STRESS ANALYSIS AND MATERIAL CERTIFICATION FOR MECHANICALLY CRITICAL PRESSURE TRANSDUCERS*

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Livermore, California 94550

INTRODUCTION

The mechanical reliability of a pressure transducer becomes of paramount importance under certain use conditions, such as a high stress level, or a toxic, corrosive, or embrittling environment. In this type of application, the potential for total loss of a costly experiment through mechanical failure tends to override other considerations, such as linearity, zero shift, and temperature compensation. This paper discusses the application of stress analysis methods to pressure transducer design and of material certification to their manufacture. These two tools appreciably increase the transducer's mechanical reliability.

PRESSURE BODY MATERIALS

The pressure transducers discussed are representative of a larger family of like instruments. These are widely used by one engineering group at Lawrence Livermore Laboratory. The application is in a field environment where the highest possible mechanical integrity is a routine requirement. These transducers have self-contained signal conditioning; excitation is 28 V DC; full scale output is 5 V DC. They are used at LLL in nine different pressure ranges — from 0 to 50 psia up to 0 to 20,000 psia. Though not routinely available, all are commercially manufactured. Figure 1 shows two members of this family. The pressure chamber or pressure body, is contained within the threaded region.

Two different pressure body materials will be discussed in this study. Berylco 33-25 is a nominal 2% beryllium in copper alloy; it contains a small percentage of lead to give good free machining characteristics. Pressure bodies are machined from solution annealed round stock. A post machining heat treat is required to give final material properties. Berylco 33-25 is obtained under the Federal Specification QQ-C-530C. 21(Cr)-6(Ni)-9(Mn) is a corrosion resistant steel manufactured by vacuum induction melting, followed by electro-slag remelting. Pressure bodies are machined from 1-1/2 in. diameter blanks, that have previously been high energy rate forged (HERF) to the desired physical properties. No post machining heat treat is required. This material is obtained under LLL specification MEL 72-001204.

This work was performed under the auspices of the US Energy Research & Development Administration under Contract No. W-7405-Eng-48.

Figure 1. Absolute Pressure Transducers with Self-Contained Signal Conditioning Electronics.

STRESS ANALYSIS

Pressure transducers utilize two fundamental quantities for the measurement of pressure: strain and displacement. These quantities produce a linear relationship with pressure for a transducer body which remains linearly elastic through its range of application. This proportionality constant can be very accurately calculated for any part of the most complex transducer body using the Finite Element Method. This analysis method yields strains, displacements, and stresses which can be used for failure criteria considerations. An extensive parameter study of various transducer configurations can be made in a matter of hours for a very minimal cost and will give a very precise transducer calibration factor. This is contrasted with the conventional fabrication and testing technique which is time consuming and costly.

The two transducers of this study had slightly different body geometries. Both had 0 to 20,000 psi ranges; one pressure body was of Berylco, the other of 21-6-9. They were analyzed using the finite element method in the elastic operating range. The analysis can be carried into the post yielding region; however, only the elastic solution will be discussed here.

A typical transducer body cross section in Figure 2 shows the region that was analyzed by the finite element method.

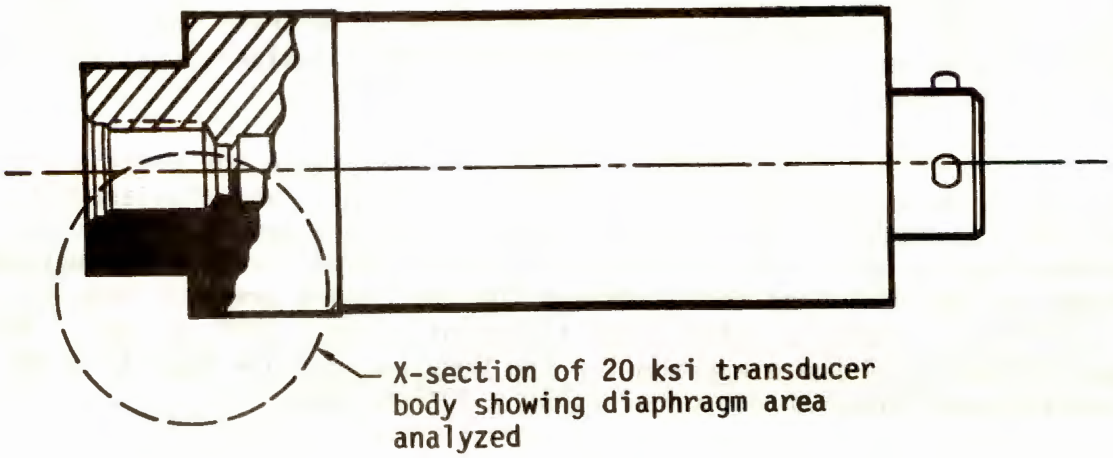
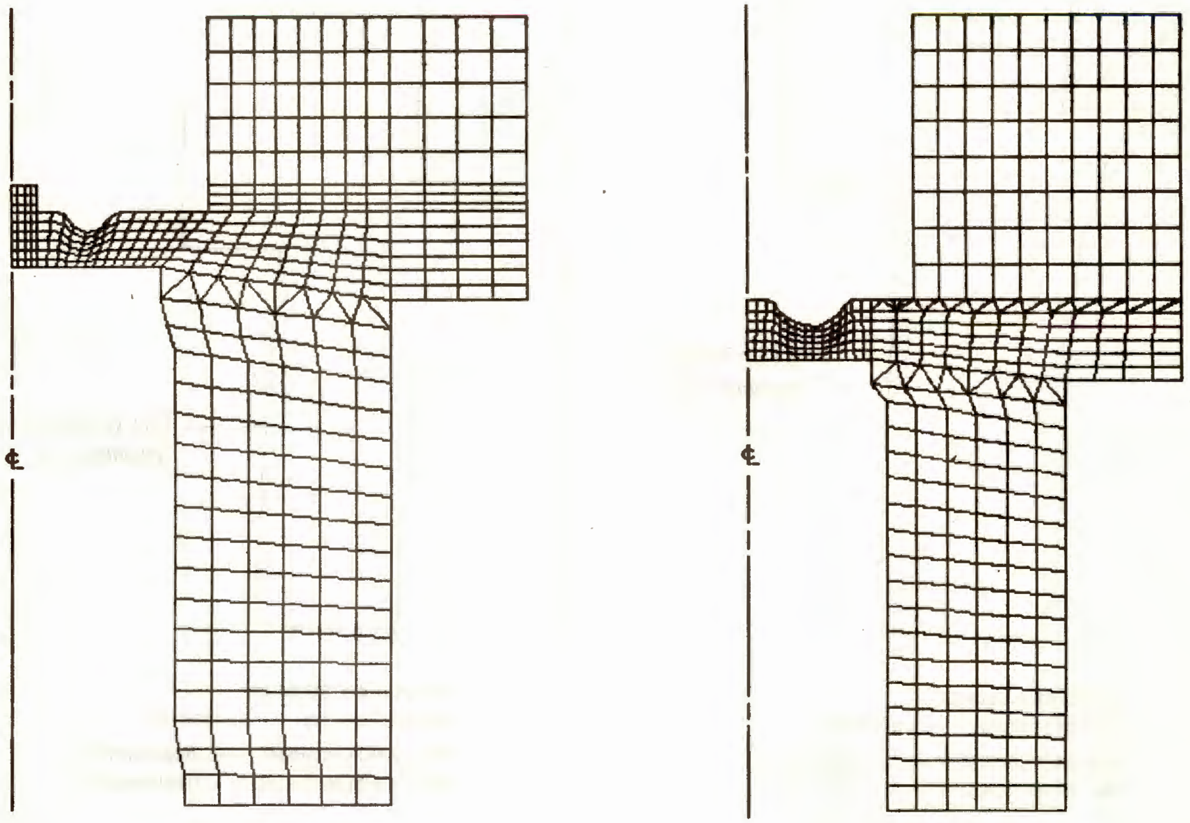


Figure 2. Typical Transducer Body Cross Section.



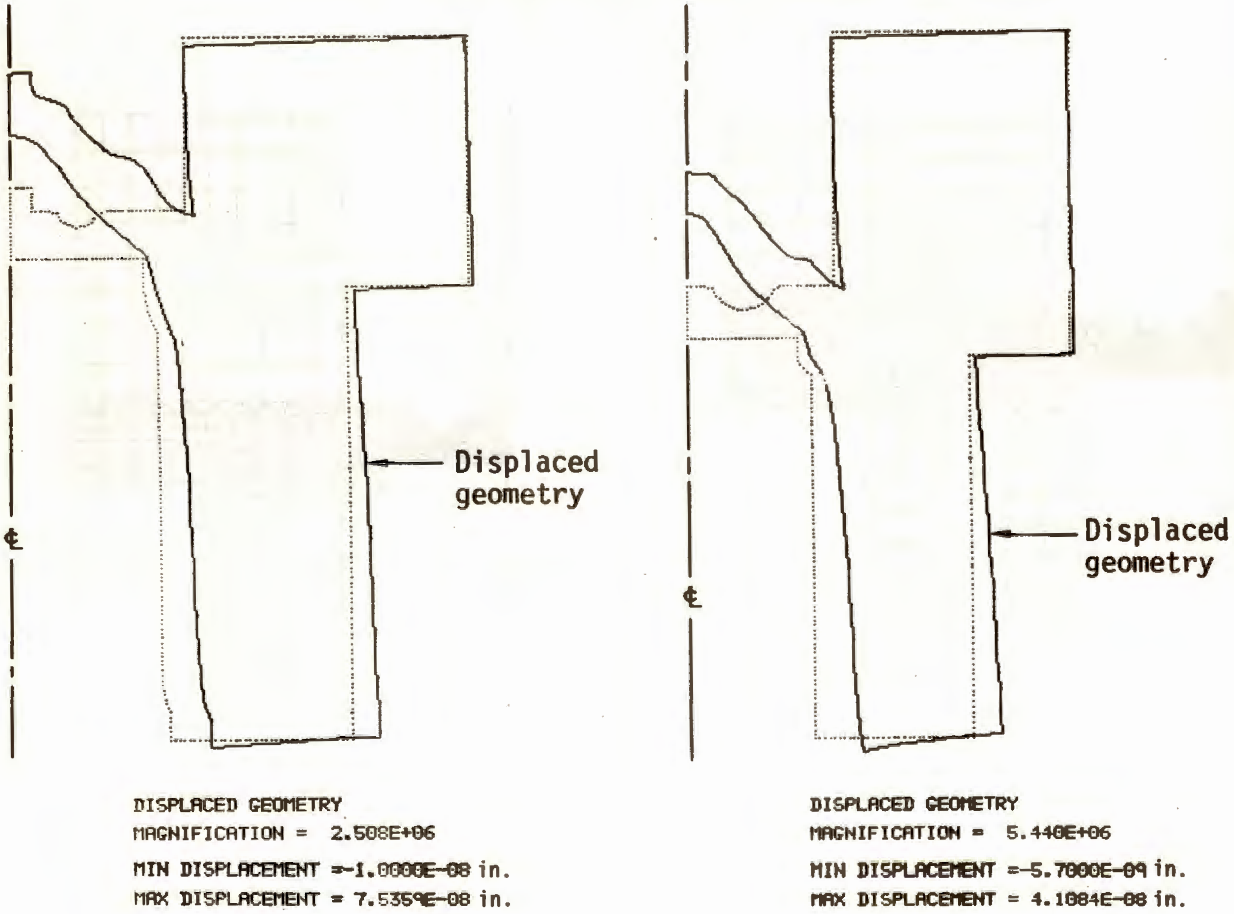
(a) Berylco 33-25

(b) 21-6-9

Figure 3. Finite Element Mesh Used in Analysis.

The finite element meshes used in the analysis are shown in Figures 3a and 3b. Only the cross sections to the right of the centerline will be shown because of axial symmetry. These geometries illustrate only two diaphragm designs of many that were analyzed. The total analysis cost for each design was less than $50.00 and took less than 2 hours.

A pressure of 1 psi was applied in both designs since the elastic solution allows us to scale all the results (strain, stress, and displacement) linearly with pressure. The displaced geometries are shown in Figures 4a and 4b magnified 2.502 x 10^6 and 5.44 x 10^6 times respectively. The maximum deflection of the center of the diaphragm for the 21-6-9 pressure body is 4.108 x 10^{-8} in., giving a calibration factor of 4.108 x 10^{-8} in./psi. The maximum deflection of the diaphragm for the Berylco pressure body is 7.536 x 10^{-8}, giving a calibration factor of 7.536 x 10^{-8} in./psi.

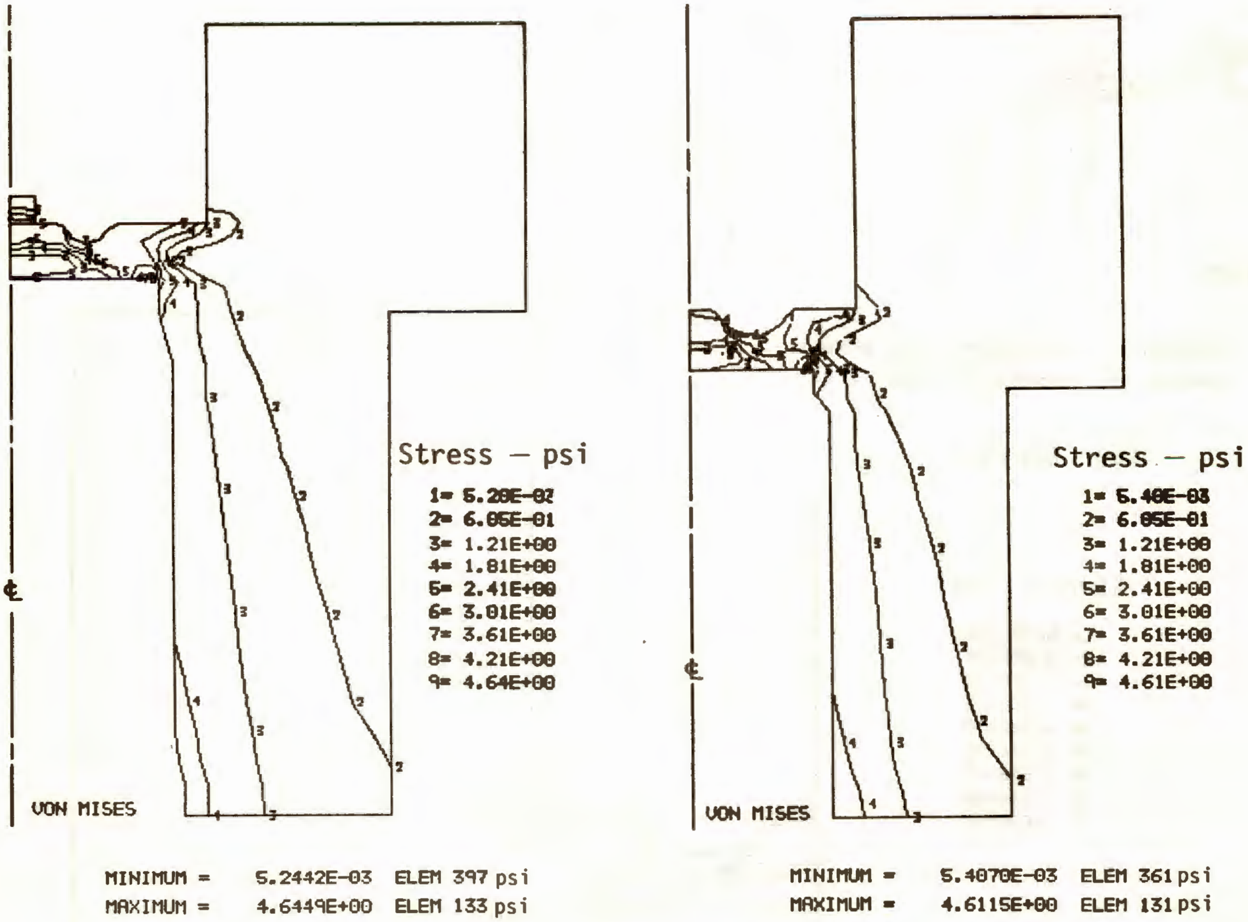


(a) Berylco 33-25

(b) 21-6-9

Figure 4. Displaced Geometry Due to a 1 psi Pressure.

The contour plots (lines of constant stress) of the von Mises stress component are shown in Figures 5a and 5b for a 1-psi pressure in both transducer bodies. A number of other stress components can be plotted, however, the von Mises stress best describes the deformation energy and is, therefore, used as yield criteria for a three dimensional stress state.

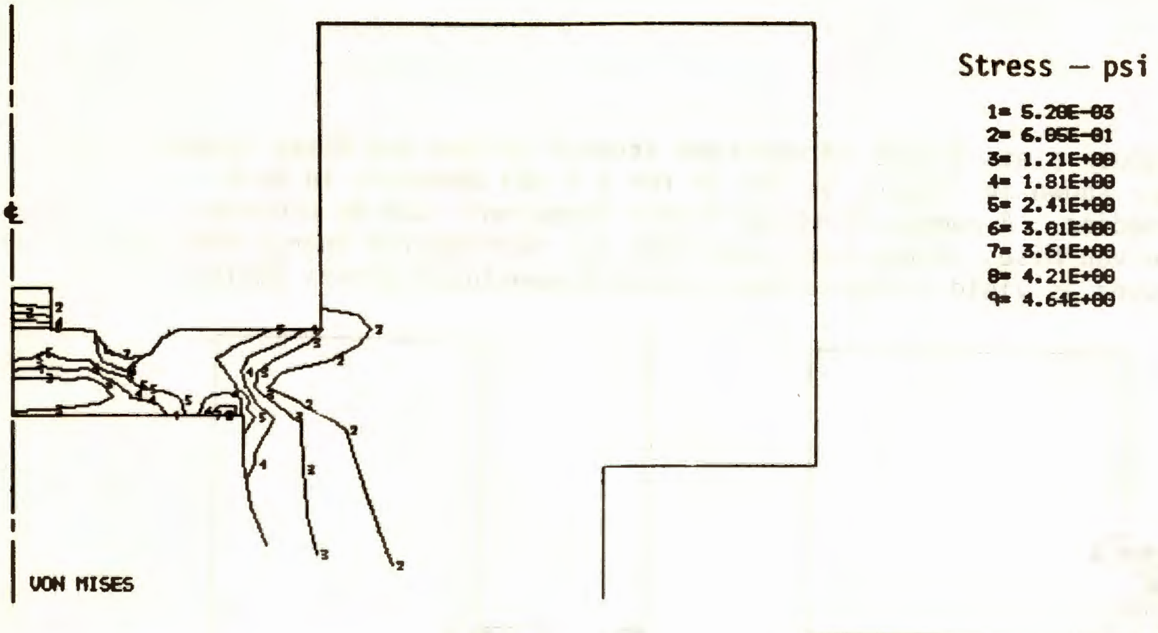


(a) Berylco 30-25

(b) 21-6-9

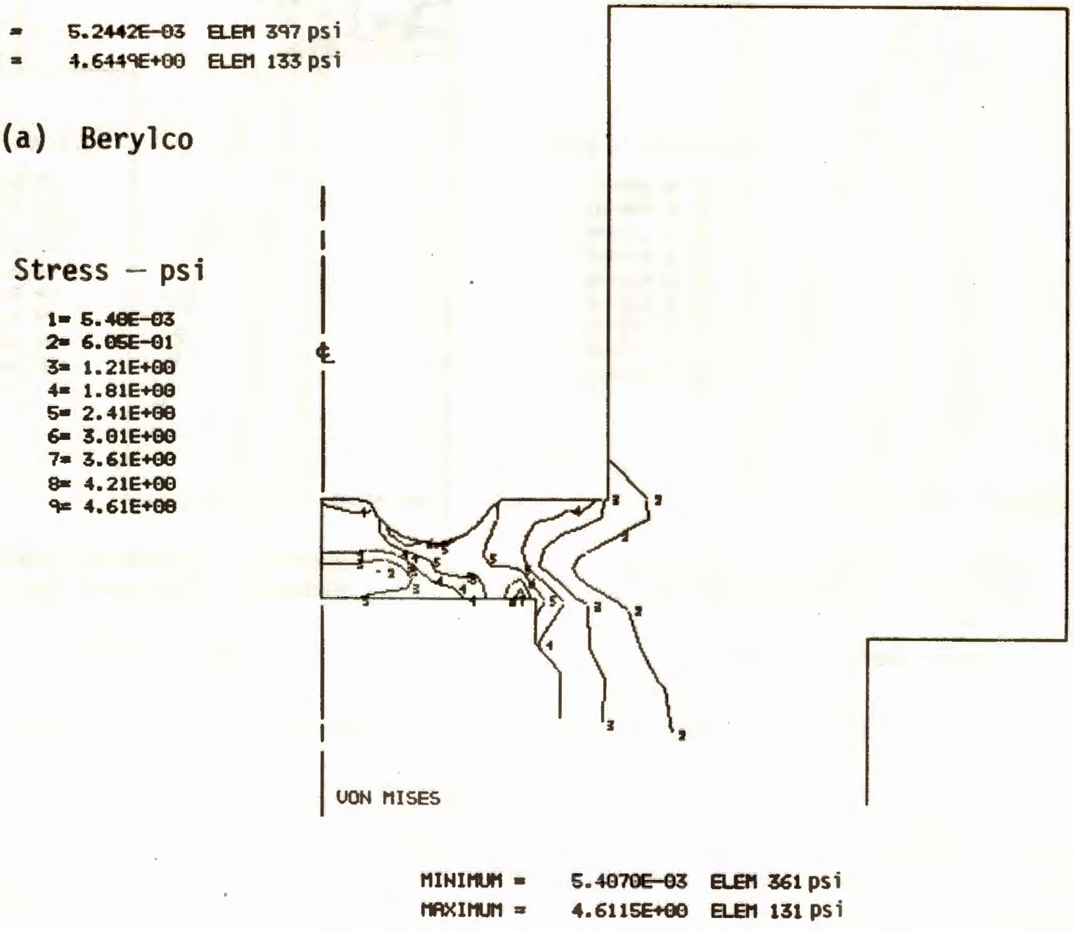
Figure 5. Von Mises Stress Contour Plots Due to a 1 psi Pressure.

From Figure 6, which shows the stress contours in greater detail, the average diaphragm cross section stress (contour line 5) is 2.4 psi in both bodies. This is a bending stress amplification of 2.4 or a nominal stress of 48,000 psi at a pressure of 20 Ksi. A stress concentration does develop at the diaphragm wall corner with an amplification of 4.2 psi stress per psi of applied pressure. At 20 Ksi pressure, this is 84 Ksi von Mises stress. This stress concentration can be used to indicate when the diaphragm begins to respond nonlinearly. When a significant region, say 5% of the diaphragm thickness in Figures 6a and 6b, has reached or exceeded the yield stress in the elastic analysis then nonlinear deflection of the diaphragm can be expected.



MINIMUM = 5.2442E-03 ELEM 397 psi
MAXIMUM = 4.6449E+00 ELEM 133 psi

(a) Berylco



(b) 21-6-9

Figure 6. Detail View of the Von Mises Stress Contours at the Diaphragm Due to a 1 psi Pressure.

MATERIAL CERTIFICATION

Though the specific material certifications for Berylco 33-25 and 21-6-9 differ slightly, both are concerned with these vital considerations:

1. Is the material provided to the manufacturer the same material from which the pressure body has been fabricated?

2. Are the mechanical properties of the finished pressure body established?

3. Is the rupture value of the finished pressure body known, at least on a lot basis?

The flow chart in Figure 7 illustrates the certification steps involved for each lot of material. For Berylco 33-25, parts cut from the same bar and given final heat treat at the same time (same oven) make up a lot. This frequently involves parts from several different pressure ranges. For 21-6-9, a lot is a parts group from the same heat and forging set-up. They need not be manufactured at the same time; they may also include multiple ranges.

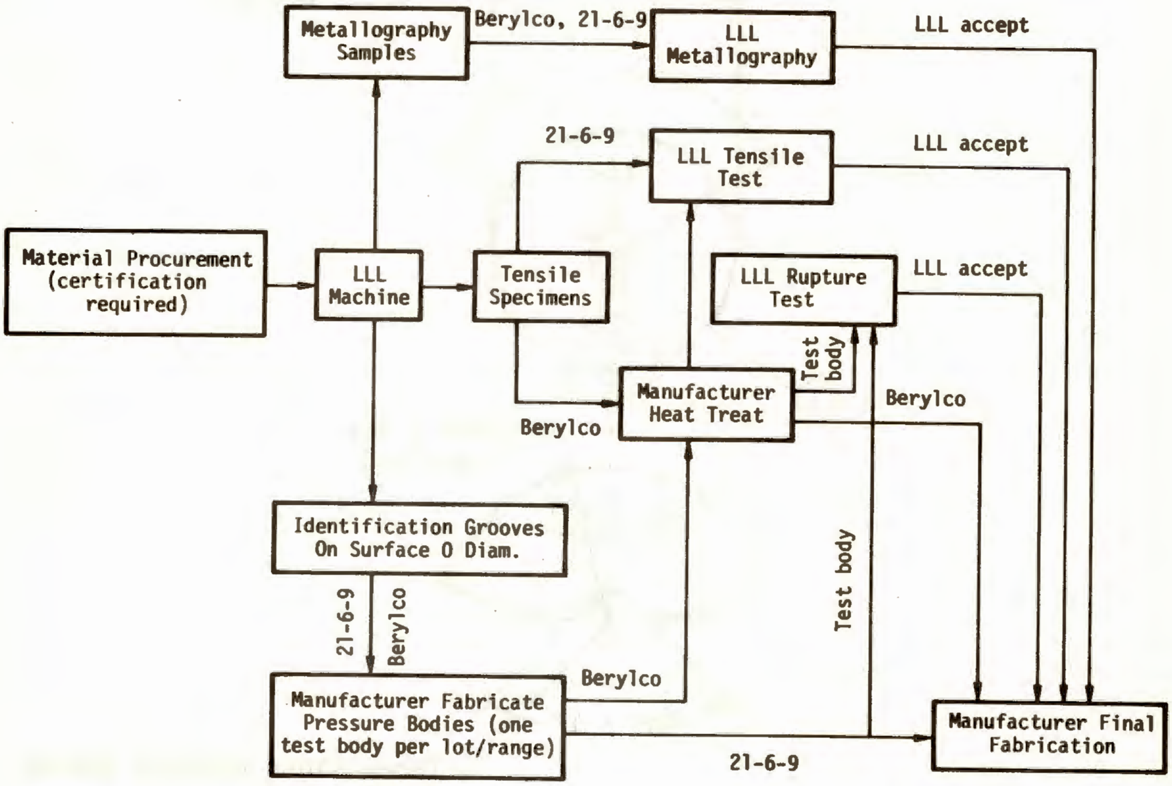


Figure 7. Material Certification Flow Chart.

Starting from the left in Figure 7, material is procured with complete supplier certification, as 1-1/2 in. round stock. Typical in-house machining is shown in Figures 8 and 9.

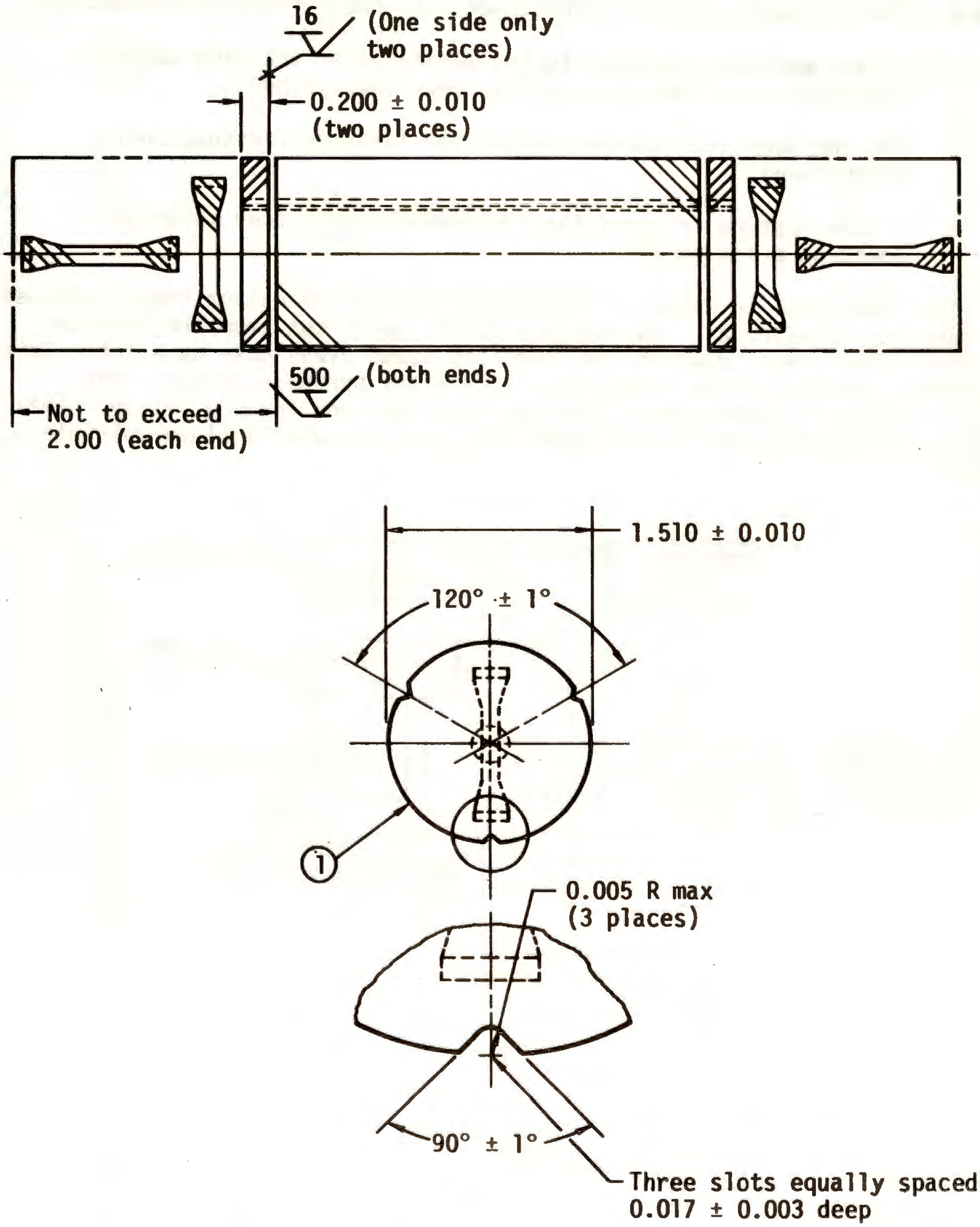


Figure 8. In-House Machining for Material Certification.

Notes:

1. Dimensioning and tolerances are per USASI-Y14.5
2. Positional and form tolerances apply R.F.S.
3. Surface texture per USAS-B46.1
4. 63/ except where noted
5. The reduced section shall be tapered from the ends to the center with the diameter of the ends 0.6 to 1.0% larger than the center
6. Polish reduced section to remove tool marks
7. Bag and tag with drawing number, tab and material identification

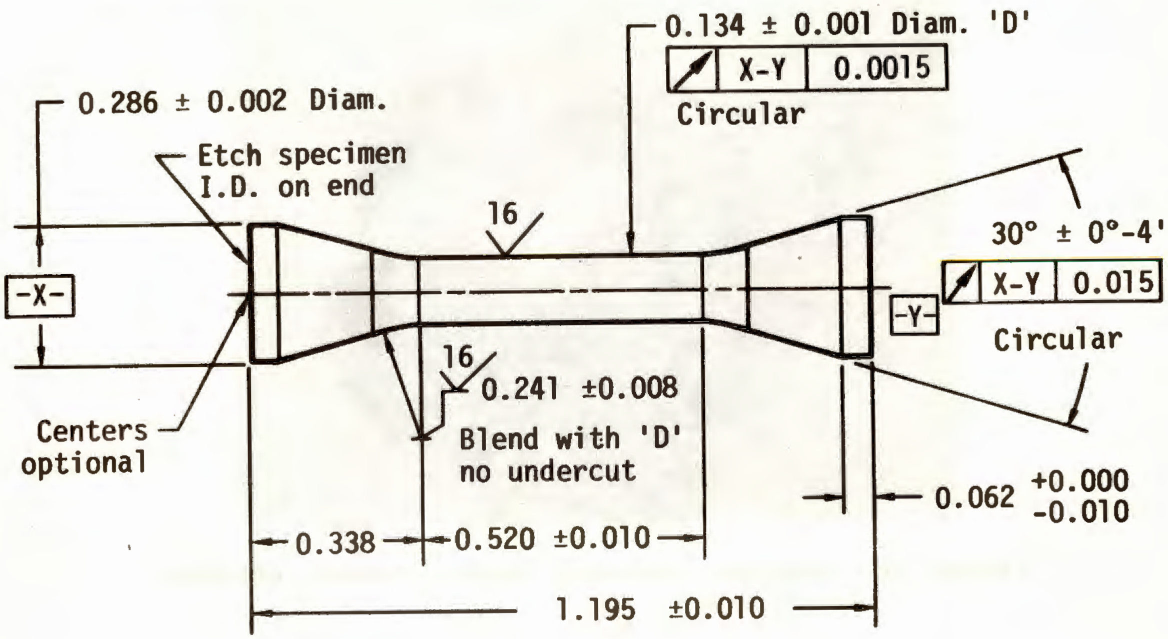


Figure 9. Micro Tensile Specimen.

These operations yield:

1. tensile specimens representative of both transverse and longitudinal mechanical properties,

2. cross section disks which when metallographically evaluated, will indicate the microstructure of the bar. With Berylco 33-25 a fine, homogeneous structure, without phase segregation or high oxide content, is desired. This insures good later response to the post machining heat treat. In the case of 21-6-9, the concern is a poor forging. This is indicated by microrecrystalization along grain boundaries, with serious effect upon the fracture toughness of the material, and

3. a bulk length of round stock containing three grooved surface slots. These are equally spaced around and extend the length of the bar. This outer cylindrical surface ends up as the maximum outer diameter of the finished pressure body. As such, the grooves provide a reasonable assurance of correct material identity. This detail is shown pictorially in Figure 10.

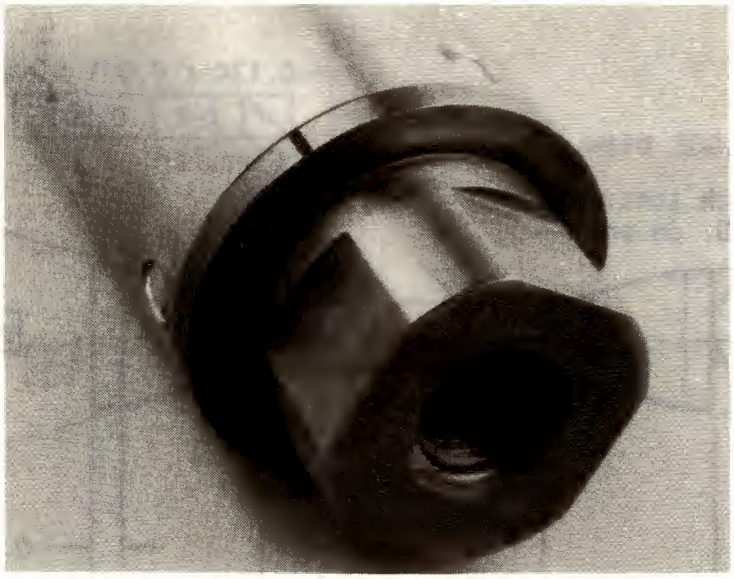


Figure 10. Pressure Body with Identification Grooves.

For each pressure range within each lot of material, the manufacturer is required to fabricate one extra pressure body. With Berylco, all parts of the lot-manufacturer produced pressure bodies, and LLL fabricated tensile specimens, are given the required post machining heat treat together. This operation is done by the manufacturer. Following this (or directly after machining for 21-6-9), the tensile specimens are tested, and the extra pressure bodies (representative of each pressure range within the lot) are ruptured. Both of these operations are done at LLL. When these tests establish that the materials have (1) satisfactory microstructure, (2) mechanical properties within specification, and (3) acceptable rupture values, LLL gives structural acceptance for all pressure bodies in the lot. The manufacturer is then free to proceed with final assembly of all units involved.

MECHANICAL PROPERTIES DATA

Table 1 gives the mechanical properties typical of these materials. No values were specified for 21-6-9 following the HERF process. There are significant property differences between transverse and longitudinal stress values. Hence, a "worst condition" stress analysis should use minimum values.

Material	Specification		Measured Mechanical Properties				Pressure Body Rupture (measured) ksi
	Ultimate Strength ksi	Elongation	Ultimate Strength ksi		Elongation %		
			Longitudinal	Transverse	Longitudinal	Transverse	
Berylco 33-25 (condition AT)	$\frac{165}{190}$	3% min	171	165	9	4	40.8 40.0
21-6-9 (HERF)			156	139	31	29	56.0 (O-ring blowout)

Table 1. Mechanical Properties Tabulation for Pressure Body Materials.

SUMMARY

The finite element method can be used very efficiently to help in the design and calibration of pressure transducer bodies. For a minimal cost, a complete series of parameter studies can be done which will quickly define the elastic properties of any pressure transducer body. These parameters, including diaphragm deflection, stress state, and strain, can be determined for the complete elastic operating range by simply scaling the unit pressure values to the operating pressure. Calibration constants may be very precisely determined from diaphragm deflection data. Indications of the onset of diaphragm nonlinear response can be obtained from the stress concentrations of the von Mises stress component. Post yielding and rupture analysis is also available using the finite element method, but with less precision and increased cost.

A detailed material certification program can lengthen the procurement of usable pressure transducers. Four actions will shorten this lead time:

1. The user maintains a supply of previously certified pressure body material.
2. The user maintains a larger pressure transducer inventory.
3. The user and the manufacturer efficiently move parts to one another.
4. User "in-house" operations (i.e., metallography, testing) are quickly accomplished.

These actions may be viewed as penalties. They should rather be weighed against the benefit of such a program, and whether the pressure transducer application requires this benefit — in this case, very high mechanical integrity in an environment totally intolerant of structural failure.

There has been
an alarming increase
in the number of things
I know nothing about

LOW AIRSPEED SYSTEMS

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ABSTRACT

The purpose of this report is to provide an overview of low airspeed transducers which have been evaluated by NATC and other organizations. By using a variety of techniques other than differential pitot static pressure, these transducers overcome the low speed threshold problems of the traditional aircraft airspeed system. These transducers measure either unidirectional or omnidirectional airspeed, and have been installed in both helicopters and VSTOL aircraft. Omnidirectional systems provide outputs resolved into longitudinal and lateral components.

Various LORAS systems have been evaluated. This is an omnidirectional system which derives airspeed by measuring differential pressure between the tips of two rotating arms.

J-TEC systems, which measure airspeed by counting vortices shed behind a strut in an airflow, are available in either unidirectional or omnidirectional models. These systems have no moving parts.

The LASSIE system is a swivelling pitot static tube which measures downwash angle, which can then be related to omnidirectional airspeed. This technique limits the LASSIE system to helicopter installations.

The Rosemount Orthogonal Airspeed Sensor measures differential pressures on a strut mounted along the aircraft vertical axis. These pressures are proportional to longitudinal and lateral airspeed.

Various other types of systems are discussed briefly.

It is desirable that a calibration, usually in a wind tunnel, precede the aircraft installation. The aircraft installation must then be evaluated for position error. The search for a satisfactory mounting location is time consuming. It often requires extensive aircraft modifications, such as a standpipe installation, and optimization of the transducer design for each aircraft type.

There are no ideal low airspeed systems available today. Each system has distinct advantages and disadvantages in transducer design and ease of installation.

INTRODUCTION

The purpose of this report is to provide an overview of the field of low airspeed measurement systems. A number of systems which have been evaluated by various organizations will be described in terms of theory of operation, installation requirements and resulting problems, evaluation results, and state of development. The development history of one system will be given as an example of the development process from original concept to the marketing of a limited production system. This will help place the growing pains of other systems in the proper perspective.

DISCUSSION

a. General Requirements and Uses

There exists in the helicopter and VSTOL communities a requirement for an operational quality low range omnidirectional airspeed system.

Helicopters and VSTOL aircraft are capable of operating at low airspeeds, down to a low wind hover and in most cases rearward flight. Conventional pitot-static airspeed systems do not operate satisfactorily at either low airspeeds or large angles of sideslip. The small magnitude of impact pressure at low airspeeds (i.e., .0023 psi at 10 kts; .0094 psi at 20 kts) makes any mechanical friction in a transducer or airspeed indicator a significant source of error. Static source errors due to sideslip, downwash at low speeds, and in ground effect (IGE) ambient pressure fluctuations, contribute to the overall problem. Another problem with standard airspeed systems is that they utilize pitot tubes aligned parallel to the centerline of the aircraft. The pitot tube will operate satisfactorily only at relatively small angles of sideslip and angles of attack. These characteristics permit the standard pitot-static system to operate in only a portion of the helicopter or VSTOL flight envelope.

Knowledge of lateral airspeed is needed during cross wind landings and especially during shipboard approach work. Helicopter flight envelopes specify lateral airspeed limits, however, the pilot has no indication of his lateral airspeed component. Helicopter operations manuals specify a permissible wind envelope for the engagement or disengagement of the rotor blades. Helicopter IFR operations are restricted by the lack of any low airspeed indication. Two, and in same cases, three dimensional airspeed inputs are needed for fire control systems. This particular requirement has been the driving force behind much of the testing performed by the Army. VSTOL aircraft experience problems similar to those of the helicopter, as well as low speed controlability problems and exhaust gas reingestion.

Aside from the operational requirements, there is a need for low airspeed systems in flight test work. This need led to the initial involvement of the Technical Support Directorate (TSD) of the Naval Air Test Center (NATC) in this field. The U.S. Naval Test Pilot School (USNTPS) at Patuxent River, Maryland is currently examining low airspeed systems as an instructional tool.

There exist systems in various stages of development today which measure low airspeed in one, two, or three axes. System outputs can drive various cockpit indicators including HUDS. The outputs are also available for fire control systems, automatic flight control systems, navigation systems, and telemetry. The advanced state of avionics design allows for almost unlimited output options. Analog or digital processing of the basic transducer output can result in airspeed information presented in various combinations of axial components, total flight path airspeed, or angle of attack (α) and sideslip (β). Computation of indicated airspeed or true airspeed is possible. Signal processing to eliminate position error will be a necessity in any operational system. Miniaturization will allow the signal conditioning to be reduced to an acceptable size.

Low airspeed systems have undergone qualitative, quantitave, and operational evaluations on a variety of helicopters and VSTOL aircraft. This is a rapidly expanding field with an increasing number of manufacturers developing equipment.

b. Types of Systems

(1) LORAS (Figure 1)

The Low Range Airspeed System (LORAS) is manufactured by Pacer Systems, Inc. of Burlington, Massachusetts. The LORAS system currently being marketed (LORAS 1000) is the result of a long development program. LORAS hardware has undergone more evaluations than any other low airspeed system.

The main element of the LORAS system is the rotating sensor assembly, which is commonly referred to as the LORAS head (Figure 2). This unit consists of a rotating arm assembly and a stationary base structure. The rotating portion of the head contains a differential pressure transducer mounted in a cylindrical hub, two hollow arms connected to the hub 180° apart, and two tubular shrouds mounted on the arm tips. The base contains a drive motor, slip rings, and a temperature sensor.

The basis of the LORAS concept is the measurement of instantaneous differential airspeed at the arm tips. Two pressure ports located on the arm tips, inside the shrouds, measure the pressure on the top and bottom of the arm. This pressure is proportional to the airflow across the tip. Any pressure difference between the two arms is measured by the differential pressure transducer. The magnitude and phase of the transducer output can therefore be related to the airspeed of the arm tips.

If the airspeed of the head is zero, the only airspeed at the tips is due to the rotation of the arms and the differential airspeed is zero. The rotational tip speed of current LORAS units is approximately 22 kts. If the head is experiencing an airspeed, the advancing tip will have a greater airspeed than the retreating tip. The magnitude and phase of this differential airspeed can be related to the magnitude and direction of the airspeed of the head. Using a resolver and appropriate signal conditioning, this information can be presented as longitudinal and lateral airspeed components. It is also possible with this information to derive the angle of sideslip (β). If the head is mounted in an unobstructed location, airspeed can be measured from any direction in the plane of rotation.

The airspeed information can be displayed as indicated airspeed or true airspeed. In LORAS II, this display option is selected by a switch located on the cockpit control panel. Density compensation is accomplished in the electronics unit. Temperature information is obtained with a thermister located on the base portion of the rotating head assembly. Static pressure is sensed with a potentiometer type absolute pressure transducer located in the electronics unit.

The original LORAS was a result of research conducted by Cornell Aeronautical Laboratory, Inc. of Buffalo, New York, beginning in 1951. The original LORAS was intended to provide inputs to the variable stability system installed in the X-22A VSTOL research aircraft.

The first design used rotating pitot tubes to sense tip speed, and a differential pressure transducer mounted in the hub to obtain the required differential airspeed information. The diameter of the arms was 2.26 feet long and was driven at 1560 rpm, resulting in a tip speed of 110 knots. Tests showed that the pitot tube design was very susceptible to icing. Another disadvantage of the pitot tube was that the airspeed being measured could not be greater than the tip speed.

The next design used symmetrical venturi tubes in place of the pitot tubes. This eliminated the icing problem. The use of venturi tubes also permitted, in theory, the measurement of airspeeds greater than the tip speed. This configuration was tested on a station wagon at Buffalo prior to installation on the X-22A in March of 1966. It was installed on the vertical stabilizer of the X-22A. A LORAS of this type was also installed on a P-1127 (XV-6A Harrier prototype) in June of 1967. A flight calibration of this system was conducted at Edwards AFB on June 30, 1967. This system was installed on top of the fuselage aft of the trailing edge wing root.

In October of 1971, LORAS Instruments, Inc. of Lancaster, New York announced the development of LORAS II. This system was based on the same concepts as the original LORAS, however, there were several significant physical differences. The rotor diameter was reduced to 14 inches, to minimize the area of required constant pressure about the rotor tips. By reducing the rotor diameter, the required power of the motor could be reduced. This decreased the size and weight of the LORAS head assembly. The electronics unit was also redesigned with a corresponding reduction in size.

LORAS II is now manufactured by Pacer Systems, Inc. of Burlington, Massachusetts. This system has a rotor diameter of 13 inches. A LORAS II is mounted on a nose boom on the X-22A. It is oriented with the plane of rotation in the YZ plane. Mounted in this fashion the system measures the lateral and vertical components of airspeed.

In November 1972, in conjunction with the AH-1G/AH-1J Attack Helicopter Lateral Maneuvering Evaluation (Airtask A510-5104/053-2/3255-000-110), a LORAS II system was installed at NATC in an AH-1J (BuNo 157794). This system was installed by the Technical Support Division in cooperation with Pacer Systems' representatives. The test program was conducted by the Rotary Wing Branch of Flight Test Division.

A cockpit display of lateral airspeed was necessary because this aircraft was to be operated near its lateral airspeed limits. The first location attempted was on a nose boom (Figure 3). The center line of the LORAS head was 24 inches forward of the nose. This system was calibrated in flight using a pace truck equipped with a "fifth wheel" speedometer. A calibrated anemometer was used to determine ambient wind conditions. The data obtained from this flight calibration had enough scatter and unexplainable readings to justify trying another mounting location. This location was above the ADF antenna which is located on the pilot's canopy (Figure 4). The center line of the LORAS head was located at station 147.7. This location was chosen because it would not be affected by rotor tip shear and is also theoretically an area of minimum downwash. The system was calibrated in flight using the "fifth wheel"/anemometer technique. This location proved to be less satisfactory than the boom location. During these calibrations it was noted that the system outputs in ground effect were different than those for the same airspeed out of ground effect. At this point the decision was made to return the LORAS head to the boom, since this appeared to be the better of the two locations. No further calibrations were attempted because of time constraints. The remainder of the program was conducted at Hunter-Liggett Army Air Base, California, using the AH-1J with the nose boom mount and a similarly configured Army AH-1G. Upon completion of the program the AH-1J was returned to NATC. The LORAS system was removed from the aircraft before any further evaluations could be conducted. During the AH-1J tests, the mounting location was shown to affect the LORAS system output, however the exact cause of this problem was not isolated. System performance in ground effect was also not explained at that time.

Early in 1973 TSD began tests of two LORAS II units purchased for use at NATC. The intent was to perform a complete evaluation of the equipment under controlled conditions. This evaluation would result in base line data which could be used to explain position errors encountered during aircraft installation. This approach reflected the instrumentation background of the organization and was in contrast to evaluations performed by other organizations.

After correcting the initial problems usually associated with new equipment, a LORAS unit was installed in the ram air turbine test wind tunnel located at NATC. This tunnel was neither designed nor instrumented for use as a standard. It was intended to use this tunnel for functional checks of the LORAS system throughout its full advertised range (0-200 knots). This tunnel is an open circuit tunnel with no provisions for temperature control. The test section is a cylinder 6 feet long and 3 feet in diameter (Figure 5).

Initial tests showed that the longitudinal and magnitude outputs of the system appeared to saturate at approximately 140 knots input (Figures 6 & 7). The lateral output varied as a function of longitudinal input (Figure 8) and fluctuated as much as 10 knots at a given point. These problems were observed in both systems. The saturation problem was solved by the manufacturer with a design change in the electronics unit. The cross-axis sensitivity and fluctuation problems remained.

It was obvious that the two units purchased by NATC had not been tested to the full advertised airspeed range (200 knots) prior to delivery. Conversations with representatives of the manufacturer revealed that the units purchased by NATC had been adjusted using a truck as the airspeed source. This method was obviously limited to lower speeds.

The problems of cross-axis sensitivity and lateral fluctuations were not solved during this first series of wind tunnel tests. The severity and characteristics of these problems could be varied but not eliminated by adjustments to the resolver or to the various signal conditioning cards. The problems appeared to be centered around the design of the tip shrouds.

Using the NATC wind tunnel as an airspeed source, representatives of the manufacturer and TSD personnel evaluated a series of modifications to the LORAS head. During this period, 10 head configurations were tested, with varying results. Data was recorded with a two-channel brush recorder since the output fluctuations frequently made the cockpit indicators unusable.

The magnitude of the lateral fluctuations and the cross-axis sensitivity could be varied by changing arm length or shroud design, however these problems could not be eliminated. The plots of cross-axis sensitivity all had the same characteristic shape. The zero crossings could be shifted by adjustments to the resolver but the shape of the curves remained the same. The magnitude of the fluctuations increased with longitudinal input in all designs.

During this series of tests, the opinion was offered that the fluctuations were caused by turbulence or pressure gradients in the wind tunnel and that the cross-axis sensitivity was caused by swirl in the tunnel. Since the NATC wind tunnel had never been presented as a precision airspeed source, this argument could not easily be discounted. It was decided to test the most successful design in another wind tunnel. This design used 12-inch arms, 1.2-inch shrouds with a knife edge baffle installed in the shroud ends.

A LORAS system with these modifications was tested in the wind tunnel located at Harvard University. This tunnel is a closed circuit type with provisions for cooling. The test section is 5 feet long, 30.5 inches by 39.5 inches with filled corners. The results were not in exact agreement with the NATC wind tunnel data, however the trends were similar, even though there were substantial design differences between the two tunnels.

The calibration data for the lateral channel indicated that the longitudinal channel varied as a function of lateral input. The manufacturer suggested that the problems experienced were caused by the wind tunnel environment and would not be seen in "free air." At this point, the two NATC systems were modified with 1.2-inch shrouds with baffles and returned to NATC (Figure 9).

To verify that the accuracy and turbulent characteristics of both the Harvard and NATC tunnels were not adversely affecting the LORAS test results, it was decided to run a final calibration of LORAS at the National Bureau of Standards (NBS) at Gaithersburg, Maryland. One system was calibrated in the 5- by 7-foot rectangular test section of the NBS dual test section wind tunnel (Figure 10). This tunnel was chosen because it has a large test section, it is capable of covering 75% of the advertised airspeed range of LORAS, and it was designed to provide a low turbulence airflow. The longitudinal turbulence component in this tunnel has been determined to be less than .04% of mean speed. Stagnation temperature and stagnation pressure were recorded and used to compute true airspeed. The resulting data had the same characteristics (fluctuations, cross-axis sensitivity) as previous data (Figure 11).

At this point the manufacturer offered to provide aircraft data to verify system performance. Data was collected on an Army AH-1G at Hunter-Liggett Army Air Base. The LORAS in this test was mounted on a nose boom. Beechcraft Baron, under contract to the manufacturer, was flown at NATC to demonstrate the system. This aircraft had also had a nose boom mounted LORAS (Figure 12). Neither of these one day tests disproved any of the wind tunnel data. Plans to install a LORAS on a VSTOL aircraft (the Canadian CL-84) were cancelled when the aircraft was lost in an accident. During the first half of 1973 the U.S. Army Aviation Systems Test Activity (USAASTA) at Edwards Air Force Base, was conducting flight tests of a LORAS system on a UH-1C helicopter. The LORAS was mounted in three different locations to determine position error. At the time, this project was unknown to the NATC personnel involved in the LORAS evaluation.

In the winter of 1973 a LORAS was installed on a chin boom of a NCH-46A operated by the Test Pilot School (TPS) (Figure 13). This helicopter has a magnetic tape instrumentation system capable of recording aircraft attitude and motion. It also has a variable stability system (VSS) used in the TPS curriculum. The ultimate intent was to use the LORAS outputs as inputs to the VSS. The chin boom location was not entirely successful. Later, data from the Army test programs was to demonstrate that the best location for a low airspeed system on a helicopter is over the rotor hub.

The unit not installed on the NCH-46A was returned to the manufacturer for what turned out to be a major modification. The tip shrouds were removed, leaving just the arms with the two pressure ports in each tip (Figures 14 & 15). The signal processing equipment had been completely redesigned. The system on the NCH-46A was returned for a similar modification.

Wind tunnel tests at NATC and NBS showed that the redesign had reduced the fluctuations, however the cross-axis sensitivity remained (Figure 16). During these tests it was discovered that variations in frequency of the AC input power severly affected the system outputs (Figures 17, 18 & 19). Since aircraft power is not always exactly 400 Hz, a static inverter had to be installed with the LORAS to provide the precise power required. One of the systems was shipped to VFW in Germany for use during the Joint Developmental Flight Test Program on the VAK-191B. This system was first mounted on the tail, however interference from the APU necessitated that the head be moved to a location on top of the fuselage even with the wings. This system provided useful information in low speed forward flight, but was limited in lateral flight due to flow around the wings.

The remaining system was installed over the rotor hub of a UH-1N operated by the Rotary Wing Aircraft Test Directorate at NATC. A standpipe provided by the manufacturer was used in this installation. The outputs were displayed on cockpit indicators. Later the LORAS outputs were connected to the Hovering Vehicle Versitile Automatic Control System (HOVVAC). This low airspeed information permitted landing approaches to a hover in IFR conditions. The LORAS II and the HOVVAC equipment were removed from the helicopter prior to its transfer to a rework facility. When the helicopter returned from rework it was configured as a low airspeed system test bed. It is instrumented with a pulse code modulation (PCM) system and a magnetic tape recorder (Figure 20). The original plans called for the reinstallation of the LORAS II system, however Pacer Systems offered NATC the use of a LORAS 1000 system. This is a redesigned system incorporating modifications to eliminate the undesirable characteristics of LORAS II, such as cross-axis sensitivity and power frequency sensitivity. The LORAS 1000 contained a circuit change which provides position error correction for the UH-1N rotor hub locations. Another change was "hover meter" type of omnidirectional airspeed display (Figure 21). This installation provided acceptable results during a limited evaluation, however there is a need to conduct a more thorough evaluation under controlled conditions to verify these improvements.

During the period of May to October of 1976, the Air Force 1st Helicopter Squadron (MAC) located at Andrews Air Force Base, Maryland conducted an operational evaluation of LORAS 1000-T using a squadron UH-1N. The LORAS was mounted over the rotor hub using a standpipe. The report issued as a result of this evaluation contains many comments favorable to LORAS.

USAASTA at Edwards Air Force Base has run a flight evaluation of a LORAS 1000. To date a report of the results has not been issued.

On the recommendation of the manufacturer, the LORAS II which was returned from Germany has had the arms with no tip shrouds removed and replaced with arms with shrouds, but no baffles. This system is now installed on a OH-58 operated by TPS (Figures 22 & 23).

In 1972, LORAS II was presented as a universal low airspeed system. It was advertised to be impervious to downwash, insensitive to mounting location with a large speed range, and it could be moved between different helicopters and aircraft without any modifications. It didn't work. Only through agressive testing by the the customer and continuous modifications and improvements by the manufacturer has this system attained the level of maturity it enjoys today. The primary disadvantage of LORAS is that it has moving parts, adding to the cost and complexity of the system.

(2) J-TEC

J-TEC Associates, Inc. located in Cedar Rapids, Iowa manufacturers various low airspeed measuring devices. These systems determine airspeed by measuring airflow across a rod or strut. Vortices are shed behind this rod. The frequency of these vortices is directly proportional to the velocity of flow across the strut. This relationship is independent of air density, thus resulting in a vortex frequency linearly proportional to true airspeed. This vortex street modulates a ultrasonic beam between two transducers located behind the strut. This modulated signal is then detected and processed to provide a square wave, the frequency of which is equal to the vortex frequency and proportional to true airspeed. J-TEC originally used this technique to design anemometers and water current measurement systems.

In 1970 the Air Force Flight Dynamics Laboratory (AFFDL) purchased a prototype true airspeed transducer manufactured by J-TEC. This transducer was evaluated in a wind tunnel. The results were published in Technical Memorandum AFFDL - TM - 70-1-FGS. In 1971 AFFDL mounted this unit under the nose of a CH-3E for a flight evaluation. Initial flight testing revealed problems which were corrected by redesign. The improved system was then flight tested. The results of these efforts were reported in Technical Report AFFDL - TR - 72-131. The Army evaluated a J-TEC sensor as part of a Collins avionics system at Fort Rucker. The Army Aviation Systems Test Activity (USAASTA) at Edwards Air Force Base evaluated a model VA-210 system. In 1973 NATC purchased a VA 210 system for evaluation as a flight test transducer (Figure 24). All of these systems measured unidirectional airspeed.

Over a period of time, J-TEC developed an omnidirectional low airspeed sensor based on the same vortex counting technique. This is the model VT-1003 (Figure 25). The original units of this design were built for NATC. The VT-1003 consists of a sensor head with six tubes mounted 60° apart on a plane parallel to the waterline of the aircraft, and a signal processing unit with interconnecting cables. Each tube contains a vortex generating strut and a pair of ultrasonic transducers. The signal processing unit selects the two tubes with the highest indicated flow. The airspeed seen by these two tubes are then resolved into longitudinal and lateral outputs. This system has been wind tunnel tested at NBS. These tests showed repeatable data, a slight non-linearity, and a discontinuity every 60° of azimuth (Figures 26, 27, 28 & 29). This discontinuity occurs when one tube is aligned with the airstream, and the two adjacent tubes are exactly 60° from the airstream. The signal processing unit cannot distinguish which of these two tubes is experiencing the higher airflow, causing it to switch back and forth between these tubes. The effects of this problem can be minimized by installing the sensor head in such a way that none of the six tubes is aligned in a direction of expected flow. These discontinuities in all probability would have not been discovered if the system had not been calibrated in a wind tunnel.

The VT-1003 has been installed over the rotor hub on the UH-1N low airspeed test bed at NATC, and a AH-1G operated by TPS. Both installations have shown that the system is highly sensitive to electrical noise, thus severly limiting the collection of airborne data. The UH-1N installation is inactive due to lack of funds. Troubleshooting of the system on the AH-1G is being actively pursued. The feasibility of using the instrumentation package installed in the helicopter to record intermediate signals in the signal processing unit to help diagnose this problem is being studied. A potential problem with the VT-1003 is the quality of construction of the signal processing unit. Although the specifications called for an airborne quality package, it appears that the signal processing unit has a potential for vibration induced failures.

USAASTA at Edwards Air Force Base has completed a flight evaluation of a VT-1003. To date, no formal report has been issued. The VT-1003 is still in the prototype stage. The above mentioned problems would limit its usefulness as either an operational system or a flight test measuring system at this time, however the concept shows much promise. The primary advantages of this system are simplicity, no moving parts, and a potential for being an inexpensive and reliable system.

J-TEC also manufactures the VA-220 which is a refinement of the VA-210 (Figure 30). This is a unidirectional airspeed system with a linear output throughout the range of 2 to 200 kts true airspeed. A VA-220 has been purchased by TSD. Although this unit has not been tested in flight, it has been successfully calibrated in the NBS wind tunnel (Figure 31). This unit may have application in VSTOL testing programs.

(3) LASSIE (Figure 32)

The problem of finding a suitable mounting location for a low airspeed system on helicopters has led to extensive flight test programs, primarily conducted by USAASTA. Errors are more severe when the airspeed transducer is mounted in the rotor downwash. A unique approach to this problem is LASSIE (Low Airspeed Sensing and Indicating Equipment) manufactured by Marconi-Elliott Avionics Systems Limited of England. This system derives low airspeed information by measuring rotor downwash angle. The system consists of a swiveling pitot static tube (Figure 33), an air data converter and various indicators.

This method was conceived in 1956 and since then has been in various stages of development. The original system measured only longitudinal airspeed. Additional refinements led to a system capable of measuring longitudinal and lateral airspeed as well as rate of climb. LASSIE is currently being marketed as a component in an advanced helicopter flight data system.

USAASTA has tested an early version of LASSIE and an improved version. A LASSIE was used by the Army during the UTTAS test program. NATC has a LASSIE installed on the low airspeed test bed UH-1N (Figure 34). A preliminary flight calibration has been conducted. The air data converter is now being optimized for the specific probe location based on this flight calibration. This is necessary because of a discontinuity which appears when the probe passes out of the downwash in foward flight.

There are two primary advantages to the LASSIE system, both due to the unique design. The system is capable of providing information in three axes, and it does not require a rotor hub mounting location. The hub mounting location becomes a problem in some flight test programs since slip rings may be required at the hub as part of the data gathering system. Also in some types of helicopters a standpipe installation is impractical since the rotor mast contains blade fold equipment.

Two disadvantages of LASSIE are complexity and cost. A characteristic of LASSIE related to the basic design concept and mounting location, is the effect of a "dead area" in which the downwash is disturbed by the transmission housing. As the LASSIE system is presently constructed it cannot measure airspeed when the probe lies in this area of disturbed flow. Also due to the design concept (measurment of downwash) LASSIE cannot be used on VSTOL aircraft. LASSIE also could not be used to provide needed wind information to a helicopter pilot prior to rotor engagement.

(4) ROSEMOUNT

The Rosemount Engineering Company builds Model 853 Orthogonal Airspeed System. This system consists of a sensor, pressure tubing, a combined pressure transducer-signal conditioner and a "hover indicator." The Rosemount sensor is a cylindrical strut mounted perpendicular to the plane of desired information. For example, to measure longitudinal and lateral airspeed the sensor must be mounted along the aircraft vertical axis. Four flush ports are equally spaced around this strut. The ports are normally aligned with the longitudinal and lateral axis of the helicopter. The ports are connected to chambers inside of the strut which in turn are connected to the pressure transducer-signal conditioner by four pressure tubes. The differential pressures between opposing pressure ports is then measured by the transducers. The electrical output of these differential pressure transducers is then processed in an analog form to obtain longitudinal and lateral airspeed components. Rosemount offers versions of this system which will measure airspeed in three axes. Various output options have been offered. The Rosemount two-axis system was tested in 1974 by the U.S. Army Aviation Engineering Flight Activity (formerly USAASTA) at Edwards Air Force Base. Prior to the flight tests, wind tunnel tests were conducted at NASA Ames. Three mounting locations were evaluated during the flight tests on a UH-1M helicopter. Mounting the sensor on the cabin roof produced unsatisfactory results. The sensor was mounted at two heights over the rotor hub. In this location the higher of the two mounts produced better data. These results were reported in USAAEFA Project No. 71-30-5, Final Report V.

The Army has since flight tested an improved Rosemount system. The results of these tests have not yet been published. Advantages of the Rosemount system are that it has no moving parts and it has the potential to provide three-axis airspeed information. A disadvantage is potential maintenance problems associated with running at least four pressure lines from the rotor head to the airspeed transducer.

(5) ULTASONIC WIND VECTOR SENSOR

The ultrasonic wind vector sensor (UWVS) was originally developed as part of a helicopter fire control system by Honeywell, Inc. of St. Louis Park, Minnesota. This sensor measures wind speed in three axes. The UWVS derives this information by measuring the relative delays of three ultrasonic signals. This system has been flight tested by the Army (USAAEFA) at Edwards Air Force Base. The results of these tests have not been published, however unofficial comments from USAAEFA personnel indicate that this system shows promise. The best mounting location was determined to be above the rotor hub.

The advantages of this system are three-axis data, no moving parts, and good reliability based on several years of flight testing. The original intent of this system was to measure local airflow, which is not necessarily airspeed. There are some mounting problems due to the sensor head interfering with itself aerodynamically. The UWVS system is still in the process of being developed, however, it shows much promise for the future.

(6) OPTICAL CONVOLUTION VELOCIMETER

The optical convolution velocimeter (OCV) is an electro-optical airspeed sensor. This system has been developed jointly by the Air Force Flight Dynamics Laboratory and Bolt, Besanek and Newman, Inc. of Cambridge, Massachusetts. It is still in the early stages of development. Very limited flight tests have been conducted. The development of this system will be followed with interest.

(7) ION DRIFT ANEMOMETER (Figure 35)

The ion drift anemometer works on a principle similar to the UWVS, using ionized air instead of ultrasonic signals. An ion drift anemometer constructed at Princeton University was flight tested at NATC in 1973. This system measured longitudinal and vertical airspeed. For the evaluation it was mounted on the nose boom of an OH-58A operated by TPS. The item tested was not airborne quality, having been constructed for wind tunnel use. The results of this one flight evaluation were published in Flight Test Technical Memorandum No. 7-73.

(8) AEROFLEX TAVS

The Aeroflex True Airspeed Vector System (TAVS) was a self-aligning hot wire anemometer system tested by the Army from 1970 to 1972. The system provided airspeed and angle of sideslip information. The system was extremely complex when compared to other systems available in 1972 (the completion of testing on TAVS). The outputs drifted. The hot wire elements proved fragile and the entire system appeared to be a potential maintenance problem.

Although the Aeroflex TAVS was one of the earliest low airspeed systems it fell by the wayside, a victim of competition from better and simplier systems.

MOUNTING

With the exception of LASSIE which must be mounted in the downwash, to date, the best mounting location is over the rotor hub. To mount a sensor in this location requires a relatively major modification to the helicopter. A standpipe is inserted through the hollow center of the rotor mast. The bottom of this pipe passes through the transmission where it is secured to prevent rotation or other movement.

An opening in the bottom of the standpipe allows the sensor wires or tubes to be routed to the electronics package. The lower end of the standpipe has an oil seal to prevent loss of the transmission oil (Figure 36). The upper end of the standpipe passes through a modified mast nut and is supported by a bearing (Figure 37). The outer race of this bearing is attached to the mast nut, the inner race to the standpipe. Since the mast nut holds the hub and rotor blades onto the rotor shaft, great care must be taken with this modification. The sensor head is mounted to an adapter on the upper end of the standpipe. The sensor must be aligned with the center line of the helicopter. The accuracy of this alignment will be the limiting factor of the accuracy of the entire system. To date, an optimum alignment technique has not been developed.

Because of the critical location of the sensor, standpipe assembly requires a high degree of structural integrity. The sensor head and adapter must mate with a close tolerance. All fasteners must be safety wired. A turn-up and tear down inspection of the adapter and bearing is required for all new installations.

Questions about the fatigue life of standpipes led to an internal program at NATC to gather data to determine this fatigue life. A small instrumentation package was designed for this program (Figure 38). This package is capable of conditioning and multiplexing, using PBW/FM, up to 14 strain gage measurements. The package outputs are two 7-channel FM multiplexes which can be either recorded or telemetered. This package has been used on a UH-1N and a OH-58A. The results of this standpipe certification program have been satisfactory.

In order to provide a linear output in any location, many low airspeed systems must receive circuit modifications. In general, if the sensor is moved to another type of helicopter or to another location on the same helicopter, the system must again be modified.

TEST METHODS

All of the previously described systems measure airflow in the area of the sensor. This local flow is usually not the same as the relative velocity of the airframe with respect to the air mass (airspeed). This difference is referred to as position error. One approach to the evaluation of low airspeed systems was to fly repeated flights with the sensor mounted in different locations around the helicopter and compare the results. The location with the fewest problems such as cross-axis sensitivity, non linearity, discontinuities, or random noise, would be used for further evaluation. The only trouble with this approach is that each of these problems can also be a charteristic of the basic system. Thus the basic system errors can be masked by the position errors. The reverse is also true. Also the effects on the system of such variables as temperature, altitude, vibration, and electrical power are nearly impossible to isolate in an aircraft installation.

A more logical approach is to perform a complete evaluation of the low airspeed system on the ground, prior to installing it on an aircraft. This approach is the least expensive in both the long and short run. The only disadvantage is that it takes time. The heart of the ground evaluation is a complete wind tunnel calibration. Here, under controlled conditions, the sensor can be checked for linearity, hysteresis, repeatability, cross-axis sensitivity, and the effects of angle of attack, sideslip, and variations in input power. By identifying the system errors in this manner, they can be corrected prior to the first flight. Errors not corrected can at least be documented. The calibration data obtained during the ground evaluation can be used to evaluate position error data from the flight tests.

CONCLUSION

There are no ideal airspeed systems available today. Each system has distinct advantages and disadvantages such as cost, complexity, reliability, and ease of installation. The field is becoming more competitive with many diverse technologies being applied to the problem of low airspeed measurement.

With the advent of these new measurement techniques comes the requirement to test the resulting equipment. An organization involved in the evaluation of low airspeed systems, or anything else for that matter, must develop a philosophy of testing. Infinite laboratory testing must be tempered by the constraints of time, money and manpower. The evaluation process must be aimed at providing the end user with properly functioning equipment with any unresolved problems carefully documented. A ground evaluation with emphasis on wind tunnel testing, as advocated by TSD appears to be the most logical approach to the testing of low speed airspeed systems. The ultimate test is, of course, an evaluation of the aircraft installation.

It would also be advantageous for test activities engaged in similar testing to maintain informal contact with each other. This mutual exchange of information would eliminate some of the duplications and false starts found in test work.

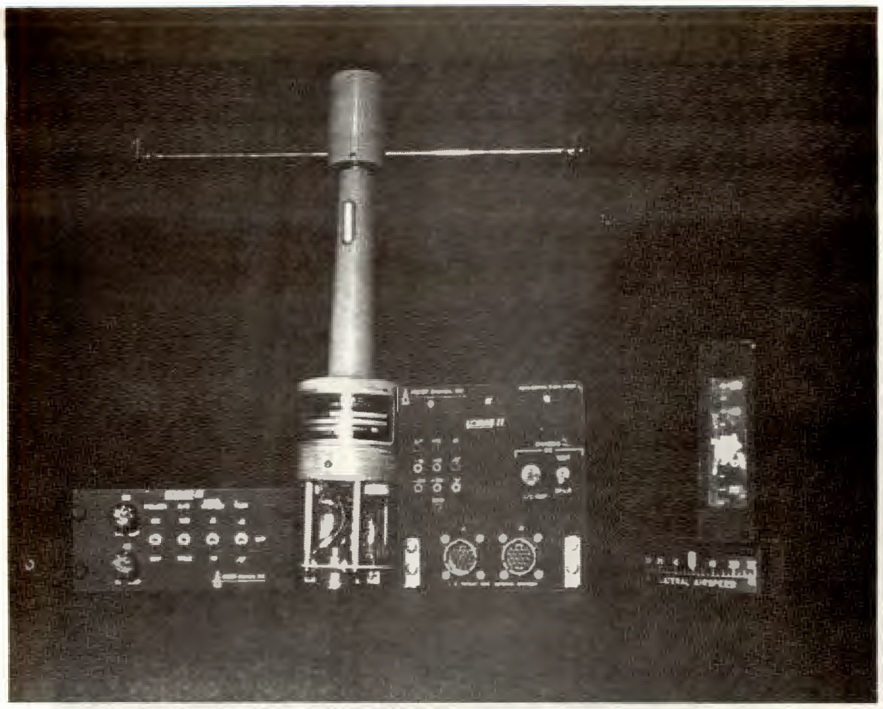


FIGURE 1
LORAS II SYSTEM

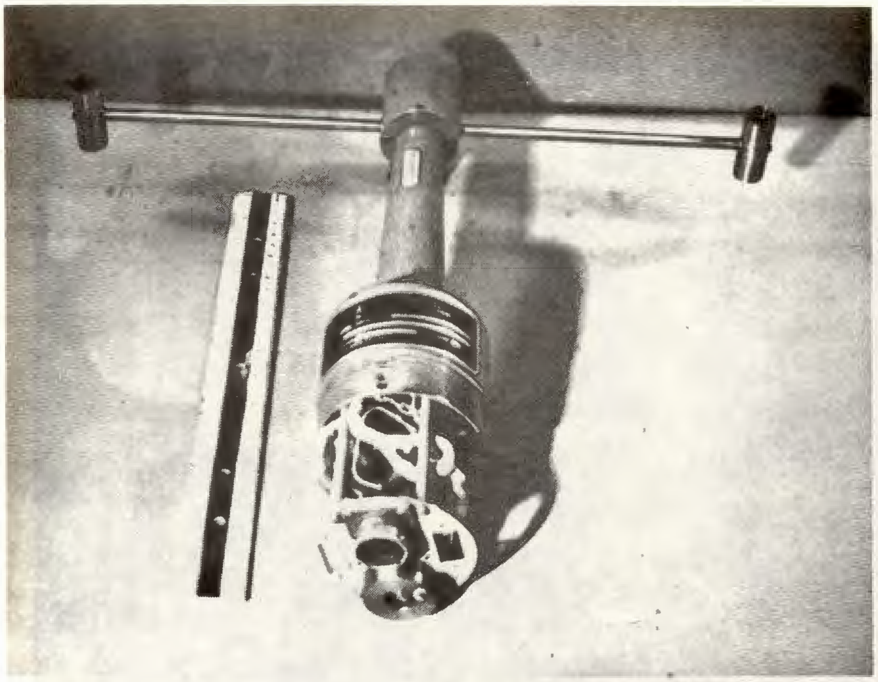


FIGURE 2
DETAIL OF LORAS HEAD

FIGURE 3
AH-1J (BuNo 157794) LORAS II MOUNTED ON NOSE BOOM, NOV 1972

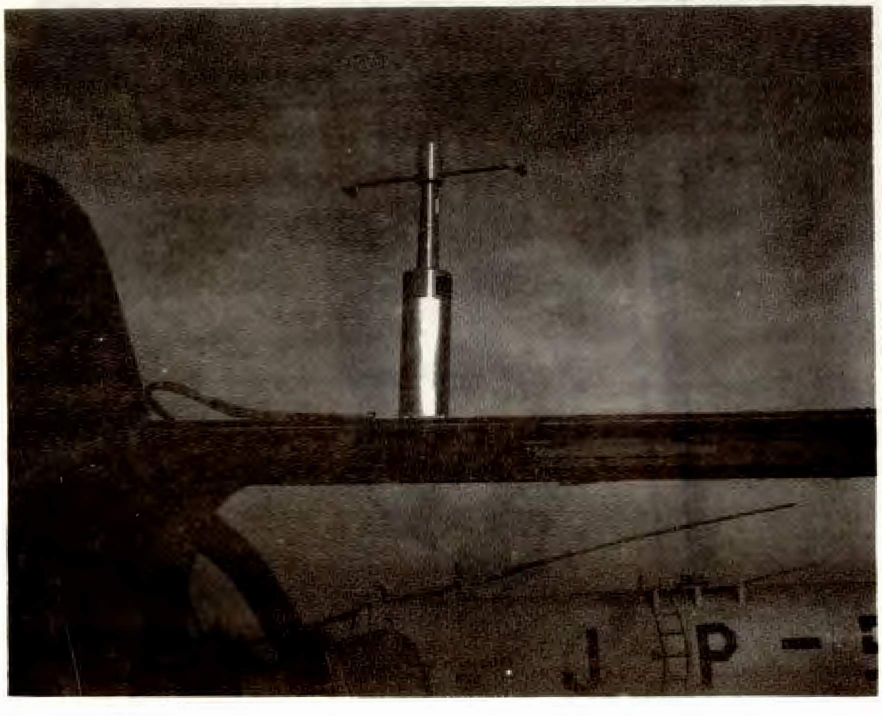


FIGURE 4
AH-1J (BuNo 157794) LORAS II MOUNTED ON CANOPY, NOV 1972

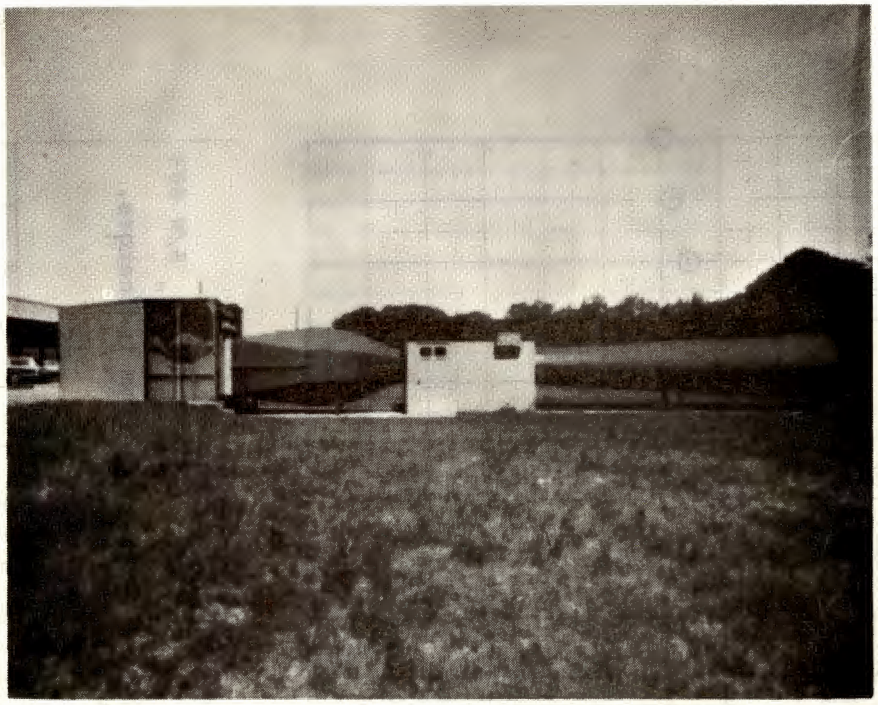
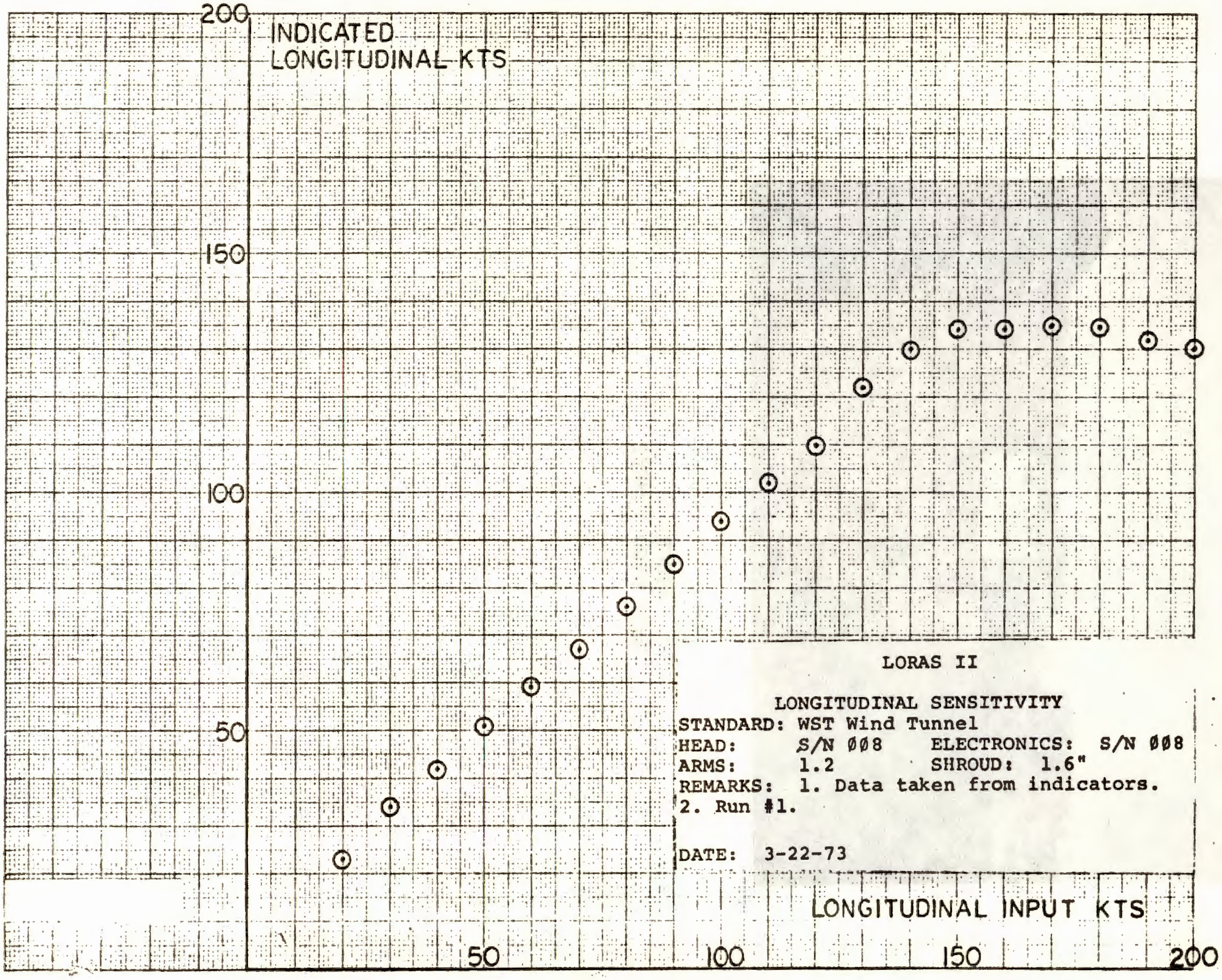


FIGURE 5
NATC WIND TUNNEL

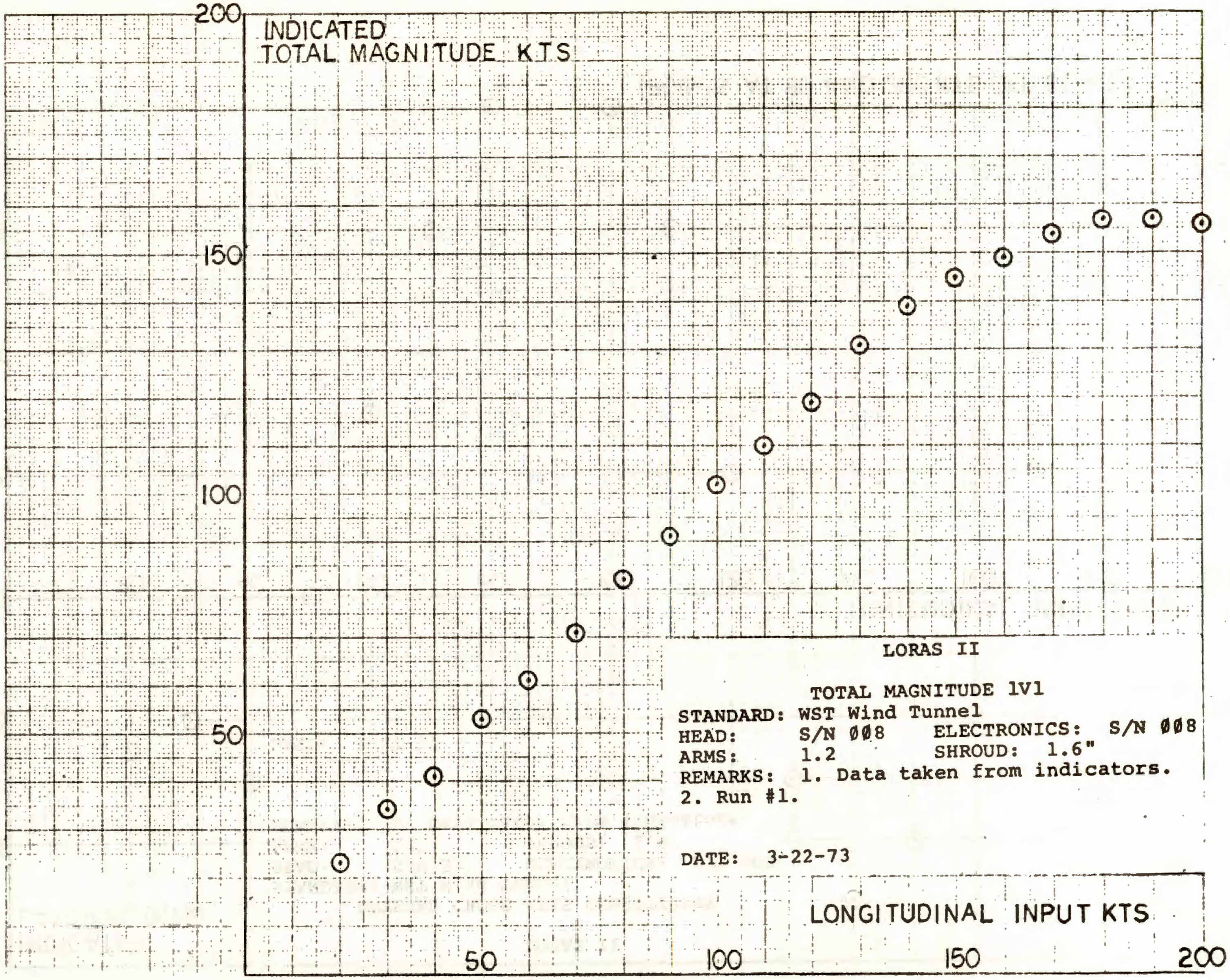


LORAS II

LONGITUDINAL SENSITIVITY
STANDARD: WST Wind Tunnel
HEAD: S/N 008 ELECTRONICS: S/N 008
ARMS: 1.2 SHROUD: 1.6"
REMARKS: 1. Data taken from indicators.
2. Run #1.

DATE: 3-22-73

FIGURE 6



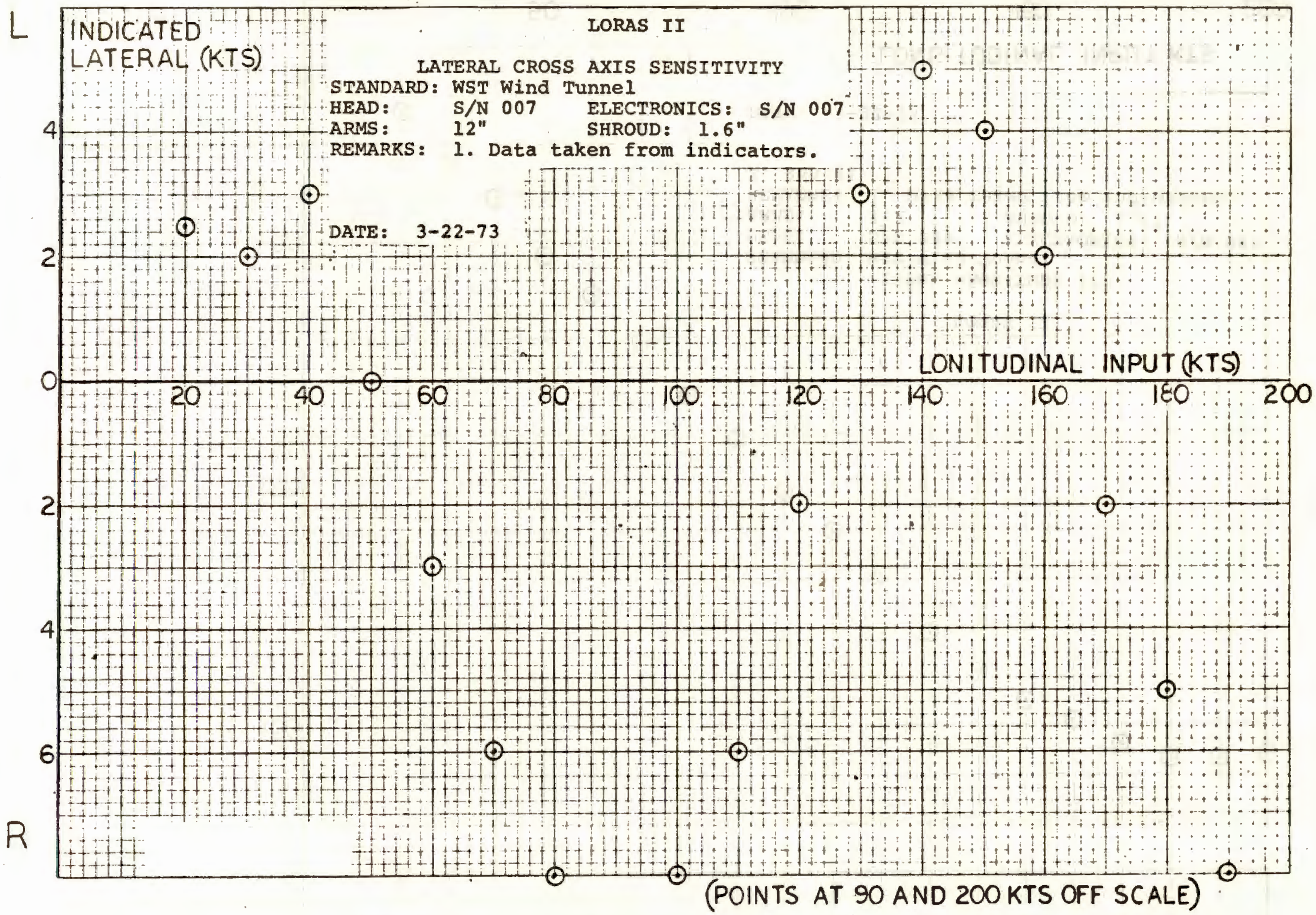
LORAS II

TOTAL MAGNITUDE lVl

STANDARD: WST Wind Tunnel
HEAD: S/N 008 ELECTRONICS: S/N 008
ARMS: 1.2 SHROUD: 1.6"
REMARKS: 1. Data taken from indicators.
2. Run #1.

DATE: 3-22-73

FIGURE 7



LORAS II

LATERAL CROSS AXIS SENSITIVITY
STANDARD: WST Wind Tunnel
HEAD: S/N 007 ELECTRONICS: S/N 007
ARMS: 12" SHROUD: 1.6"
REMARKS: 1. Data taken from indicators.

DATE: 3-22-73

FIGURE 8

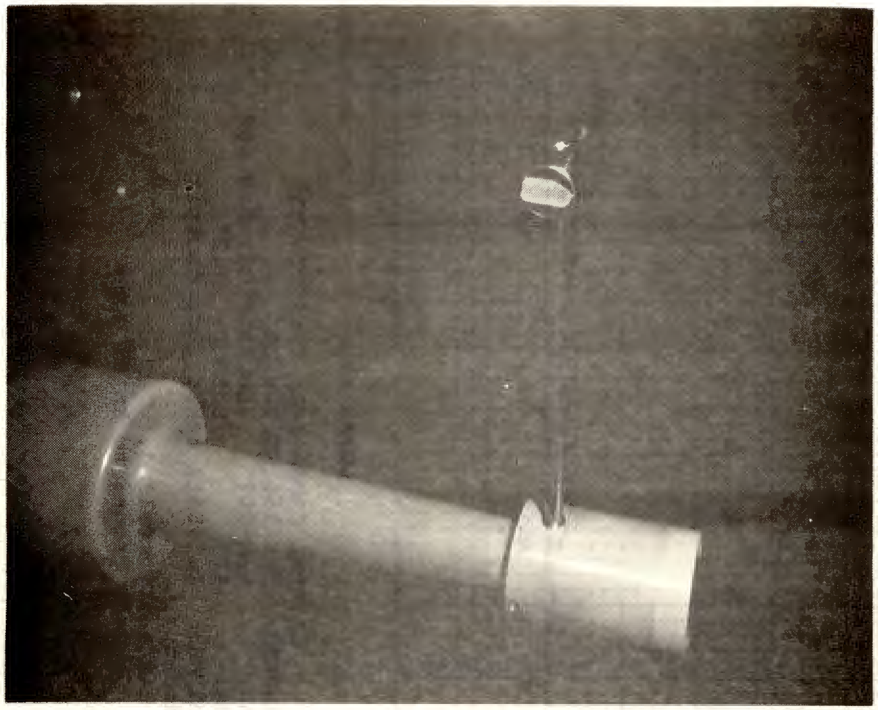


FIGURE 9
MODIFIED LORAS HEAD SHOWING SHROUD AND BAFFLE

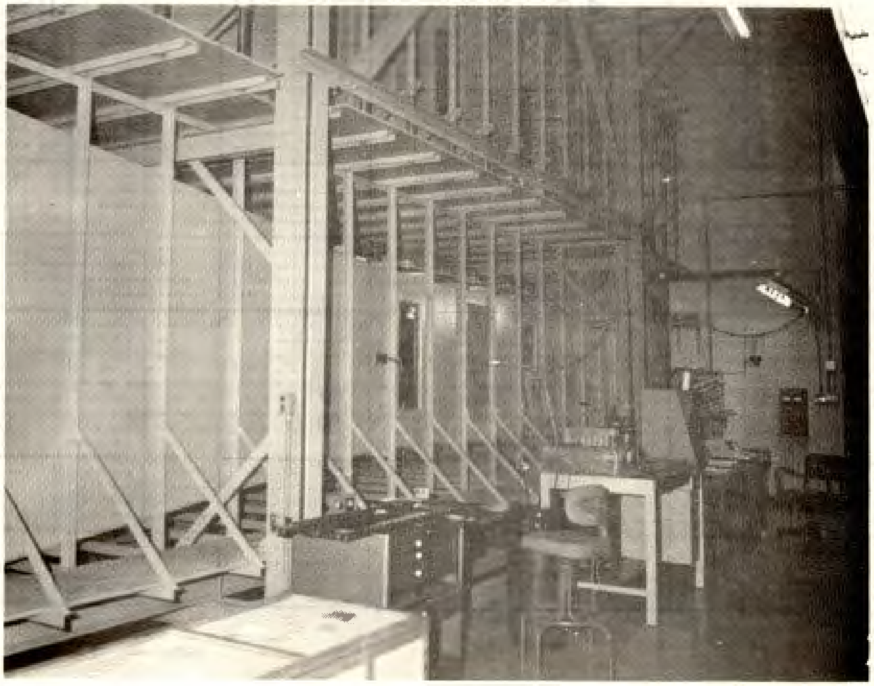
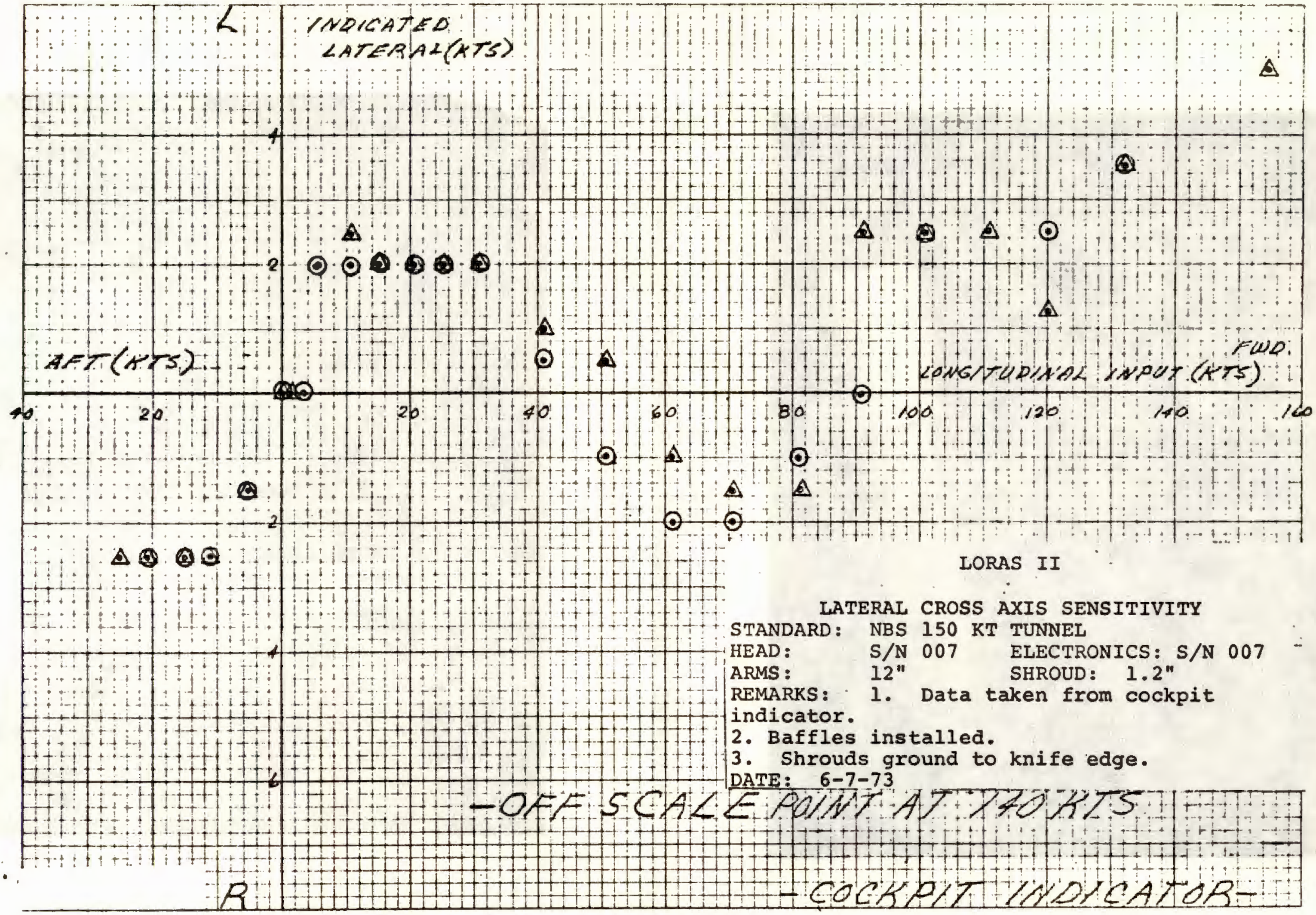


FIGURE 10
NBS DUAL SECTION WIND TUNNEL



LORAS II

LATERAL CROSS AXIS SENSITIVITY
STANDARD: NBS 150 KT TUNNEL
HEAD: S/N 007 ELECTRONICS: S/N 007
ARMS: 12" SHROUD: 1.2"
REMARKS: 1. Data taken from cockpit indicator.
2. Baffles installed.
3. Shrouds ground to knife edge.
DATE: 6-7-73

FIGURE 11

FIGURE 12
LORAS MOUNTED ON BEECH BARON NOSE BOOM

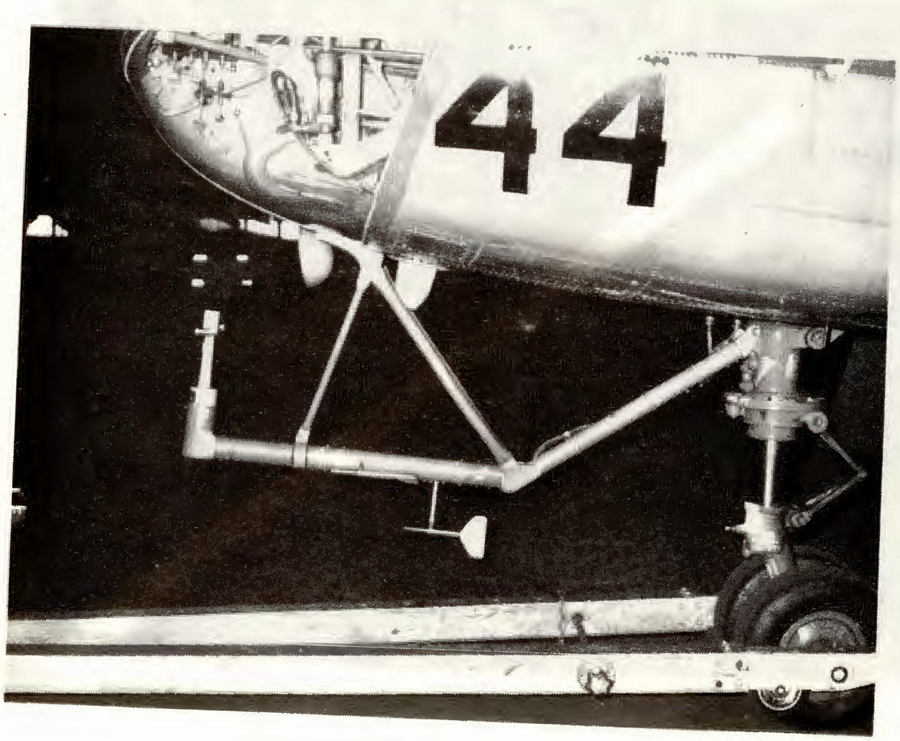


FIGURE 13
NCH-46A LORAS MOUNTED ON CHIN BOOM, NOV 1973

FIGURE 14
MODIFIED LORAS II SYSTEM

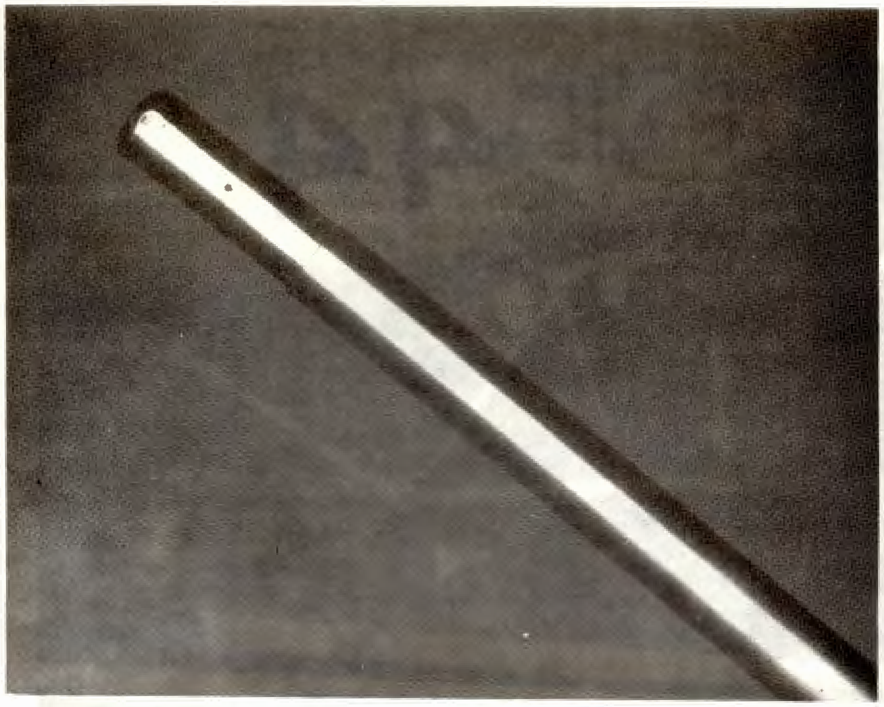
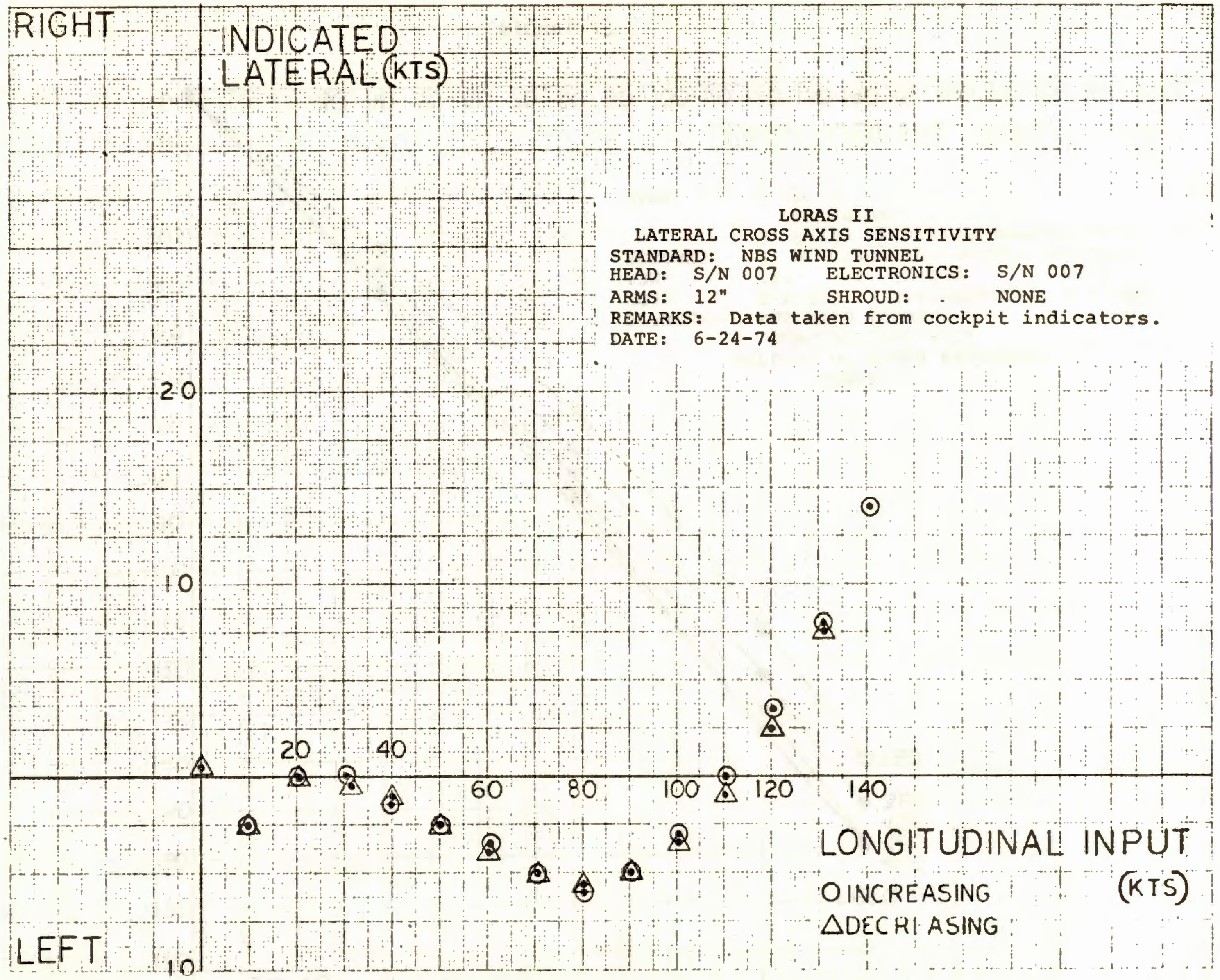
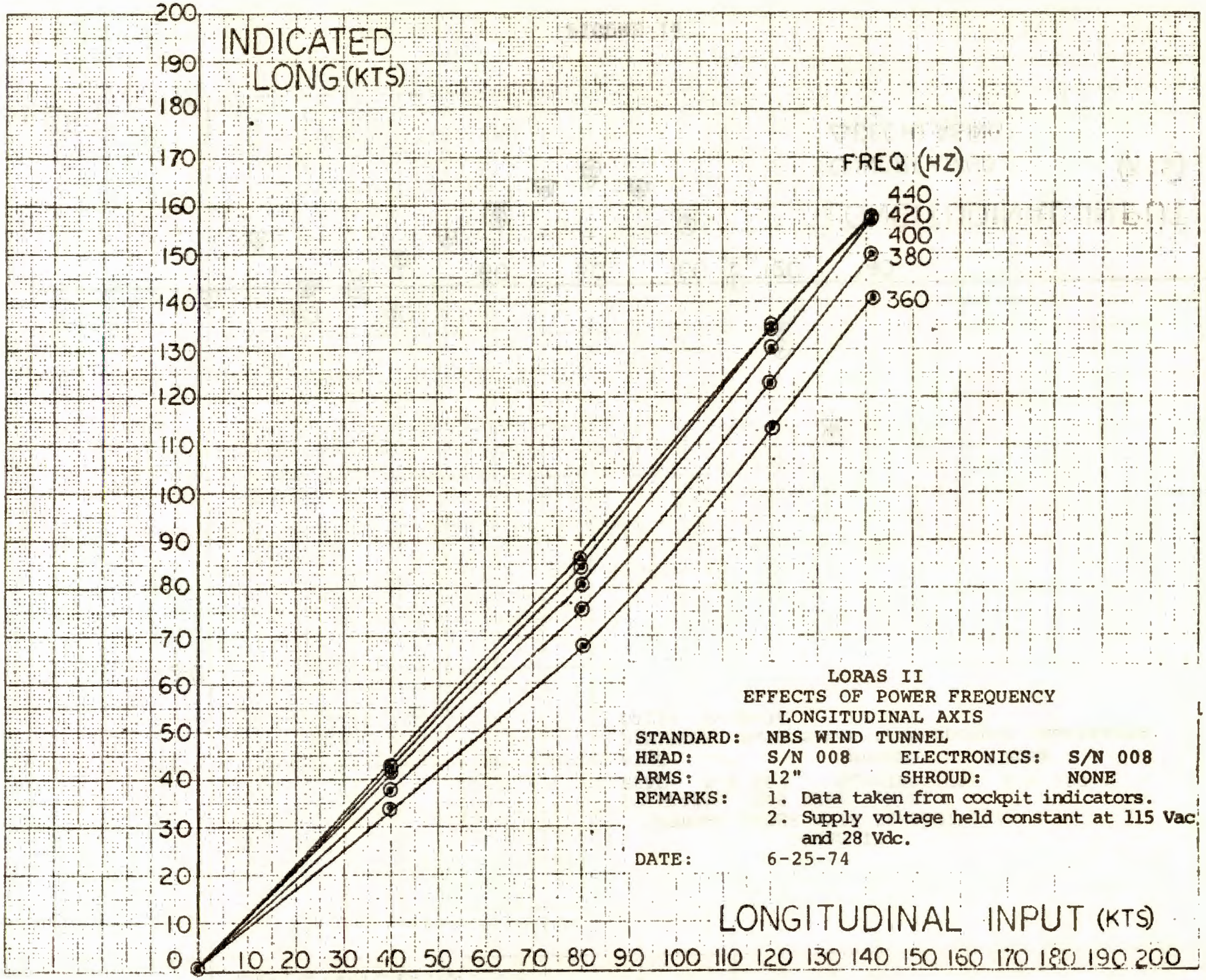


FIGURE 15
CLOSE-UP OF LORAS II ARM TIP



LORAS II
LATERAL CROSS AXIS SENSITIVITY
STANDARD: NBS WIND TUNNEL
HEAD: S/N 007 ELECTRONICS: S/N 007
ARMS: 12" SHROUD: NONE
REMARKS: Data taken from cockpit indicators.
DATE: 6-24-74

FIGURE 16

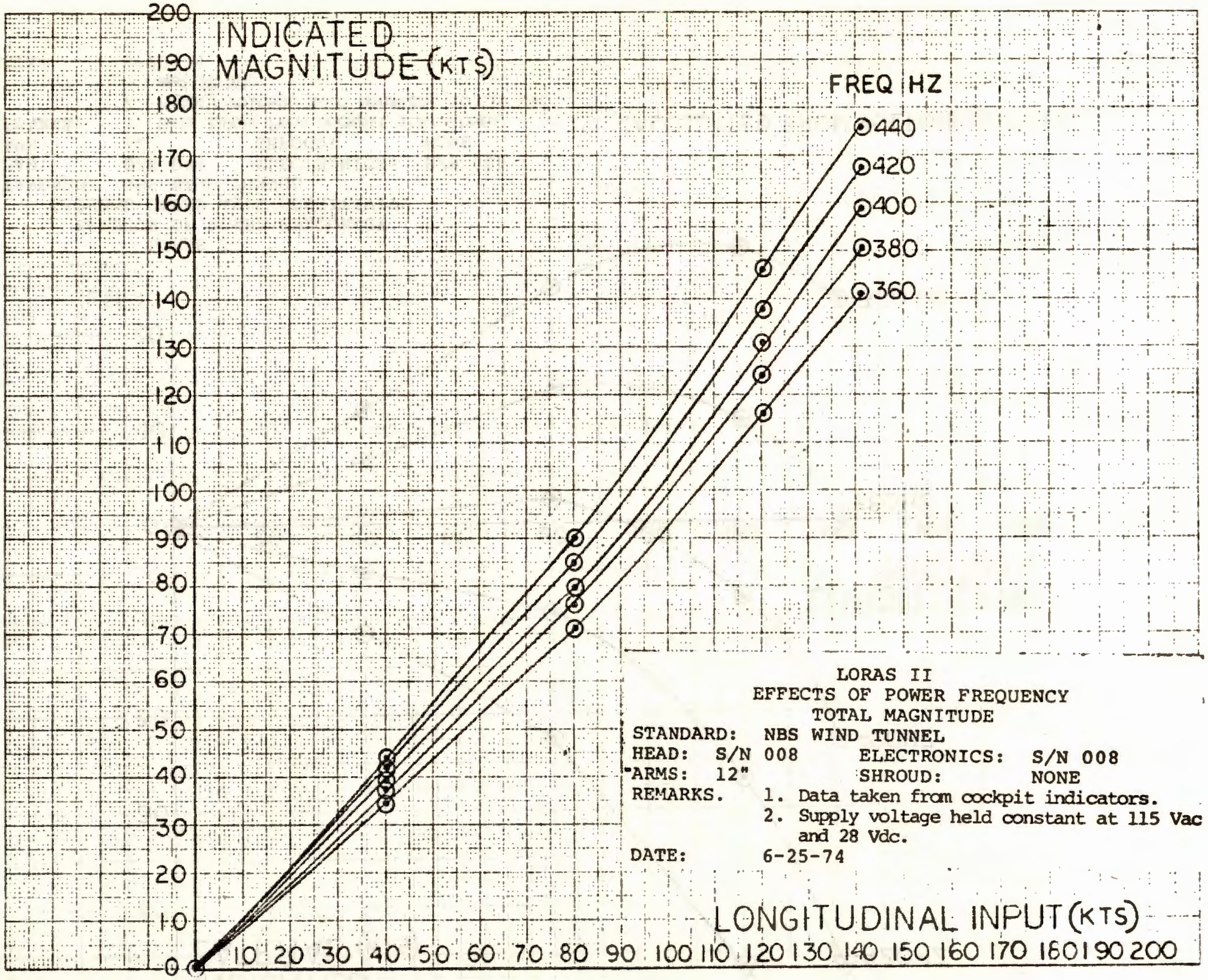


LORAS II
EFFECTS OF POWER FREQUENCY
LONGITUDINAL AXIS

STANDARD: NBS WIND TUNNEL
HEAD: S/N 008 ELECTRONICS: S/N 008
ARMS: 12" SHROUD: NONE
REMARKS:
1. Data taken from cockpit indicators.
2. Supply voltage held constant at 115 Vac and 28 Vdc.

DATE: 6-25-74

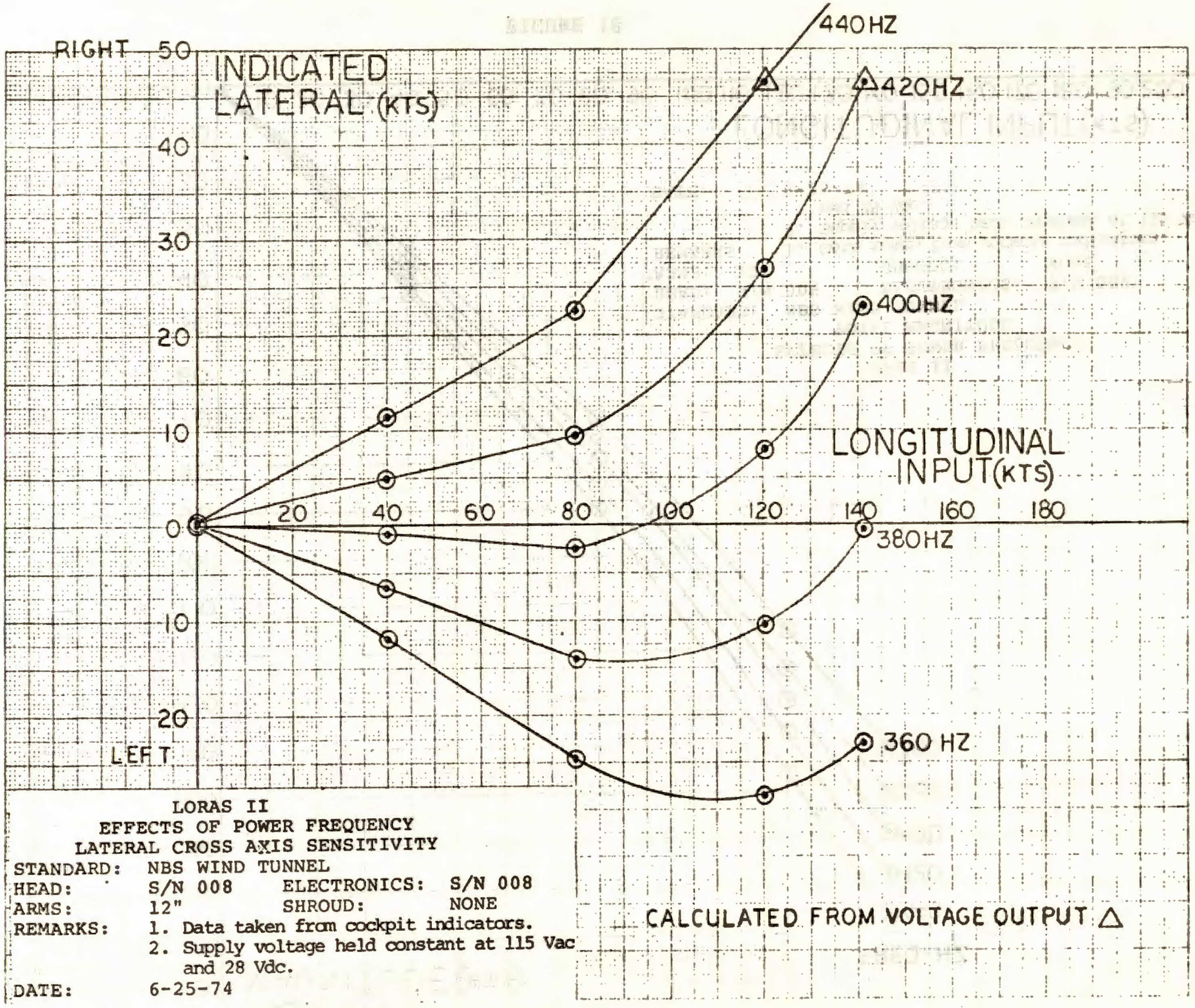
FIGURE 17



LORAS II
EFFECTS OF POWER FREQUENCY
TOTAL MAGNITUDE

STANDARD: NBS WIND TUNNEL
HEAD: S/N 008 ELECTRONICS: S/N 008
ARMS: 12" SHROUD: NONE
REMARKS. 1. Data taken from cockpit indicators.
2. Supply voltage held constant at 115 Vac and 28 Vdc.
DATE: 6-25-74

FIGURE 18



LORAS II
EFFECTS OF POWER FREQUENCY
LATERAL CROSS AXIS SENSITIVITY

STANDARD: NBS WIND TUNNEL
HEAD: S/N 008 ELECTRONICS: S/N 008
ARMS: 12" SHROUD: NONE
REMARKS: 1. Data taken from cockpit indicators.
2. Supply voltage held constant at 115 Vac and 28 Vdc.
DATE: 6-25-74

FIGURE 19

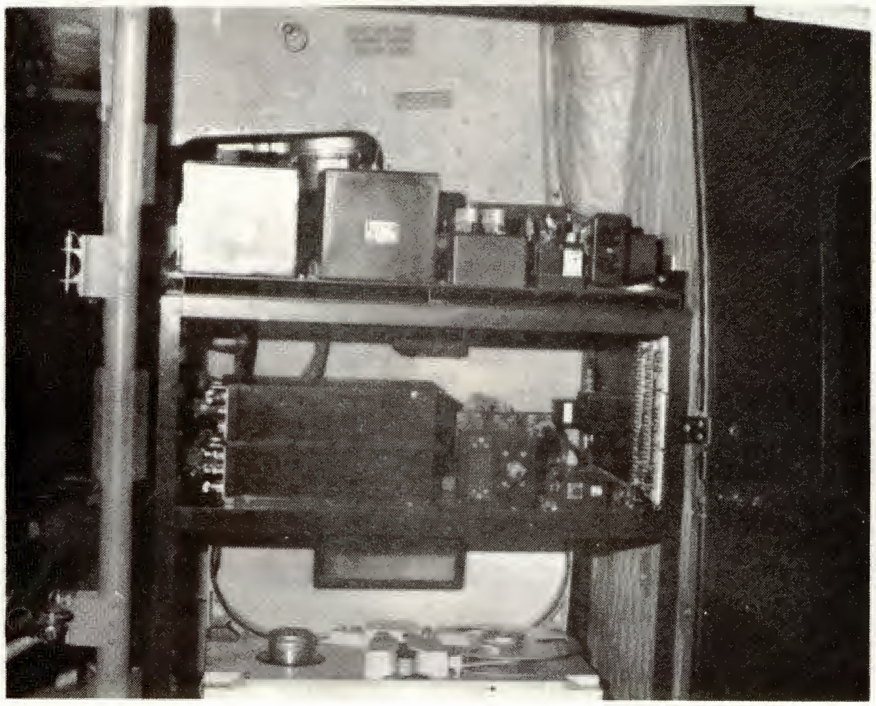


FIGURE 20
NATC LOW AIRSPEED TEST BED HELICOPTER INSTRUMENTATION PACKAGE

FIGURE 21
LORAS 1000 "HOVER INDICATOR"

FIGURE 22
TPS OH-58 WITH LORAS MOUNTED ON STANDPIPE, MARCH 1977

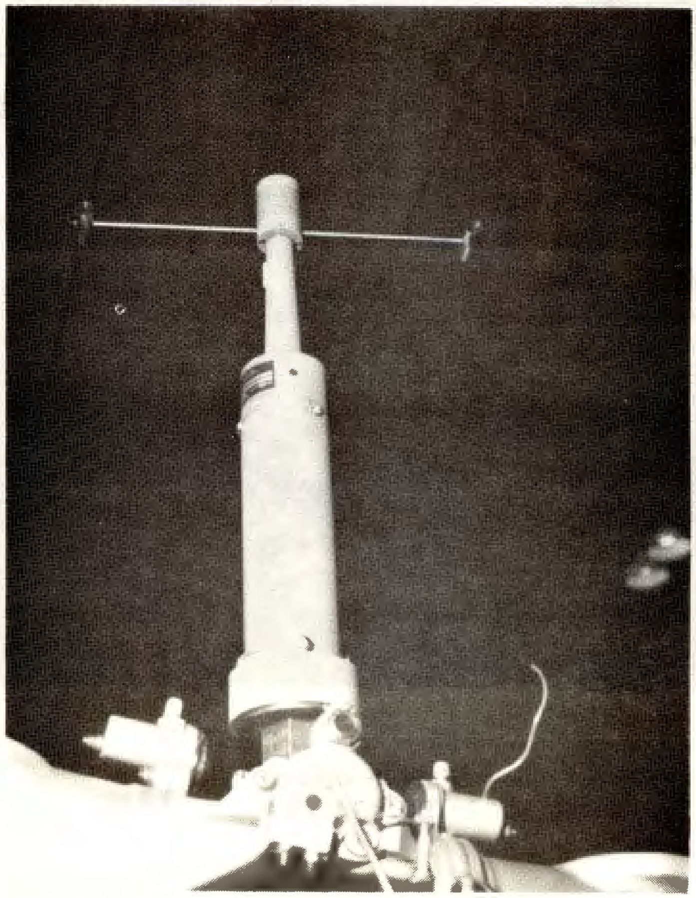


FIGURE 23
CLOSE-UP OF A LORAS HEAD SHOWING REINSTALLED SHROUDS

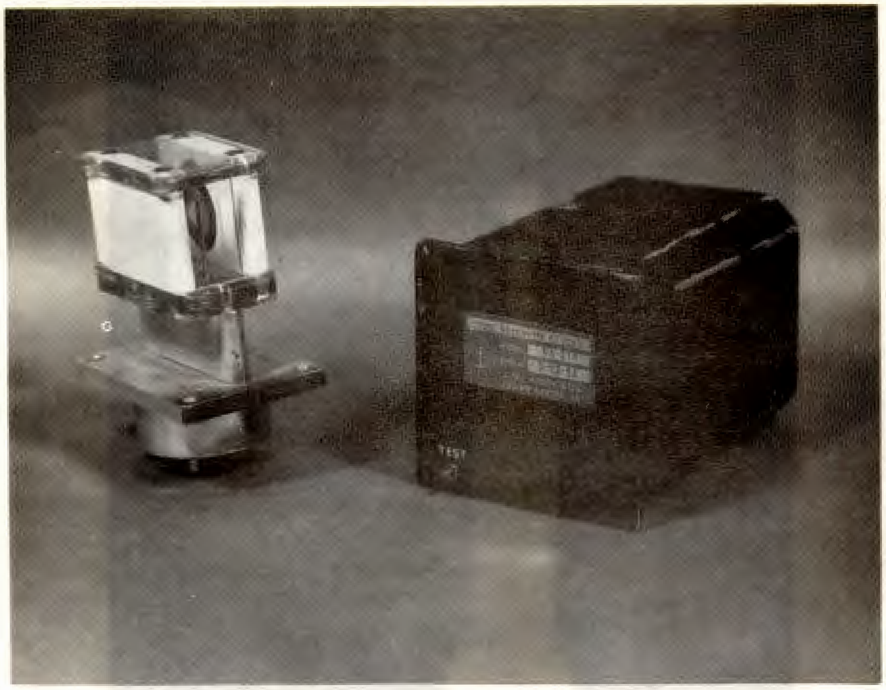


FIGURE 24
J-TEC VA 210

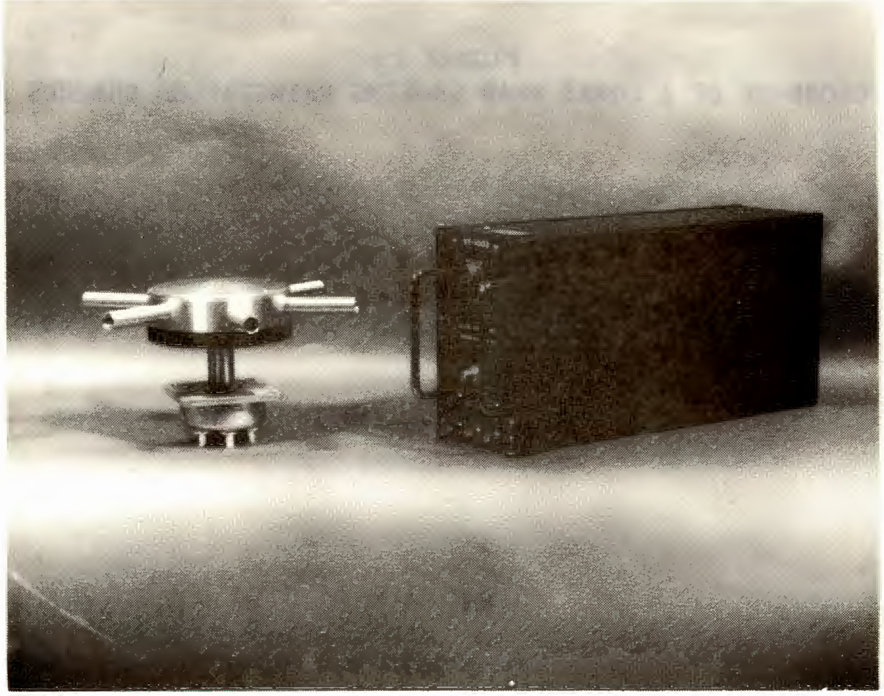
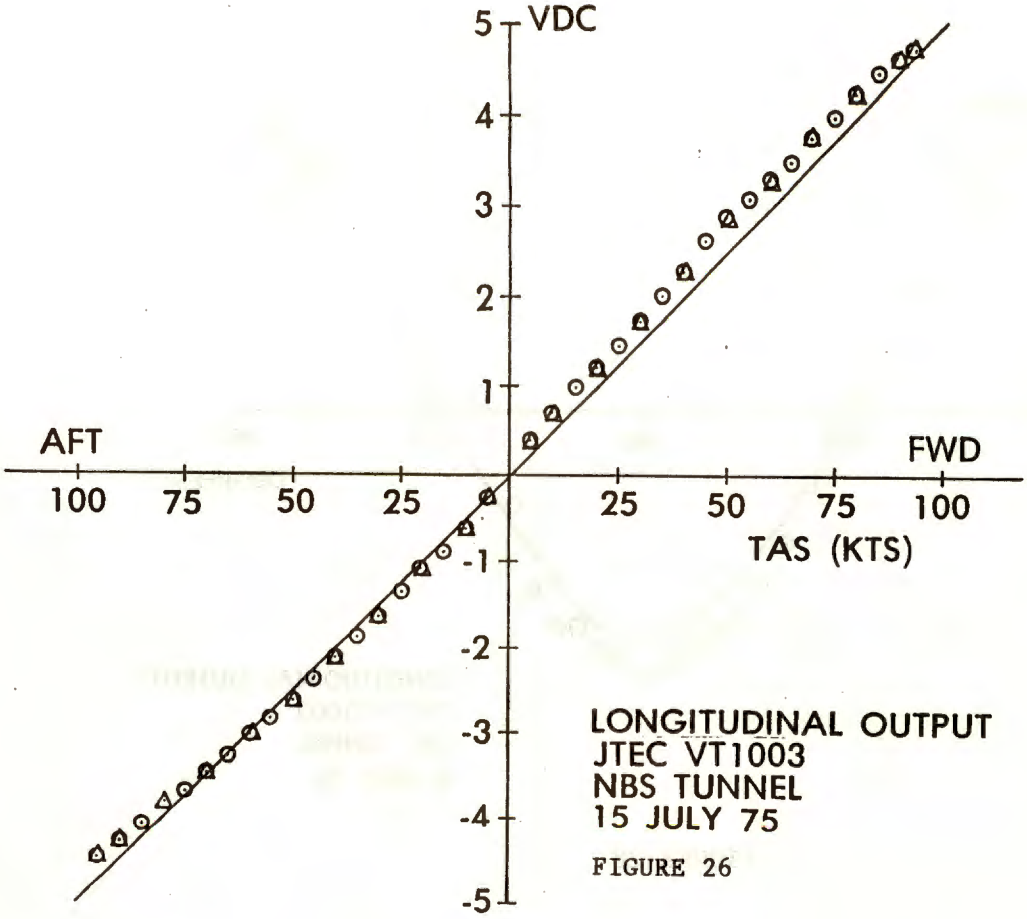
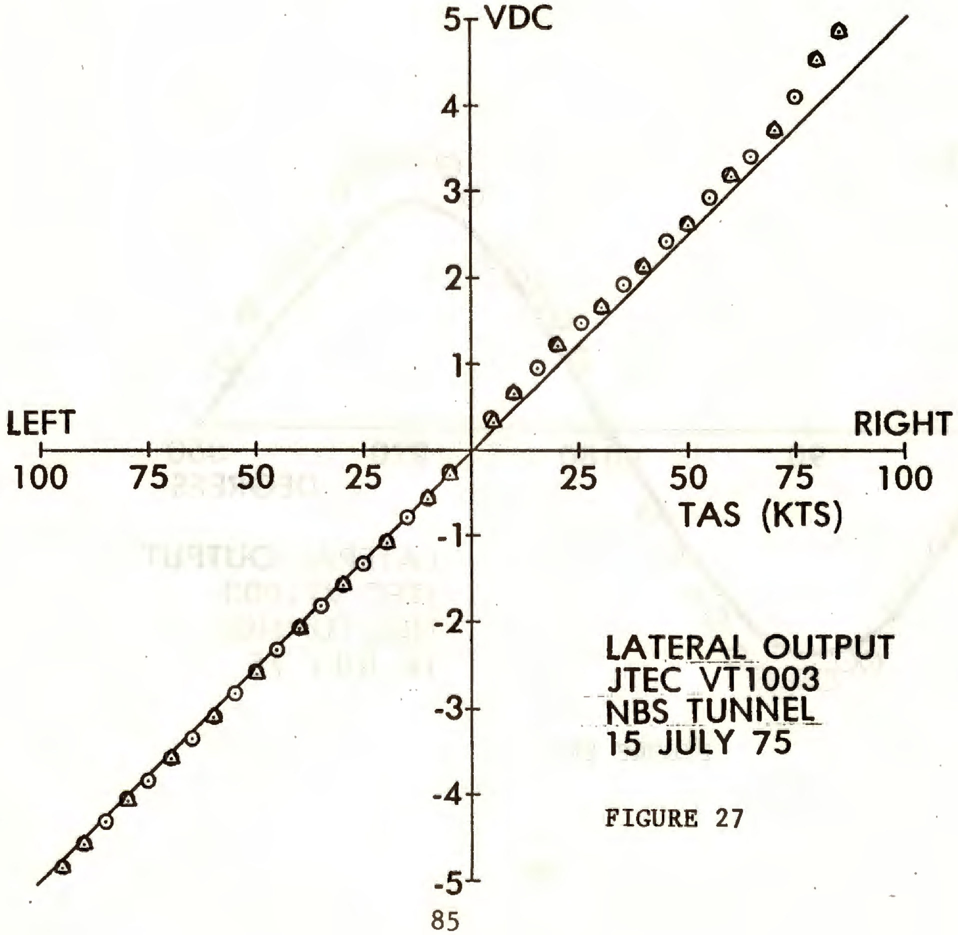


FIGURE 25
J-TEC VT 1003



LONGITUDINAL OUTPUT
JTEC VT1003
NBS TUNNEL
15 JULY 75

FIGURE 26



LATERAL OUTPUT
JTEC VT1003
NBS TUNNEL
15 JULY 75

FIGURE 27

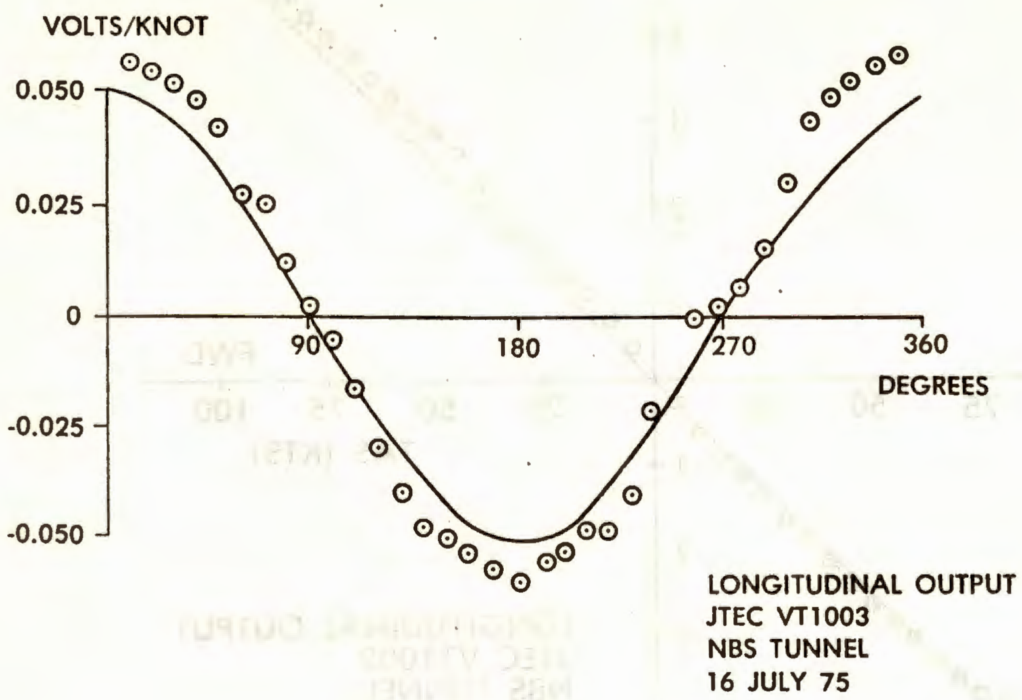


FIGURE 28

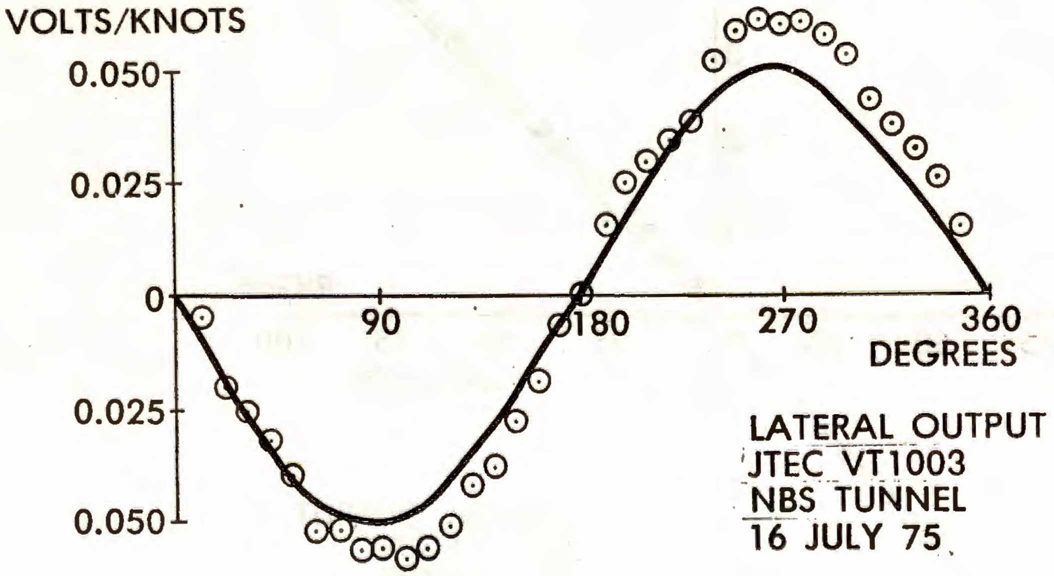


FIGURE 29

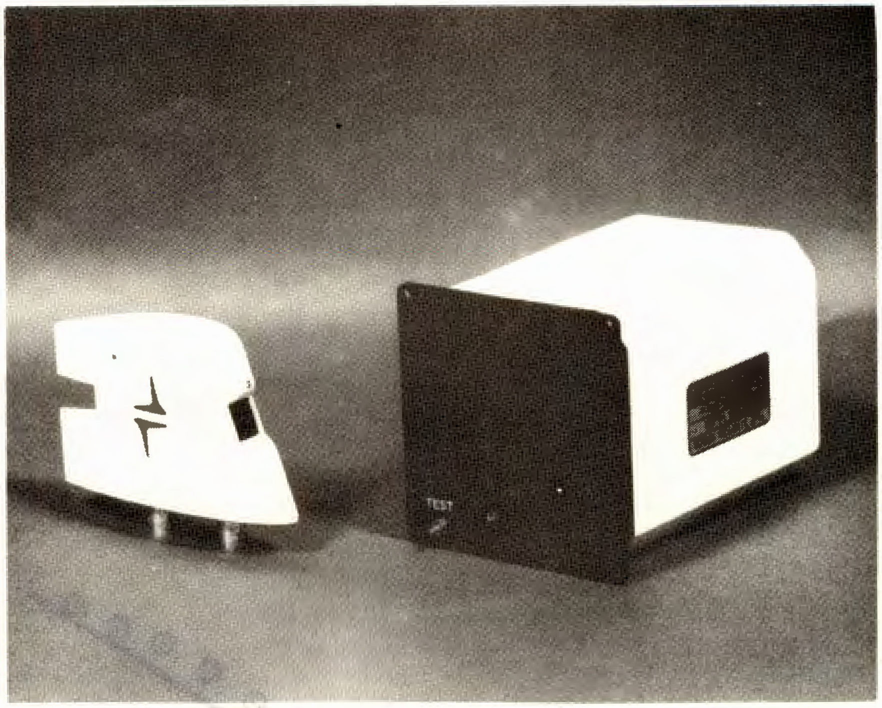


FIGURE 30
J-TEC VA 220

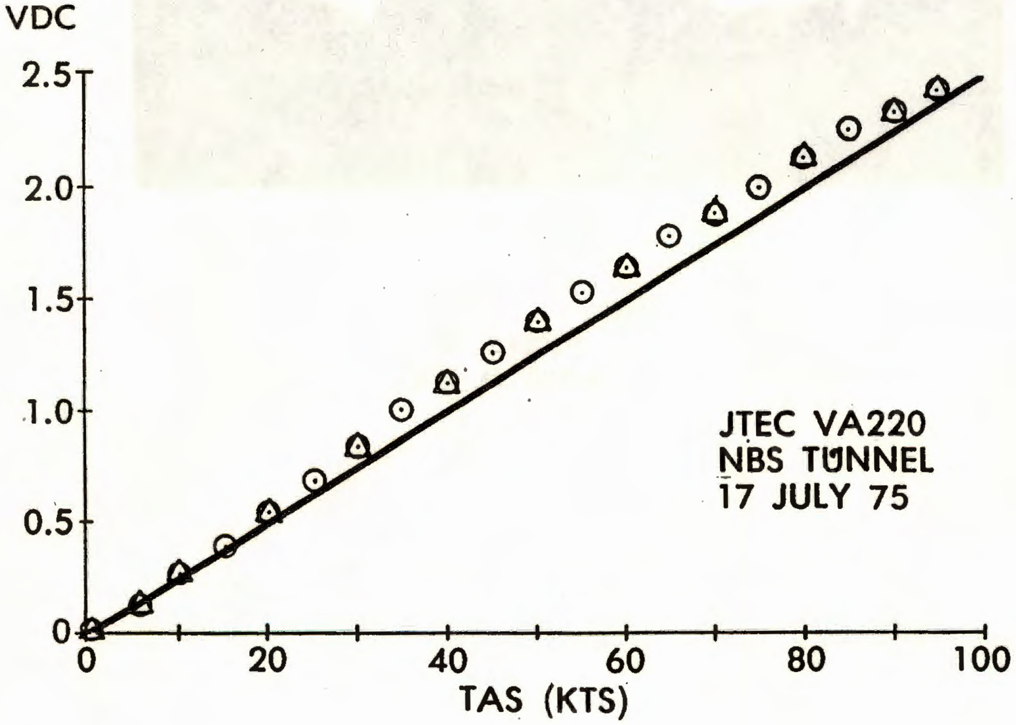

VDC
2.5
2.0
1.5
1.0
0.5
0
0
20
40
60
80
100
TAS (KTS)
JTEC VA220
NBS TUNNEL
17 JULY 75

FIGURE 31

FIGURE 32
LASSIE SYSTEM

FIGURE 33
LASSIE SWIVEL HEAD

FIGURE 34
LASSIE MOUNTED ON NATC LOW AIRSPEED TEST BED HELICOPTER

FIGURE 35
ION DRIFT ANEMOMETER MOUNTED ON NOSE BOOM OF OH-58A, 1973

FIGURE 36
UH-1N STANDPIPE MOUNTING PLUG

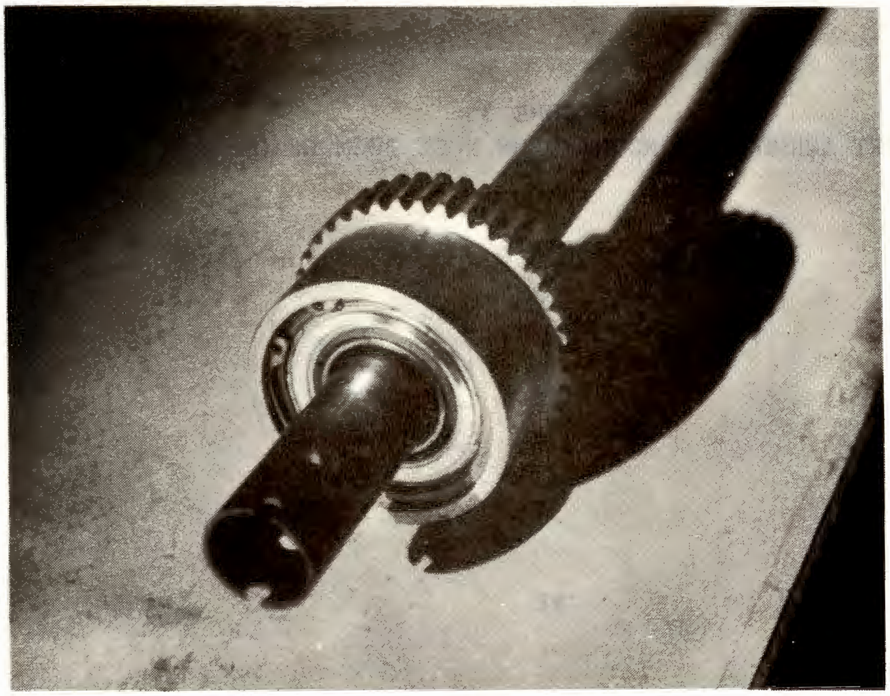


FIGURE 37
UH-1N STANDPIPE - MAST NUT ASSEMBLY

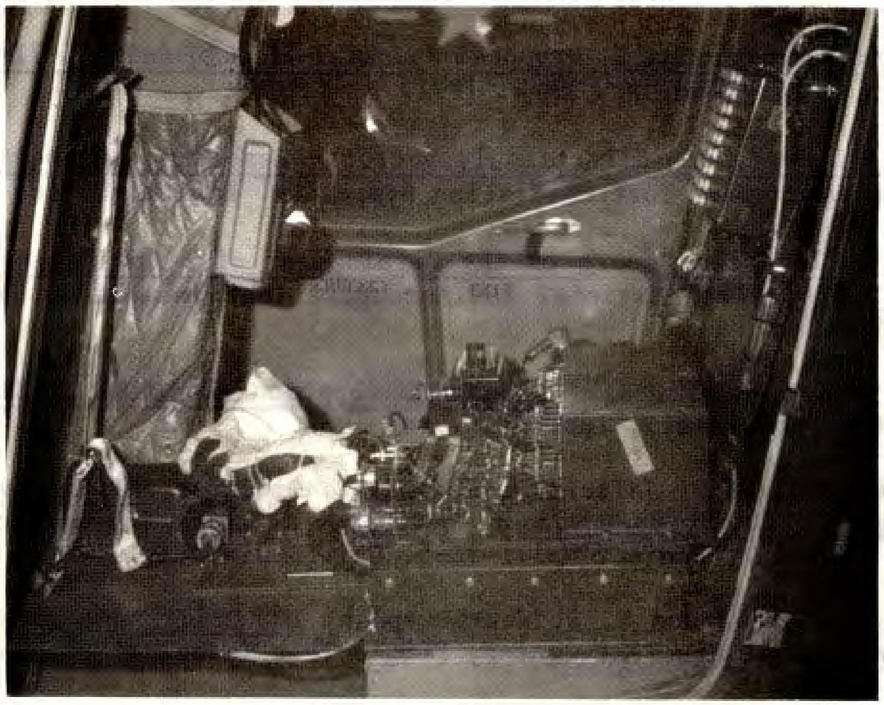


FIGURE 38
STANDPIPE CERTIFICATION INSTRUMENTATION PACKAGE MOUNTED IN OH-58A

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We the unwilling
led by the unqualified
have been doing the unbelievable
so long with so little
we now attempt the impossible
with nothing

AIRCRAFT EQUIPMENT COOLING FLOW MEASUREMENTS

by

L. J. Mertaugh
Aerospace Engineer
Technical Support Directorate
Naval Air Test Center
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ABSTRACT

This paper describes the techniques used at the Naval Air Test Center to measure the cooling air flows through aircraft equipment components on flight test aircraft. Comments on the utility of the various methods are provided. The development of a calibration parameter for the presentation of flow calibration data is also given.

INTRODUCTION

The increased emphasis being placed on system performance and reliability in weapon system development has brought about an increasing need for the evaluation of the amount of cooling air flow being provided to various equipment units in test aircraft. Pertinent projects at the Naval Air Test Center (NATC) have included measuring the cooling air flows to generator heat exchangers, computer cold plates, radar control units and battery compartments. Often, these installations involve hard-to-get-at locations with additional complications brought about by relatively low air flows, ill defined flow paths and large variations in flow pressure and temperature. Although the techniques used to provide these flow measurements do not, in general, represent any major departure from normal laboratory flow measurements, they do require rather careful consideration and implementation in the flight test environment in order that reasonable accuracy can be obtained. An additional aspect of this type of instrumentation problem is the selection of a suitable method of presenting the calibration data. The desired objective of the presentation is to provide a single, linear calibration curve that is valid for all flight conditions.

The intent of this paper is to share some of the NATC experiences with this type of measurement and to present a brief development of a method of presentation for the resulting calibration data that would seem to satisfy the above stated objective quite well. It is felt that this information may be useful to flight test instrumentation people and will possibly provide some additional insight into the physics of this type of installation.

TEST METHODS

The methods that have been used or studied at NATC for the measurement of cooling air flows have included total/static pressure rakes, venturis, hot-film anemometers and a technique that will be herein referred to as the pressure-drop method. The pressure-drop method involves the calibration of the pressure drop across the unit against the mass flow rate of cooling air passing through the unit. A major portion of this paper will be devoted to the pressure-drop method because it has proven to be a versatile technique that lends itself to the flight test constraints and environment. It should be noted that only subsonic flows are being considered. Considerations of pressure recovery, mechanical strength of unit components and heat transfer normally dictate that moderate to low flow velocities be used for equipment cooling flows.

Pressure rakes have been used in a number of cooling flow installations at NATC, but the results are sometimes subject to question because of the vulnerability of the probes to flow misalignment. These rakes have often been of the integrating type and, therefore, are also open to errors due to unexpected variations in the velocity profiles. The integrating rakes do require some form of calibration, and sufficient care must be taken in the calibration to provide accurate duplication of the actual test ducting. It is seldom that a test situation allows a sufficient number of measurements across the flow path to allow for an accurate numerical integration of the rake data.

The ability to perform a numerical integration would eliminate the need for calibration of the installation, assuming that the probes were aligned properly. A traversing probe would also allow numerical integrations, but such installations are also seldom practical for airborne equipment cooling-flow measurements. At the present time pressure rakes are not used at NATC unless it is desirable to identify the local average velocity over a particular component within a unit. A sketch of such an installation is shown in Figure 1. Even with care in the design of the probes and the installation, this type of measurement is best used for comparison type measurements where one might be trying to evaluate the merits of different methods of providing a cooling air flow.

A few installations at NATC have allowed the insertion of a venturi into the cooling flow ducting as a means of measuring the flow rate. The venturi is usually built to fit the particular installation and must be calibrated. As in the case of the integrating rake, care must be taken in the calibration setup so that the airflow entry into the venturi is an accurate duplication of the intended test installation. The ideal arrangement would provide a long straight section of ducting just upstream of the venturi, but this is not possible in most aircraft. Although the pressure loss through a venturi is small and can be minimized by proper design, the pressure loss may represent a departure from the original duct configuration and may produce a reduction in air flow due to the presence of the test instrumentation. A representative venturi installation is shown in Figure 2.

The use of hot-film anemometers for certain types of cooling flow measurements at NATC has been limited, to date, to laboratory studies. The particular application that was being considered involved the measurement of very low cooling flow velocities within equipment racks on a test aircraft. The test involved large variations in flow temperature, and laboratory tests were conducted to evaluate the temperature compensation provided in a particular commercial probe. The probe was a relatively simple unit designed for heating and ventilating work. The temperature compensation proved to be inadequate with the probe, and it was not felt that the generation or use of calibration curves for a number of flow temperatures was feasible for the intended test program. An additional problem with most probes of this type is the alignment of the probe normal to the flow velocity. The hand-held utilization of the anemometer allows maximum readings to be sought - a maxmimum reading indicating flow normal to the probe sensor. The fixed installation normally required in flight test does not provide this flexibility, and an appropriate solution to this problem is still being sought. It should be pointed out the more sophisticated three-dimensional anemometer equipment is available and that at least some of this equipment does provide reasonable temperature compensation. This equipment tends to be rather bulky and costly and was not felt to be appropriate for the test programs considered to date.

The pressure-drop method utilizes the test equipment as a flow restriction device and has proven to be a very versatile technique. The method introduces a minimum of protuberances into the flow path and normally does not require additional ducting. As the pressure-drop method is often used by the equipment manufacturer (with a great number of variations) the use of this method in flight test does provide a better opportunity for direct comparison with the manufacturer's data. In this respect, care must be taken to establish the exact test arrangement used by the manufacturer and the correct definitions of the terms used in presenting his data. A calibration of the variation in pressure differential across the unit as a function of the mass flow of cooling air is required with this method. A typical installation for the pressure-drop method is illustrated in Figure 3. This particular installation was considered ideal since the connection of the equipment to the aircraft environmental control system provided an inlet plenum which allowed a simple pressure tap installation. The lower unit employed a flush static pressure tap in the plenum. A total pressure probe, directed at the plenum inlet, was used on the upper unit to provide a larger differential pressure across the unit. This was necessary because of the limited range of pressure transducers available at the time of this test. Both differential pressure measurements were referenced to the static pressure at the rear of the units. Because of the close proximity of the rear of the units to the rear wall of the aircraft compartment, a static pressure in the immediate vicinity of the cooling flow exit from the unit was considered to be more desirable than using a static pressure obtained at some other location in the compartment (i.e., just opening the reference side of the differential pressure transducer to the ambient conditions at the transducer location). For the work at NATC, the average of the inlet and exit pressure and temperature is used in computing the density of the flow. The use of a total pressure probe for the inlet pressure makes the determination of an average pressure more difficult (unless an additional static pressure is measured at the inlet), but the use of the total pressure probe implies a small differential pressure across the unit so that only small error would result from using the exit static pressure as an "average" value. The pressure-drop method is currently the most used method for measuring cooling flows in test aircraft at NATC.

DATA ANALYSIS

The initial efforts at working with the pressure-drop method calibration data followed the approaches of some of the electronic equipment manufacturers and resulted in a series of nonlinear curves - a curve for each of a series of flow temperatures. The best results were obtained with calibration plots of mass flow as a function of the square root of the product of the differential pressure and the flow density. This presentation is still non-linear and has a small, but identifiable, trend with flow temperature. Such a calibration plot is shown in Figure 4 for a generator heat exchanger configuration. A log-log plot of the calibration data would identify an appropriate exponent for the product, $\rho \Delta P$, so as to give a linear relationship, but it was felt that more meaningful results might be obtained if the problem were considered in more basic terms. In attempting to provide an understanding of the mechanism of the flow resistance through a piece of equipment, it was felt that there was reason to expect that the flow restrictions associated with the interior of a piece of equipment (such as a piece of electronic equipment) would have characteristics similar to a sharp-edged orifice plate. In particular, it was felt that the role of viscosity and the trend of flow resistance with Reynolds number would be similar. A typical variation of an orifice discharge coefficient (a measure of flow resistance) with Reynolds number is shown in Figure 5. The discharge coefficient, K, is defined by the volume flow relationships

$$Q \sim K\sqrt{\Delta P/(g\rho)} \qquad (1)$$

where ΔP is the pressure drop across the orifice, ρ is the flowing fluid density and g is the acceleration due to the local gravity. The data in Figure 5 were obtained from Reference 1. Following the assumption that the flow characteristics should be similar, it might be logical to attempt to generate data similar to Figure 5 for the particular piece of equipment of interest. Unfortunately, obtaining such data for a piece of equipment is difficult because of problems with defining an appropriate characteristic length and a velocity required to define a Reynolds number. Fortunately, we are only looking to define a proportionality relationship and, as it turns out, are only interested in a restricted portion of the full curve given in Figure 5. The final presentation of the cooling flow calibration data does in fact represent an alternate presentation of a limited portion of the data in Figure 5 for an equipment unit.

Lacking more appropriate data, and needing more immediate results, it was decided to simply assume that the orifice relationship shown in Figure 5 was valid, at least in a limited range of Reynolds numbers, for a general equipment unit. Further, because of the low flow rates normally used for equipment cooling, it was assumed that only the initial, low Reynolds number, portion of Figure 5 was appropriate. Approximating the initial curved portion of Figure 5 with a straight line yields

$$K \sim R_N^{0.2} = (\bar{V}D\rho/\mu)^{0.2}. \tag{2}$$

In equation (2) $\bar{V}$ is a representative velocity and D is a characteristic length. Multiplying equation (1) by the fluid density gives the mass flow ($\dot{m}$), and subsitution of equation (2) into equation (1) gives

$$\dot{m} \sim \rho(\bar{V}D\rho/\mu)^{0.2}(\Delta P/\rho)^{0.5}.$$

Although we are not really able to define $\bar{V}$ or D, we can absorb D into a constant of proportionality and utilizing the fact that $\rho\bar{V}$ is proportional to the mass flow obtain

$$\dot{m} \sim (\dot{m}/\mu)^{0.2}(\rho\Delta P)^{0.5}$$

or

$$m \sim (\rho\Delta P)^{0.625}/\mu^{0.25}. \tag{3}$$

For most atmospheric operations the viscosity can be assumed to be a function of only temperature. Based on data found in References 2 and 3,

$$\mu \sim T^{0.82}. \qquad (4)$$

Substituting equation (4) into (3) gives the final formulation

$$\dot{m} \sim (\rho \Delta P)^{0.625} / T^{0.205}. \qquad (5)$$

If the various assumptions that have been made are reasonably valid, it should be expected that plotting the calibration mass flow data against the pressure parameter,

$$(\rho \Delta P)^{0.625} / T^{0.205},$$

will yield a single straight line for a given piece of equipment. Further, this single calibration line should be valid for all conditions of $\rho, \Delta P$ and T. The effectiveness of this method of presentation is shown in Figure 6. These are the same data as were used in Figure 4. Similar results have been obtained for a number of different equipment configurations. Although the limits of mass flow or pressure parameter for which these results are valid can not be defined, the limits will be apparent as the calibration data start to deviate from a straight line. Such a deviation may be illustrated near the origin in Figure 6, however, this deviation may only represent the inaccuracy of the flow meter or manometers at very low flow rates.

CONCLUSIONS

Although certain test situations may lend themselves to the use of one of the other methods, the pressure-drop method for measuring cooling air flows through equipment units has proven to be a versatile flight test technique.

The use of the pressure parameter developed in this paper provides a simple presentation of cooling flow calibration data. The parameter should allow a minimum number of calibration points. The linearity of the resulting data also allows the use of the linear regression methods to obtain a relatively simple calibration equation.

REFERENCES

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2. "Aerospace Propulsion Data Book," Flight Propulsion Division, The General Electric Company, Cincinnati, Ohio, 1961

3. "Heat Transfer, Volume I," Max Jakob, John Wiley and Son, New York, New York, 1949

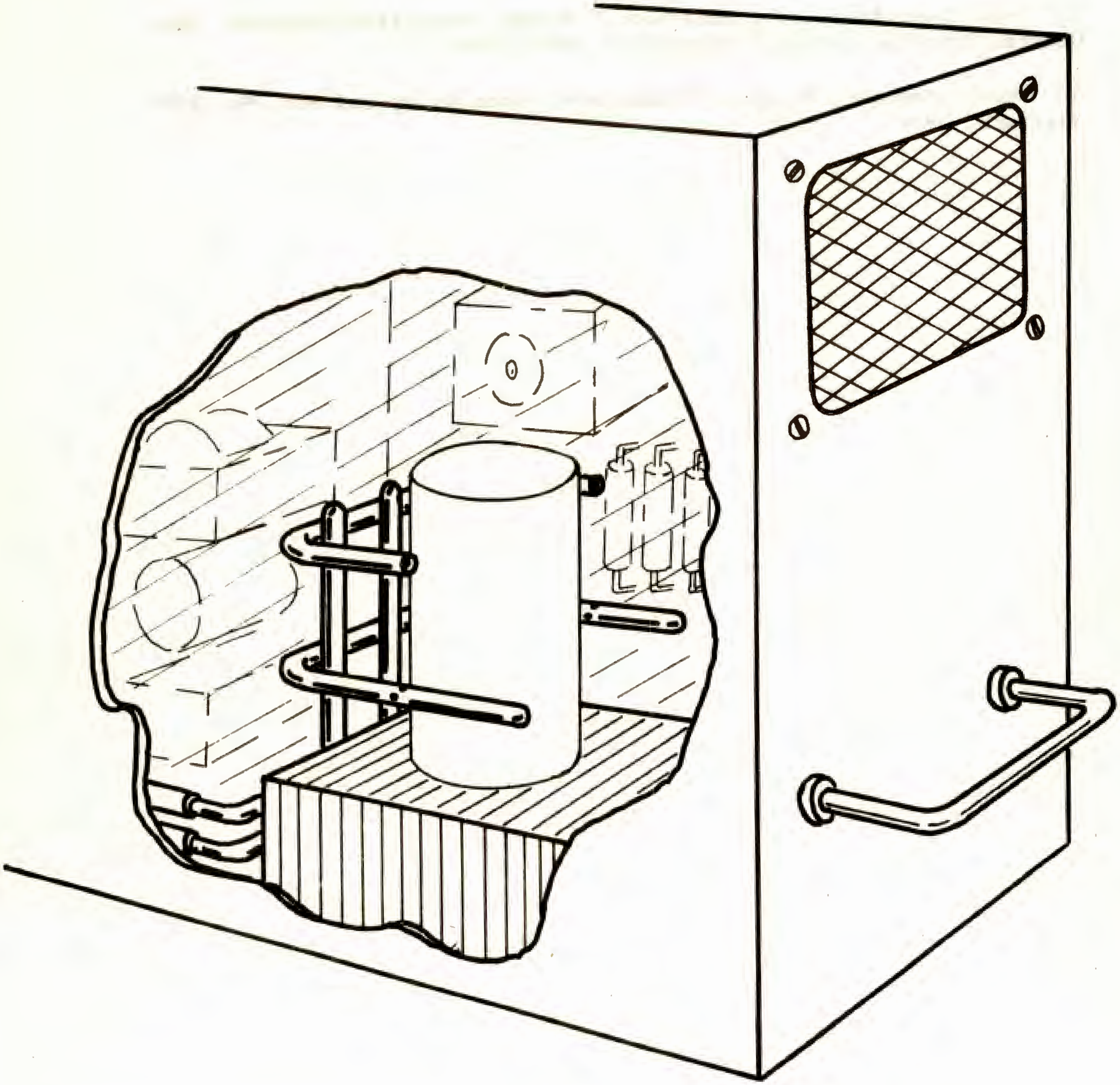

Figure 1. Pitot-Static Rake to Measuring Cooling Flow Over Component.

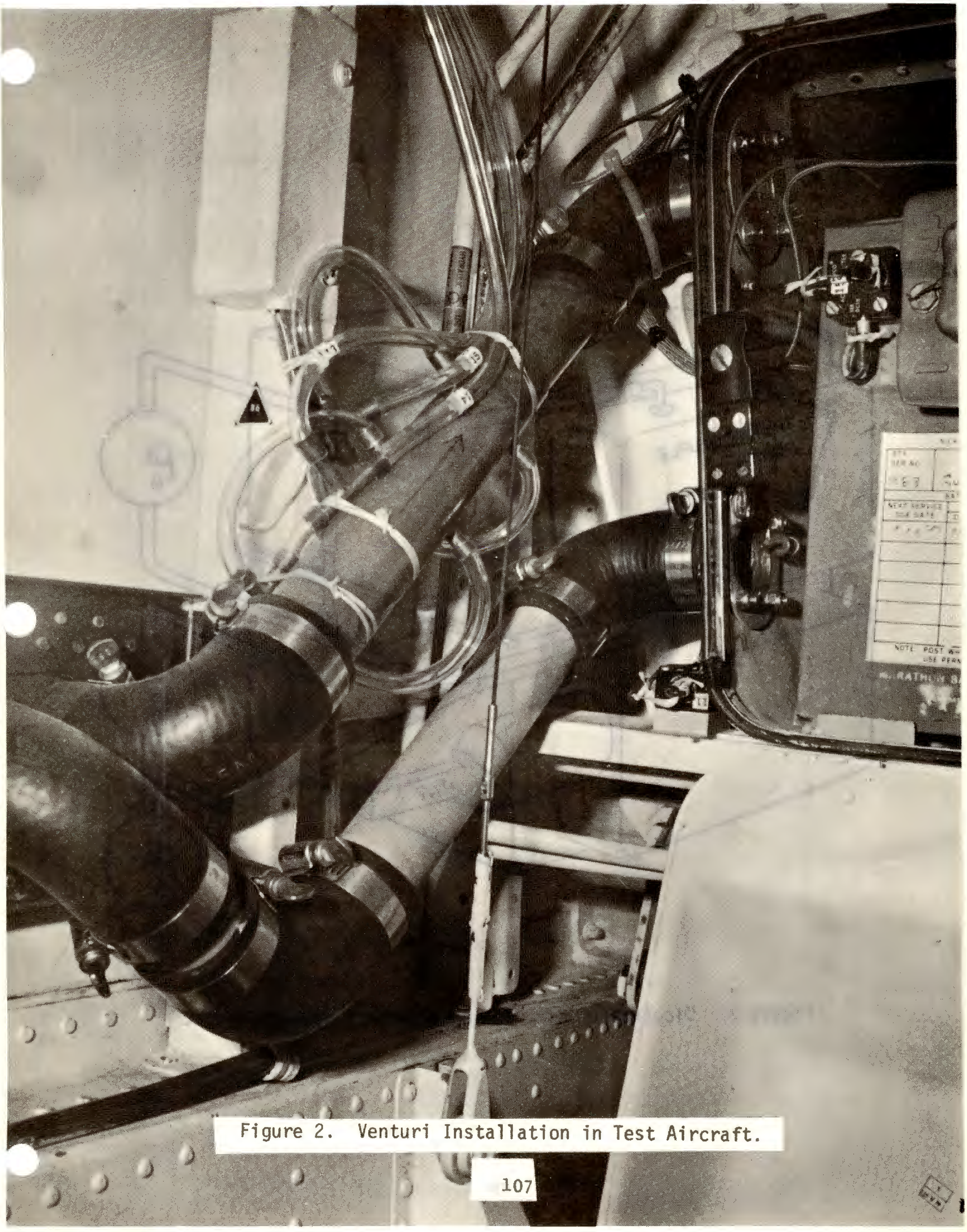

Figure 2. Venturi Installation in Test Aircraft.

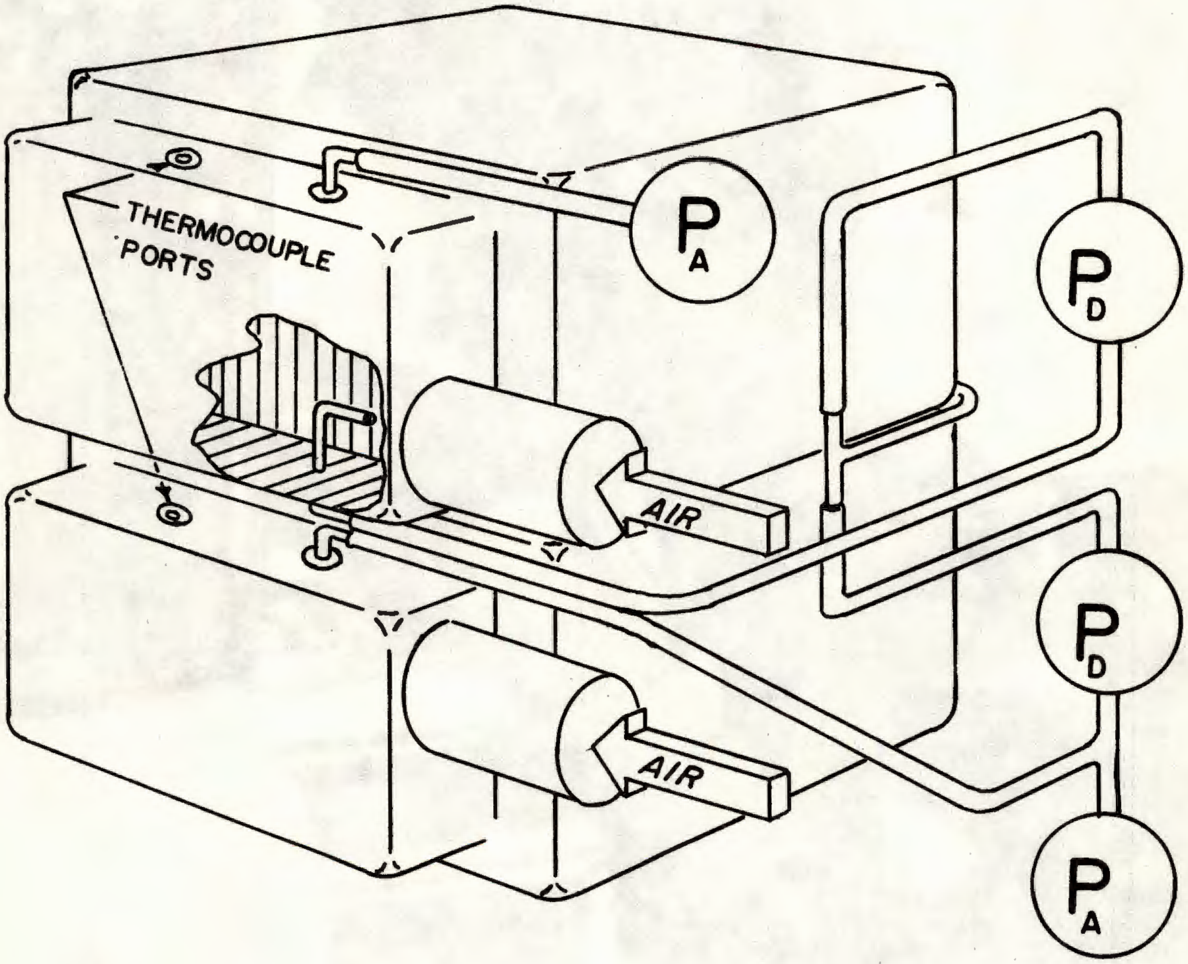

Figure 3. Electronic Unit Cooling Flow Instrumentation.

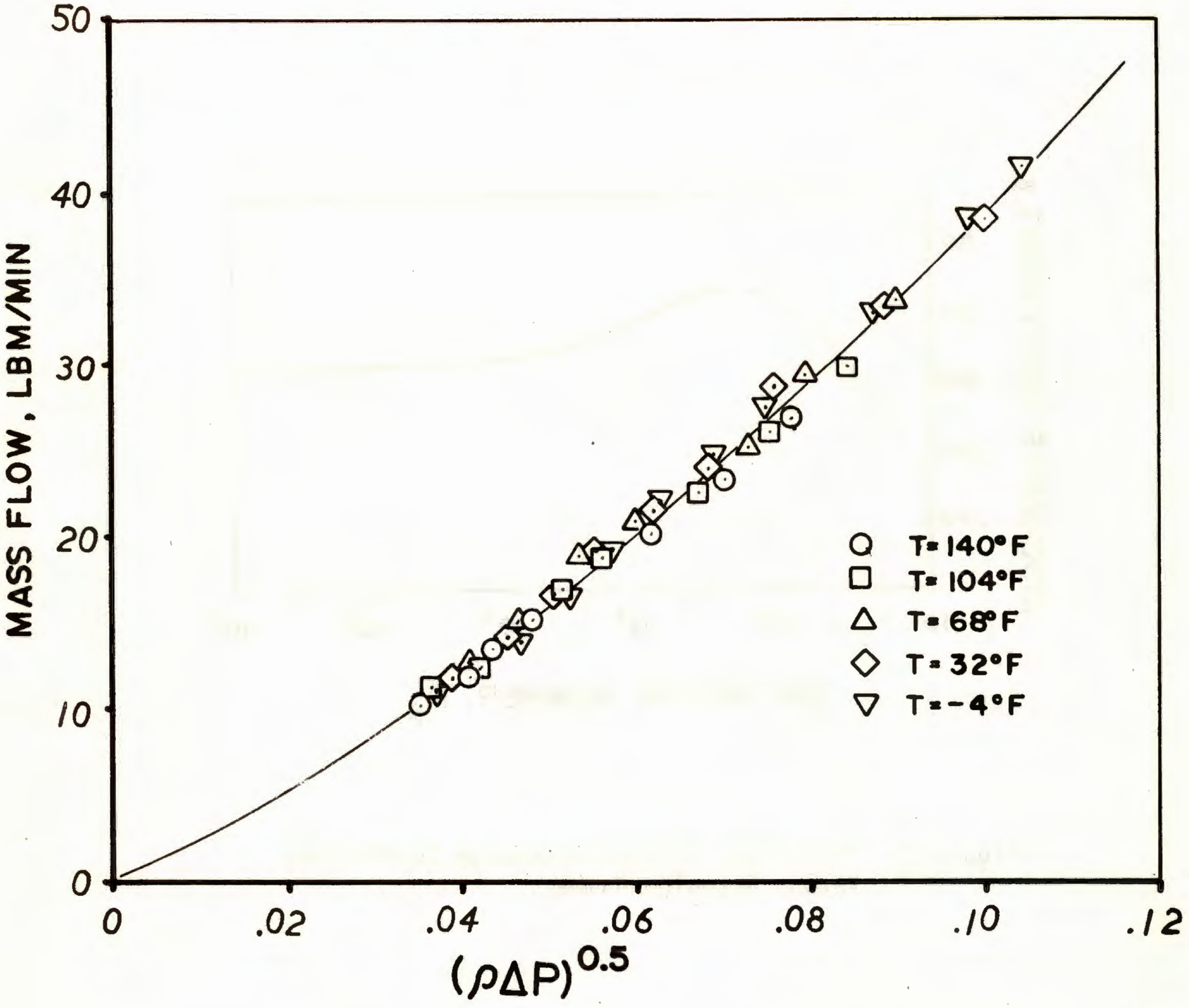

Figure 4. Mass Flow Calibration, Typical Presentation.

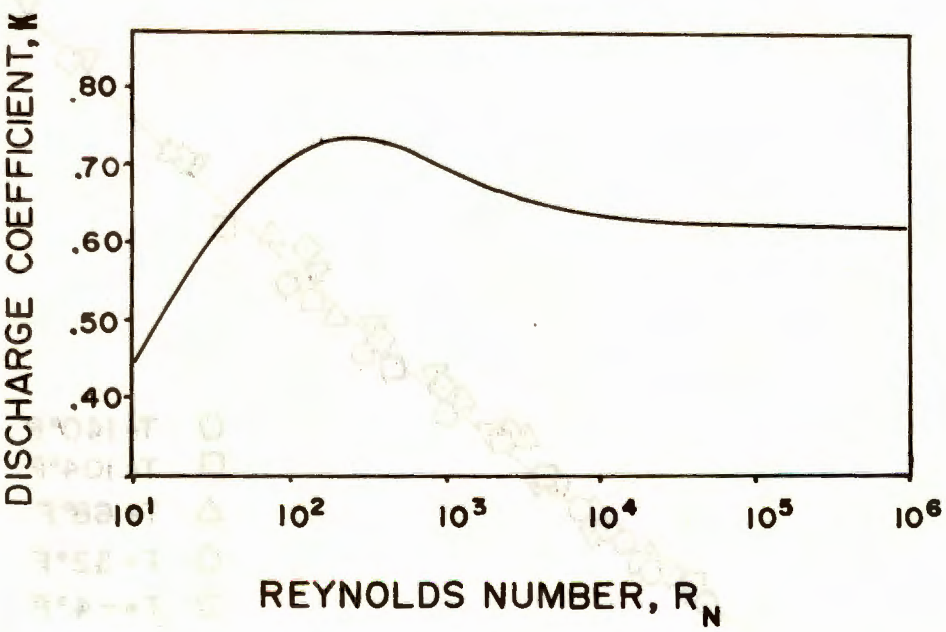

Figure 5. Thin Plate Orifice Discharge Coefficient Versus Reynolds Number.

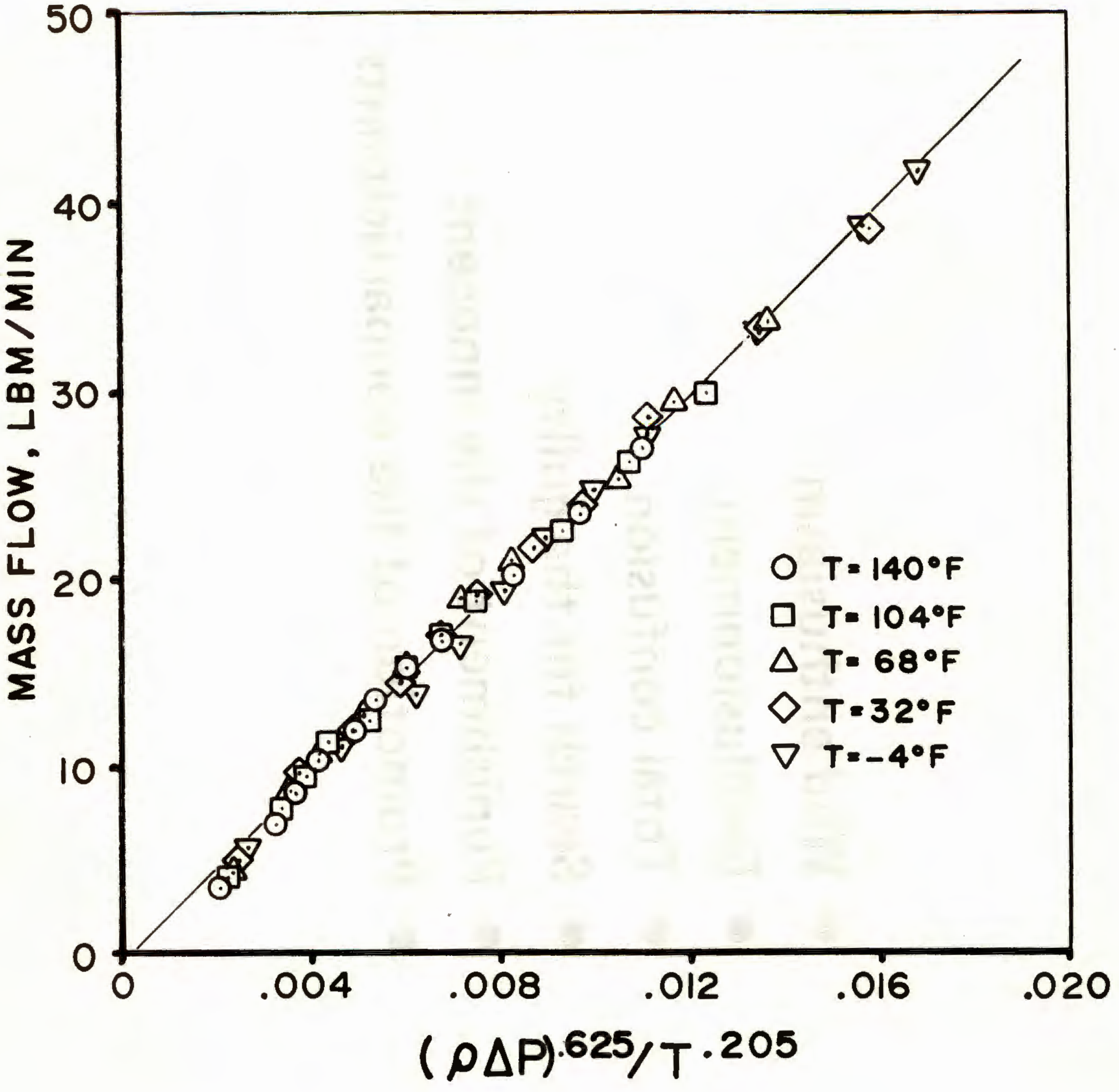

Figure 6. Mass Flow Calibration, Pressure Parameter.

THE SIX PROJECT STAGES

- Wild enthusiasm
- Disillusionment
- Total confusion
- Search for the guilty
- Punishment of the innocent
- Promotion of the nonparticipants

GUIDE TO EMPLOYEE PERFORMANCE APPRAISAL

	Degrees		
Factors	Far exceeds job requirements	Meets job requirements	Needs some improvement
Quality	Leaps tall buildings with a single bound	Leaps over short buildings only	Cannot recognize buildings at all
Timeliness	Is faster than a speeding bullet	Would you believe a slow bullet	Wounds self attempting to shoot
Initiative	Is stronger than a bull	Shoots the bull	Smells like a bull
Communication	Talks with the angels	Talks to himself	Argues with himself; loses
Adaptability	Walks on water in emergencies	Drinks water	Passes water in emergencies

SESSION I

DISCUSSION SUMMARY

Session Chairman: Pierre Fuselier

Papers: Hasbrouck, Brandt, Burger and Holten, Crosby, Mertaugh

DISCUSSION:

Alan Holmes, Lockheed Missiles and Space Company: My questions are directed at Mr. Crosby. It seems to me that downwash is the big problem. If that's the case, is it totally out of the question in evaluating an instrument to put the gages out of the downwash? That's question 1, and secondly, anemometers are good low speed devices and generally speaking they do confine measurement at least to a plane. You could, having three of them oriented at various axes, probably resolve your total effective motion. Have these things been considered and if not, what do you intend to do?

Thomas Crosby, Naval Air Test Center: The problem has to do primarily with structurally unsound locations. The downwash tracks the speed of the helicopter in the low region, and that's the principal we're using. It's actually measuring the downwash. As far as the anemoters, I think it might be a structural problem to mount them. Any measurements you make on a helicopter, you're measuring local air flow.

James Rieger, Naval Weapons Center: Question for Mr. Brandt. You said that the pressure frequency response ends at about 3 to 4 kc. I'm wondering if that's a characteristic of the thing you're measuring with or the thing you're measuring. And how do you know?

William Brandt, Boeing Commercial Airplane Company: With flush diaphragm, flush mounted transducers you get flat frequency response to one-fifth the natural frequency. When you use transducers that have tubulation, frequency response becomes a function of the tubulation and we have to tune the tubulation.

Robert George, AMRDL, Ames Directorate: Our interest is in helicopter research and my background is in wind tunnel application. Much like Bill (Brandt), I'm interested in pressure dynamics using pressure transducers. We have used the same transducers Bill used, the difference being I've used surface mounted and the little canister type. We have found that the small tubes are frequency limited. The size of the container and the size of the transducer, the length of the tube and the diameter of the tube, are all factors. After installation the dynamic frequency of the transducer with the tubing attached is about 1,000 cycles. So you figure 20-25% of the resonant frequency is the limiting point we want to measure out to. You have to know the frequency of the thing you're trying to measure before you apply the transducer. Because the transducer is limited, you have to know its limitations so you can tailor it to your needs.

Paul Lederer, National Bureau of Standards: Question for Mr. Brandt. How do you experimentally ascertain the acoustic frequency response of these transducer installations?

Brandt: Our method is to build a pneumatic model of what we are using in the test, and subject it to an air jet. I might say where double tube transducer technique is generally used, back-to-back measurement can't be used. A finite amount of room is required for back-to-back. When we have to get in closer quarters we use tubulation. Needless to say that method cuts the frequency response. With flush mounted transducers our problems with frequency response generally aren't there. When you use tubulation, you have to worry about it.

George: Bill (Brandt) said that he calibrated with water method. Another method is a flight test program where you don't have access to the transducer, in the laboratory sense, and you want to determine frequency response of the transducers installed. You know what the resonance frequencies are, you can test out your technique on a flush mounted transducer so you can actually determine your acoustic profile. Use an unknown transducer, say tubular mounted with tubing as short as possible, and your opening as large as possible. Attach a pump and pump it up. A strip chart tape recorder analyzer actually will get signature profile of the transducer installed in place.

Lawrence Mertaugh, Naval Air Test Center, to Brandt: Do you attempt to measure phase angles in your frequency response testing?

Brandt: Yes, we often do in two areas. We use delta phase measurement when phase relationships of both sides of the transducer have to be the same. We balance the pneumatic pressure on both sides using two different methods. Blowing an air jet is a fast easy way of determining the natural frequency of the pneumatic system. If you get the same resonant frequency on both sides you will find that the phase shift inside is the same. We found proof of that by putting the system model into the chamber and then pulsing the chamber with a pressure and observing that the output of the delta pressure transducer remains at zero. That's one phase relationship we have to consider. The other is where we've got more than one transducer in the air flow. The phase relationship between one transducer and the others also has to be the same.

Peter Hsu, EG&G Idaho: I'm particularly interested in work Mr. Brandt is talking about, small pressure transducers. We do a lot of experiments inside of reactors, usually at 650°F and 2200 psi. The question is, what's the possibility of using the fused silicon?

Brandt: Off-the-shelf transducers usually have an upper limit of 250°F, I believe. However there is a high temperature transducer that is generally available from the suppliers, I think it's around 550°F. So we're getting close, but we aren't there. What I would suggest to you is to look at the infinite tube technique for measuring these dynamic pressures. I'm no expert on this particular subject but I'll tell you what I do know about it. It allows you to locate the transducer at more remote locations and maybe offers you the chance to cool the transducer and still make your dynamic measurements. It will probably involve a correction once you make the measurement, but I would think maybe infinite tube techniques might work for your application.

Walter Kistler, Kistler-Morse: What is the pressure range of these transducers and what is the lowest full scale measurement?

Brandt: The transducers that we generally use in this delta "P" application are rated at 10 psi. We don't use them at that high a range but that's what their upper range is. The sensitivity of these devices runs around 20 mv/psi. The smallest pressure range that comes to mind is a test which required pressure readings at around 3 to 5 thousandths of a psi. And it really adds up to how quiet is your recording system. You can get down into the small thousandths of a psi for sure. But just how far you can go down I think is really a matter of where is your noise barrier.

George: (Comment) I don't want to keep talking along the same line, but I know this application with small transducers and I just want to point out one other factor that I found to be a problem area and you don't want to be led down the primrose path. You not only want to tune the transducer, you want to use both sides of it and be cognizant that there is a frequency problem. You have problems with electronics also. You have to use linear phase amplifiers. Otherwise you're really sending up a lot of smoke. As soon as you start making comparative measurements, even if it's FFT or computation where you're trying to find functions or cross correlations, auto correlations, you've got to have equipment in phase, otherwise you don't know what you are doing.

Pete Stein, Stein Engineering Serv. Inc.: I have a comment for Tom Crosby and a question for Dave Holten. Tom, you made the comment at the end of your paper that it is a great value to be able to calibrate a test in a low speed wind tunnel and that you use the NBS tunnel. Is that right?

Crosby: Yes.

Stein: You're aware that it has been declared surplus and is about to be dismantled. I'm making that comment in preparation for the discussion Thursday morning, when Dave Goldman, the Deputy Director of the Institute for Basic Standards will be here to explain some of the changes that have occurred in that Institute. The fluid dynamics section effectively will be nonexistent after this year. You may want to make some inputs there. Now the question I have for Dave Holten. Your prime interest was the safety of the pressure transducer. What does that mean in sacrifice or in gain of things like elastic modulus, linearity and hysteresis in the material that you ended up instrumentating as pressure transducers?

Holten: We have generated a set of specifications which address both electrical and mechanical sides of the unit. The inaccuracy of the unit is 1-1½% full scale including temperature. It operates strictly in the linear range. The hysteresis of the instrument is extremely small. I guess I can quote room temperature error band typically as a few tenths of a percent. Ray Cornell operates the pressure calibration laboratory and probably knows.

Ray Cornell, Lawrence Livermore Laboratory: The error band is on the order of 2/10 of a percent.

Holten: The angle we're trying to emphasize is that we go to great lengths to insure the structural integrity and the pressure measuring diaphragm itself.

Stein: Did those two materials that you tested turn out be very good transducer materials? They are perhaps not very widely used as yet, especially those steels you mentioned.

Holten: Yes, we have been using these types of transducers for about 16 years. We have gone through a number of different materials, 304, 316 and stainless steel, A286, several alloys that manufacturers presented to us and we would evaluate them. We put them through a pretty hard test series, and if they fail we reject them. We are still finding some that fail. We are very happy with veryllium copper and with 21-6-9. 316 is still a very good material also, I might add.

Fuselier: (Comment) I'd like to comment on that. I essentially agree with him. The transducer manufacturers favor the material 17-4 PH.

It has a lot of good things going for it. It's probably capable of slightly better performance than these other two materials. There is only one problem as far as we're concerned, it's totally unsuitable because of hydrogen embrittlement. In selecting a beryllium copper alloy and steels such as 316 or 21-6-9, the chemistry is specified to .01% or less for every constituent. It's manufactured under rigid control, certified every step of the way all the way to the laboratory and put in bonded storage in the laboratory. So you don't just go down to the local hardware store and get a bar of 316.

Holten: This is not done for properties, but for integrity with the various gas media.

Binneg Lao, Bendix, to Holten: What is the thermoelastic coefficient?

Mike Burger, Lawrence Livermore Laboratory, to Holten: I think I understood him to say how does the modulus and the elasticity change with the temperature? Well, it doesn't change very much, certainly it doesn't change at all in the region we're operating in. We are operating essentially at room temperature or maybe 100-160°F. But essentially very moderate temperatures and down to maybe zero. So we're not talking about a wide temperature range. However 21-6-9 is a very good material. Also very expensive to use in a wide range of temperatures. It all depends on what kind of treatment that steel has had. If it's been forged as ours has it will have a very high elastic modulus and therefore it becomes more brittle. However, if you don't need such a high yield point, you can operate on a much wider range in temperature.

LAO: Does the modulus of elasticity and coefficient of thermal expansion affect the transducer performance?

Burger: Well the first question: The modulus of elasticity in the range that we're operating doesn't change at all. The other question is, Yes. The characteristics of the transducer will definitely change with temperature. But you compensate for the temperature change. All you're talking about there is the linear relationship with temperature. You're talking about a range of the coefficient of thermal expansion between 0 and 160°. If you're talking about a temperature range higher, then you have to know what the coefficient of thermal expansion is and it's not necessarily linear, but you can correct for it. It's a known characteristic of the material. Incidentally, those properties can be included in the analysis, as part of the analysis, so that when you're doing a finite analysis you'll find pressure, temperature, and your final result all in one analysis.

Alan Holmes, Lockheed Missiles & Space Co., to Burger & Holten: You gave an ultimate strength. What elastic limit do you apply?

Holten: We generally end up by analyzing those designs presented by the manufacturers. We do like to get at least a safety factor of 2. So that means we are probably operating half way up to yield point. Of course it gets worse in the higher pressure range. Our highest range is 20,000 psi and we have barely a safety factor of 2. The 500 psi transducer has a very healthy safety factor of 4 or 5.

Holmes: I guess you don't understand the point of my question. You are talking about yield. That may concern me when it starts to fail, but on a lot of these materials the full stress-strain curve is a gently curving line rather than the sharp line you get from mild strain. In such cases there is quite a distinction between what we call the yield point, which is taken at 2/10's of a percent offset, and the elastic limit which is the point where you have filter. I was just curious whether you could give me some approximate number for that elastic limit.

Burger: I think it's probably around 60,000 psi for 21-6-9. You're right, there is a proportional limit, it starts to change around 60,000 psi. Variation is small, you might be able to live with the variation. It's going to be very small. If you're really interested in knowing what the calibration is, if it's a nonlinear calibration at that point, you can handle that as I said with the nonlinear analysis.

Norman Muelleman, GARD Inc.: A question for Mr. Hasbrouck. I'd like to have comments on accuracy numbers that have been generated and how pressure transducers are being used. What was the final accuracy of the transducer being used down hole?

Richard Hasbrouck, Lawrence Livermore Laboratory: The transducers that are finally being considered are the ones that are ostensibly ± 1% transducers. You want your ideal standard to be 10 times better if you can get it. We are pushing that. We will take a real good look at a stack of ± 1% transducers, if we can qualify one or two out of the batch of 10 that exhibit much better characteristics, we will select those units and make them transfer standards. These standards come up with the final numbers which are having typically ± .2% as total error band. The other option is to go to the manufacturer and say "build me a secondary standard".

Muelleman: You're measuring static pressures in radians, very accurate. But are you recording signals down there?

Hasbrouck: Our particular application is for static pressure. There is a need to record a time pressure signature, but this is a fairly long time. Our interest is in knowing our static pressures are fairly accurate.

Bill Anderson, Naval Air Test Center: I have a question for Richard Hasbrouck, how about field calibration?

Hasbrouck: The transducers that we purchased for our normal field applications include a shunt calibration feature producing a signal of 50% of full scale output. You have to know what it means. That gives you a fair check on how well the transducer is working. In our case it's a ½ scale, with zero measurand applied. You don't imply a high degree of accuracy.

Mike Obal, Wright-Patterson AFB: Another question for Dick. Given a 95% confidence band, I understand what they are but I didn't understand why they were off center.

Hasbrouck: You were talking about that one plot, error versus pressure? Normal distribution curves?

Obal: Yes.

Hasbrouck: My particular approach is to look at statistical things. I consider each cardinal point, apply pressure, and get 20 samples over a period of time. That would indicate data scatter...and the data scatter followed a normal distribution about that point. If I apply an unknown pressure to this gadget, I would like to know if the number says 117 psi. How well can I believe that? Is that 117 ± 1 or ± 10 psi? That was the intent of getting all this data.

Jim Miller, Army Calibration Center to Hasbrouck: I wonder about the pressure range of your instrumentation, is it all one pressure?

Hasbrouck: No, we have 0-50 to 0 to 20,000 psi transducers. They are all utilized. I had a complete set of data available on it. The higher range error bands tend to be somewhat larger. Somebody asked about hysteresis effects. The computer gave a print-out for all of the 20 calibration points giving you a brief look at just how the transducer did respond to increasing and decreasing cycles of pressure. It looked remarkably good. The error band ± the hysteresis was very small, 2% maximum error band and .01% —.02% hysteresis. Kind of gave you a good look at how transducer performed over a period of time.

Miller: How was the data obtained?

Hasbrouck: Unfortunately, at the time it was taken it was logged manually on the Gilmore or Ruska. If it was a Ruska 40,000 psi standard they have to hand load the weights. Now if it was Gilmore 0-10,000 psi it is automatic, but they did not at the time have provision for taking BCD output. It's there, but they hand logged.

Roger Noyes, EG&G Las Vegas: For Mr. Brandt - In the calibration of your differential transducers, you said that they were typically 10 psi full scale. A little later on I heard you mention that you're measuring a few hundredths psi or a few thousandths psi. Is that differential pressure that you're measuring? If so, how do you take care of the non-linearity and hysteresis? Which is probably typically 1% of 10 psi? How do you calibrate around that?

Brandt: We do indeed have 10 psi transducers. We rarely calibrate over a 10 psi range. We determine the sensitivity over a 1 psi range which would be a high level of pressure, oftentimes 1/10 of a psi full scale. Hysteresis turns out to be far less than 1% of full scale. With these silicon deposited transducers, I'm reluctant to even state a value for the hysteresis. They are very good. The linearity is probably the major concern, and over a small range is very linear.

Noyes: What happens if you get two transducers that have slightly different temperature directions?

Brandt: You'll generate a number. This will certainly add to your error. The effect of temperature on this transducer sensitivity has been much improved in the past year. Over a 10-25°F temperature interval, the error is pretty negligible. It can be corrected, and it is repeatable.

Fuselier: Was the thin film transducer you used an evaporated or sputtered type?

Brandt: I'm not an expert on manufacturing these transducers, but it is actually a silicon diaphragm, extremely thin. The sensing element is deposited on the diaphragm. It is an extremely thin diaphragm with very low mass.

Fuselier: Bill, can you give us a manufacturer and model number?

Brandt: Certainly, Kulite, Endevco and Entran are manufacturers.

Lederer to Brandt: In this technique you mentioned for determining the acoustic response, you essentially looked for the resonant frequency of the system, and then you use this technique up to a quarter of that frequency. Correct me if I'm wrong, but it seems to me that resonant frequency is a function of the density of the air or gas as well as the temperature. What is the effect of your operating conditions on the value of that resonant frequency? What do you do about it?

Brandt: Our testing is generally near or at sea level conditions so we don't do anything about it. Correct, resonant frequency is influenced by density. In our particular applications we don't make any corrections for density changes.

Don Vollmer, Lawrence Livermore Laboratory: Mr. Brandt, will you tell us what excitation levels you use?

Brandt: I'll stick my neck out here a little bit. I don't have a current catalog for all these companies, but the nominal sensitivity that one runs into from off-the-shelf transducers is around 3mv/psi and they require an excitation voltage of around 7-10 volts. We have pushed vendors into supplying transducers with higher outputs. We apply 20 V excitation voltage on most of our transducers.

Vollmer: What would be the typical resistance value?

Brandt: Output impedance is 1500 ohms or smaller with 1,000 ohms being nominal. The input impedance is around 2000 ohms.

Holmes: Question for Bill Brandt - I gather from what you are saying these are basically semiconductor devices, where the diaphragm is part of the strain gage and the configuration is a full bridge.

Brandt: Yes, as far as I know it is a full bridge. Their output is somewhere between 3 and 20 mv/psi. We're pushing up the scale and have actually had a prototype to 50 mv/per psi. That gets into a region where you can start using these pressure transducers as a microphone.

Henry Freynik, Lawrence Livermore Laboratory for Holten: What is the design strain in the diaphragm and what kind of strain gages do you use? Are they metal foil or deposited metal or semiconductor? The third question is how do you balance the strain in the diaphragm versus the output you're trying to achieve?

Holten: First, we're basically analyzing the manufacturer's design. Each seems to require a different strain. The ones that use semiconductors, if the semiconductor is not directly coupled to the flexing metal diaphragm, they seem to like strains up to 6-7 tenths of a mil. If the pick-off is connected directly to the diaphragm, it appears again from my observation that they can live with much less deflection, perhaps 2 or 3 tenths of a mil. We don't dictate one over the other. What it comes down to is whether or not the transducer meets performance specifications.

Jim Birdsall, Pratt Whitney, to Hasbrouck: On our transducers we almost always experience a long-term zero shift. How long do you expect a box to go without recalibrating?

Hasbrouck: We expect the zero shift to be normal. We haven't determined long-term zero shift.

SESSION II

TRANSDUCER SIGNAL CONDITIONING AND GENERAL TRANSDUCER TOPICS

Steve Rogero, Chairman

CLOSE-COUPLED STRAIN GAGE SIGNAL CONDITIONERS

by

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Presented to the

NINTH TRANSDUCER WORKSHOP

Transducer Committee of the Telemetry Group
Range Commander's Council

CLOSE-COUPLED STRAIN GAGE SIGNAL CONDITIONERS

David J. Ray
Air Force Weapons Laboratory
Kirtland AFB, NM

ABSTRACT

The low level outputs of structure strain gage bridges, even when configured in double active arm schemes, may contribute little to understanding structure response if these outputs must be recorded at great distances. Noise levels over great distances, combined with distortion caused by limited bandwidth, may serve to obfuscate the data beyond usefulness. These problems have been faced by the Air Force Weapons Laboratory (AFWL) on many occasions, but the most severe test to date was the 6 October 1976 Dice Throw high explosive event at White Sands Missile Range. The Air Force Weapons Laboratory made structure strain measurements on scaled model aircraft shelters over 6,000 feet (1,829 meters) of cable.

The approach used to solve this problem was to develop and use close-coupled strain gage signal conditioners on 1 1/2" x 2" printed circuit boards placed inside the structures. These conditioners provided the following features:

1. Bridge completion
2. Regulated bridge power
3. Signal amplification
4. Bridge calibration
5. Bridge balance

To insure compatability with standard voltage-controlled oscillators in the recording vans, a special interface circuit was also developed.

This paper compares the model of normal strain measurement techniques with the model of the AFWL-developed technique. Further, the close-coupled strain gage signal conditioner is explained in detail. Its advantages and disadvantages are discussed, along with practical field applications and problems. Sample "Dice Throw" channels are presented.

INTRODUCTION

The necessity for making remote measurements of physical phenomena may be dictated for reasons of safety for personnel and equipment. Such is typically the case when making measurements of structure response to air-blast and ground shock in explosive tests.

The Air Force Weapons Laboratory, located at Kirtland AFB, NM, participated in the Defense Nuclear Agency-conducted 500-ton "Dice Throw" explosive test on 6 Oct 76 at the White Sands Missile Range. The test objective was to observe the structural response of various aircraft shelters to blast and shock inputs. The test required a large number of strain information channels on steel reinforcing bar, steel doors, and metal structure components.

Preliminary instrumentation system designs revealed problems associated with the large distances required for equipment safety. The test required that strain measurements be made at a distance of 6,000 feet (1,829 meters). Even at this distance, personnel safety required that the recording vans be remotely controlled from an even greater distance.

With this line length, great concern centered on the use of strain gage bridges with attendant large line resistance, capacitance, and induced noise. The line resistance and capacitance, combined with the 350 ohm strain gages, would reduce the overall system frequency response to approximately 1.6 kHz. The noise pickup of such long line lengths would obfuscate data and certainly cover up any underrange signals.

The approach used to help alleviate such problems was to develop what is referred to here as "close-coupled" strain gage signal conditioners.

THE CLOSE-COUPLED SIGNAL CONDITIONER

Recent advances in integrated circuit (IC) technology have produced such devices as the instrumentation amplifier on silicon chips that measure approximately 2 mm x 3 mm and are mounted in standard 14-pin dual inline packages.

For proper operation, the resistance bridge transducer requires power, a high input impedance double-ended instrumentation amplifier and, in many cases, a shunt calibration. A device to perform these three operations is referred to in this paper as a signal conditioner. Figure 1 is an example of a signal conditioner that operates with a single active arm bridge. The bridge completion may or may not be part of the signal conditioner.

If the long line parameters are lumped and included with the transducer, figure 2 is an appropriate model. Distortion caused by limited system bandwidth may or may not be a problem depending on the data frequency content. Attenuation and noise may cause the effects indicated in figure 3. The long line effects vary in degree depending on the cable and the strain gage resistance. Reference 1 provides detailed analysis of the causes and counter measures of long line effects.

With this background, the Air Force Weapons Laboratory exploited the preliminary development work it had already done on miniature signal conditioners to produce the circuit of figure 4 on a 2 1/8-inch (5.4 cm) by 1 1/16-inch (2.7 cm) printed circuit board. The circuit can easily be understood if it is analyzed in three parts. The components associated with VR1 and Q2 form a simple voltage regulator to provide bridge power. A1, and those items connected to it, is the instrumentation amplifier. The gain is fixed at about 30. Amplifier A2 is simply a positive feedback comparitor which provides a sharp turn-on to switch Q1 after the calibration trigger pulse is severely rounded by filter R17 and C4. This filter is provided to prevent spurious noise pulses on the calibrate line from initiating unwanted calibrate turn-on's of Q1. The calibrate line is normally left open until calibration is desired, and it is then connected to the -15 volt supply to initiate turn-on of Q1.

THE FIRST FIELD TEST

The first large-scale use of this signal conditioner was on the Dice Throw high explosive test. Figure 5 shows the test configuration for the AFWL portion. The Sample Aircraft Shelter was also used to house the close-coupled signal conditioners. The cable used was buried for the run from the shelter to the junction box (approximately 330 meters). Power for three strain gage bridges was provided over one pair of wires and the shield was used for common. Three wires were then used for the three signals, and one wire used for the calibrate signal for the three signal conditioners. Since the driving impedance is now 10 ohms, the system response is 4.3 kHz. Figure 6 is a diagram of this wiring. Each bridge (double active arm) was powered by 15 volt regulated. The normal practice is to run four (sometimes six) wires to each set of two gages. In that case, for each three sets of measurements, twelve wires would be required. With the close-coupled signal conditioners, however, only seven were required (six plus shield). This resulted in a savings of over 500,000 meters of wire since 143 channels were required. Among the instrumentation officers involved, there was a decided preference for not using the shield for the common return line; however, lack of available cable necessitated this practice.

The close-coupled signal conditioners required that an interface circuit be used to set correct levels into the voltage-controlled oscillators (VCO's) in the recording van. Figure 7 diagrams the circuit used. It consists of an integrated circuit operational amplifier in the inverting configuration with offset and gain adjustment.

The VCO's and all subsequent recording and data reduction procedures provided at least 5 kHz data bandwidth on each channel.

TEST RESULTS

Of 143 strain channels assigned, 139 were operating during the test event. The following analysis includes what were judged to be some of the worst and some of the best data as regards to noise content. The calibration levels are within a few percent of the predicted values, and the VCO bandedges were set at twice the calibration levels.

It is obvious from the data that the worst signal-to-noise ratios were on those channels that were grossly overpredicted. Figure 8 shows three examples of such channels. For these three cases (channels 499, 500, and 506), the calibration levels were 16,755, 16,172, and 16,132 microstrain. The peak signal levels actually encountered were approximately 1,800, 2,000, and 500 microstrain, respectively. Noise spikes occurred on all data channels at 102 and 175 milliseconds.

If the overpredicted signals have the worst SNR, then the underpredicted signals should have the best. However, if the signal level exceeds that required to drive the VCO's to bandedge, the data are of less value. Figure 9 presents channel 459, which was underpredicted, but within bandedge (1,511 microstrain predicted). Here, the signal reached approximately 2,800 microstrain. Samples of those signals, which were within fifty percent of the predicted value, are shown in figure 10. These are channels 486, 489, and 502.

CONCLUSIONS TO DATE

Due to heavy test commitments, the field crews were not exposed to the miniature close-coupled signal conditioners until they arrived "on site" some four short months before the test date. In this time they were, however, able to learn, hook up, check out, and adjust the 139 operable strain gages, conditioners, and van interface circuits, in addition to the over 170 other channels of blast pressure, acceleration, and velocity measurements required on the test.

Certain structure components were found to have resonances in excess of 2 kHz. The amplitude of these resonances was clearly measurable with the close-coupled conditioner and most certainly would have been distorted by connecting the 350 ohm strain gages directly to the long lines.

Through an oversight, no strain channels were conditioned and recorded directly over the long line with standard van conditioning. Therefore, no direct comparison is available. However, presuming that the continuous noise levels of 17 sample channels are the same as that on the long lines, then the ratio of noise amplitude to 100 percent bandedge level varies from 0.5 percent to 1.8 percent. This is somewhat optimistic in that it does not count the obvious noise spikes on the data.

The use of the cable shield for the common return line certainly increased the probability of ground loop and noise pickup. As far as is presently known, this may have been the source of obvious electrical interference in the data. This unfortunate choice was, as indicated earlier, made due to lack of available cable.

The voltage drop over the long lines to the signal conditioners was compensated for by using adjustable voltage supplies. Part of the design used the same supplies to power the van interface circuits. The amount of long line drop was unfortunately underestimated and, in order to achieve the normal voltage at the close-coupled conditioners, the voltage ratings of the operational amplifiers in the van circuits were in some cases exceeded. This caused several failures of the van interface circuits until the problem was discovered. When this was realized, it was a simple matter to run the close-coupled conditioners at a reduced, but acceptable, voltage.

The use of this new technique was successful in both the data and the learning experience. This experience once again proved that laboratory testing is not a complete substitute for field experience.

ACKNOWLEDGEMENTS

The useful test data and new knowledge of close-coupled signal conditioning techniques were made possible only through the long hours of toil provided by TSgt Jerry Nixon, SSgt Jay Hum, and the other field crew members of AFWL/DE who provided considerable extra effort on this test.

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1. Ray, D. J., "Self-Conditioning Transducers," Instrumentation Technology, September 1976.

2. Carlson, A. B., Communications Systems: An Introduction to Signals and Noise in Electrical Communication, McGraw-Hill, New York, 1968.

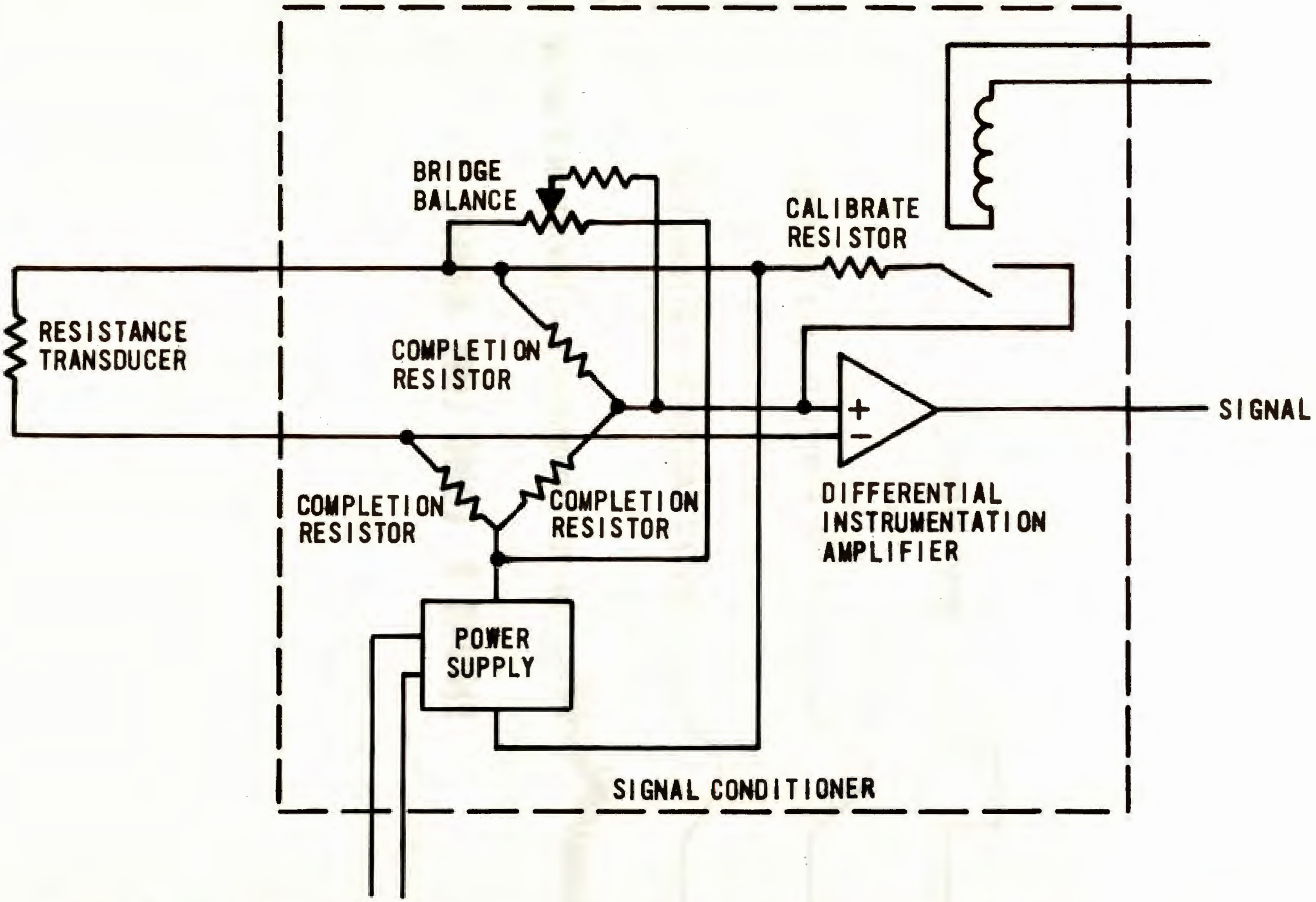


FIGURE 1 RESISTANCE TRANSDUCER WITH TYPICAL SIGNAL CONDITIONER

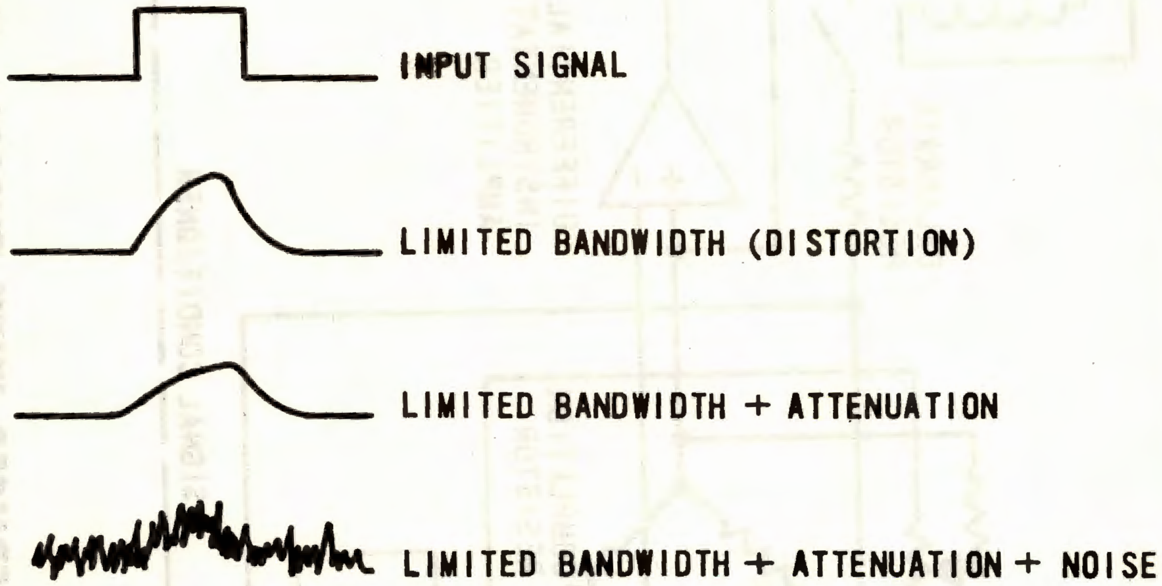


FIGURE 2 LONG LINE MODEL

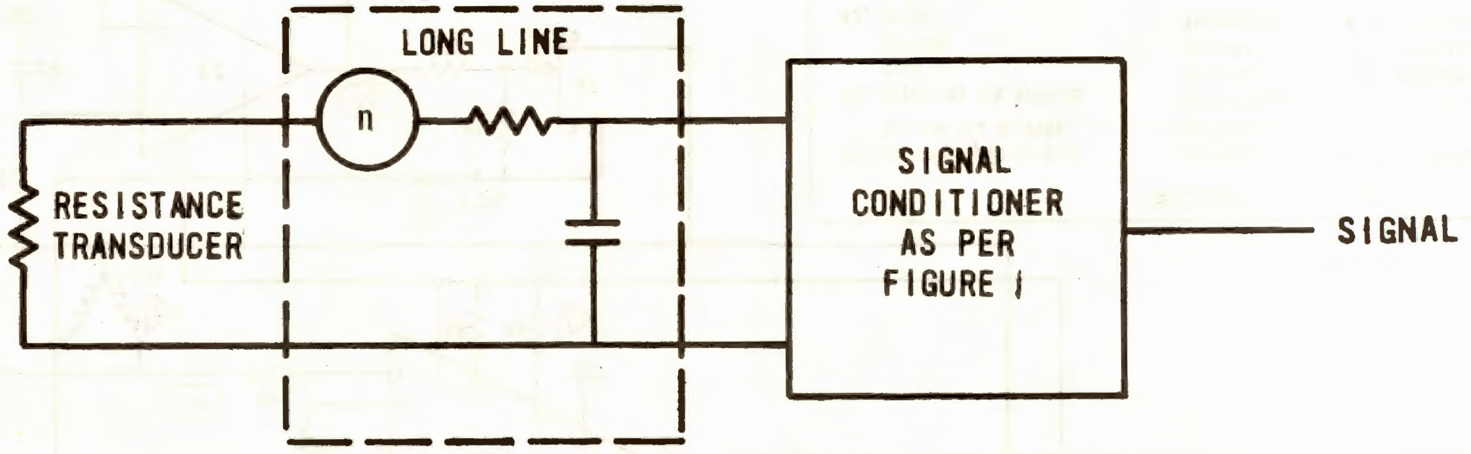
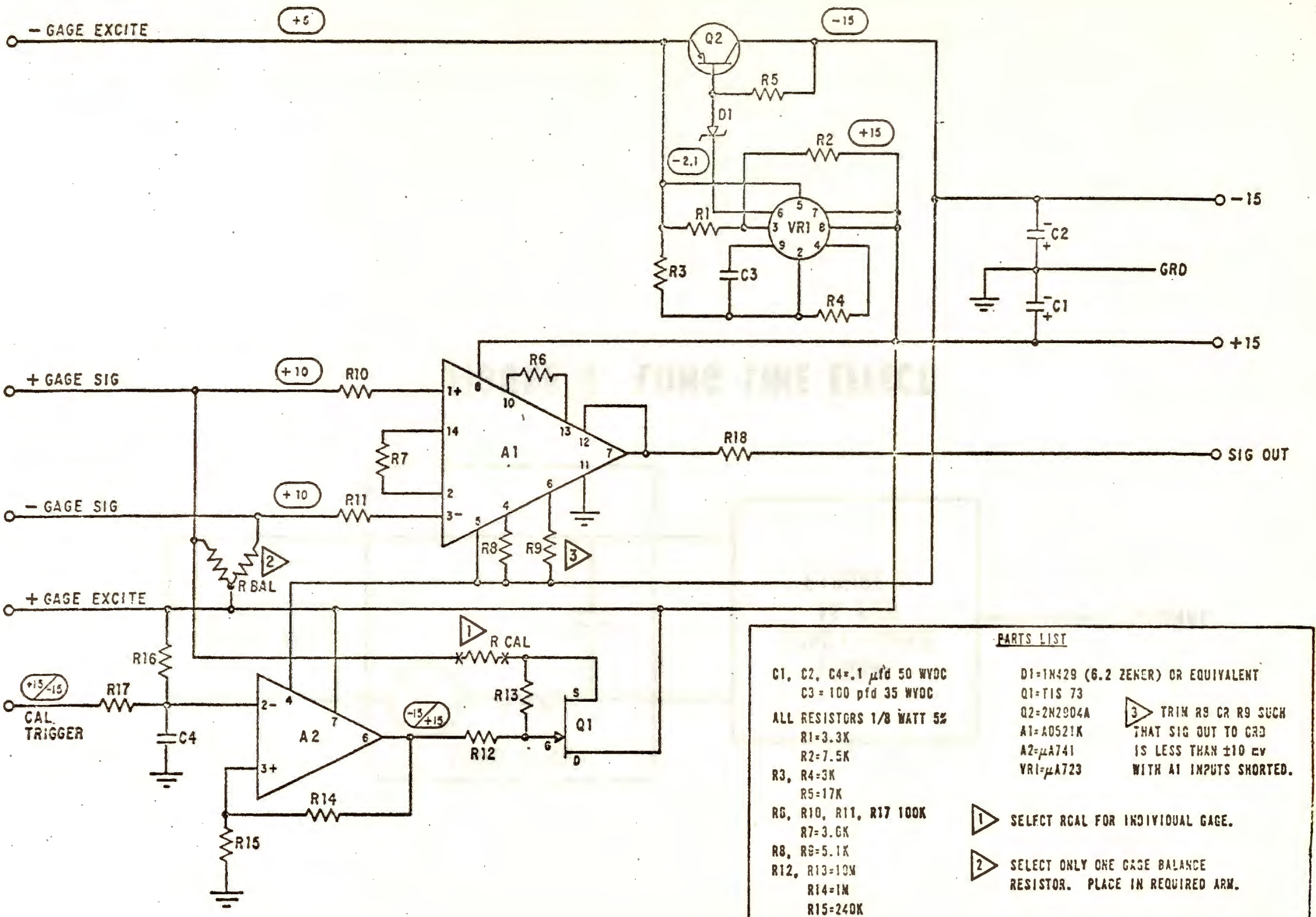


FIGURE 3 LONG LINE EFFECT



<u>PARTS LIST</u>

C1, C2, C4=.1 μfd 50 WVDC
C3 = 100 pfd 35 WVDC

ALL RESISTORS 1/8 WATT 5%
R1=3.3K
R2=7.5K
R3, R4=3K
R5=17K
R6, R10, R11, R17 100K
R7=3.6K
R8, R9=5.1K
R12, R13=10M
R14=1M
R15=240K
R16=200K
R18=10

D1=1N429 (6.2 ZENER) OR EQUIVALENT
Q1=TIS 73
Q2=2N2904A
A1=AD521K
A2=μA741
VR1=μA723

3 TRIM R8 OR R9 SUCH THAT SIG OUT TO GRD IS LESS THAN ±10 mv WITH A1 INPUTS SHORTED.

1 SELECT RCAL FOR INDIVIDUAL GAGE.

2 SELECT ONLY ONE GAGE BALANCE RESISTOR. PLACE IN REQUIRED ARM.

AUG 29, 1975 DAVID J. RAY

FIGURE 4 AFWL CLOSE COUPLED SIGNAL CONDITIONER

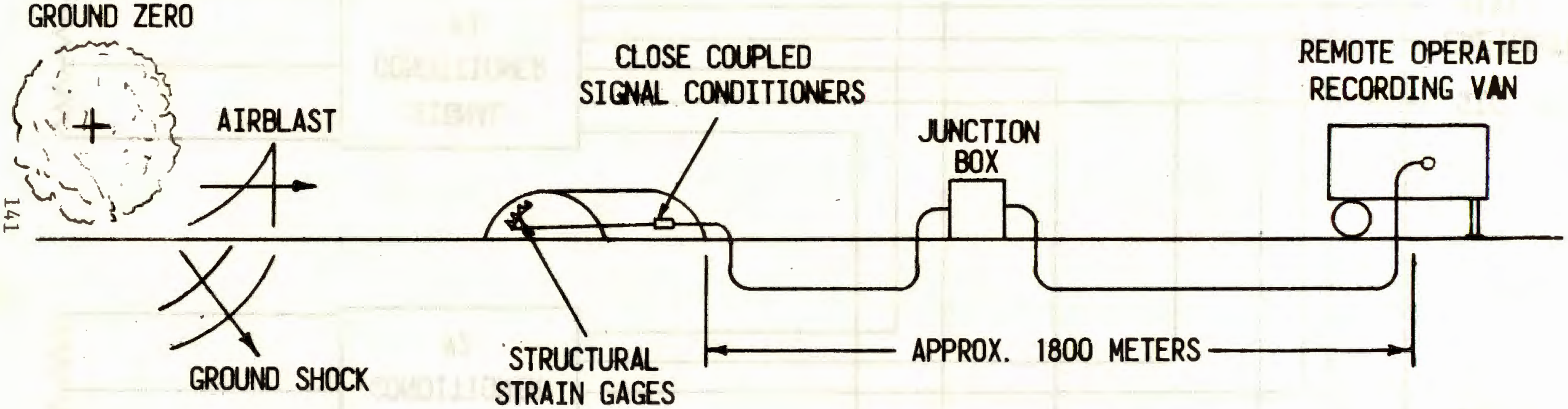


FIGURE 5 INSTRUMENTATION SYSTEN LAYOUT

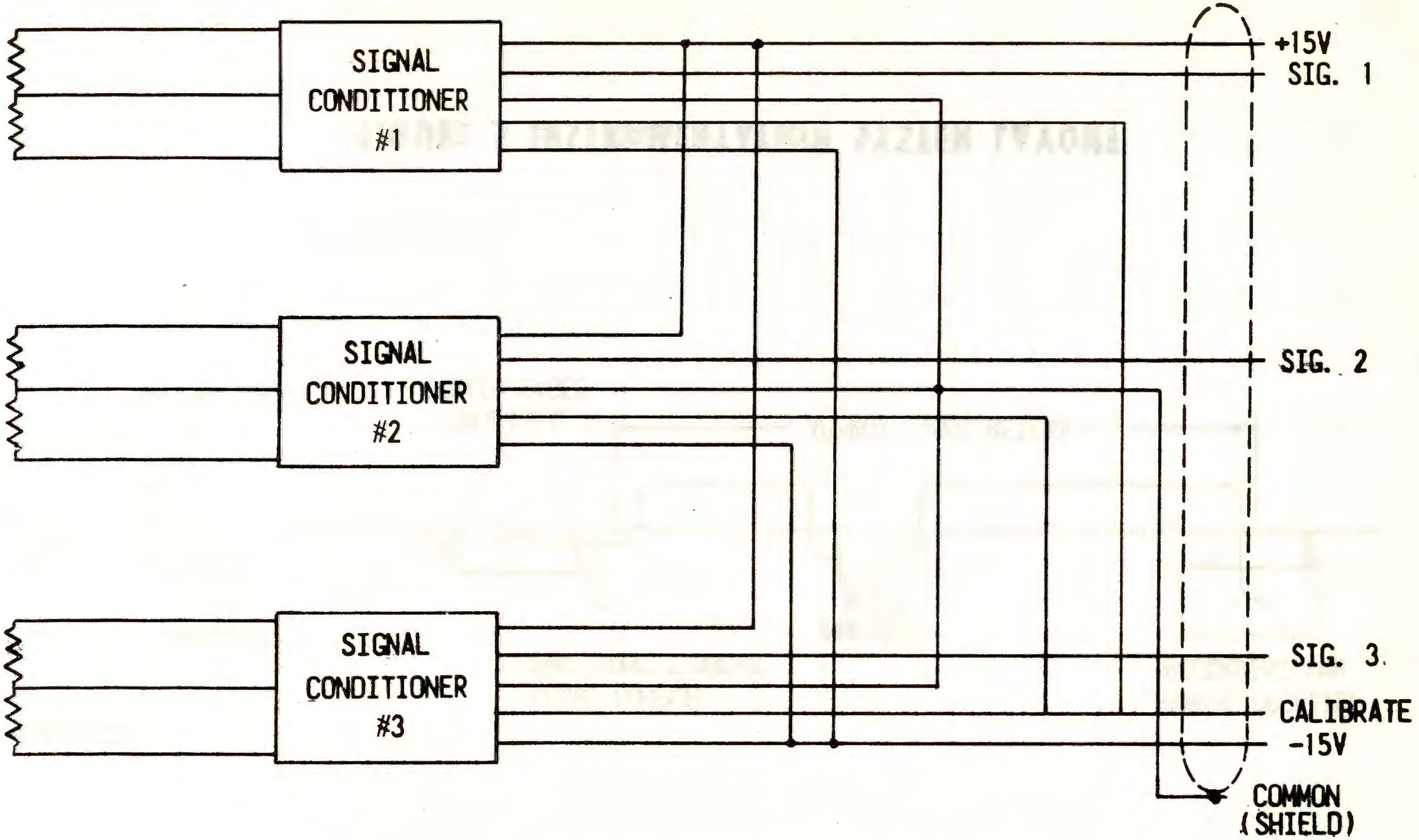


FIGURE 6 SIGNAL CONDITIONER CABLING DIAGRAM

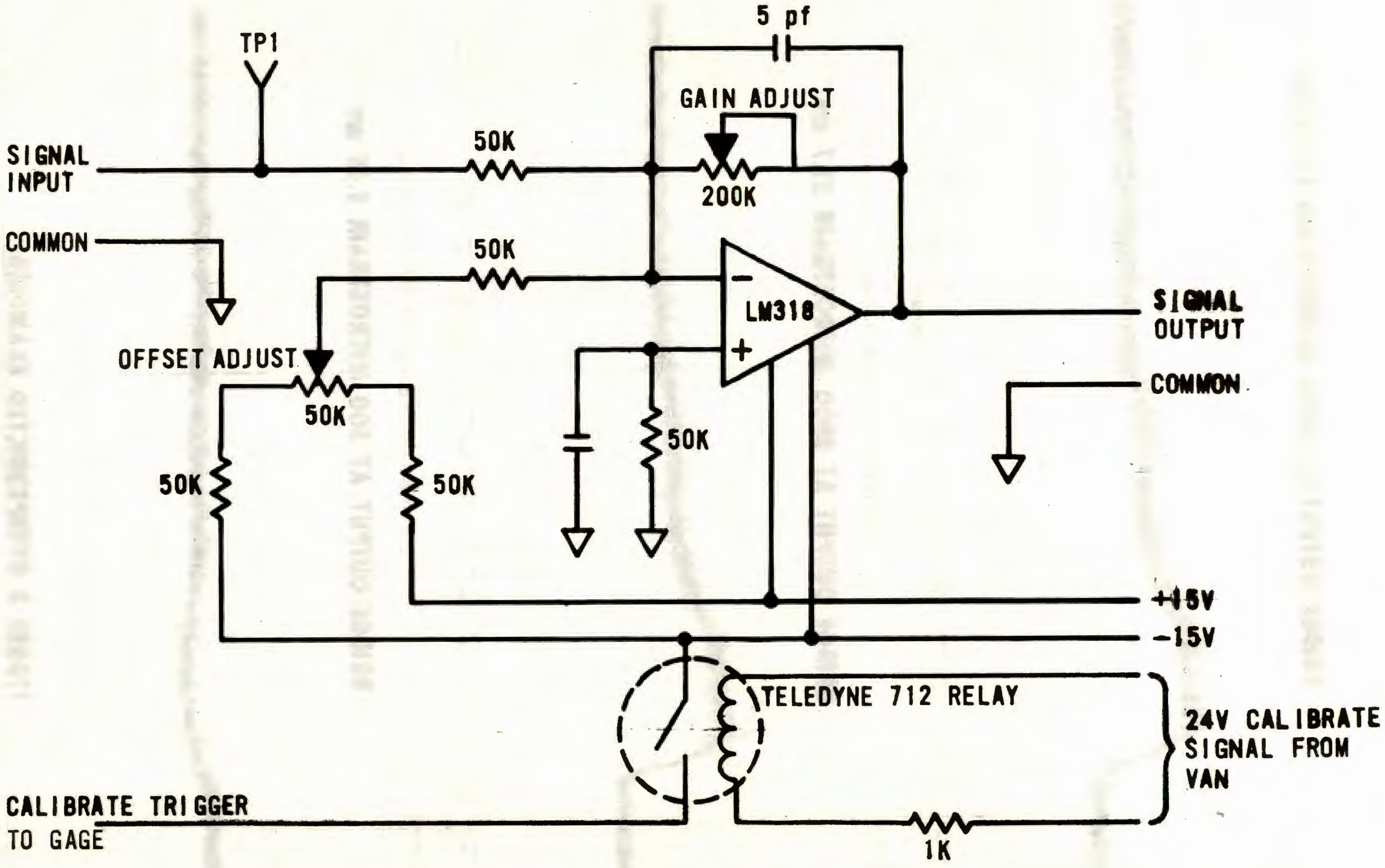
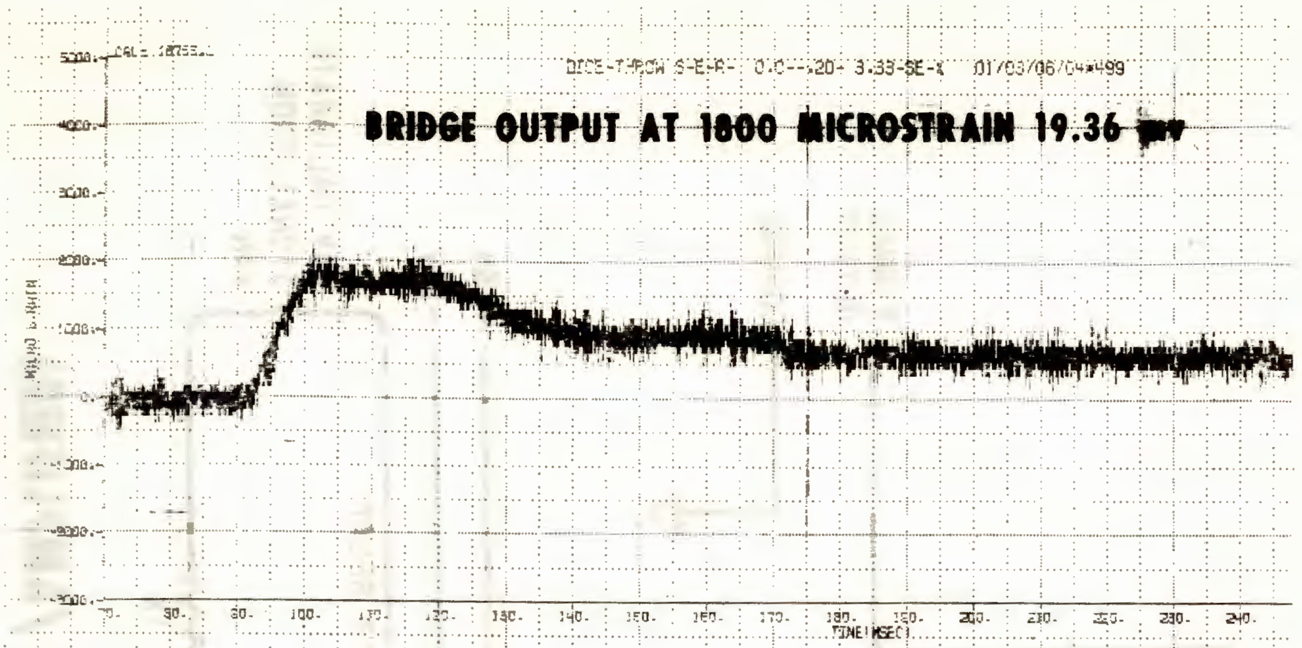
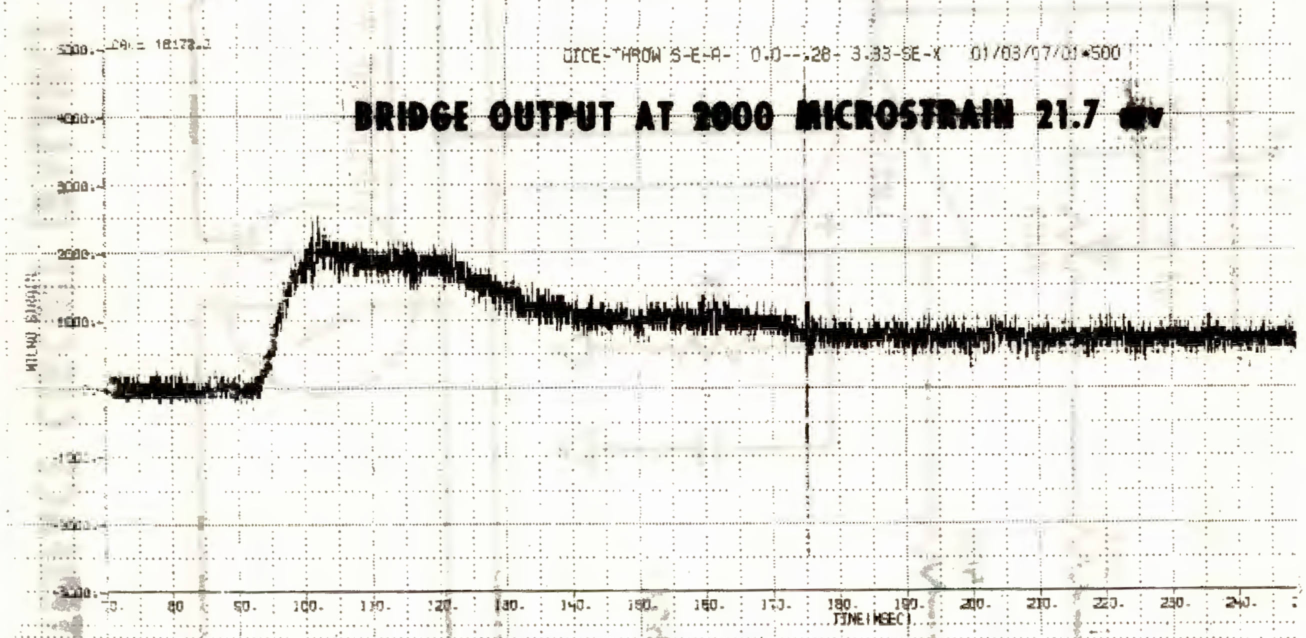


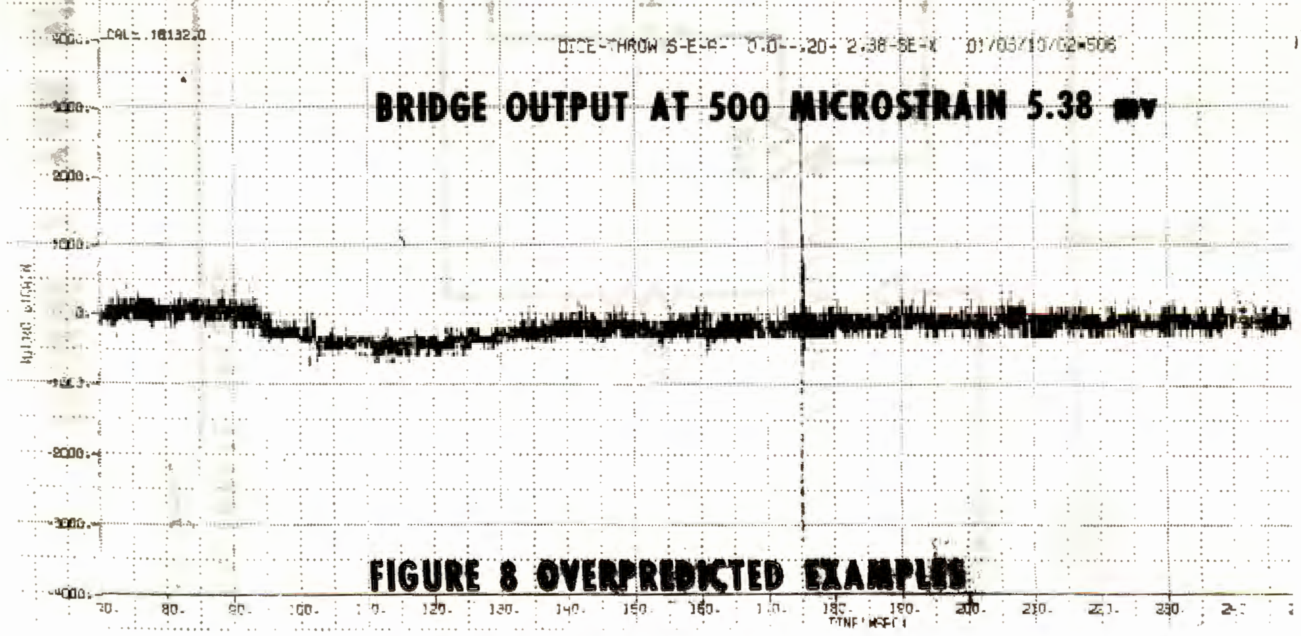
FIGURE 7 VAN INTERFACE CIRCUIT (BALANCE /AMPLIFIER)



BRIDGE OUTPUT AT 1800 MICROSTRAIN 19.36 mv



BRIDGE OUTPUT AT 2000 MICROSTRAIN 21.7 mv



BRIDGE OUTPUT AT 500 MICROSTRAIN 5.38 mv

FIGURE 8 OVERPREDICTED EXAMPLES

BRIDGE OUTPUT AT 2800 MICROSTRAIN 30.1 mv

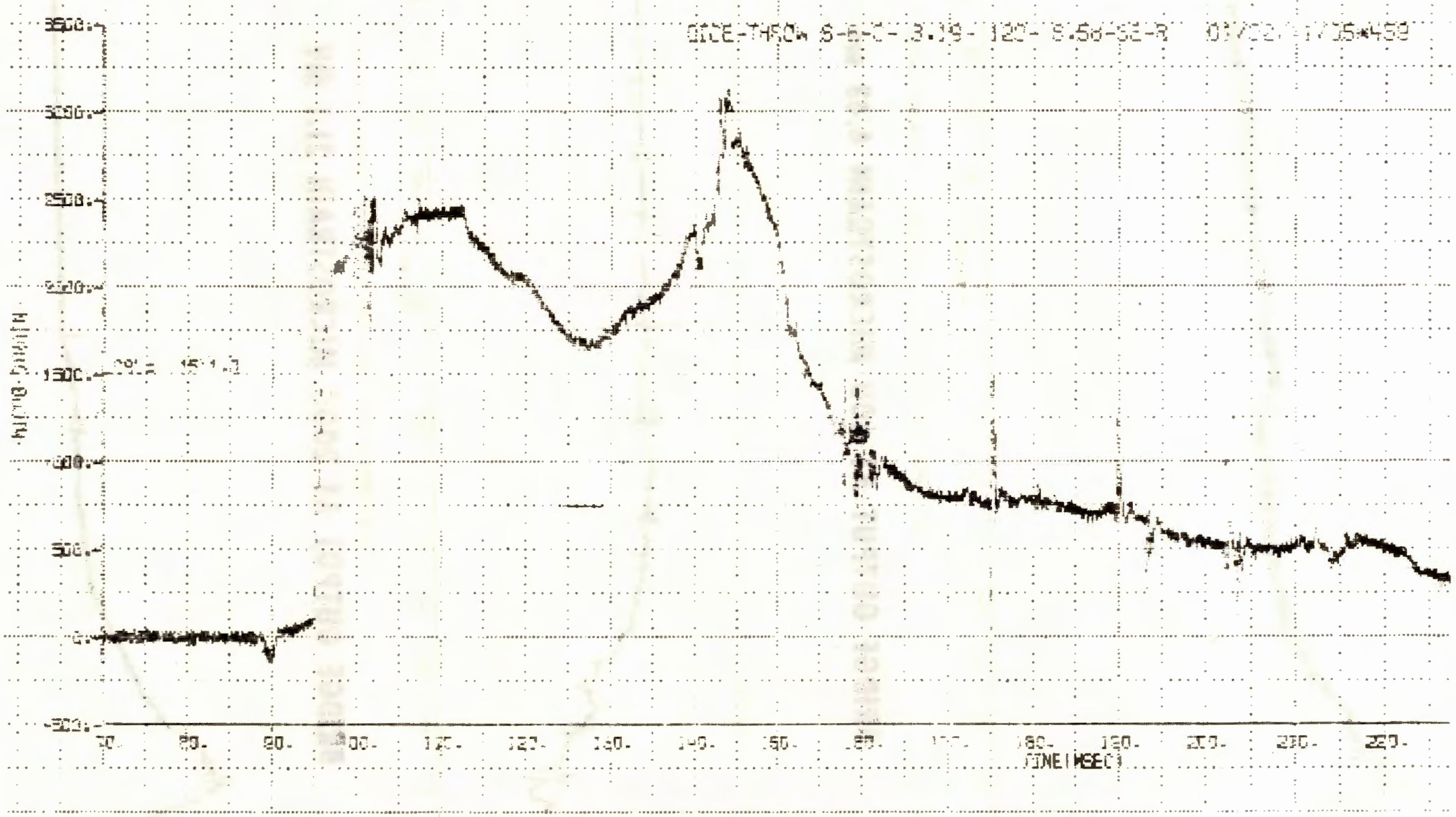
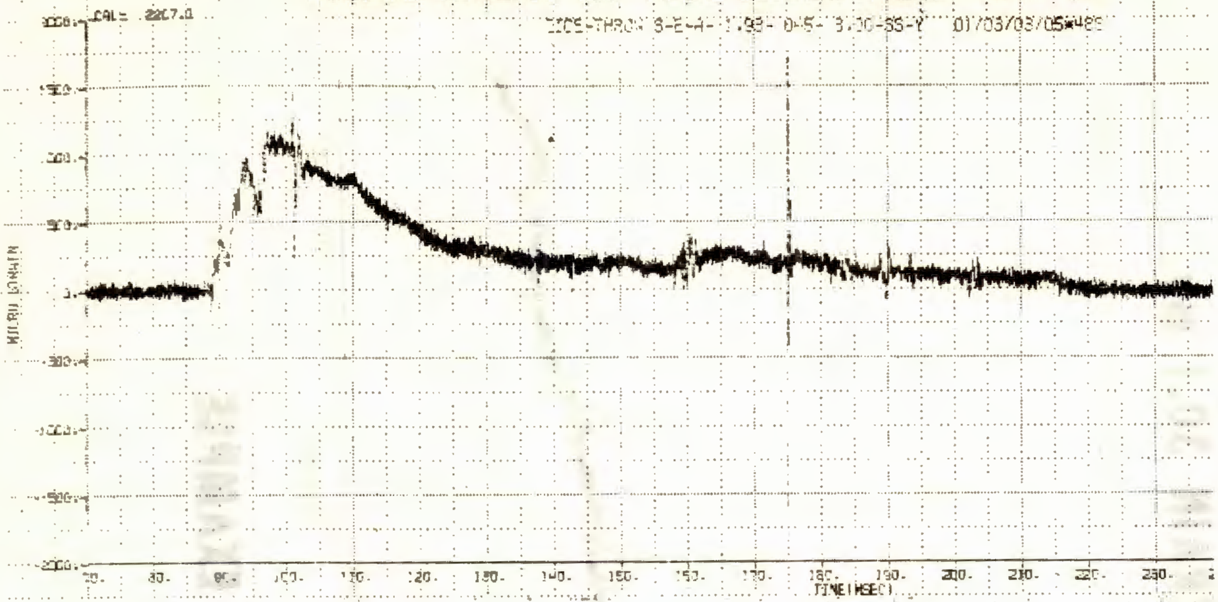
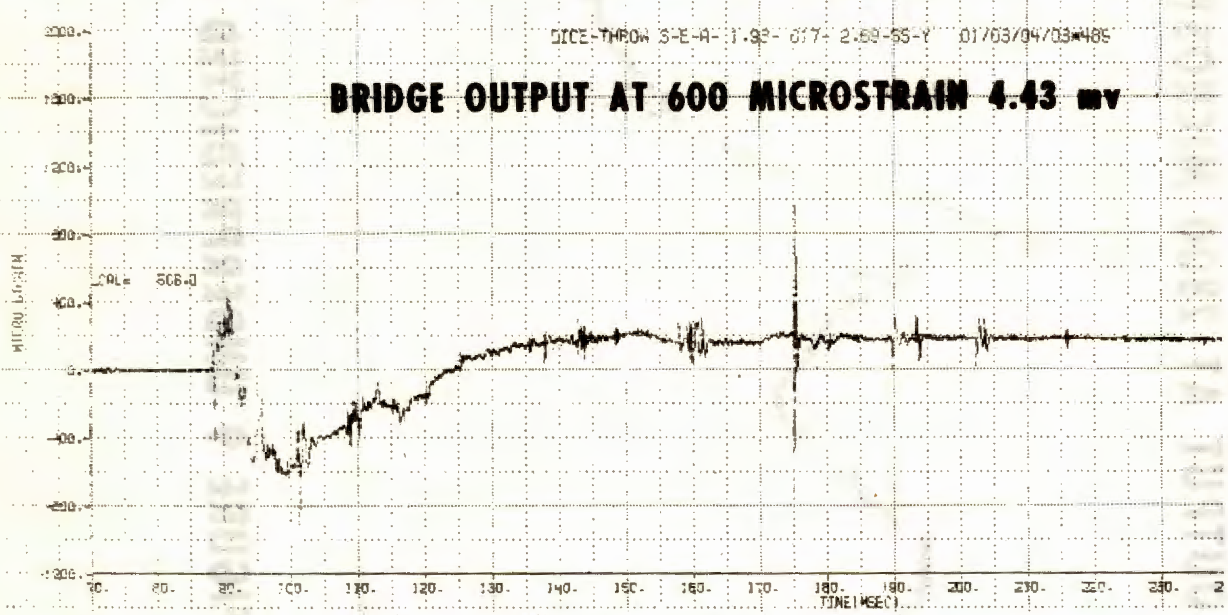


FIGURE 9 UNDERPREDICTED EXAMPLE


BRIDGE OUTPUT AT 1000 MICROSTRAIN 4.43 mv


BRIDGE OUTPUT AT 600 MICROSTRAIN 4.43 mv

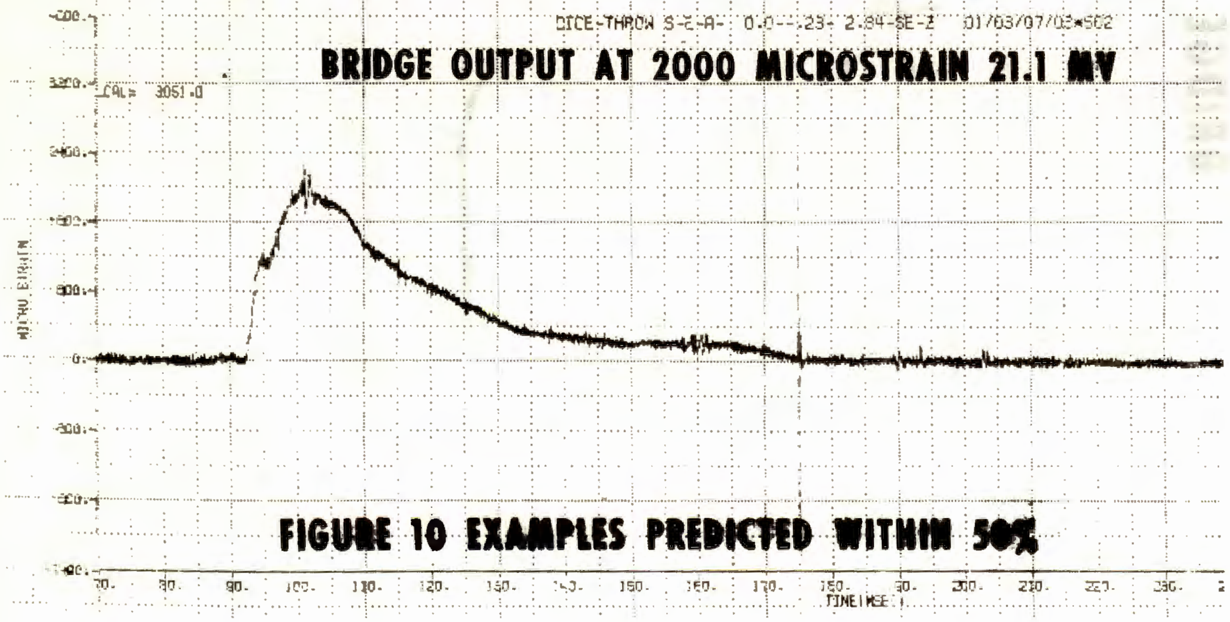

BRIDGE OUTPUT AT 2000 MICROSTRAIN 21.1 MV

FIGURE 10 EXAMPLES PREDICTED WITHIN 50%

PROCESSING OF WIDE DYNAMIC RANGE SIGNALS WITH COMPANDOR CIRCUITS

J. Reiger, NWC

Certain transducers exist which produce outputs over a very wide dynamic range. Examples of transducers commonly found in instrumentation systems, with wide dynamic range, are: accelerometers, microphones, light sensors, hydrophones, radar, sonar and IR sensing. Slowly-varying ("DC") signals can be processed for transmission by logarithmic or nth-root black boxes.[1] A process such as this is stationary in that there is a one-to-one correspondence between the input and output voltages up to the frequency limitation of the black box. At the other end of the instrumentation system the signal can either be reconverted to the original wide dynamic range signal, or interpreted directly in accordance with a calibration.

When the signal is AC, however, the situation is more complicated. Since the AC signal is more or less symmetrical about the zero crossing, (what happens when it isn't very symmetrical will be discussed later), any waveform present at the input is distorted in shape[2] and the resulting shape changes with input amplitude.[3] Soft-limiting of the peaks can be used for compression but is difficult to reconstruct. However, there are two classes of circuits, intended mainly for audio processing, that are nearly ideal. These techniques, previously implemented with push-pull triode tubes, light bulbs, panelescent surfaces, FETs, and more recently integrated circuits, work on the envelope of the AC signal, thus preserving the ratio between components of different frequencies as well as the shape of the waveform. Ideally, only the long-term average peak-to-peak variation is reduced or removed.

The first, and least sophisticated version of this process is the AGC circuit which uses feedback to decrease the gain of an amplifier in the AC signal path. The AC signal is amplified a sample of the output is rectified, filtered and fed back to the input in such a manner so as to decrease the gain. Depending upon the "gain" of the feedback loop, an increase in input level will cause only a small change on the output. Signals below the threshold at which the AGC operates are not affected; also, the attack and release times of the gain control circuit are not necessarily the same. For example, when an AGC circuit is used, in a tape recorder, the AGC charges up rapidly to the level of the voice peaks, and discharges slowly to prevent background noise from coming up between words or sentences. The original dynamic range of the input signal is irreversably altered--but the waveforms of the original signal are preserved without distortion except perhaps at times when the AGC control voltage is changing. Thus, although the scale is arbitrary, an instantaneous frequency plot of the signal preserves the ratios between individual frequency components. This would be the optimum way of determining the frequency content of the transducer signal where signal strength information is not required.

An example of a simple (although admittedly not very good) AGC circuit is shown.[4] As the input voltage rises to the point where the

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[1] Footnotes in text correspond to figure numbers at the end of this paper.

output has a peak-to-peak voltage greater than two diode drops, the light bulb will flicker with the peaks of the signal. As the light bulb flickers, it lowers the resistance of the light-dependent resistor, thus shunting part of the signal to the amplifier. As the input voltage rises still further, the light increases in brightness and reduces the signal still further. If the gain of the amplifier is high, the increase in output level for increases in input level above the threshold is very slight. In this example, the charge time of the AGC control is limited by the thermal time constants of the light bulb (and to a much lesser degree by the lag of the photoresistor). If the signal fed to the light bulb were rectified first, for example, it would be possible to cause the AGC to react ("attack") rapidly, and release slowly, as would be desired on certain audio signals. Use of analog multipliers can speed reaction times by a great deal--but there's a limit; the frequency response of the control signal must be lower in general than the lowest expected data frequency, or low frequencies at the input will be detected, fed back, and will cause the otherwise leveled output to have falling low-frequency response. Again, the AGC circuits are used when maximum S/N (signal-to-noise ratio) and FM transmission channel is the most important attribute, and unless additional information is also transmitted on a separate channel, the use of such a circuit destroys or distorts amplitude information irreversably.

If we take the control circuit out of the feedback loop and remove the threshold diodes, we can produce a situation where an increase in input level will produce a smaller increase in level on the output.[5] For special situations, we could even arrange things such that an increase in input level would cause a decrease in output level. With the setup as shown we are in effect AMing (amplitude modulating) the input signal by its long-term average. Still adjustable and negotiable are the AGC attack and release times which establishes the effective frequency resolution that can be recovered from the transmitted data. Also a new variable, compression ratio, is now available for consideration.

One possible algorithm is the square root function--that is, if the input peak-to-peak amplitude increases by a factor of four, the output peak-to-peak amplitude increases by a factor of two. Looked at logarithmically, that means that a 12 dB input increase is a 6 dB output increase--a two-to-one compression in decibels. This also means that, all other things being equal (which of course they are not), a signal-to-noise ratio of 80 dB can be theoretically provided in an analog transmission channel limited to a 40 dB S/N. Of course, other compression ratios could be used--the cube root (which is a 3:1 compression in dB) is easy to produce, as is a 3:2 compression. It's also possible (and perhaps inevitable) that a compression ratio which varies with the input level could be produced, although that defeats the purpose if the purpose is reconstructing the data as accurately as possible.

Limitations on the accuracy of such a system include the linearity (or accuracy of the transfer function for compression ratios other than 2:1), and the rise- and fall-time and overshoot/undershoot of the control function voltage. The light bulb is an incredibly non-linear device, for example, since its "color temperature" increases as the voltage across it increases, and the light output, which is more or less proportional to power dissipated, increases roughly with the square of the input voltage. Further, the light bulb's output at very low input voltage is not visible light, and a certain amount of hysteresis exists. The light bulb, at best, is a less than optimum control element. If optical control is used, a panelescent surface or an LED is more predictable (the LED being a linear device, and the panelescent surface a square-law responder). Neither has "memory" problems, but both still require some power to drive.[6] Integrated circuit mulitpliers, connected as dividers, perform the action more conveniently, and removal of the optical link also removes some of the less-desirable characteristics of light-dependent resistors.

Recently, several integrated circuit manufacturers have come out with "compandor" chips, notably Exar, Signetics, and National Semiconductor.[7] All of these manufacturers show applications of compression and expansion of telephone audio circuits but are vague about any other uses (although a preliminary application note showing other audio processing applications has been made available for the Signetics 570/571).[8] Most of the applications information is based upon use of two units connected together as a compressor on one end and an expander at the other end of the circuit, so let's consider what happens when we do that, and then what happens when we don't.

First of all, if everything worked perfect, we could transmit an 80 dB dynamic range in a system with 40 dB S/N. However, the circuit itself has an S/N of about 70 dB, which is present before the compression takes place, thereby restrictions the range over which the system can work. Another limitation is called "low-level tracking"--the levels where the output of the rectifier is close to zero and offset errors can introduce ambiguity in reconstruction of the original level. If the output of the instrumentation is going to be displayed on an oscillograph (or some other copy machine) though, the biggest problem comes in displaying such a dynamic range--for example, 60 dB is a 1000:1 voltage ratio. If the instrumentation output is to go to a spectrum analyzer though, the dynamic range presents no severe handicap, especially if the spectral responses are displayed logarithimcally the distortion due to the process tends to cancel out between the compressor and expander. To see why, consider a gated burst of sine waves.[9] Since a signal of that nature represents a sine wave with a modulation envelope of a square wave, and the control voltage will not respond instantaneously to the square wave signal, the compressed audio signal will have a non-square-wave envelope. But as long as system capacity is not exceeded by the uncompressed parts of the signal, the expander on the receiving end (if its time constant is the same as the transmitter's) will have an undershoot which will compensate for the transmitter overshoot.[10] However, if the initial overshoot did, in fact, overmodulate the channel, the signal on the channel itself would be clipped for the first few cycles, and the output after the expander would have an envelope on it which would look rather odd, but for reasons which are obvious. Spectral analysis of this transistion would show a rapidly decreasing third, fifth, seventh, and so on, harmonic of the input signal. This problem isn't as serious as it sounds, since real signals don't generally go from nothing to something instantaneously for roughly the same reasons that mathematical functions don't.

A more serious problem exists at low frequencies, where the input frequency is so low that it is lower than the corner frequency of the filter of the control section. When this happens, the input waveform is reduced in gain more at the peaks than at the zero crossings, creating a signal that looks rounded-off and, containing odd-order harmonics, which are apparent, although lower in magnitude, than the harmonics generated by clipping. This phenomenon places a limit on the speed of the response of the control generator just opposite the requirement for fast attack times to reduce clipping.

Clearly a compromise is suggested. First, if any frequency below a certain point is possible from the transducer but not required on the data, low pass filtering of the data at the compressor input is recommended. Since the compressors discussed here use fullwave rectification, if the low pass filter's 3 dB point is set at the lowest expected frequency, actual suppression of that frequency at the filter will be about 7 dB. For audio signals, a frequency around 15 to 20 Hz is used. For a crystal accelerometer, a frequency around 40 to 50 Hz should be appropriate.

If the signal entering the compressor is asymmetrical, it follows that one set of peaks is farther from the zero crossing than the other, which in turn implies that the voltage at the output of the control voltage filter is the average of the two--and contains a proportionately higher ripple. This condition, especially common on things like human voices, presents no problem if known--the bias point at the output of the compressor is set at a point which allows both positive and negative peaks to be within the linear range of the channel. Often the asymmetrical signal exists only at the extremes of the range of the transducer. This would perhaps require that the bias point be 33% of full scale, rather than 50%. Signals lower than the peak would be reproduced. If the expander is used at the ground station, since it is AC-coupled, asymmetrical signals will be reproduced as sent, with the DC shift like the original.

A test circuit was built at the Naval Weapons Center to evaluate the performance of the XR-2216 at room temperature and at elevated and depressed temperatures, with power supplies different from the one at which it was characterized and without trimming the required external components. We also tested it to see what would happen when we fed an input voltage higher than specified. It is to the credit of Exar that the test units survived. Without the required trimming, the compressor/expander as a unit still exhibits 70 db dynamic range, and an improvement can be made by tweaking. Unfortunately, the output of the compressor is just slightly above +1 dbm, or about 2 volts peak-to-peak (referred to 500-ohm load), and a standard telemetry channel normally requires a 5 volt peak-to-peak input, so additional amplification would normally be required. If an expander is used at the receiving end, a 5 volt peak-to-peak output can be obtained, but normally a buffer would follow for lowering the impedance. Variation of gain over temperature from $-20^{\circ}C$ to $+70^{\circ}C$ was less than ±1 dB from the room temeprature value. Although performance with a 6 volt power supply instead of the recommended 12 volt or higher supply resulted in somewhat degraded performance, that only occurs near maximum output, and is predictable.

Summarizing, compression of wide dynamic range signals has advantages over AGC for many applications since it preserves the amplitude of the original signal in recoverable form; the results of such compression are predictable and reversible electronically if desired. Monolithic IC versions of such circuits are straightforward, inexpensive, and present no unpleasant surprises.

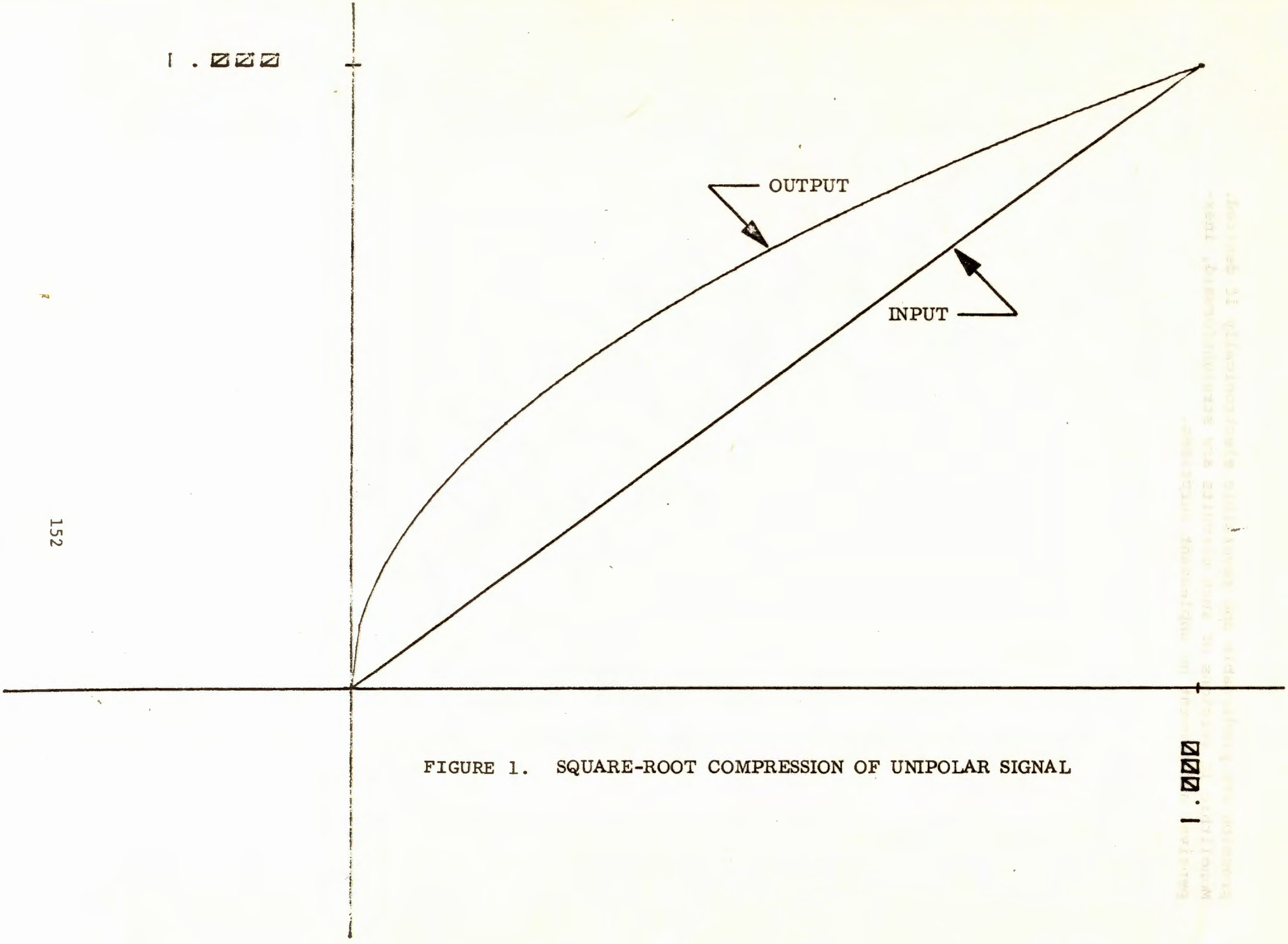

FIGURE 1. SQUARE-ROOT COMPRESSION OF UNIPOLAR SIGNAL

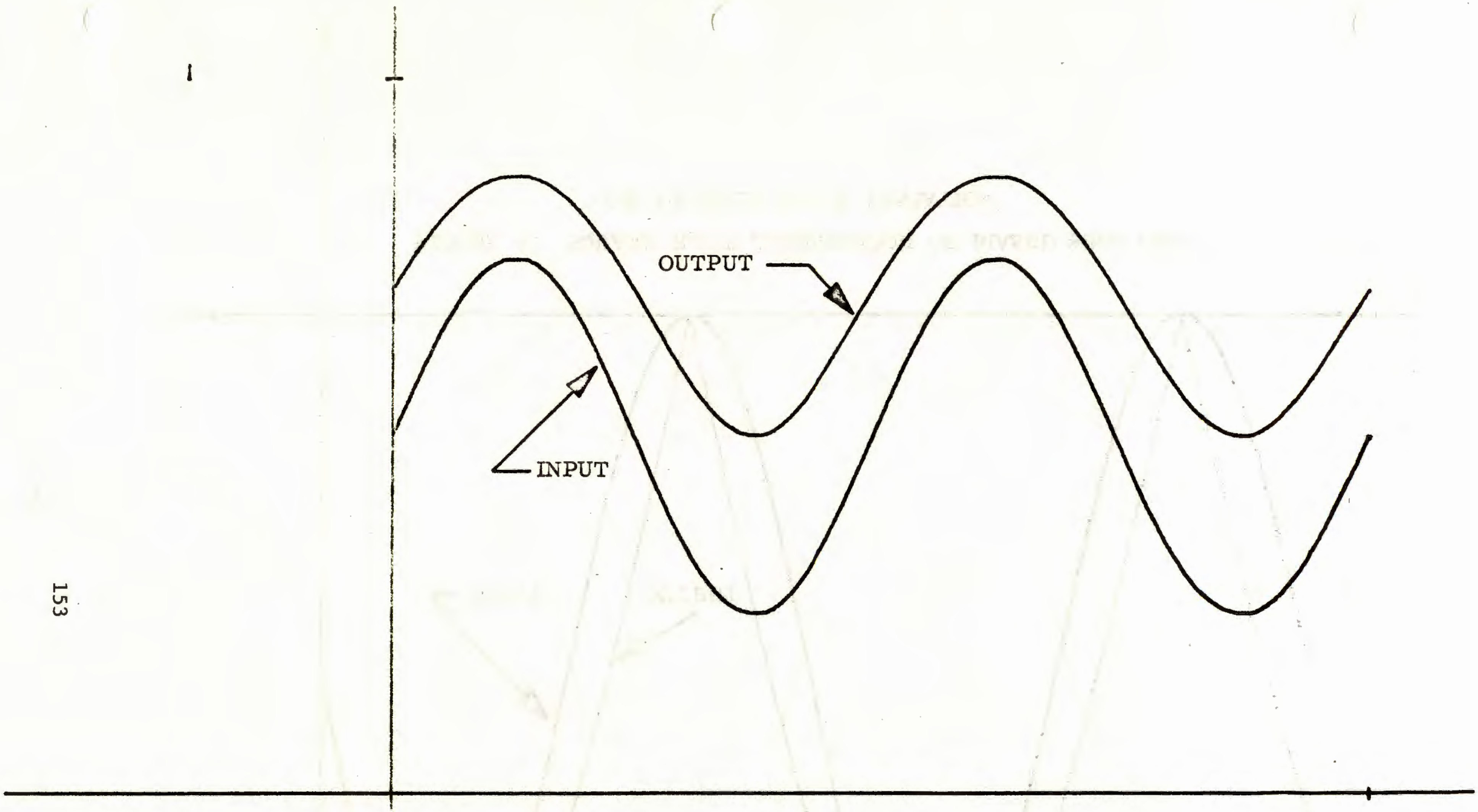

FIGURE 2. SQUARE-ROOT COMPRESSION OF BIASED SINE WAVE,
50% OF FULL SCALE DEVIATION

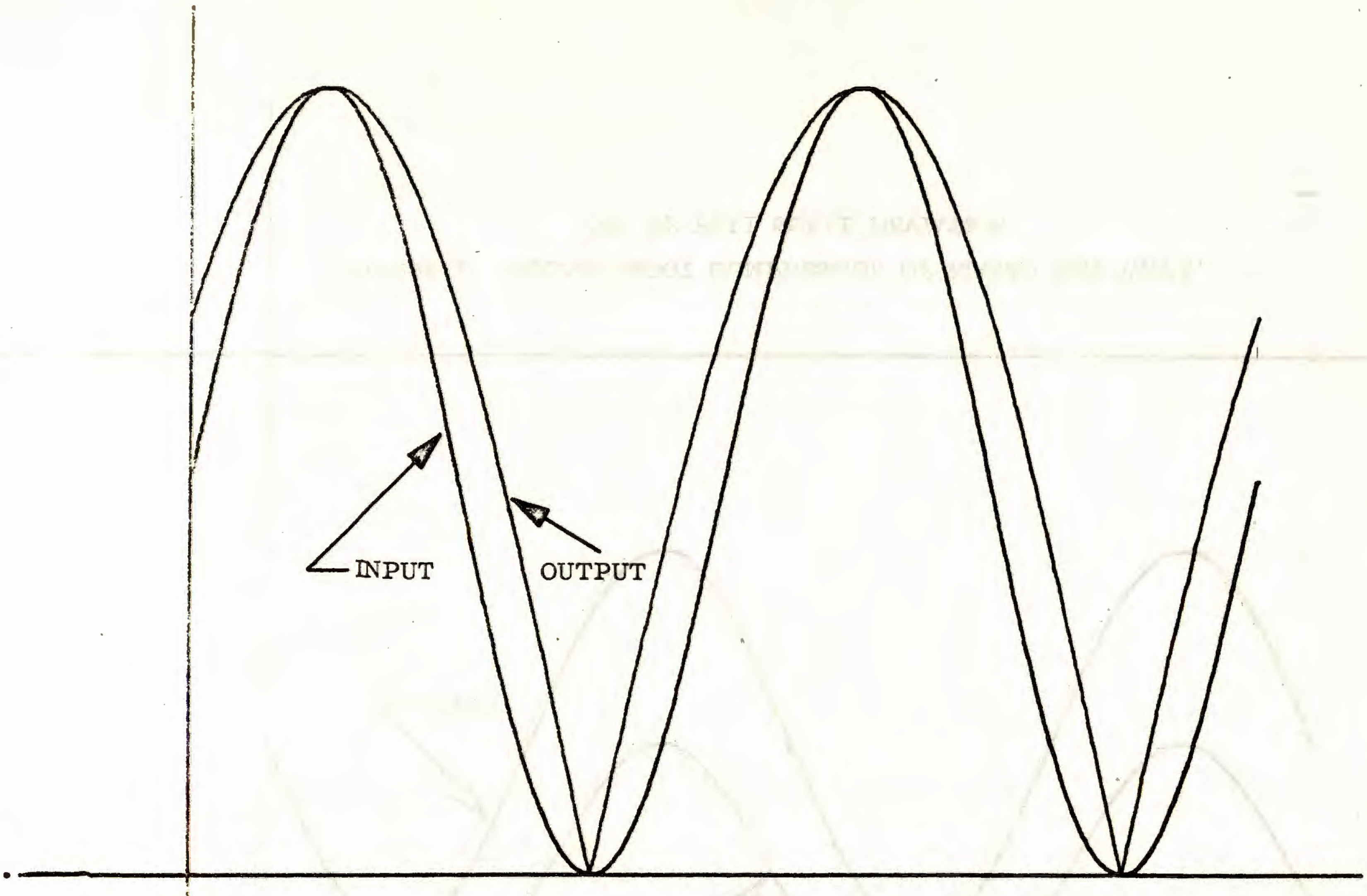

FIGURE 3. SQUARE-ROOT COMPRESSION OF BIASED SINE WAVE, 100% OF FULL SCALE DEVIATION

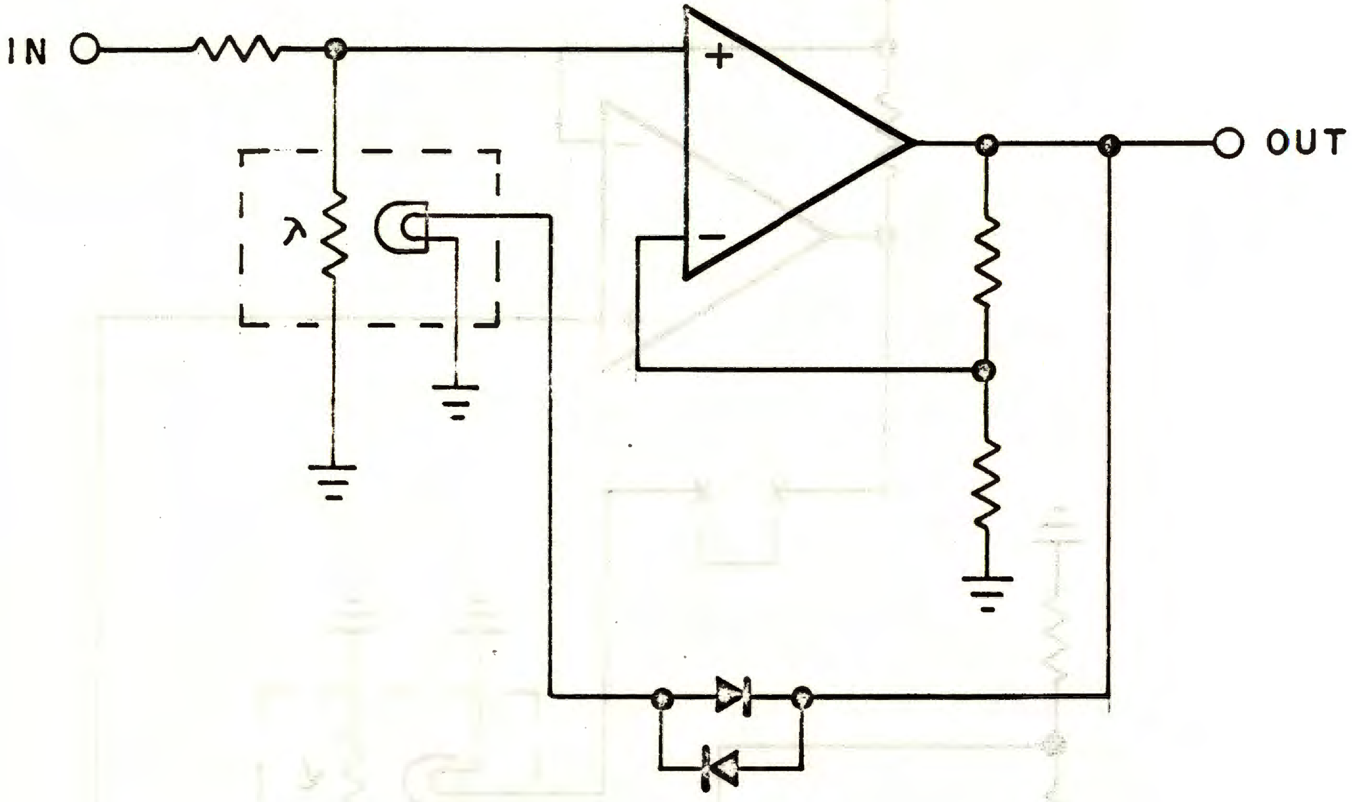

FIGURE 4. VERY SIMPLE AGC SYSTEM (OPTICAL FEEDBACK)

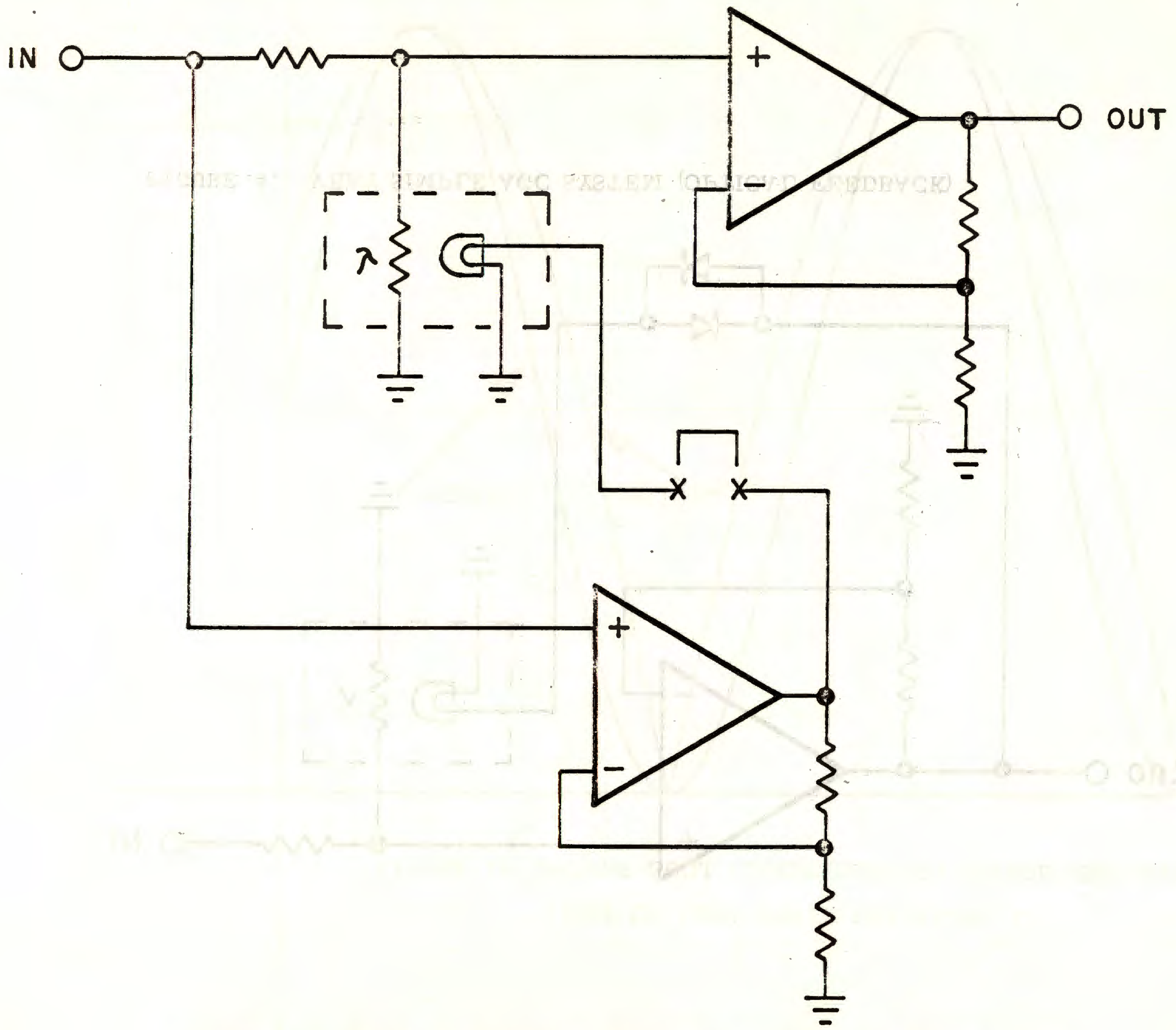

FIGURE 5. SIMPLE COMPRESSOR CIRCUIT (OPTICAL CONTROLLED)

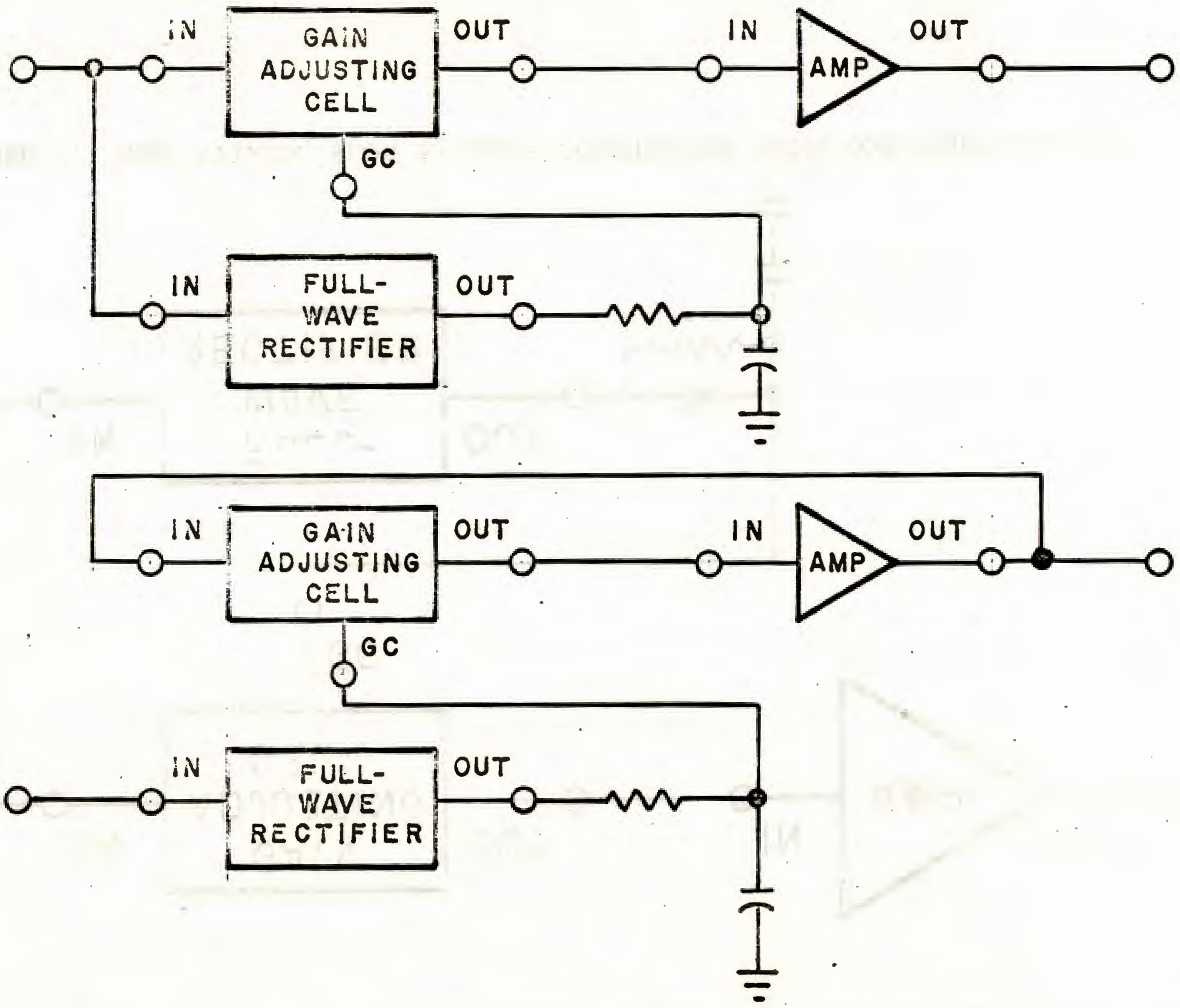

Figure 6. CONNECTION OF COMPANDOR CIRCUITS AS COMPRESSOR AND EXPANDER

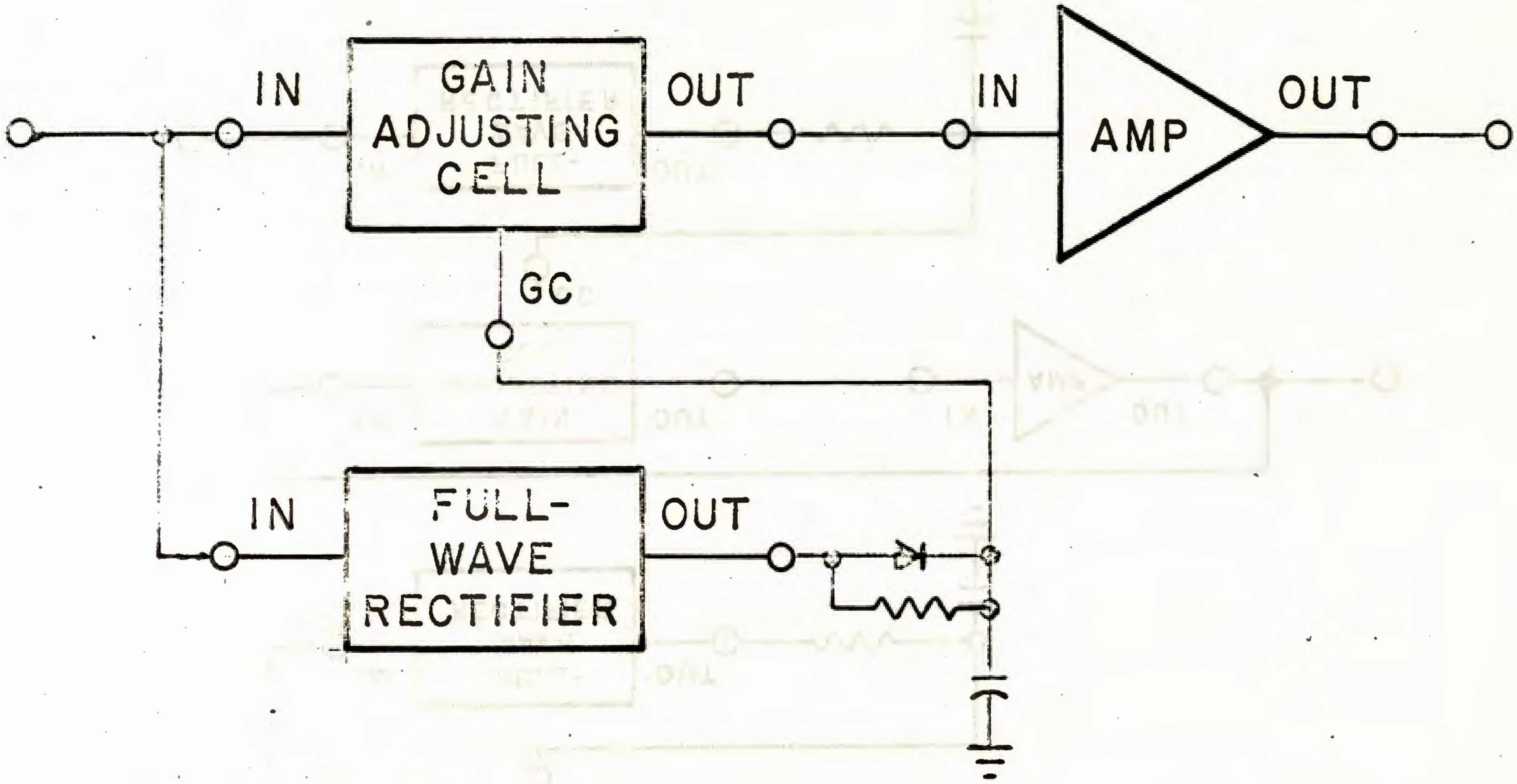

FIGURE 7. FAST ATTACK, SLOW RELEASE COMPRESSOR USING COMPANDOR CIRCUIT

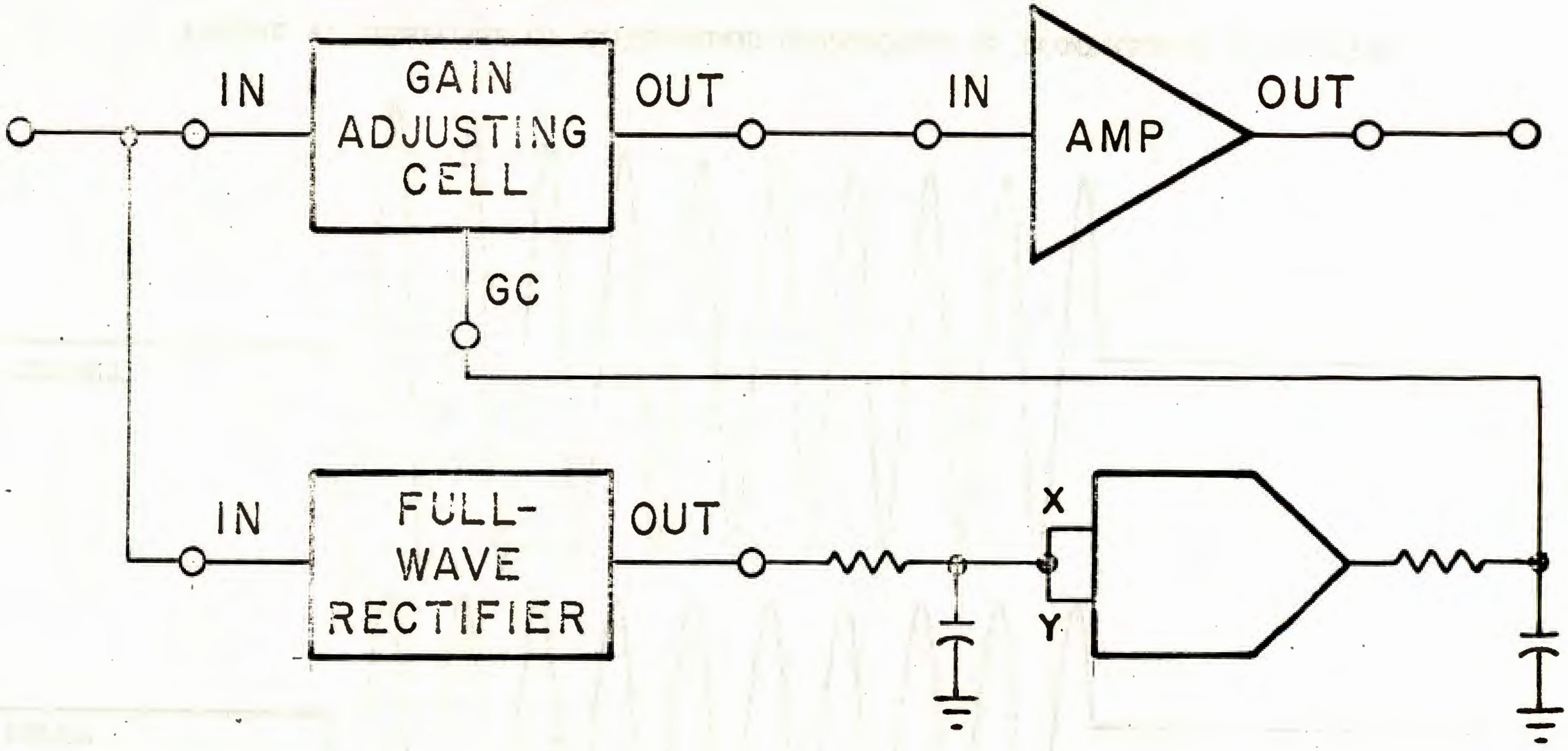

FIGURE 8. THREE-TO-ONE COMPRESSOR USING COMPANDOR CIRCUIT AND MONOLITHIC MULTIPLIER

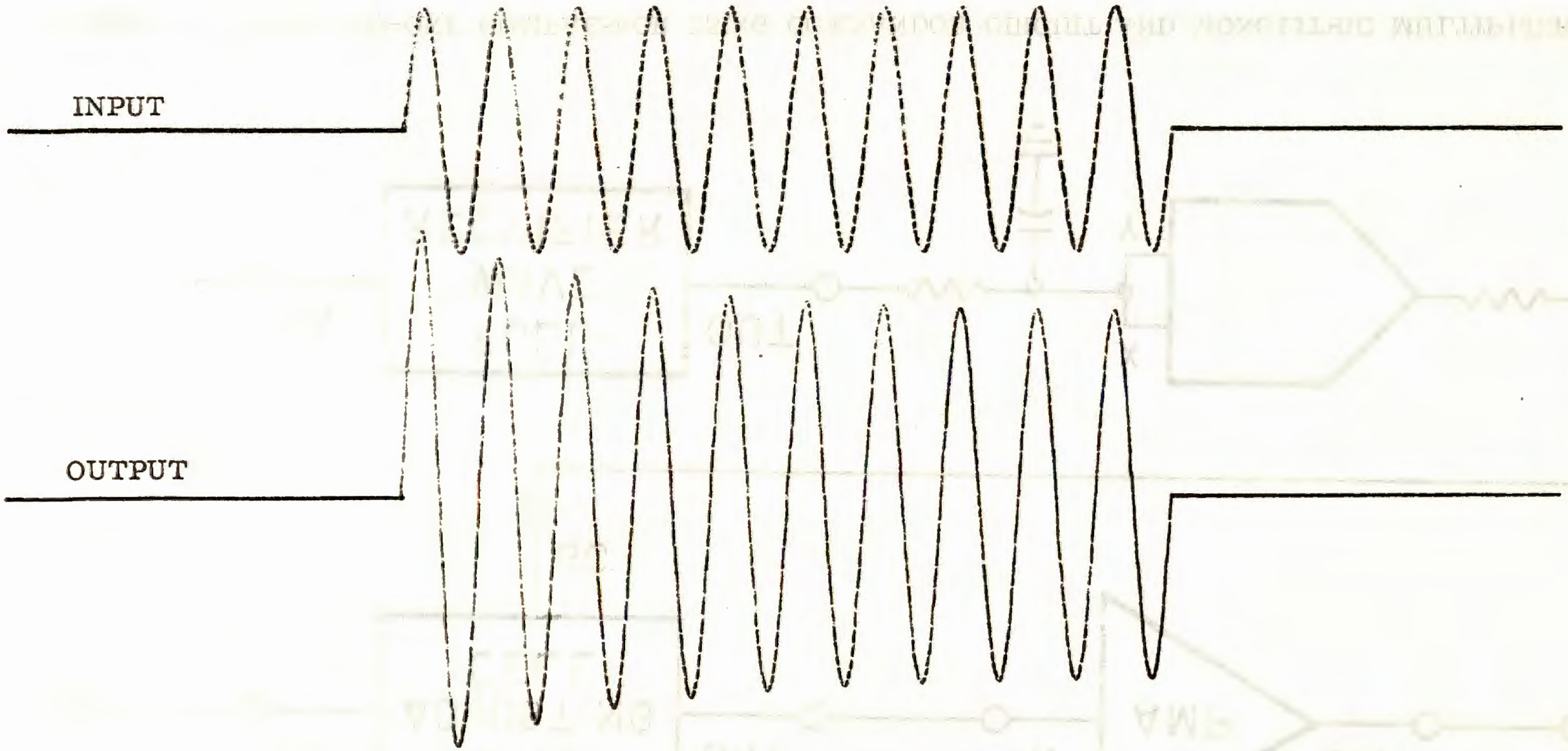

FIGURE 9. RESPONSE OF COMPANDOR CONNECTED AS COMPRESSOR TO GATED BURST OF SINE WAVES

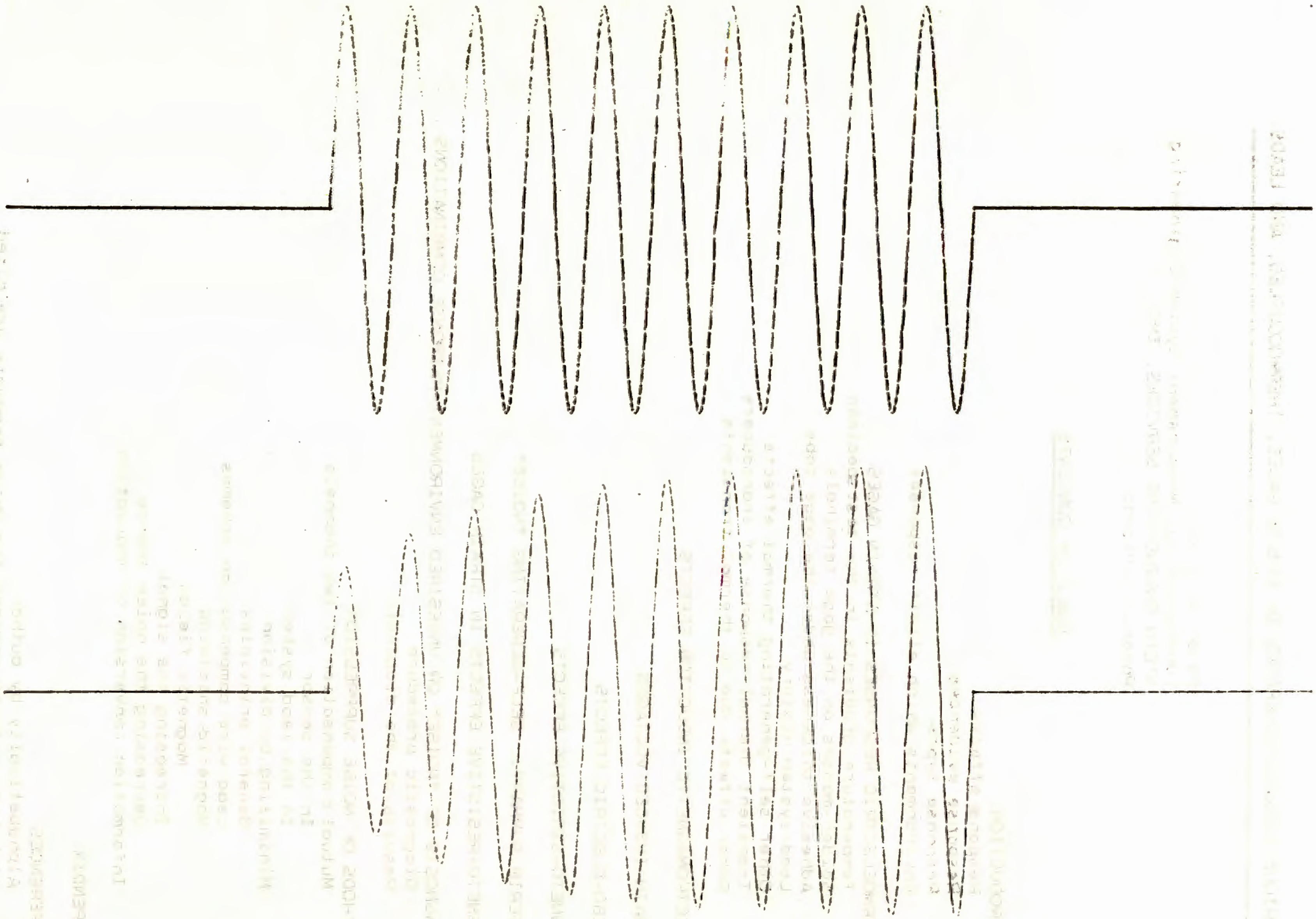

FIGURE 10. RESPONSE OF EXPANDER TO GATED BURST OF SINE WAVES

SPURIOUS SIGNALS GENERATED IN STRAIN GAGES, THERMOCOUPLES, AND LEADS

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INTRODUCTION

Strain gages, thermocouples, and the connecting lead systems, like all transducers, tell the truth, the whole truth, and nothing but the truth. The measurement engineer, however, always wants only a specific, small part of the whole truth. Thereby hang many problems.

The "whole truth" of responses of any transducer to its environment consists of at least 16 environment-response combinations (STEIN 1972D) which include:

RESPONSE EFFECTS

Additive Effects, which are changes in output level such as resistance level, voltage level, etc.
Multiplicative Effects, which are changes in calibration factor such as expressed in millivolts/volt/full scale, in units of gage factor, in millivolts/degree temperature change, etc.

RESPONSE EVIDENCES

Temporary Responses, which disappear when the excitation is removed, such as normally desired for strain gages and thermocouples, for example.
Permanent Responses, which remain after the excitation is removed, such as is normally desired in fatigue gages and in magnetic tape of tape recorders.

RESPONSE TYPES

Self-Generating Responses, which are the production of energy-related components directly in the transducer, such as voltages, currents and charges in electrical devices; displacements, forces, stresses, strains, etc., in mechanical devices. These are the desired response types in thermocouples, for example.
Non-Self-Generating Responses, which are the production of impedance changes in the transducer, such as electrical, mechanical, thermal, acoustic, etc., impedances, mobilities, etc. These, in turn, may be sub-divided into three categories:

Energy-Storing Responses
  Potential, stored as a level, such as in capacitors, springs, compliances, fluidic volumes, etc.
  Kinetic, stored as a rate of change or of accumulation, as in masses, inductors, etc.
Power Dissipating Responses
  Such as in resistors, dash-pots or other damping mechanisms.

In resistance strain gages, for example, the desired response is resistive. Any changes in capacitance or inductance are undesired.

ENVIRONMENTS ELICITING THESE RESPONSES, INPUTS, STIMULI, FORCING FUNCTIONS, SERVICE CONDITIONS, EXCITATIONS.

Desired Environments, or Measurands, the quantities which are to be observed by the measurement engineer's choice and definition, such as mechanical strain in on direction -- one of the examples to be discussed in this paper.
Undesired Environments, which also elicit responses from a strain gage, such as strain in directions other than the desired one, temperature, pressure, magnetic fields, electric fields, etc.

Of the 16 combinations possible from the above considerations, almost all have been documented as existing in strain gages, thermocouples and leads. The only desired environment-response combinations, all temporary, are:

STRAIN GAGES: The additive, non-self-generating, power-dissipating response to strain in one direction only.

THERMOCOUPLES: The additive, self-generating response to temperature.

LEAD WIRES: no response whatsoever to any environment whatsoever.

In each case, the (minimum) 15 other environment-response combinations are normally lumped under the category of "noise". They do, however, represent 15 different transducing processes which, although of criminal nature for strain gages, thermocouples and leads, have been rehabilitated for sometimes spectacular service elsewhere.

One of the missions of the measurement engineer is to be able to:

1/ Document, during the actual running of a test, whether or not such undesired environment-response combinations exist in his measuring system -- or better still, document beyond shadow of reasonable doubt, their absence.
2/ Offer viable methods of suppressing those combinations which are documented as existing.

The Unified Approach to the Engineering of Measuring Systems, developed by the author over the past quarter century, is uniquely adaptable to fulfilling these requirements. This paper will indicate some of the problems, causes, and cures for a few selected combinations illustrated with case histories and examples.

The table below includes the electrical effects in strain gages, thermocouples and lead-wire systems which often "cloud" the data with "noise". The mechanical responses are of interest in lead wires and thermocouples. Since a bonded strain gage is forced to conform to the strains in the specimen if the gage is properly attached, such effects in the <u>gage</u> itself are of secondary importance. They are of primary interest if they occur in the specimen to which the gage is attached, particularly important in strain-gage-based transducers which measure quantities other than strain itself. They will not be discussed in detail here and are cited only to encourage the strain gage user to investigate his test specimen very closely before he blames "noise levels" on the strain gages.

The boxes which have been high-lighted will be briefly surveyed in this paper. The survey will emphasize little-known effects which are not widely discussed in the literature. Well-known techniques such as the use of compensating or "dummy" strain gages will be mentioned only in passing.

TABLE 1: SELECTED ENVIRONMENT-RESPONSE COMBINATIONS

ENVIRONMENTS	RESPONSE TYPES OF THE TEMPORARY ADDITIVE VARIETY (note restriction)			
	ELECTRICAL		MECHANICAL	
	SELF GENERATING	NON-SELF-GENERATING	SELF-GENERATING	NON-SELF-GENERATING
MECHANICAL	**Piezo-electric Tribo-electric Unexplained ?**	**Resistive Capacitive Inductive**	**Strains (desired in gage, not leads)**	Piezo-elastic (yield, superplasticity)
MAGNETIC	**Electro-magnetic induction. Unexplained ?**	**Magneto-resistive**	Magneto-strictive	Magneto-elastic
THERMAL/OPTICAL	**Thermoelectric Pyroelectric Photovoltaic**	**Thermo-or photo-resistive**	Thermal expansion	Thermo-elastic

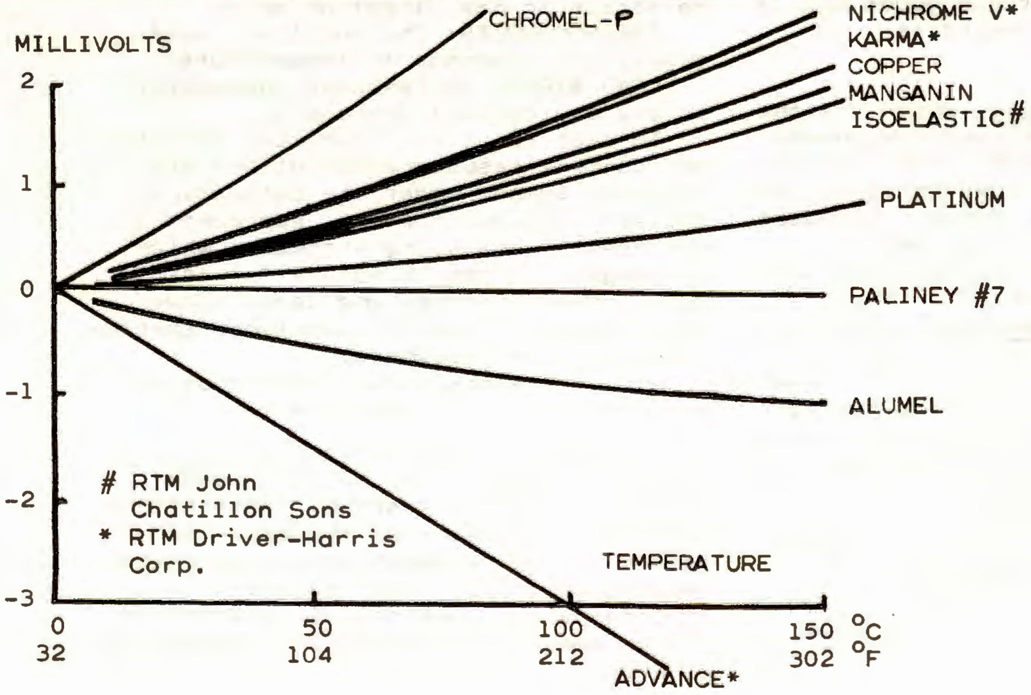


FIGURE 1

THERMOELECTRIC BEHAVIOR OF SOME ALLOYS VS PALINEY #7

from STEIN (1962B) p. 83

THERMOELECTRIC RESPONSES IN STRAIN GAGES

It is frequently assumed that since the two thermocouples (minimum) in every strain gage appear differentially coupled in the electrical circuit, that the thermoelectric emf's generated in these junctions will cancel each other out. This assumption is entirely unjustified. In fact the measurement engineer's motto should be recalled:

EXPERIMENTAL WORK DOES NOT CONSIST OF MAKING ASSUMPTIONS ABOUT WHAT SHOULD BE (we leave that for the analytical theorist). EXPERIMENTAL WORK CONSISTS OF DOCUMENTING WHAT IS.

The following phenomena may act singly or in combination to produce thermoelectric outputs due to steady or transient thermal inputs. These "noise" levels can be several 100 microstrain equivalent.

Temperature Gradients in the Test Specimen. Admittedly, most strain-gage-instrumented structures can not support temperature gradients on their surfaces sufficiently large to induce substantial temperature differences between the tabs of modern, small strain gages. Exceptions to this assumption would be radiantly heated structures (PETERSON, et al., 1962), internally heated structures by electrical methods (DODGE, 1967) or by neutron bombardment of fissionable materials (DITBENNER, 1966). It is with methods such as these, in fact, that step-changes in surface temperature are produced for evaluating transient response of temperature sensors. If the transducer is in a fluid stream, such as a resistance thermometer, then one junction may be in the shadow of the other, permitting a temperature gradient to exist and contaminating resistance-temperature data with thermoelectric effects.

Solder-Masses on the Gage Terminals. Under transient thermal input conditions, the thermal mass which governs the time constant of the lead-wire/gage-tab thermocouple is determined by the amount of solder at that junction. It is unlikely that the time constant of both junctions is the same, hence a transient net output may develop until thermal equilibrium is reached between the two gage tabs.

Adhesive Thickness under the Gage Tabs. The thermal resistance of the time constant of the lead-wire/gage-tab thermocouple is also determined by the adhesive+ backing layer thickness which separates the thermocouple from the specimen surface. It is unlikely that this quantity is the same under both gage tabs, and again, a transient differential response is possible until thermal equilibrium is reached.

Lead-System History. The fact that the leads to a transducer are usually single conductors assembled into a multi-conductor cable means that each conductor came from a different spool of wire when the cable was manufactured. It is well known that the resistance-temperature coefficient and the thermoelectric coefficient of most alloys, especially of copper, are not only inter-related, but are a strong function of minute trace elements in the chemical composition, of exact heat treatment history and of exact cold-work history among other past experiences to which the leads may have been subjected. (STEIN 1972B). In the Unified Approach, this effect of the past on the present is summarized by the integral:

$$\int_{t=-\infty}^{t=0} (\mathrm{COMETMAN})\,dt = \text{Individuality, or Childhood, or Past, History}$$

in which COMETMAN is an acronym made of the first letters of the various forms of energy which may have acted on the piece of hardware being considered since the day it was born (t = -∞) until the present (t = 0), which are:

C hemical
O ptical
M echanical
E lectrical
T hermal
M agnetic
A coustic/fluid
N uclear

It is quite possible that a thermocouple made of nothing more than the two conductors of supposedly identical materials in a single multi-conductor cable, will yield non-negligible thermal emf's. In many organizations every length of lead-conductor which will exist in high temperature gradients in service, such as on turbine blades or disks, is tested for homogeneity and identity.

The chart in Fig. 1 summarizes the thermoelectric scale of various materials often used for strain gages. It is noted that some combinations are prone to more trouble (copper/constantan) than others (copper leads with manganin, Isoelastic (RTM), or Karma (RTM), for ex.)

Both for thermocouples and for strain gages, swaged magnesium-oxide (MgO) or aluminum-oxide-insulated stainless-steel or Inconel-clad lead wire systems are sometimes used. The degree of purity of the ceramic insulation is crucial. Slight impurities cause the insulation resistance to drop markedly with rising temperature. This effect can produce a condition close to a short-circuit (a low-resistance path between conductors) if portions of such lead systems are heated in service. This condition happens when the leads are routed through a high-temperature region on their way to a lower temperature region. The low-resistance path created in the high-temperature region now creates an effective thermocouple in that region, if the two conductors are of differential materials (or the same chemical composition but of different heat treatment, cold work, etc.), and a low-resistance path. For strain gage installations, this low-leakage path across the gage may affect the data, and for thermocouples, the locally created thermocouple at the hot-spot may dominate the output from that sensor. These effects have been reported, among others, by MOFFAT (1975), and have been suggested as fire-detection method by RIEMER (1972). In that application a length of open-circuited MgO-insulated thermocouple with deliberately contaminated insulation is used. A fire-created hot-spot anywhere along the length of that open-circuited thermocouple creates a low-resistance path which, locally, now has created a thermocouple junction. So long as there is no output from this sensor, there is no fire. Any output voltage indicates a fire somewhere along the detecting line. A measure of the location of the fire can perhaps be obtained by measuring the resistance of the resulting thermocouple.

Other Self-Generating Thermal Effects. In metallic strain gages, other self-generating thermal effects are not likely to exist. In piezo-electric or semi-conductor sensors, photovoltaic of pyroelectric phenomena could produce thermally stimulated output voltages. The triggering of the emergency shut-down mechanism of a new nuclear power plant when a reporter flash-photographed its control panel (ANONYMOUS 1976A) may give some indications of unexpected results from transient thermal inputs or optical inputs.

Transient Thermal Response of Transducers

The literature on the effects of thermal inputs on transducers not supposed to measure temperature, is voluminous. The study by DEAN & FLYNN (1966), for example studies zero shifts, sensitive changes etc., of pressure transducers in cryogenic temperatures and temperature gradients. Of more interest is the recent attempt to produce a standardized method or producing temperature transients or gradients so that anyone, anywhere can test transducers in a like manner. Among the mechanisms studied are modified soldering irons with a pool of molten Wood's metal acting on pressure transducers (HORN 1969), chopped CW laser beams acting on pressure transducers (LEDERER & HILTEN 1972), incandescent lamps acting on piezoelectric accelerometers (VEZZETTI & LEDERER 1975) and the apparently most successful method of producing repeatable thermal transients by means of a photo-flash-bulb, initiated by SWISDAK (1972) and perfected into a standardized method by HILTEN et al., (1976)

Other Effects due to Thermal Transients

During a thermal transient which passes through a surface-mounted sensor such as a strain gage, due to externally applied radiant heat or internally applied nuclear or electrical heat, the gage grid is not at the same temperature at the same time as the specimen surface to which the gage is bonded. This means that all concepts of self-temperature compensation, which are predicated on a thermal equilibrium between gage grid and test specimen, are temporarily suspended. The gage grid behaves as though it were thermally isolated in an adiabatic manner, with time constants in the micro-second range; a second time-constant involves the gage grid with the backing and adhesive, in the milli-second range; and other time constants involve the medium above and below the gage -- to the specimen by conduction and through the coating on top of the gage to the ambient medium, by convection -- in minutes or hours. Until the gage is in thermal equilibrium with the specimen surface beneath it, self-temperature compensation principles are violated. Usually the elapsed time for reasonable equilibrium after a sharp thermal pulse, will be in the milli-second range. Effects such as these, including self-heating of the gage when subjected to current pulses, have been studied by GERIGK (1966), WALKER (1966) and the literature prior to 1962 is summarized by STEIN (1962B, 1962C). To these must be added the work done by Lawrence Livermore and Sandia Livermore on single-pulse operation of manganin pressure transducers where the thermal response must be taken into account.

A thermal pulse entering the specimen through a strain gage may also create thermal strains, since the surface to which the gage is attached is heated rapidly, but the specimen material away from this surface receives heat by conduction, much more slowly. The gage itself may even act as a shield, creating gradients on the surface being radiated. The thermal pulse may therefore create real mechanical strains in the specimen (bending of a plate, for ex.), which may be impossible to separate from the transient thermo-resistive responses due to the violation of the self-compensation principle as discussed above. Gradients of 100°F between the gage grid and the specimen surface have been reported at heating rates of 30°F/s (ANON. 1969). BREWER (1969) has used temperature measurements to infer the transient, thermally induced strains in the time period in which the structure responds to the thermal transients in an adiabatic manner, and BEAN (1969) has used strain gages in unheated portions of a structure to infer the thermal strains created by temperature changes elsewhere.

ELECTRO-MAGNETIC INDUCTION EFFECTS

If the lead-wire system of any transducer, and/or the grid of a strain gage interrupt a magnetic field in a time-dependent manner, then an induced voltage will be generated in the circuit, given by: $e = K.N.A.\ \partial\phi/\partial t$ where e is the generated voltage, K is a constant, N is the net number of turns, A the area of these turns which ends up being perpendicular to the magnetic flux lines, and $\partial\phi/\partial t$ the net rate at which flux linkages intercept the grid-lead system. This effect may (and has) occur in:

a./ Stationary magnetic field with leads and gages vibrating or rotating through the field.

b./ Stationary gage-lead systems with time-varying fields intercepting them, produced by motors, generators, soldering irons, welders, rotating or vibrating magnetized parts such as gears, magnetic vibration exciters, high-current-carrying lines, etc.

c./ Combinations of the above.

Even the earth's magnetic field is enough to induce significant (half to one mv) signals in cases where strain gages move very rapidly through this field such as during stationary impact tests. The traveling displacement wave which always accompanies the stress-and-strain-producing force wave, not only travels very rapidly, but has a very steep wave front as well, giving a high velocity of the gage grid through the earth's field. These transient noise-voltages appear to the same time-scale as the expected signals and are created by the same phenomenon being studied -- the impact. Thus noise can not be separated from signal by either frequency-selective filtering or by correlation techniques. The problem of impact-created noise-voltages has been extensively studied for strain gage circuits, but the literature comes to no uniform agreement. KRAFFT (1954), MEITZLER (1956), VIGNESS (1957), RIPPERGER & YEAKLEY (1963), STEIN (1963, 1962 , ND) report various experiences. The explanatory mechanism offered above for these phenomena is supported by work by FRANZ (1961) who ran tests on wave-speed propagation in metallic specimens by simply wrapping wires around the specimen at two locations, and placing the specimen in a DC magnetic field. The time-delay in the pulse outputs from the two coils a known distance apart, gave him the wave propagation speed.

Among the documented case studies in the literature are the Mach 3 sled at Holloman Air Force Base, where leads must be mounted parallel to the earth's field to avoid induction effects (PRICE 1972). Other cases involve rotating parts such as gears, made of magnetic material, and which have been magnetic-particle inspected for cracks, and not completely demagnetized. Rotating magnetized domains on such parts will couple into strain gage and thermocouple systems with whatever frequency corresponds to the net rotational speed of the gear with respect to these transducers.

In machines where hot, ionized gases flow over magnetic materials, such materials can become magnetized during engine operation, even when the parts are carefully demagnetized beforehand. Jet engines are among recognized offenders in this category. Thus vibrating, magnetized stator vanes will induce signals in near-by strain gage or thermocouple channels, producing in them all the evidences of mechanical vibrations, such as responses at various modes of vibration, and resonances -- but modes and resonances of the magnetized and un-instrumented vane ! -- by electromagnetic induction. Many parts become magnetized in service for no apparent, understandable reason. Case studies for gear trains, jet engines and compressors have been reported and the mechanisms which govern these phenomena are not fully understood (BRIDGE 1971, 1971A, 1971B; DOLLERIS 1975A, 1975B; GORTON 1975; GRISSOM, 1975; WACKER 1975). Magnetic fields created during firing of guns were reported by HENDERSON (1954) as noise sources in strain gage circuits.

In the references cited above, the magnetic-field-induced outputs from the strain gages were almost uniformly larger than the signal produced by the strain in the strain gages, and in the same frequency range, and periodic, sometimes with the same period as the dynamic strains. These are among the more difficult, but not insoluble, measurement problems. Sometimes the simplest solution is a careful de-magnetizing of all parts of the test specimen, if that is possible or economical. The only solution in some instances is the use of a high-frequency carrier system, to be discussed later (STEIN 1975A) of which the 500 kilo-Hz system used by Pratt & Whitney to solve such a problem, is an outstanding example (GORTON 1975).

Problems which appear in slipring circuits and which have not been documented before to the author's knowledge, include the generation of a magnetically induced DC voltage in the slipring when the shaft is heavily magnetized (DOLLERIS 1975B). The action is that of a homopolar tachometer, which actually uses that principle for generating its signal. The DC voltages now get into the slipring circuit and hence into the thermocouple or strain gage circuit. A 100°F error was documented due to this effect in a temperature measurement application, by Dolleris.

STRAIN-INDUCED VOLTAGES

Among the least understood, most perplexing and very common phenomena are voltages induced in conductors by dynamic strains, both in strain gages and in lead wires. They are not truly piezoelectric as that phenomenon is normally understood and defined, but they certainly appear to be electrical phenomena caused by mechanical forces.

They have been reported by a large number of strain gage and thermocouple users. Rolls-Royce (BRIDGE 1971, 1971A, 1971B) documented high emf's generated in nickel lead wires used on a rotating strain gage test in which sliprings were involved. Laboratory tests showed a 1 mv peak-peak signal in Nickel wire when knocked or vibrated in the wire axis direction, and independent of wire length. This effect was found also in Alumel, but not in Iron Copper, Platinum or Chromel at the room temperatures used during the test. The interfering signal in the specific application reported, was at 14 KHz and of such a high amplitude that it represented some 100 times the bridge output due to strain, so that the noise came through even a 100 KHz carrier system. In addition to the voltages generated in the Nickel wires, others were found to exist in other applications such as on gears.

Pratt & Whitney made similar observations at about the same time (HOFMANN 1971, BLISS 1972). An Alumel wire connected to the terminals of an oscilloscope yielded damped trains of 20 KHz pulses at room temperature when struck sharply. This voltage is apparently independent of the shape or size of the loop, and of its orientation relative to ambient magnetic fields. If a permanent magnet is brought near the loop, a train with a much longer decrement composed of much lower-frequency oscillations is produced. The effect was observed in all ferromagnetic materials tested, also in Chromel when cooled below its Curie point in liquid nitrogen (compare with the Rolls-Royce experience above). The voltage is halved by shunting it with a resistance equal to that of the wire, as might be expected. Non-ferromagnetic materials appear to behave similarly, with several orders of magnitude less voltags.

MOFFAT (1969) reported similar effects in Chromel-Alumel thermocouples, and GRISSOM (1975) and WACKER (1975) found these effects in Nickel leads on non-rotating but highly stressed parts operating at high temperature, using Karma-alloy strain gages.

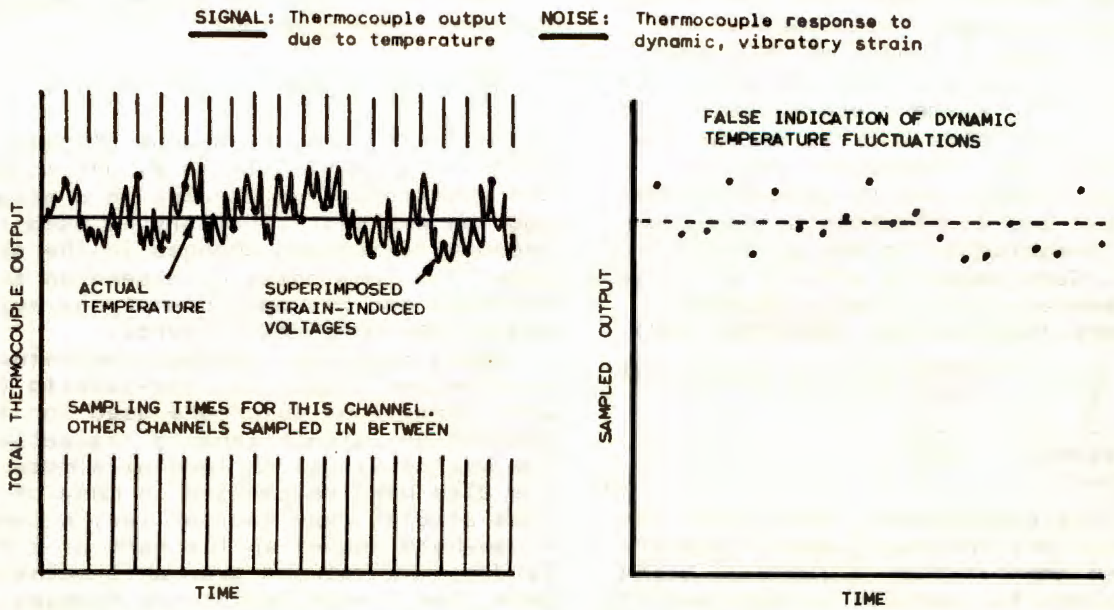
The nature of the problem for dynamic strain measurement is obvious -- spurious voltages are added to those used to interpret strains, so that an undesired, additive, temporary, self-generating response to strain is added to the desired additive, temporary, non-self-generating response to very much the same strain; this makes frequency-selective filtering and correlation techniques again inoperative.

The nature of the problem for static temperature measurement is not quite so obvious, but of much concern to organizations such as Pratt & Whitney where sophisticated, modern, computer-controlled multi-channel high-speed sampling (multiplexing) of numerous thermocouple channels is used. Whereas the high-frequency strain-induced voltages in the thermocouple data used to be averaged out by the individual low-frequency-response recording channels, the rapid sampling of the channels now allows the high-frequency noise to appear in the data, as illustrated in Fig. 2.

An understanding of the mechanism of transient-voltage generation in strain-gradient fields may be forthcoming from papers such as by ZAHARIA (1973, 1974)

FIGURE 2

SAMPLING A "DC" SIGNAL WITH SUPERIMPOSED HIGH-FREQUENCY "NOISE"



This generated emf is attributed to a charge carrier equilibrium perturbance. The author investigates the phenomenon with a view towards its exploitation as a strain measurement technique.

The problems of voltage generation in strain gages during impact tests has already been discussed in the section on electro-magnetic induction. It is another not fully explained phenomenon.

TRIBO-ELECTRIC EFFECTS

Voltages generated by rubbing or sliding surfaces, or due to the sudden separation of a conductor and an insulator, have been postulated by many investigators as the source of electrical signals in rotating machinery or gear trains, and in electrical cables. These signals are coupled to the strain gages or enter the data channels via ground loops or other as yet mysterious means. (BRIDGE 1971, RATZ 1970, MAYO-WELLS et al., 1973)

In cables, tribo-electrically generated voltages have a long and honorable history, being even used as vibration transducers, such as caused by intruders who walk or drive over buried cable (WEISS 1973) or as pressure transducers (PYE UNICAM LTD., PHILIPS INSTRUMENTATION DIV)

Cable to minimize tribo-electrically generated charges have long been available for piezoelectric accelerometers. A recent product uses semi-conducting tape wrapped around various internal portions of the cable to distribute the charges so generated (TIMES WIRE & CABLE CO, ANONYMOUS 1975), and suitable for strain gage use.

For checking tribo-electric effects generated in accelerometer cables, HARPER (1968) reports an ingenious technique. "Induced signals from cable whip and motion in the electromagnetic field of the exciter were checked by removing the pin from the coaxial connector and measuring the response signal as the frequency range was swept at maximum test level . . . Note that the mechanical arrangements of the test set-up was not altered, only the electrical circuit was affected."

MAGNETO-STRICTIVE EFFECTS

Changes in mechanical dimension due to magnetic fields may not be a problem for strain gages which are bonded to conform to the specimen dimensions, but it will affect the flexure to which the gage is attached. If this occurs in a transducer (load cell, pressure cell, accelerometer) because the flexure is magneto-strictive, the strain gage may sense strains that are due to the dimensional changes of the flexure in the magnetic field and not due to the measurand for which the flexure is designed. The author has no data on strain-gage-based transducers, but the data from SCHLOSS (1969) for piezo-electric accelerometers cited below, may give the reader a flavor of what may happen. If the electrodes plated on the piezo-electric element are Nickel or if magnetic components such as screws are used within the transducer, magnetostrictive effects may be generated and the forces and displacements thus produced, communicated to the piezoelectric element. Some magnetic effects on various accelerometers for a 1 gaus magnetic field were found to be: (SCHLOSS 1969)

Unit A	30,000 micro-g equivalent
Unit B	200 "
Unit C	20
Unshielded	200
Shielded	7

Without any endorsement, the author reports that the following accelerometers have been mentioned or claimed as being satisfactory for service in high magnetic fields: Endevco 2217E (STEIN 1975B), B&K 8308 (ANONYMOUS 1976B).

It should again be remembered that around magnetic vibration exciters the frequency of the magnetostrictive noise response will be the same as that of the signal -- or in the same range -- and directly correlated with the same excitation which forces the specimen being tested. The case for a "dummy channel", a transducer not exposed to the desired environment (such as an accelerometer suspended on rubber bands near the measuring accelerometer) but in the same undesired environment, is very strong. Dummy channels will be discussed later, (STEIN 1970A, 1974).

(Although they are not discussed in this paper, magneto-elastic effects, the changes in elastic modulus due to ambient magnetic fields, may also affect strain-gage-based transducers. The author has seen no data on this adverse effect in strain-gage transducers although transducers which operate on the magneto-elastic principle are well known.)

INTERIM SUMMARY: SELF-GENERATING "NOISE"

All the undesired environment-response combinations discussed to this point are of the self-generating variety. For strain gages in particular, and for all non-self-generating-responding transducers, such noise levels are EASY TO DOCUMENT and (in principle) EASY TO SUPPRESS IF DOCUMENTED, as will be discussed in the section on Noise Suppression.

MAGNETO-RESISTIVE EFFECTS IN STRAIN GAGES

Many materials are magneto-resistive and obey laws such as:

$$\frac{\Delta R}{R} = K_1\phi + K_2\phi^2 + K_3\phi^3 + \ldots$$

where ($\Delta R/R$) are resistance changes produced by magnetic fields ϕ, and which are interpreted as strains in a strain gage by whatever circuitry is used to measure resistance changes in the strain gage. The constants, K, depend on the material and its past history as evidenced by the COMETMAN-integral.

Among materials strongly magneto-resistive are Nickel and Iso-elastic (RTM John Chatillon) which are used for temperature and strain sensors respectively. The use of Nickel as lead-wire material has also been documented in many of the case studies reported earlier. A common house-hold magnet in the form of a stud-finder, when placed over an Isoelastic gage, can create resistance changes that are the equivalent of several 100$\mu\varepsilon$. A time-varying magnetic field can induce time-varying resistance changes which are not suppressed by the use of a carrier system (or modulation technique). A fundamental truth about carrier system operation should be noted, which applies no matter what their wave shape, frequency or manufacturer:

> Every well-designed carrier system can separate self-generating voltage responses in a strain gage circuit, from non-self-generating resistance changes. No carrier system in the world can separate resistance-responses due to strain from resistance-responses due to temperature, magnetic fields, or any other environmental factor. THAT kind of separation must be made either by mutual compensation or minimizing-by-division techniques. (STEIN 1975A).

In addition to the above problem, the fact that magneto-resistive responses for materials used in strain and temperature measurement, are governed by at least a quadratic and often a cubic equation, as shown above, means that the frequencies of resistance change produced in the strain gage are NOT those of the magnetic field !!! For a sinusoidally varying magnetic field, such as used on vibration exciters or available from the 60Hz line:

$$\phi = \phi_o . \sin\omega t$$

and the resistance changes in the gage will be:

$$\frac{\Delta R}{R} = \phi_o \quad K_1.\mathrm{Sin}\omega t + \frac{K_2}{2}(1-\mathrm{Cos}2\omega t) + \frac{K_3}{4}(3\mathrm{Sin}\omega t-\mathrm{Sin}3\omega t) + . . .$$

showing clearly the creation of DC levels which will appear as static strains ($K_2/2$ in the above equation); of 2nd harmonics ($2\omega t$) and third harmonics ($3\omega t$), but also through the K_3 coefficient of the cubic term, a signal at the same frequency as the exciting field $(3/4)K_3$. This is a special case of the generalized law that non-linear systems are "frequency-creative" in the sense that frequencies at the output do NOT correspond to frequencies of excitation, (STEIN 1976). Filtering the output to excitation frequency, ω, a trick often used to "get rid of the non-linearity effects" will not work because of the $(3/4)K_3$ term at that frequency, created by the non-linear transfer function. This property should be remembered on vibration tests where tracking filters are a common and dangerous occurrence.

Magneto-resistive effects are, of course, expected in magnetic alloys such as Isoelastic and Nickel, and in materials used for linear magneto-resistive sensors such as Bismuth (AMERICAN AEROSPACE CONTROLS INC.) and certain semiconductors (BORCKE 1972). They also exist, however, in unsuspected alloys, such as constantan. For the variety of constantan copper-nickel alloys manufactured by Wilbur B. Driver Co., as Cupron, information is given by WANG & KUBILINS (1957)(see also STEIN 1962B) which permits an understanding of the strange phenomenon occasionally reported, such as by GUNN (1960), GUNN & BILLINGHURST (1957), HARTWIG & WÜCHNER (1976), STRAUSS (1958), TAKAKI & TSUJI (1958), PETERS (1977), WALSTROM (1974) which indicate magneto-resistive responses in constantan.

The resistance-temperature coefficient of at least partially cold-worked Cupron wire or foil, after any heat-treat-annealing cycle can be expressed as a function of the alloying constituents deliberately introduced into the melt to control the resistance-temperature coefficient (RTC) in any temperature span.

$$\mathrm{RTC} = K + A(\%Fe) + B(\%Mn) + C(\%Co) +$$

K depends on the copper/nickel ratio. A, B, C are constants up to a certain limit of percentage composition of the additive elements. Calculations of RTC can be expected to be within $\pm 10\times10^{-6}/^{\circ}C$ of the measured value. RTC does not even have to be measured since for constantan of the Cupron variety: (WANG & KUBILINS)

$$(\mathrm{Resistivity})(\mathrm{RTC}) = 0.07$$

Since strain gage manufacturers select melts with different compositions, for in-house heat-treatment to produce self-temperature compensation characteristics for these gages mounted on materials with a very high variety of thermal expansion coefficients, constantan gages with different self-compensation characteristics may be expected to have slightly different chemical compositions, which explains what PETERS (1977) found.

Constantan gages of the Micro-Measurements EA-06-125-BB variety had been used in measuring the magnetostriction of pure nickel up to fields of about 6 kOe (HUSTON et al., 1973) without showing any anomalies. In more recent work up to 15 kOe, large apparent strains of the same sign and about the same magnitude as the real strain, were observed in the DC magnetic fields. Measurements were made with a BLH SR-4 carrier indicator. Two Karma alloy gages, WK-05 and WK-15 showed no apparent strain to above 15 kOe.

"A pure nonferromagnetic alloy such as 55%Cu-45%Ni alloy should not show a magnetoresistive effect, at least at temperatures much above the Curie temperature. Just above the Curie temperature, a small decrease in resistance with increasing H has been observed for a number of materials. In this region, the magnetoresistive effect is independent of the relative directions of the magnetic field and the current. This we found to be true; turning the gage 90° did not affect the sign or size of the apparent strain.

"Although the base alloy is well above its Curie temperature, a precitipate of iron and nickel would have a much higher Curie temperature. Most copper-nickel alloys contain small amounts of iron as well as manganese. The energy dispersive X-ray spectrum (Figure 3) of our EA-06 gage clearly shows that both iron and manganese are present. I would guess that it is the presence of this precipitated phase that is responsible for the magneto-resistive effect. . . The magnitude of the effect should thus depend on the heat treatment and would, in fact, disappear in the solution treated and rapidly quenched condition. Considerably more work would have to be done to experimentally establish this correlation and confirm the theory" (PETERS 1977).

Thus both the in-house heat treatment given to strain gage wires and foils by the strain-gage manufacturer, and the different composition of different lots would affect magneto-resistive response.

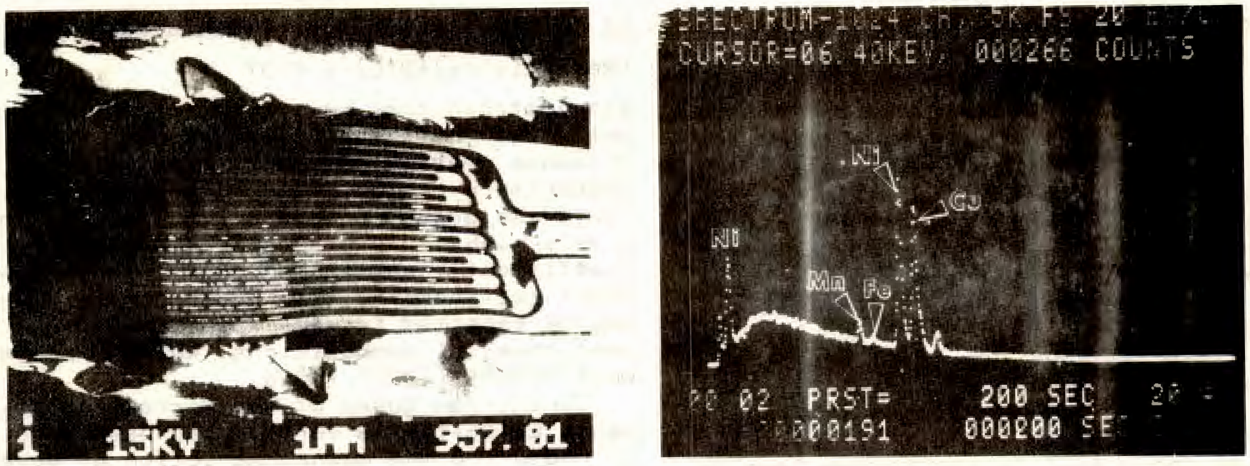


FIGURE 3: SCANNING ELECTRON MICROGRAPH (x18) OF EA-06 MICROMEASUREMENTS STRAIN GAGE WITH ITS ENERGY DISPERSIVE X-RAY SPECTRUM (PETERS 1977 WITH PERM.)

TABLE 2: STRAIN VS. MAGNETIC FIELD FOR 3 CONSTANTAN STRAIN GAGES

H (kOe)	STRAIN (10^{-6}), MICROSTRAIN		
	EA-06	EA-05	EA-40
0	0	0	0
1.93	0	1	0
3.85	2.2	1.5	0
5.87	4	3	0
7.70	5.5	5	0
9.50	10	8	1.5
11.3	13.2	10.5	4
12.7	16.2	14	5
14.0	19.5	15	6.5
15.0	22.3	18	8
15.7	24.5	20	9

(From PETERS 1977, with permission. Data are averages of at least 2 runs).

Magneto-resistive effects in constantan are also reported by TAKAKI & TSUJI (1958), showing an increase in electrical conductivity in the presence of magnetic fields. The temperature dependence of this effect is reported for strain gages bonded on copper strip in magnetic fields. Fields up to 20 kOe were used at temperatures from 100°C to -182°C:

$$\Delta R/R = -0.011\ H^2\ T \times 10^{-6}$$

with H in Oersteds and T in Kelvin. Although at room temperature the authors could eliminate the effect by means of a compensating gage arrangement, at low temperatures the differences between the measuring and compensating gages were so large as to make compensation almost impossible, as might be expected (STEIN 1962B).

WALSTROM (1974) tested Micro Measurements EA gages up to 25 kGauss and to temperatures as low as the boiling point of liquid nitrogen, showing a quadratic dependence of the strain error with increasing magnetic field up to 50 με at 25 kGauss. He, as had PETERS (1977) found no effect for the WK gages for perpendicular and parallel fields and for strains up to 1000 με both tension and compression. A WK gage which was strained to over 2000με (but without knowledge of how high) showed a small effect of 10με, which saturates with increasing field at about 15 kGauss and is independent of orientation and strain.

TSUJI (1958) found a strain gage alloy of 57%Ni, 16.5%Cr, 24Mn, 2.5%Mo as being satisfactory for measurement of forced magnetostriction above technical saturation on some ferromagnetic materials at various temperatures from the boiling point of liquid oxygen to about 250°C. He could not use constantan gages because of magnetoresistive effects.

HARTWIG & WÜCHNER investigated WK alloy Micro-Measurements gages up to 70,000 Gauss and appear to substantiate the work of Peters and Tsuji cited above, and by WALSTROM (1975) and BIRSS & LEE (1960). Tested at 4.2°K the WK-06-250BG-350 gages showed a quadratic magneto-resistive effect with 200με indicated strain at 70 kGauss with little directional effect and negligible hysteresis with magnetic field. The non-inductive WK-13-125WJ-700 gage showed a -100με indicated strain at 70 kGauss at 4.2°K. GREENOUGH & LEE (1967) showed that the exact composition of these Ni-Cr-based alloys will affect the sign of the magneto-resistive effect.

DIAGNOSIS OF "NOISE" OR UNDESIRED ENVIRONMENT-RESPONSE COMBINATIONS

Extensive discussions of diagnostic procedures based on the Unified Approach to the Engineering of Measuring Systems are given in past publications on this topic (STEIN, 1962A, 1963, 1970A, 1971A, 1972A, 1972B, 1972D, 1973, 1974, no date) Only a basic review will be given here, primarily as these methods apply to strain gage circuits, although they also apply to piezoelectric transducers (STEIN 1962A) and to thermocouples (STEIN 1962A, 1973) as specific examples of other transducers. The method, of course, is general.

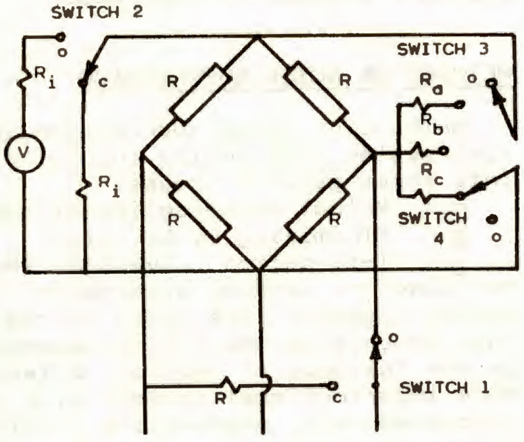
Impedance-based transducers utilize their non-self-generating response as the desired response. This means that they have to be interrogated from an external energy or power source such as a bridge supply voltage, a current source, etc. Their output is fed through "input-conditioning" circuitry usually to an amplifier. It is this set of circumstances which are illustrated in Figure 4. The perfectly general method is given in the various references cited above.

STEP 1: Always incorporate a switch shown as 1 in the circuit. In the CHECK position it creates zero input (or differential input) to the amplifier but maintains any existing common mode signal and the impedance levels which the amplifier "sees". Any problems which arise during this check -- which should give zero output -- are surely NOT due to the bridge circuit, which has, in fact, been disconnected. For diagnosis and solution of the problems thus documented see STEIN (1972B 1972C),

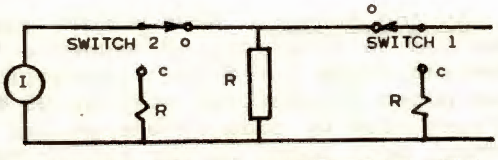
STEP 2: always incorporate either a bridge-power disconnect switch or a polarity reversal switch-pair shown as 2 and 2'. Any output from a power-disconnected circuit is by definition self-generating (see Table 1) voltage responses from the circuit. Polarity reversal may be more convenient in certain cases, especially digital systems, so that the system does not have to range back and forth. For polarity-reversal, the self-generating noise is half the difference between the check and operate readings.

FIGURE 4

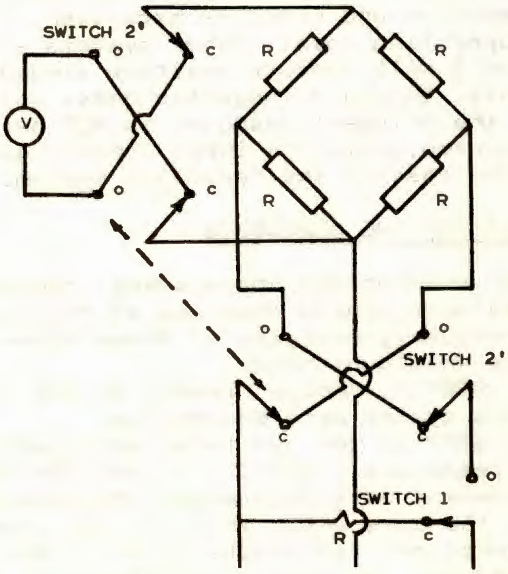
INPUT CONDITIONING CHECKING CIRCUITS SHOWING DIFFERENT FEATURES



AMPLIFIER CHECK (1), INTERROGATING INPUT DISCONNECT (2), SHUNT CALIBRATION POSITIVE (3), AND NEGATIVE (4) THREE-POINT EACH WAY



AMPLIFIER CHECK (1), INTERROGATING INPUT DISCONNECT (2). SHUNT CALIBRATION CAN BE ADDED FOR NEGATIVE THREE-POINT CHECK.



AMPLIFIER CHECK (1), INTERROGATING INPUT AND OUTPUT REVERSAL (2'). SHUNT CALIBRATION SHOULD BE ADDED AS ABOVE.

o = OPERATE POSITION c = CHECK POSITION

STEP 3: Always try to obtain one of the following conditions:

a./ Remove the measurand, the quantity to be observed, without changing any of the other operating conditions of the test, or, if this is not possible, as is often the case

b./ Utilize a "dummy" or "check" channel, which is a second transducer as identically as possible a twin to the measuring transducer. The object is to keep both of them in the same undesired environment, but to expose ONLY the measuring transducer to the desired one. The references cited above have collected a number of ingenious methods thought up by various investigators to accomplish this aim. The "dummy" strain gage is, of course, the simplest case commonly used.

This check will produce the sum total responses (self-and non-self-generating) to the undesired environment.

STEP 4: Always attempt a calibration, with the actual measurand if possible, or with injected resistance signals or injected voltage signals where actual application of the measurand is not possible. In a surprising number of case studies cited in the references, ingenious methods for calibration during an actual test, with the actual measurand, were found feasible. A typical resistance-injection system is shown in Fig. 4 as shunt calibration resistors, of which a set of three will give 3-point calibrations (which are also linearity checks) in positive and negative excursions from the operating point. Other systems exist. Switch 3 will produce positive simulated outputs, Switch 4, negative ones. Note that the bridge-transducer is NOT being calibrated, only the input conditioning and the rest of the recording system.

RESULTS OF THE DIAGNOSIS

For each of the above steps, records of the wave shapes observed at the output and frequency-analyses of these waveshapes should be made.

In STEP 1, "noise" levels in the recording system were documented.

In STEP 2, the sum total self-generating responses (Table 1) to the sum total environment were documented. These are often the ones which exist to the same frequency and time scale as the signal to be observed, and are correlated with it because both are caused by common phenomena, such as magnetic fields, thermal transients, strains, etc.

In STEP 3 the sum total responses but only to the undesired environment are recorded.

In STEP 4 any multiplicative responses (changes in transfer ratio or calibration factor) of the recording system are documented. Steps 1-3 only documented temporary additive effects only, in their simplest application.

It is important to note that the principle on which this facet of the Unified Approach is based is to try to document, beyond reasonable doubt, the innocence of the measuring system. This is expressed by DRANETZ (1974) in the fundamental, if anti-Western-civilization philosophy:
A MEASURING SYSTEM IS ASSUMED GUILTY UNTIL IT IS PROVEN INNOCENT beyond a reasonable doubt. It is also commensurate with the analogy between measuring systems, which exist for the sole purpose of obtaining information about some on-going process, and spy organizations, which exist for the same purpose (STEIN 1971C): both must use second-order spies to spy on the spies. Thus every channel of information must be checked up on some of the time, and critical channels must be checked up on all of the time. This recipe demands either test time or channel capacity. Sometimes neither of these is made available by the management responsible for the entire test program. It is then wise to recall VERHAVE's (1961) definition of the HUM-level in organizations of human organisms: Higher Up Management. For this kind of noise level there are also recipes, but they are beyond the scope of this paper.

METHODS OF NOISE SUPPRESSION

Methods of noise suppression in measuring system with analog inputs, divide into three general groups:

a./ Mutual Compensation of two channels
b./ Minimizing by Division
c./ Information Conversion (Modulation).

Detailed and general discussions with numerous examples, are cited in the literature describing the Unified Approach cited in the "Diagnosis" section. A few of the more important applications will be briefly discussed here, emphasizing little recognized problems which arise in their use.

Mutual Compensation of Two Channels

In the Sensor: When strain gages are exposed to time-varying magnetic fields, non-inductive (bi-filar) connection of two gages is possible with grids mounted either side by side or one on top of the other. Both forms can be either home-made or purchased commercially either in wire or foil configurations. The principle applies to temperature sensors as well.

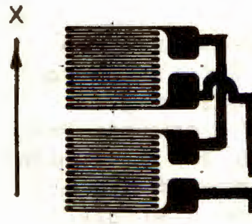


FIG. 5

NON-INDUCTIVE SENSOR ARRANGEMENT

May be side by side as shown here, or on top of one another.

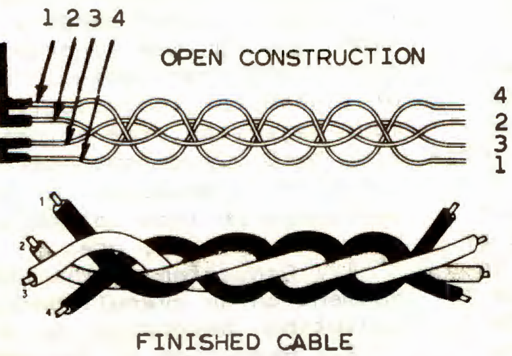
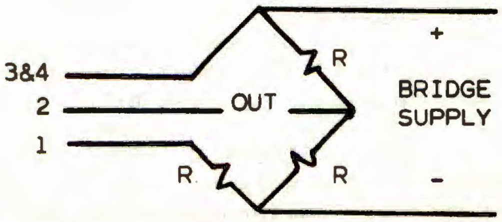


FIG. 6 INTER-8-WEAVE CABLE

(Manufactured by Magnetic Shield Division, Perfection Mica Co.)

In normal use with 2-conductor connections, wires 1&2 and 3&4 are connected together at the terminals. Shown here in a 3-wire connection with a non-inductively connected transducer.



R = bridge completion resistors

FIG. 7 THREE-WIRE CIRCUIT

The 3-wire circuit minimizes impedance changes in the leads provided all three leads experience the same environment and respond to it in the same manner. This figure shows the principle only. For practical details see other illustration.

The expected magnetic field gradients must be considered in selecting between the side-by-side method, where gradients in the X-direction (Fig. 5) are dangerous, and the super-posed method where gradients through the gage thickness may destroy the expected compensation (less likely). It is well to remember that two transducers can only produce mutual compensation by subtraction -- or common mode rejection, if: they are:

REQUIREMENTS	PROBLEM AREAS
in the same environment at the same time	Gradients and transients in that environment
responding the same way	Individuality (COMETMAN integral)
Arranged in a differentially coupled structure which "knows" how to subtract	Common mode rejection ratio must be adequate

The problems associated with the mutual compensation of two transducers in hostile environments, studied according to the Unified Approach, have been discussed by STEIN (1972A, 1972B, 1972C, 1973).

Although strain-relief loops are often suggested for the wiring between the sensor tabs and the lead-wire anchor-tabs, LOOPS should be avoided for sensor service in magnetic fields. One must be very careful not to compromise the fatigue life of the installation in the wiring lay-out.

In the Lead System: Twisted leads to subtractively cancel the effects of magnetic fields on these leads, are an age-old solution which is not always successful for the following reasons:

1./ Magnetic fields have gradients in linear directions. Unless the magnetic field gradient per wave-length of twist is acceptably small, adjacent twists will not be in the same environment at the same time and cancellation can not occur.

2./ Magnetic fields also have gradients in angular directions. Adjacent loops simply can not be in the same plane -- they will be along a spiral. Thus adjacent loops will not experience the same magnetic field because of the directional properties of these fields. The Magnetic Shield Division of Perfection Mica Co., for many years, has had a patented cable construction available to minimize the angular gradient problem (Fig. 6).

3./ Practical considerations such as being on the surface of high-speed rotating blades or disks, may preclude twisted cables. It is difficult enough to keep parallel-wire configurations on the surface of high-speed rotating parts with high centrifugal forces tending to tear the cable away from the surface. Anchoring twisted cable may not be technologically feasible. Sometimes it is possible to arrange for ONE cross-over point of the wiring. If this is suitably selected, then the resulting loops may, in fact, cancel their magnetic pick-up.

The author saw such an application involving thermocouple leads routed along a turbine disk during his 1962 visit to Rolls Royce Aero Engines in Derby, England.

The coiling of cables which connect transducers to the input conditioning instrumentation may also be a problem when these cables are routed through magnetic fields. SCRIBNER (1976) cites the example of a precision load cell which was sent for calibration with 200 ft of cable attached, which would be used in the final installation. The cable had to be coiled in the calibration facility. The inductive nature of the resulting coil permitted evidence of ambient magnetic fields to be introduced into the calibration instrumentation. The calibration for the load cell at the end of a straight 200 ft run of that cable did not match the laboratory data. Note that cables can easily be coiled in a non-inductive manner along the same principle as shown in Fig. 5. The center of the cable is used to start the coil, resulting in an equal number of loops of the same area going in both directions.

Mechanical strains in lead wires may cause resistance changes which can be confused with those produced by a strain gage. Although copper has a low resistivity, it does have a gage factor of 2, and enough instrument sensitivity or lead length may cause problems. The first mention of strains in lead wires creating bridge unbalances is probably WHEATSTONE (1843) who writes in the paragraph which follows the description of his "Differential Resistance Measurer" (which we know today as Wheatstone's Bridge):

> "slight differences in the lengths, and even in the tensions of the wires, are sufficient to disturb the equilibrium" of the circuit.

Had be "patented" or taken advantage of the opposite of this problem, it would be Wheatstone and not William Thompson (Lord Kelvin) who would be credited with the discovery of the principles on which today's resistance strain gage operates.

MEIER (1956) provides a more recent example of strain gages on the bucket of an excavating machine, with cables hanging from that bucket without strain relief, creating cable-strain-induced signals which depended on the swing and sway of the cable during operation. The cable for the compensating "dummy" gage had been routed along the structure, thus assuring strain-relief of the cable. The use of a single compensating sensor for many measuring channels sometimes results in problems of this type.

Minimizing by Division

Whereas the Mutual Compensation method requires two sensors for its operation, giving rise to problems of gradients and transients in the environment, of transducer individuality and of common mode rejection, the principle of Minimizing by Division operates on a single sensor or transducer. Of the techniques available in this category of noise suppression methods, are:

1./ Reduction of the undesired environment on an overall basis: Shielding isolating, absorbing, or screening.

2./ Reduction of the undesired environment on a selective bases: frequency-selective filtering, for example

3./ Self-compensation, such as the non-inductive gages already discussed, temperature-compensated strain gages or strain-compensated resistance thermometers, etc.

3./ "Side-ways Promotion" through reduction of the interrogating input. One example is the wire shown as 2 in Fig. 7: a resistor through which there is no current can not generate a voltage. Thus that third wire from the strain gage, in the output circuit of the bridge where, at balance, no current flows, can not convert any environment-created resistance change into a noise-voltage. The voltage-output terminals of a 4-terminal resistor are another of numerous applications of this principle which is: Place a necessary but obstreperous member of your team in that part of the system organization chart (circuit) where there is no action -- he then can not contribute to the output but is still there doing his job. (Sideways promotion)

4./ "Jail" through reduction of the effect of the noise level. The social parallel would be the placing of a necessary but highly obstreperous member of the team in a "padded cell" and not allowing contact between him and the customer (system output recipient) to occur. The bridge-input lead resistance when fed from constant-current source conditions, will show a large voltage drop along these lead wires, but no evidence of this will be seen at the bridge output. The power-input terminals of a 4-terminal resistor utilize the same principle.

A detailed discussion of the principle and numerous applications of this noise suppression method are given elsewhere (STEIN 1962A Chapter 6, 1974, 1976). In this paper only a few of the more pertinent examples will be cited, emphasizing often-neglected problems.

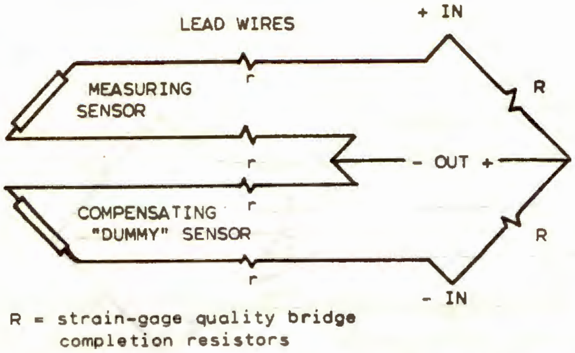


FIG. 8: COMPENSATING OR "DUMMY"-SENSOR METHOD OF LEAD COMPENSATION

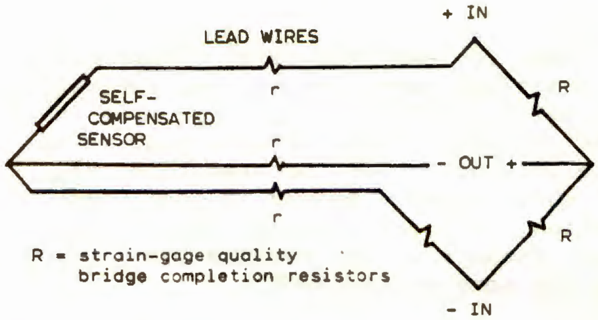


FIG. 9: "THREE-LEAD-WIRE" CIRCUIT FOR LEAD COMPENSATION FOR A SELF-COMPENSATED SENSOR

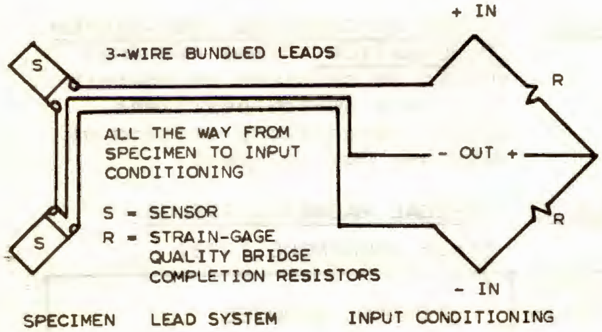


FIG. 10: THREE-WIRE CIRCUIT: PRACTICAL EXECUTION WHEN "DUMMY" SENSOR IS USED. (HASTINGS, 1976)

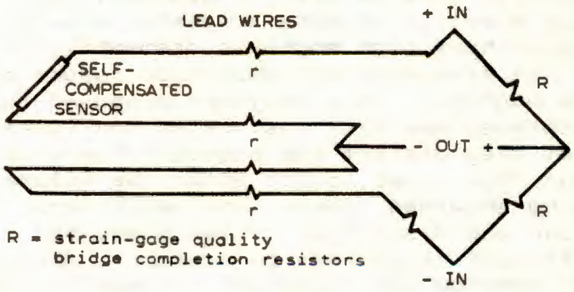


FIG. 11: TWO LOOPS OF LEAD-WIRE COMPENSATE EACH OTHER IN ADJACENT ARMS

Lead-Wire Compensation Schemes: Since many lead-wire compensation schemes utilize a combination of noise suppression methods, some of these will be discussed separately here. A common arrangement using mutual compensation of the effects of undesired environments on the SENSORS is shown in Fig. 8. Users are well aware of the necessity of keeping the measuring and compensating SENSORS in the same environment at the same time, etc. The same care must be taken with the LEAD SYSTEM. The effect of mechanical strains on one set of leads and not the other has already been mentioned (WHEATSTONE 1843, MEIER 1956). HASTINGS (1976) presents sample calculations of the effects of only 10 ft difference in length of No. 26 AWG copper wire, and a 5°F temperature change in that 10 ft length as producing:

a./ A zero shift due to the 10 ft extra lead length of 3583µε at a gage factor of 2

b./ A zero shift of 39µε due to the effect of a 5°F temperature change on that 10 ft of extra lead length. Temperature gradients of that order of magnitude exist due to thermal lag in the wire insulation, and between floor and ceiling in even a closed room (HASTINGS).

The identity of resistance and resistance temperature coefficient of conductors in a multi-conductor cable has already been mentioned earlier in this paper. A difference of 5% between conductors is not unusual (HASTINGS, 1976) which means that only about 95% of the common-mode effects due to resistance or resistance changes will be cancelled out if cables of the same LENGTH are used.

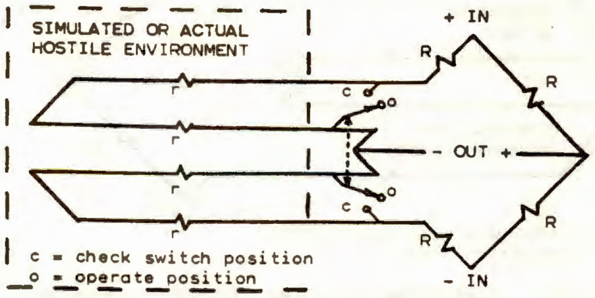
Fig. 9 illustrates the 3-wire circuit which, if precautions on identity of cable length, resistance and resistance-temperature coefficient are taken, will yield a compensated arrangement for lead effects. A simple way of checking such effects is shown in Fig. 12 where identity of readings in the operate and check positions over the temperature range of the test, applied to the cables, assures lead-effect cancellation in service. The SAME cable which was tested, can then be used with assurance, in service. DORSEY (1976) points out that the Micro-Measurements strain gage calibration apparatus always uses the SAME wires which are soldered and unsoldered as the gages are tested and removed. Thus the strain gage calibration data is not contaminated with differential lead system effects.

Fig. 10 (HASTINGS 1976) illustrates a practical execution of a wiring system in which identical length of wire in the critical bridge arms is assured by the very nature of the execution. Fig. 11 shows another method of lead compensation, provided that all four conductors experience and respond identically to the same environment.

Magnetic Shielding: is a well-known method of keeping magnetic fields in certain amplitude - and-frequency ranges away from components of a measuring system. It should be emphasized that magnetic shields are, in general, not good electric shields. Magnetic shields should have high magnetic premeability in the frequency and amplitude ranges of the magnetic field they are supposed to suppress, and they must be so configured that they distort the magnetic field so that flux lines to not enter the volume being shielded. These combined attenuation and distortion criteria may be difficult to achieve in practice, and the attenuation criterion is usually the predominant one. Magnetically shielding foils which are optimized for various field levels are available from many vendors including AD-VANCE MAGNETICS, MAGNETIC SHIELD DIVISION OF PERFECTION MICA CO., and JAMES MILLEN MANUF. CO., among others.

Recently a spray has become available which is claimed to accomplish the same function (ACME CHEMICALS & INSULATION). Flexible, magnetically shielding conduit is also available (BOLL & KELLER 1975, VACUUMSCHMELZE). Cables with multiple shields, some electrical and some magnetic, are also available (CALMONT DIV VARADYNE) and have been successfully used by TELINDE (1971) on tests involving high transient magnetic fields during capacitor discharge on flyer-plate testing.

It should be remembered that the criteria for electrical shielding involve a high degree of coverage of the sensitive portions of the measuring system in order to catch the charges induced by the environment on the system, and a high electrical conductivity in order to leak these charges off to a reference potential or ground, of which there should be only ONE. Ungrounded electrical shields or shields of low conductivity will allow residual charges accumulated along the shield to capacitively couple themselves into the measuring system. These criteria are quite different from those required of magnetic shields and one should not be expected to do both.



R = strain-gage quality bridge completion resistors

FIG. 12 METHOD OF CHECKING LEAD-SYSTEM COMPENSATION. (Same readings should be obtained on operate and check switch positions. Solder connections instead of switches may be used)

TABLE 3: TYPICAL MAGNETIC FIELDS (from ANONYMOUS 1958)

MAGNETIC FIELD in Gauss	TYPICAL SOURCES
Low Under 20	Power transformers, fluorescent lights, about 3 ft from power lines, 6-12 ft from a soldering gun
Medium 20-500	Anything closer than 3 ft from power line, 6" to 6ft from soldering gun, 10 to 50 ft from busbars, cables, wires, conductors, power lines and outlets carrying 5000 amps, within 6 ft of 1000 amps
High 500 - 32,000	Shake tables within 10 ft. Any motor is medium to low intensity but may be high in the area immediately adjacent to the motor. The same is true of power generating equipment.

Magnetic Fields often cause both the signal and the noise as in electro-magnetic vibration exciters, flyer-plate studies, explosions, traveling-wave impact studies, etc. The magnetically induced noise levels then appear to the same time and frequency scale as the signals, and are correlated with them. In such situations, frequency selective filtering and correlation techniques are powerless as noise suppression methods. Noise documentation such as by the Unified Approach, is essential to prove the integrity and validity of the data.

Increasing the Signal: The effective noise levels may conveniently be reduced by raising the signal level. Although this possibility seems obvious it is often neglected. Thus the same engine tested at the manufacturers' facility was found to have acceptably low noise levels in the strain gage data with 25ma gage current and a constant voltage supply; the same engine tested at an Air Force facility was found to have excessive noise in the strain gage data with 5 ma gage current from a constant current supply to the bridge. (1976, author is not at liberty to divulge the source of this information).

The generalized equation for the voltage output Δe of any resistance sensor which responds to a variable Q in the

$\frac{\Delta R}{R} = K.\ \Delta Q$ manner, where Δ indicates a change in the variables involved, can be expressed as:

$$\frac{\Delta e}{\Delta Q} = I.\ R.\ K.\ \gamma \quad \text{in units of micro-volt per microstrain}$$

if Q = strain, or volts/°C if Q = temperature, etc. I is the current through the sensor, R is the initial sensor resistance, K its resistance-Q coefficient (gage factor for strain gages, for ex.) and γ is the circuit efficiency which varies from 0.5 for an equal-arm bridge to 1.0 for constant current conditions in the sensor (STEIN 1962B, 1963).(Note that constant-current conditions in the sensor is NOT the same as constant current fed to a bridge !)

The upper limit on current is governed by the self-heating effect giving a temperature rise $\Delta\theta$ in the gage of

$\Delta\theta = U.\ I^2_{rms}.\ R$ where U is a coefficient which depends on local heat transfer conditions in the sensor.

Several possibilities now emerge for maximumizing peak gage output without over-heating the sensor:

a./ Use as high a sensor resistance as can conveniently be put in place. HASTINGS (1976) has long been a proponent of 350 ohm gages over 120 ohm ones, for example.

b./ Use an interrogating input (supply) wave shape with high peak/rms ratio.

WAVE SHAPE	PEAK/RMS RATIO
DC	1
Sine	2.8
Pulse train	1/(Duty Cycle)

or use single-pulse operation for transient measurements (STEIN 1962B, 1962C)

Since gage factor is selected by so many other criteria, the use of high K-factor sensors is not discussed here.

The use of constant-current conditions in the sensor, however, makes a great deal of sense, especially for dynamic steady state or dynamic transient tests.

Decreasing the Noise Source: Although it sounds obvious, the suppression of noise at its source will be specifically mentioned. This, of course, assumes that the source of the noise is known. In a recent case where the noise level exceeded that of the signal and both covered identical frequency ranges, a carrier system was used to solve the problem. It had to be home-made because the 500 kHz carrier frequency needed was not commercially available. The problem was, in fact, solved. The later recognition that the noise source appeared to be magnetic led to the demagnetization of the parts involved in the test, with the result that the noise levels all but disappeared even with DC input conditioning. (GORTON 1975).

Information Conversion or Modulation

The final method of noise suppression available to the measurement engineer for analog measurands is to convert the information in the measurand to some other form such as sine wave amplitude, frequency or phase modulated, pulse amplitude modulated or any of the many other information forms (STEIN 1970B for information forms according to the Unified Approach. STEIN 1975A for information conversion methods;also 1970C)

Information conversion, modulation, or the use of carrier systems other than DC, will accomplish the following, if the system is properly designed for the problem to be solved:

a./ It will separate self-generating responses from non-self-generating responses (see Table 1 for definitions).

b./ Coupled with a frequency-selective filter, it will suppress the self-generating responses.

c./ A separation is also possible in the time domain without filtering.

d./ Depending on the wave shape selected (sine or pulse) it may even be possible to separate capacitive effects from resistive ones in the sensor-cable system. (Pulses will, sine waves will not), (STEIN 1970C).

A detailed discussion of the principles and applications of information conversion will not be given here. The basic procedure for evaluating its applicability to a problem is as follows:

a./ After the application of the other noise suppression methods such as those discussed above, have failed to solve the problem in a resistance-based transducer,

b./ And if the measuring system output in the Switch-1-operate, Switch-2-check position is to the same amplitude and frequency or time scale as the signal to be measured (See Fig. 4)

c./ The problem is diagnosed without shadow of reasonable doubt as self-generating responses obscuring the desired non-self-generating responses

d./ And a suitably designed sine wave or pulse modulated carrier system can be just about guaranteed to solve the problem.

e./ It is necessary to obtain a wave shape photograph and a frequency analysis of the self-generating responses alone; Fig. 4, Switch-1-operate, Switch-2-check.

f./ It is necessary to obtain a waveshape photograph and a frequency analysis of the total transducer response. Fig. 4, all switches in the operate position

g./ With the above data a carrier system can be designed.

Case studies are cited in STEIN (1971A 1970C, 1962D) by PETERSON et al. (1962) and by GORTON (1975). The fact that the carrier frequency can be less than signal frequency so long as the data are left in the time domain and NOT filtered, is shown by STEIN (1962D) for a mechanical 2kHz pulse-carrier system which is both amplitude and frequency modulated, and for an electrical sine-wave AM system in STEIN (1970C).

Unless the above steps are carried out with a reasonable understanding of what the method really is and does, even the best experimental work can lead to quite erroneous generalizations (SAUER et al. 1971 as discussed in STEIN 1971B and 1976).

It must be emphasized again that NO carrier system can separate resistance changes due to strain from resistance changes due to other environments. EVERY carrier system is potentially suitable for separating SELF-GENERATING from NON-SELF-GENERATING responses, which is why the diagnostic procedures outlines in this paper are so important.

APPENDIX:

The author has been in the teaching field for 27 years, and has developed several simple experimental illustrations of some of the problems discussed in this paper.

The strain-induced voltages generated during traveling-wave impact tests are illustrated in a simple experiment in (STEIN no date, 1962A Ch. 14, 1963).

Magnetically induced voltages in strain gage circuits and where the noise amplitude is larger than that of the signal, with both covering overlapping frequency ranges, are illustrated in STEIN (1962A Ch. 6, 1971A). One important noise suppression technique for such a case, not mentioned in the body of the paper, is the reversal of the connections for a single sensor. The principle is that resistance changes are not polarized (unless a non-ohmic connection exists in a semiconductor sensor, which should always be checked). That is, regardless of which way the terminals are connected to an ohm-meter, the resistance will be the same. Self-generating voltages of the magnetic or thermoelectric variety, however, ARE polarized. Thus by reversing the sensor connection to the circuit it is possible to perhaps diminish the noise by having it subtract from some other noise-voltage. A reduction by a factor of two is achieved in the experiment described. (STEIN 1971A)

A demonstration not as yet written up, illustrates strain gage response to a photo-flash cube illustrating both thermo electric and thermo-resistive responses; magneto-strictive responses in Isoelastic gages are demonstrated as well as the frequency-creating ability of non-linear magneto-resistive responses in time-varying magnetic fields. The author's favorite magnetic field generator is a magnetic tape eraser-degausser. Twisted vs. parallel leads illustrate the advantage of the twisted ones, and the remarkable advantage of the Inter-8-Weave cable is shown, as well as non-inductive gages.

The power of a high-frequency (25kHz) carrier to solve the dynamic-strains-in-a-magnetic-field problem described above (STEIN 1971A) is illustrated there, complete with a detailed description of the noise-documentation and system design methodology used (also STEIN 1970C, 1975A).

The author wishes to thank the many individuals who have contributed their ideas and experiences over the years, so that a systematic study of problems such as reported here, have been made possible.

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References to SGR Reviews are made to enable those readers who have a set of Strain Gage Readings to locate some of the information rapidly.

Numerous references are made to personal letters, private conversations, and telegrams so that due credit can be given to the many sources of information which made this paper possible.

My personal gratitude to everyone who was willing to share their problems and case studies with the author !

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1974 Letter from Dr. Zaharia, Univ. of the Negev, Israel, to C. P. Wright TRW Systems, Redondo Beach, CA Jan 1, 1974 enclosing manuscript Direct Shock Effect in Metals.

MANUFACTURERS' LITERATURE CITED

ACME CHEMICALS & INSULATION CO
P.O. Box 1404, New Haven, CT 06505
Conductive paints & adhesives. E-Kote 3062 for EMI/RFI shielding materials.

AD-VANCE MAGNETICS
226 E. Seventh St., Rochester, IN 46975
Magnetic shielding materials

AMERICAN AEROSPACE CONTROLS INC.
129 Verdi St., Farmingdale, NY 11735
Magnetic flux sensitive resistor: MistoR. Bismuth insulated in mylar.

CALMONT DIVISION OF VARADYNE CORP.
420 E. Alton St., Santa Ana, CA 92707
Electromagnetic interference shielding with high and low permeability shields combined with electrical shields, on cables.

MAGNETIC SHIELD DIVISION, PREFECTION MICA
1322 N. Elston Ave., Chicago, IL
Inter-8-Weave Cable

JAMES MILLEN MANUF CO., INC.
150 Exchange St., Malden, MA 02148
Millen Suggestion Box: Selection of Magnetic Materials for Shielding

PYE UNICAM LTD / PHILIPS INSTRUMENTATION
York St., Cambridge CB1 2PX, England
VIBRACOAX, Vibration and Dynamic Pressure Detector.

TIMES WIRE & CABLE CO.
358 Hall Ave., Wallingford, CT 06492
Low Noise Instrument Cable Product Bulletin.

VACUUMSCHMELZE GmbH
Hanau, Federal Republic of Germany
Flexible magnetically shielding conduit.

NBS Pressure Transducer Characterization Program

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ABSTRACT

As a step toward improved pressure measurement, the NBS Pressure and Vacuum Section has developed a program designed to determine the long-term performance of pressure transducers. Among the parameters determined in the eight months test are: warm up, zero drift, supply voltage dependence, calibration, precision, pressure hysteresis, short and long term stability, shift of sensitivity and zero with temperature, temperature hysteresis, full scale drift, relaxation effects, effects due to pressure cycling, pressure fluid dependence, attitude dependence, etc. . The philosophy of the program, the test schedule and examples of the data obtained are discussed.

INTRODUCTION

The Pressure and Vacuum Section of the National Bureau of Standards (NBS) presently maintains primary pressure standards spanning the range from 1 Pa to 420 MPa. The calibration chains depending upon these primary standards serve the nation's manufacturing industry, the transportation industry, the armed forces and the general public. Within this system there are great demands for increased accuracy, speed and economy of pressure measurements. Such demands for increased accuracy can no longer be met only by improving the accuracy of the primary standards but must be satisfied also by improvements in the calibration chains extending from the primary standards to the end point of use. One possible improvement is to shorten the calibration chain by shipping accurate, portable transfer standards from a primary standards laboratory directly to the point of use to assess the accuracy of pressure measurement achieved at this point and to compare it with the accuracy expected or needed. The problem is to identify and characterize the long term behavior of the pressure transducers to be used as transfer standards.

NBS has developed the pressure transducer characterization program to meet this need. The program is based on a testing schedule that evolved from past experience at NBS with many helpful suggestions from manufactures and users of transducers. It was developed primarily to identify those factors having an effect on repeatibility. Since in general, temperature has the greatest long term effect, the tests involving temperature are done near the end of the schedule to reduce the complexity of separation of effects. The test schedule is detailed in the next section. Then in another section, the treatment of the data is discussed and illustrated with examples selected to show what can be learned from the tests. The concluding section offers some observations and contributions made already by this new program.

TEST SCHEDULE

1. Observe zero drift for a 3 day period taking readings after turn-on each 500 seconds for the first 20,000 seconds and each 10,000 seconds thereafter. Leak tight absolute transducers are evacuated to a pressure of 0.1 Pa or less.
2. Zero the transducers as required. Zero adjustments will not be made again until just before the final calibration.
3. Observe zero drift every 10,000 seconds for 1 week.
4. Decrease the supply voltage to 10% below normal and calibrate against a piston gage (estimated 33 ppm accuracy, 2 ppm precision) at 0, 20, 40, 60, 80, 100% full scale with only increasing pressure allowing no overshoot. Dry nitrogen is the pressure fluid.
5. Increase the supply voltage to 10% above normal, calibrate at 0, 20, 40, 60, 80, 100% full scale with increasing pressure and no overshoot.
6. Calibrate at room temperature, nominal 23°C, with increasing pressure at 0, 10, 20, 30, 40, 50, 60, 70, 80, 90, 100% full scale and 95, 85, 75, 65, 55, 45, 35, 25, 15, 5, 0% full scale with decreasing pressure allowing no overshoot.
7. Calibrate again.
8. Calibrate using He as the pressure fluid.
9. Pressurize to full scale, hold the pressure for 3 weeks, compare full scale reading with piston gage each working day.
10. Reduce pressure to zero, observe zero drift for 1 week.
11. Repeat step 9.
12. Repeat step 10.
13. Pressure cycle from 0 to 90% of full scale 3450 times with interruptions each 690 cycles to observe zero drift for an hour at 500 second intervals. Dwell times at 0 and 90% full scale are approximately 1 minute.

14. Observe zero drift for 24 hours, then calibrate.

15. Observe zero drift for 1 week.

16. Pressure cycle from 0 to 90% of full scale 6900 additional times with interruptions each 690 cycles to observe zero drift for an hour at 500 second intervals.

17. Observe zero drift for 24 hours, then calibrate.

18. Observe zero drift for 1 week.

19. Pressurize to full scale, hold for 3 weeks, compare reading with the piston gage each working day.

20. Reduce pressure to zero, observe zero drift 1 week.

21. While making zero drift measurements, increase the transducer temperature to 40°C, decrease to 0°C, then increase to 23°C at as rapid a rate as the environmental chamber allows (several hours).

22. Check the calibration at 0, 20, 40, 60, 80, 100% full scale using only increasing pressure while at nominal 40°C.

23. Lower the temperature to nominal 31.5°C, check the calibration at 0, 20, 40, 60, 80, 100% full scale using only increasing pressure.

24. Lower the temperature to nominal 11.5°C, check the calibration at 0, 20, 40, 60, 80, 100% full scale using only increasing pressure.

25. Lower the temperature to nominal 0°C, check the calibration at 0, 20, 40, 60, 80, 100% full scale using only increasing pressure.

26. Return to room temperature, nominal 23°C, check the calibration at 0, 20, 40, 60, 80, 100% full scale using only increasing pressure.

27. Turn off the power to the transducer, increase the temperature to 50°C, hold for 4 hours, decrease the temperature to -40°C, hold for 4 hours, return to room temperature.

28. Turn on power, allow 1 week warm up while observing zero drift at 10,000 second intervals.

29. Check the calibration at 0, 20, 40, 60, 80, 100% full scale using only increasing pressure.

30. Observe zero drift for 4 weeks.

31. Calibrate.

32. Adjust the zero.

33. Calibrate again.

34. If appropriate, tilt the transducer by 1, 2, 3, 4, 5, 10, 15, 30, 60, 90° about two orthogonal horizontal axes while observing zero shift.

35. Repeat step 34 at full scale pressure.

DATA SUMMARY

The data from these 35 tests are categorized under 18 parameters. The numbers in parenthesis following each heading refer to the tests that apply. The data presented here is from a number of different transducers selected to illustrated that which might be learned from those particular tests.

Warm up (1). Figure 1 is an example of transducer reponse during warm up. The zero drift output has been converted to percent of full scale via the initial calibration equation and plotted as a function of time following the first application of power. From this data one can determine a minimum warm-up period compatible with the precision of the transducer.

Zero drift (3, 15, 18, 30). Again the transducer output is converted to percent of full scale and has been plotted as a function of time. Test 3 establishes the norm and in conjunction with the first test provides a 10 day warm up period before calibration. Tests 15 and 18 are diagnostics to examine the effect of pressure cycling. Test 30 is a search for long term (4 weeks) trends. The example in Figure 2 is from a 4 week test.

Supply voltage dependence (4, 5). The differences between the pressures generated by the piston gage and these indicated by the transducer for these two tests are expressed as a percent of full scale and are plotted as a function of full scale in Figure 3. The corresponding differences between applied and indicated pressure from the initial calibration equation are also plotted for reference. The data are obtained as pressure is increased to avoid the complication of pressure hysteresis.

Calibration (6, 7, 14, 17, 31, 33). It is desirable to have an initial calibration equation with which subsequent diagnostic calibrations can be compared in order to determine the effects of the various tests. Including zero drift in the initial calibration equation only obscures contributions from other sources. For this reason, the calibration equation has the form

$$P = b(X-X_o) + c(X-X_o)^2 + d(X-X_o)^3 + \dots n(X-X_o)^k \qquad (1)$$

where P is the pressure generated by the piston gage, X is the transducer output, X_o is the transducer output at zero pressure and b, c, d ...n are coefficients to be determined by a least squares fit.

To determine where to terminate the polynomial and thereby establish the value of k, one tests the coefficients to see whether or not they are significant. There is a standard deviation for each coefficient which is an estimate of the uncertainty associated with that coefficient. To be regarded as being significant, the coefficient must be larger than its standard deviation by a factor of 3. If any coefficient fails this test, the fit calculation is repeated omitting the highest order term.

Precision (6, 7, 14, 17, 31, 33). The residuals (ΔP) are obtained subtracting the indicated pressure from the applied pressure

$$\Delta P = P - [b(X-X_o) + c(X-X_o)^2 + \ldots n(X-X_o)^k] \tag{2}$$

The standard deviation of the residuals (σ) is an estimate of how well the equation describes the calibration data. As such, it is also an indication of the random error in pressure measurement one is likely to make using the transducer. For the present purpose, 3σ is taken to be the precision of the transducer.

Pressure hysteresis (6, 7, 14, 17, 31, 33). The magnitude and location with respect to pressure of hysteresis effects can be observed in the plot of the residuals as a function of percent of full scale shown in Figure. 4. The arrow heads indicate the order in which the measurements were made. For a normal hysteresis pattern, the ΔP values for decreasing pressure are smaller than for increasing pressure.

Short term stability, approximately 24 hours (6, 7). The first and second calibrations are done on consecutive days. A comparison of the two calibrations provides an indication of the short term stability.

In order to compare the two calibrations, let us write an equation for each:

$$P = b(X-X_o) + c(X-X_o)^2 + \ldots n(X-X_o)^k \tag{3}$$

for the first and

$$P' = b'(X'-X'_o) + c'(X-X'_o)^2 + \ldots n'(X'-X'_o)^k \tag{4}$$

for the second.

Taking the transducer output from the second calibration and the coefficients from the first, we calculate an indicated pressure which is then subtracted from the pressure applied during the second calibration, i.e.

$$\delta P = P' - [b(X'-X'_o) + c(X'-X'_o)^2 + \ldots n(X'-X'_o)^k] \tag{5}$$

These values are plotted as a function of full scale in Figure 5. If the two calibrations were in perfect agreement, the values of δp would be identical to the residuals of the first calibration (ΔP). The residuals are also plotted in Figure 5 for reference.

Long term stability, approximately 7 months (6, 33) A comparison between these calibrations, done some 7 months apart, provides an indication of the stability over the long term. This comparison is done using Equation 5 as above.

Zero drift as a function of temperature (21). The transducer output, converted to percent of full scale, is plotted as a function of temperature for the temperature cycle 23° to 40° to 0° to 23°C as in Figure 6. Appropriate polynomials fitted to the data using (T-23) as the independent variable provide a room temperature referenced correction factor for the zero drift. This data is used to obtain temperature coefficients, temperature hysteresis and optimum operating temperatures.

Temperature hysteresis (21). Any temperature hysteresis will be evident from Figure 6.

Sensitivity shift as a function of temperature (22, 23, 24, 25,). At each of the four temperatures, 0. 11.5, 31.5, and 40°C, the calibration is checked at 0, 20, 40, 60, 80, 100% full scale using the piston gage to generate the pressures. Only increasing pressures are used to avoid the complication of pressure hysteresis. The differences between the applied pressure and the indicated pressure are expressed as a percent of full scale and are plotted as a function of percent of full scale in Figure 7. These data are used to determine temperature correction factors for the composite calibration equation considered later.

Full scale drift (9, 11, 19). In each of these three tests, the transducer is held at full scale for three weeks and the indicated pressure is compared with the piston gage each working day. The deviation between the piston gage pressure and the transducer indicated pressure is plotted as a function of time as in Figure 8.

Relaxation effects (10, 12, 20). The question considered here is: How does the transducer respond to pressure at the low end of the range after long service at full scale? Relaxation to the initial state after the mechanical deformation necessary to make a pressure measurement is a time dependent phenomenon resulting in a complex zero shift. Often relaxation is also a function of the history of the transducer. Characteristic relaxation times on the order of 100 hours have been observed. A preconditioning of three weeks at pressure is therefore a reasonable way to reduce historic effects. The preconditioning is accomplished in the three full scale drift tests. Following each of these tests, the pressure is reduced to zero and zero drift measurements are made for several days. The transducer output for these tests is converted to percent of full scale and is plotted as a function of time. Figure 9 is an example wherein the indicated pressure can be approximated by an exponential function to determine the characteristic relaxation time; that of Figure 10 is random drift without a characteristic time.

Effects of pressure cycling (13, 14, 15, 16, 17, 18, 19, 20). The effects of the pressure cycling may be determined by comparing the calibration, the precision, and the hysteresis obtained from the calibration data of tests 14 and 17 with that of test 6. Also the zero drift curves from tests 15 and 18 should be compared with that of test 3. Changes in the characteristic relaxation due to the exercise will be observed by comparing the data of test 20 with tests 10 and 12. Changes in the full scale drift will be noted by comparing the plotted results of tests 19 with tests 9 and 11. The plots of the zero applied pressure measurements taken after each sequence of 690 cycles have been observed to fall into 3 different patterns:

1. Random scatter
2. Exponential relaxation superimposed on a sequence to sequence zero shift as in Figure 11.
3. Sequence to sequence zero drift with the measurements clustered about each new zero as seen in Figure 12.

The zero drift measurements in both of these graphs were taken during test 13 after each of the five sequences of 690 pressure cycles. The 23 hours during each sequence has been reduced to 1000 seconds on the plots for ease in comparison.

Dependence upon the pressure fluid (8). For those classes of transducers where the calibration equation is a function of the density of the pressure fluid, the calibration is repeated using helium and compared with the nitrogen calibration of test 6 using Equation 5. Figure 13 shows δp calculated from the helium calibration output and ΔP from the initial nitrogen calibration.

Attitude dependence (34, 35). In test 34, zero drift measurements are made while the transducer is tilted through a series of angles about 2 orthogonal horizontal axes. For test 35, full scale measurements are made as a function of tilt angle about the two orthogonal horizontal axes. The difference between the piston gage pressure and the transducer indicated pressure is plotted as a function of angle as in Figure 14.

Effects due to storage/transportation temperature cycle (27, 28, 29). The dwell times at -40°C and 50°C are an attempt to simulate the temperature cycle a transducer might experience in being transported from one standards lab to an other in the unheated cargo space of an airplane. The effects are tested by checking the calibration after the cycle at 5 applied pressures.

Composite calibration equation including temperature effects (6, 7, 14, 17, 22, 23, 24, 25, 26, 31, 33). The composite calibration equation is obtained by fitting the data from all calibrations, including those that are a function of temperature, as one data set. The value of σ from this calculation reflects the changes that have taken place in the transducer throughout the entire test schedule.

It is often instructive to note the changes in σ as the fit is determined under 6 different conditions:

1. Best fit, zero corrected, temperature dependent.
2. Best fit, zero corrected, temperature ignored.
3. Linear fit, zero corrected, temperature ignored.
4. Best fit, not zero corrected, temperature dependent.
5. Best fit, not zero corrected, temperature ignored.
6. Linear fit, not zero corrected, temperature ignored.

CONCLUSIONS

Even though the transducer characterization program is new at NBS and it is felt that a broad enough variety of transducers has not yet been tested to identify transfer standards, still there have been positive results. One manufacturer was able to locate and correct a design flaw after studying his test report. Another found he could simplify his calibration equation and his linearizing electronics.

In addition to providing data for the better understanding of transducer behavior, this program has uncovered another common problem in the transducer community: how does one computer-fit calibration data for a transducer having nonzero output for zero pressure? The situation is sketched in Figure 15. The common practice is to fit a curve of the form

$$P = A + BX + CX^2 + \ldots NX^n. \quad (6)$$

A calibration equation of this form has 2 faults.

1. The value of A is determined by extrapolation out of the range of measurement. It is often a large value and usually has a large standard deviation associated with it. This large uncertainty in A produces a large uncertainty (σ) in the calcualted pressures. By choosing a calibration equation of the form of Equation 6, one may be discarding precision that is inherent in the measurements.

2. In general, any transducer of any design may have zero drift. Often the slope of the calibration curve will not change significantly but the zero will. Should this be the case, the only ways to repair Equation 6 to account for zero drift are to zero correct each value of X before calculating the pressure or to recalculate all new coefficients for the calibration equation using zero corrected data or new calibration data. Zero correcting the data requires a knowledge of the zero output at the time of calibration which may well not be available.

Both of these problems are avoid by simply using the change of output, $X-X_o$, where X_o is the output at some easily reproduced reference pressure, as the independent variable in the calibration equation. The calibration equation then has the form of Equation 1, has no coefffficients that are determined by an extrapolation outside range of measurement, and is easily corrected for zero drift. We have seen cases where the precision associated with the calibration equation has been improved by a factor of 10 merely by selecting Equation 1 rather than Equation 6.

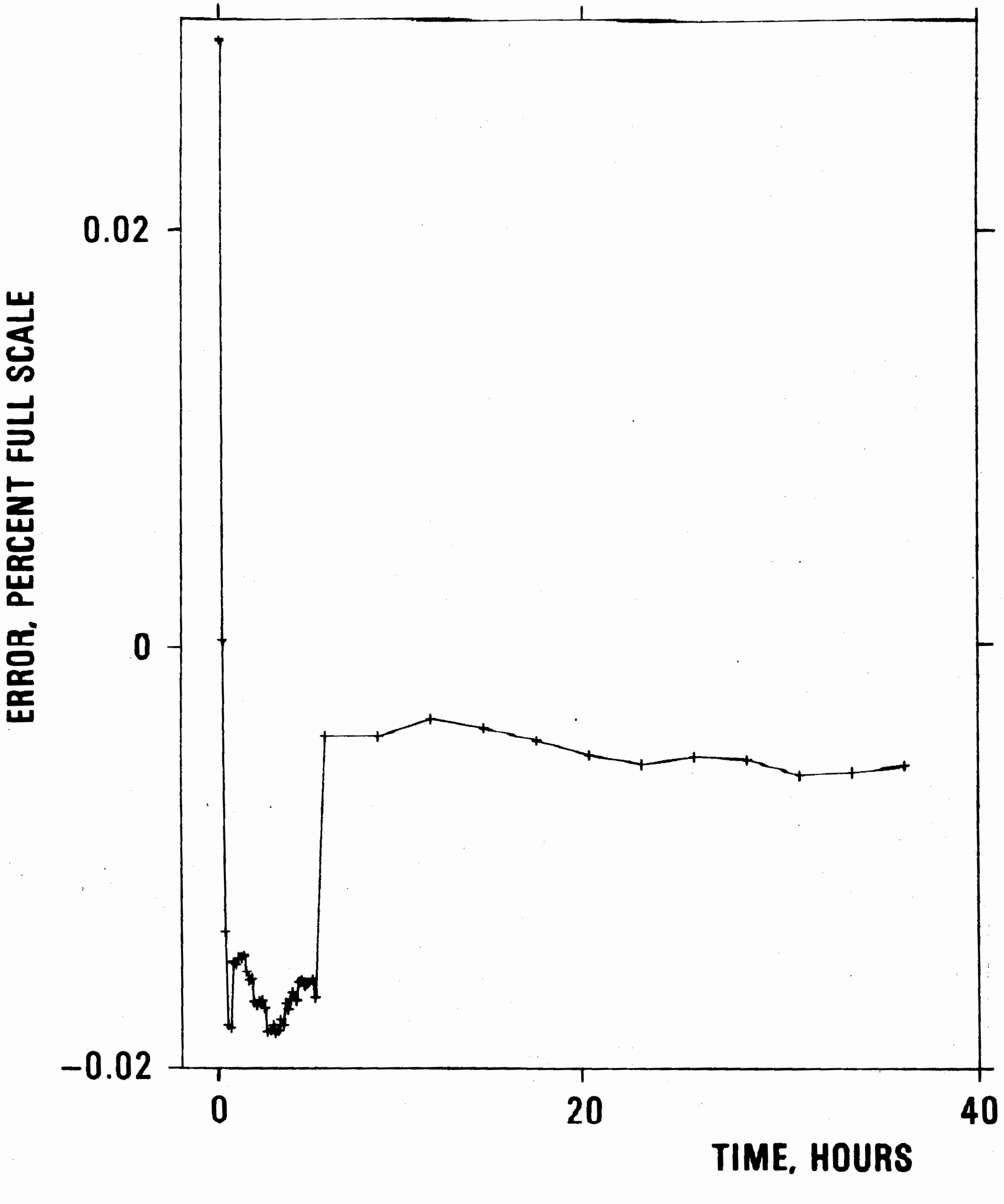

Figure 1 – Warm up.

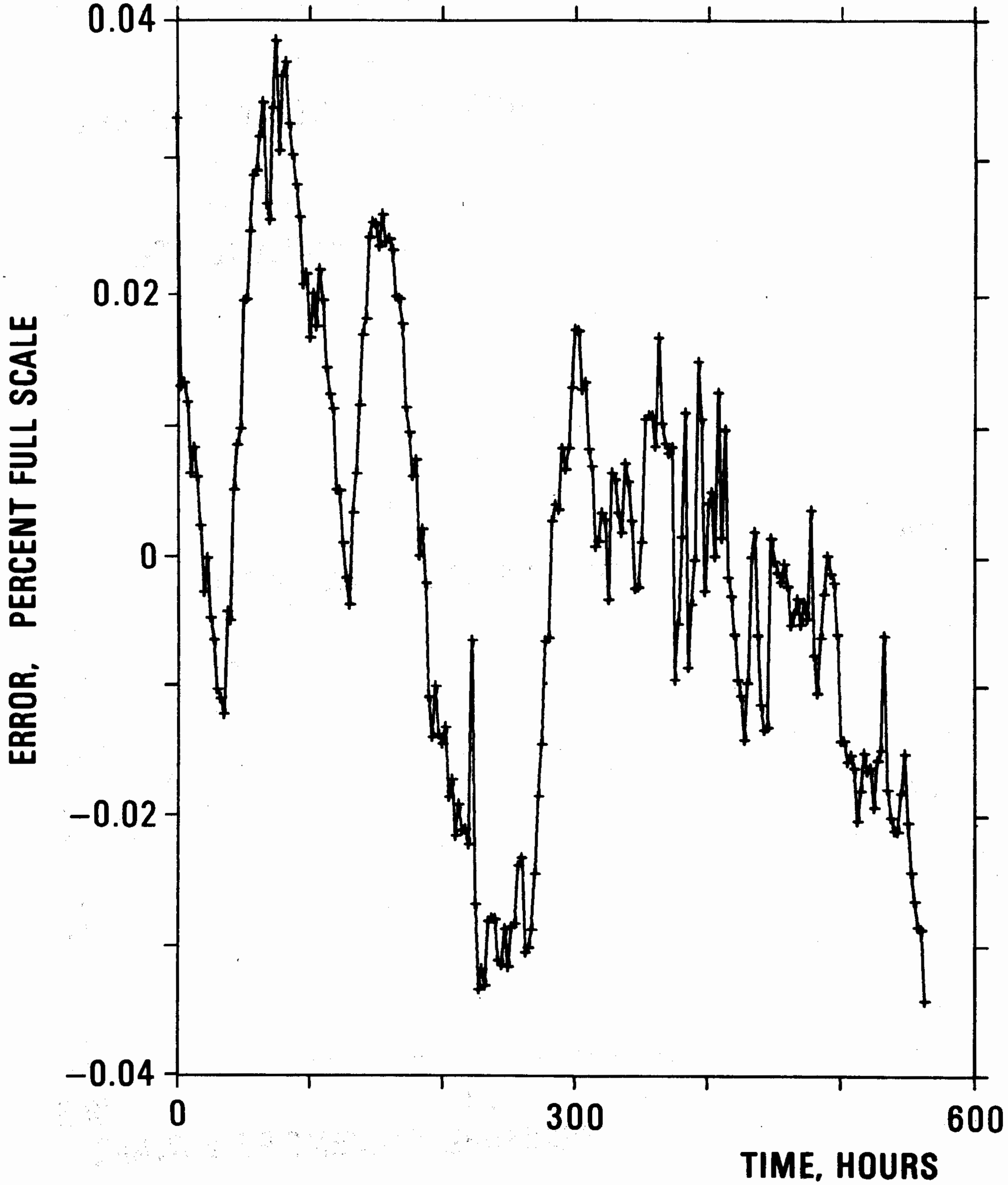

Figure 2 – Zero drift.

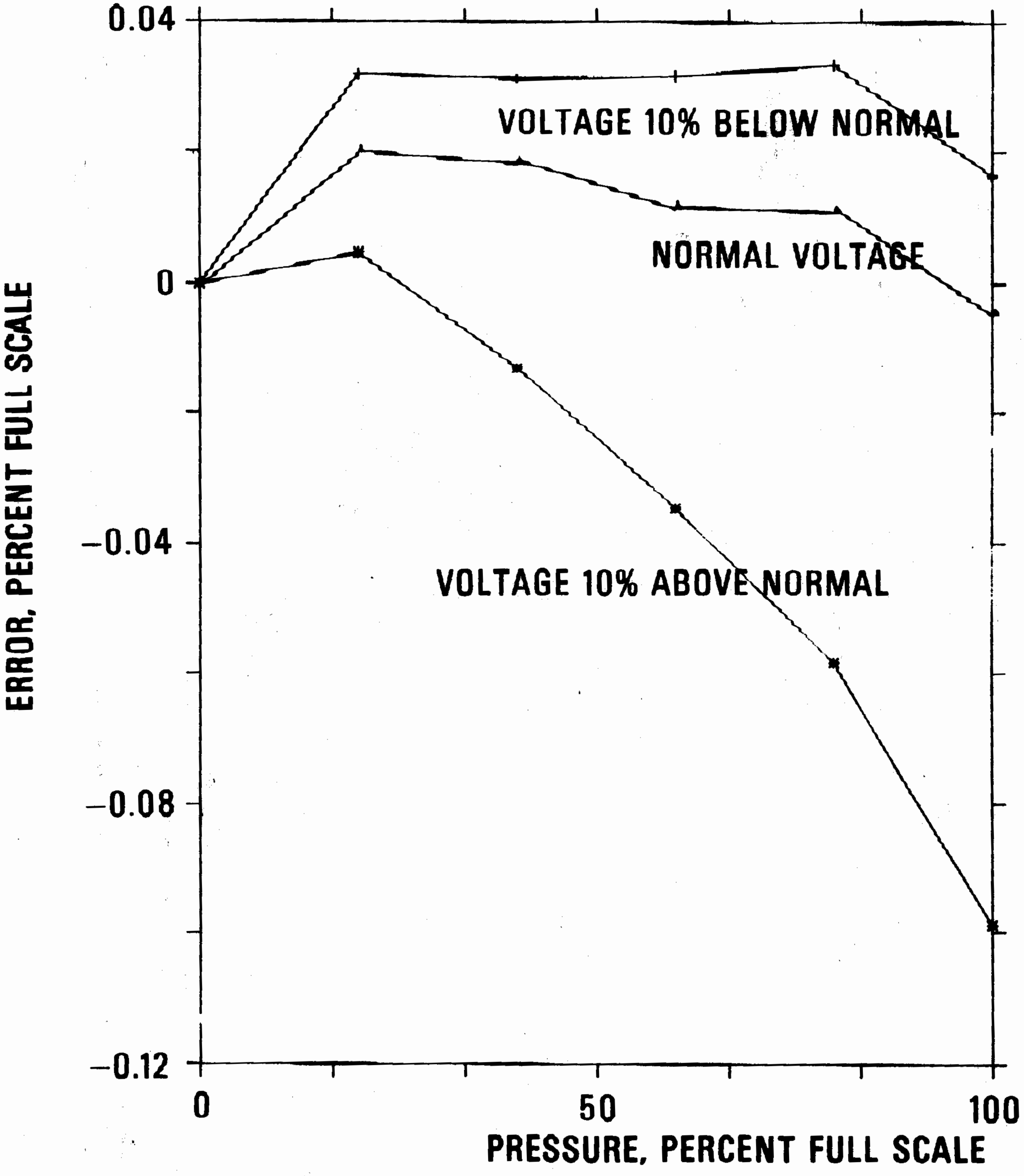

Figure 3 – Supply voltage dependence.

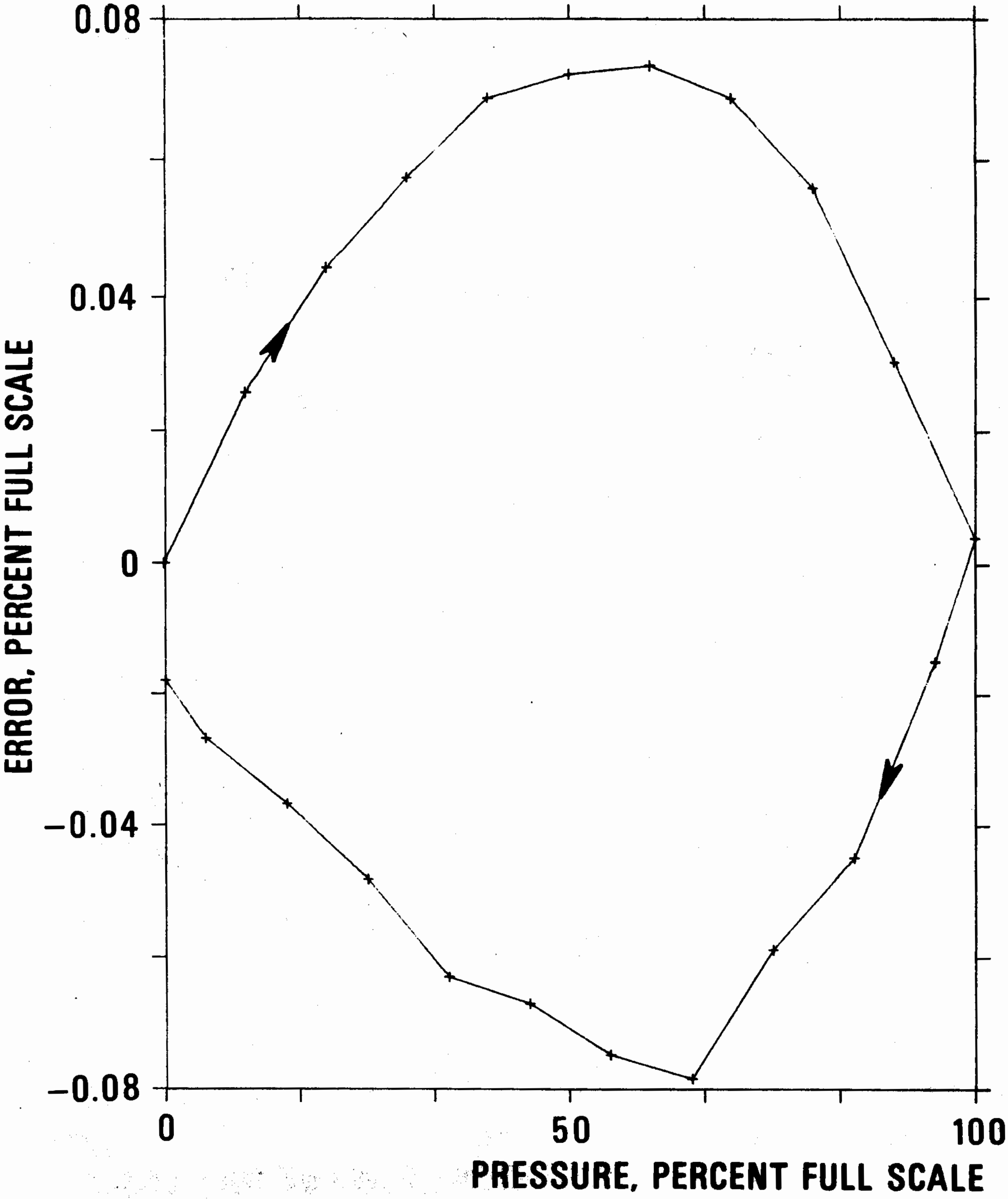

Figure 4 – Pressure hysteresis.

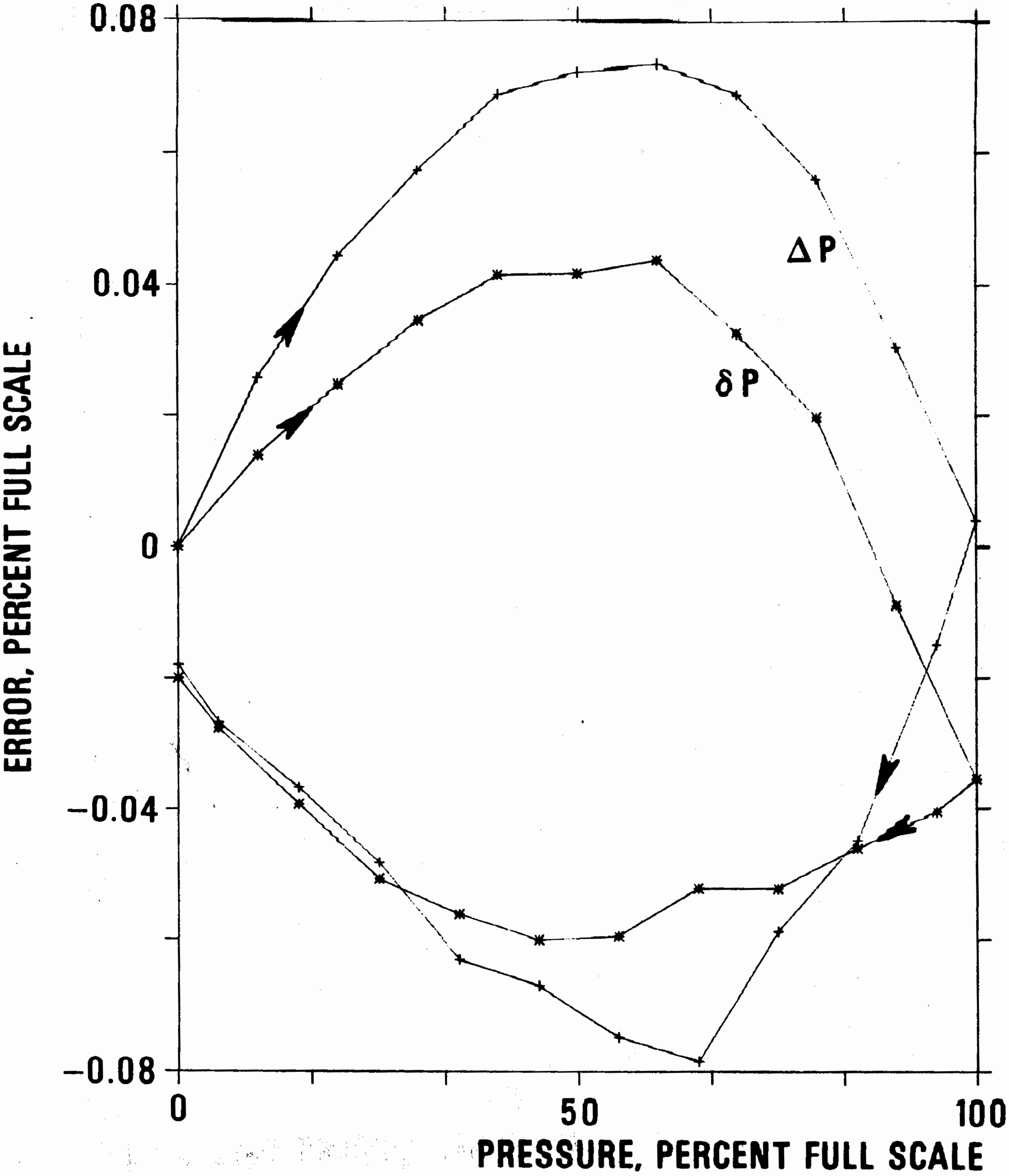

Figure 5 – Stability.

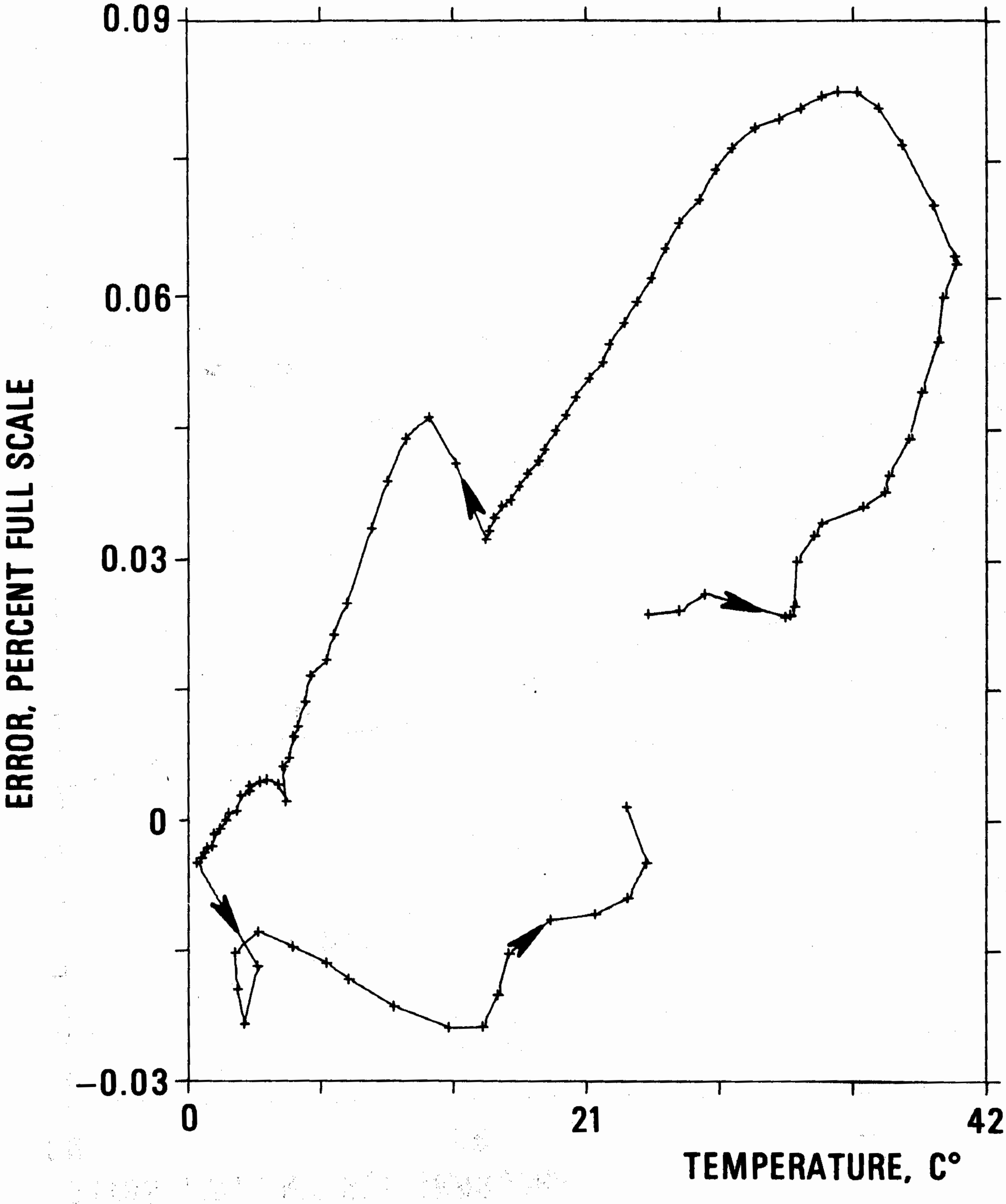

Figure 6 – Zero drift as a function of temperature.

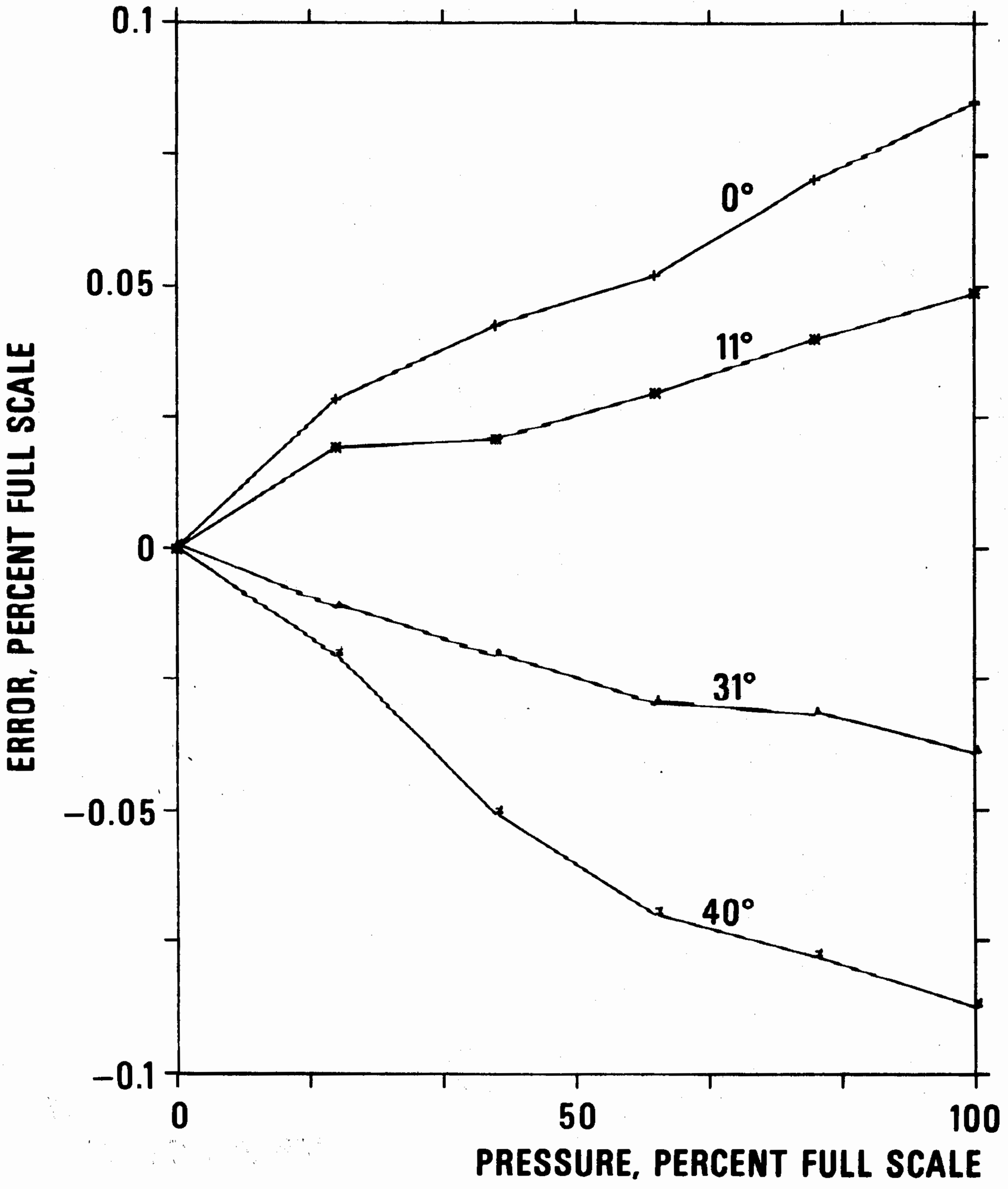

Figure 7 – Sensitivity as a function of temperature.

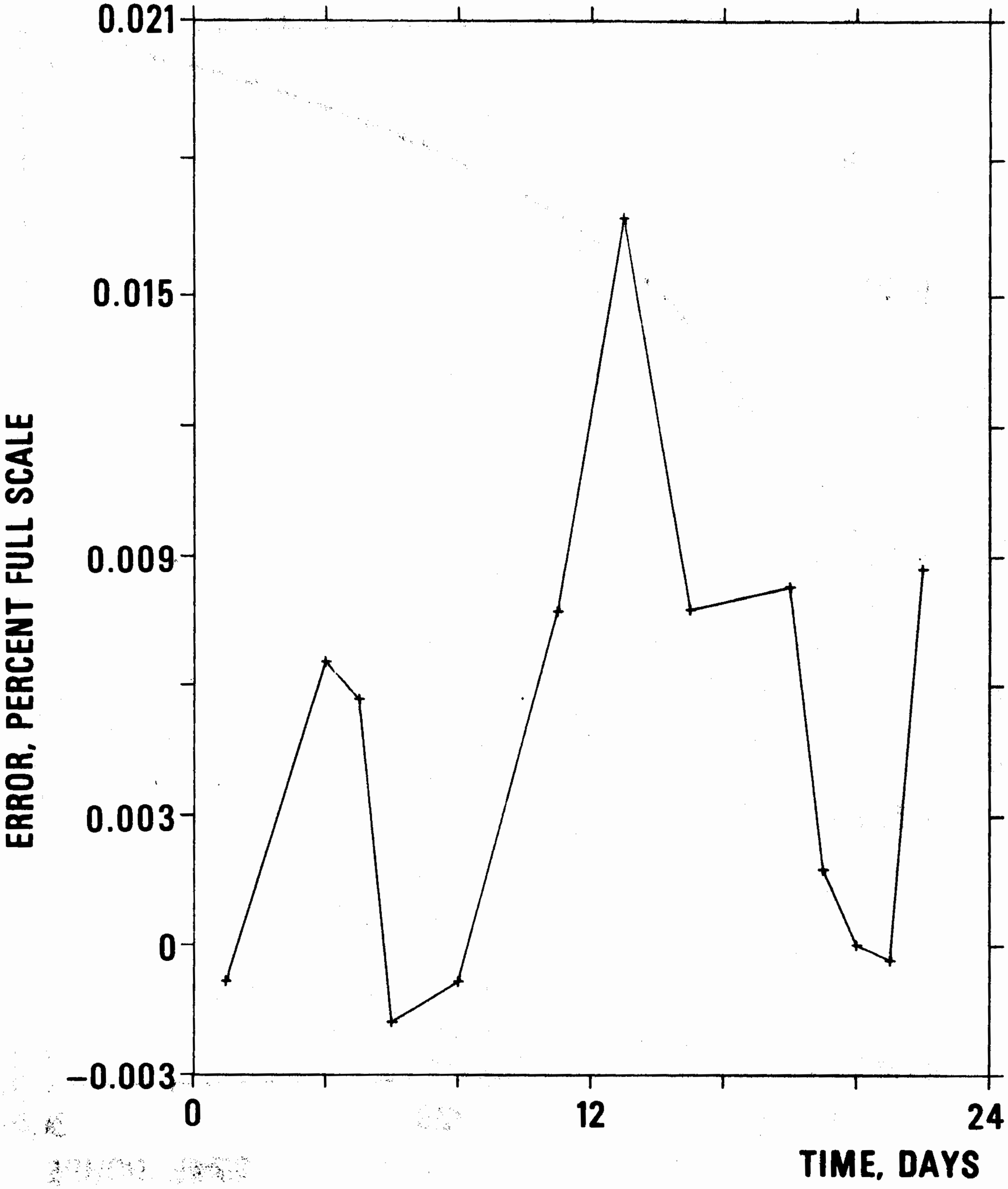

Figure 8 — Full scale drift.

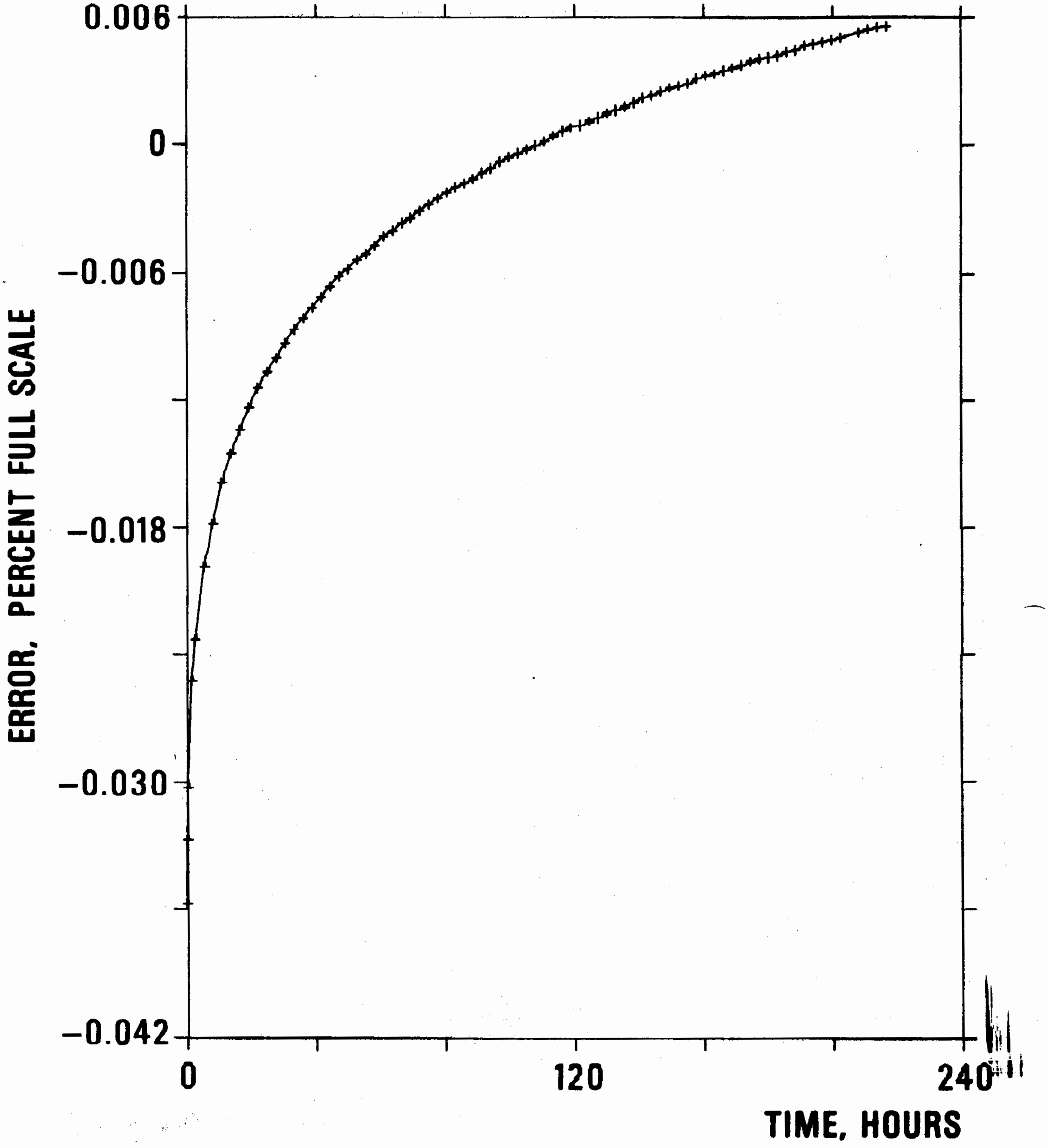

Figure 9 — Relaxation.

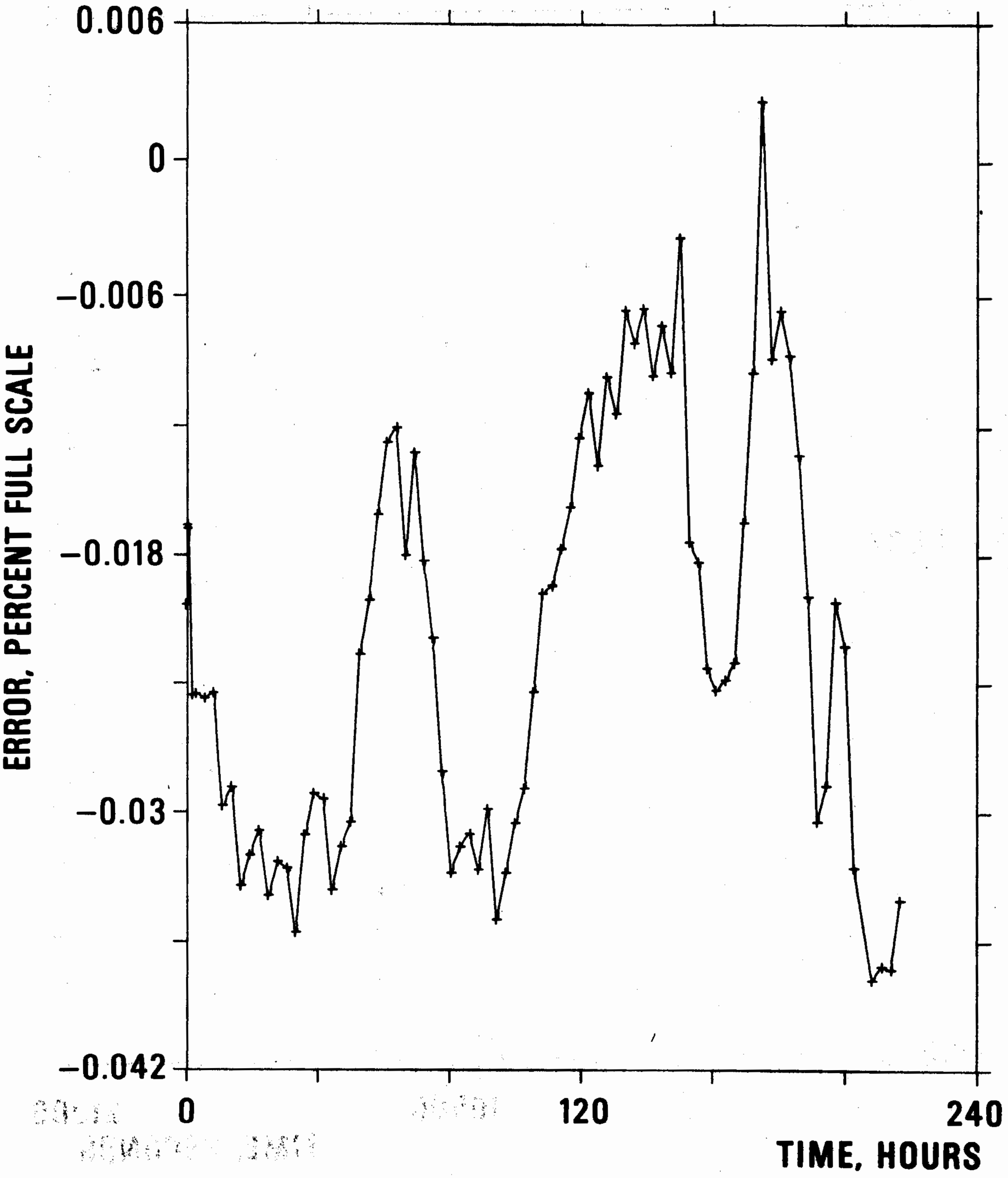

Figure 10 — Relaxation.

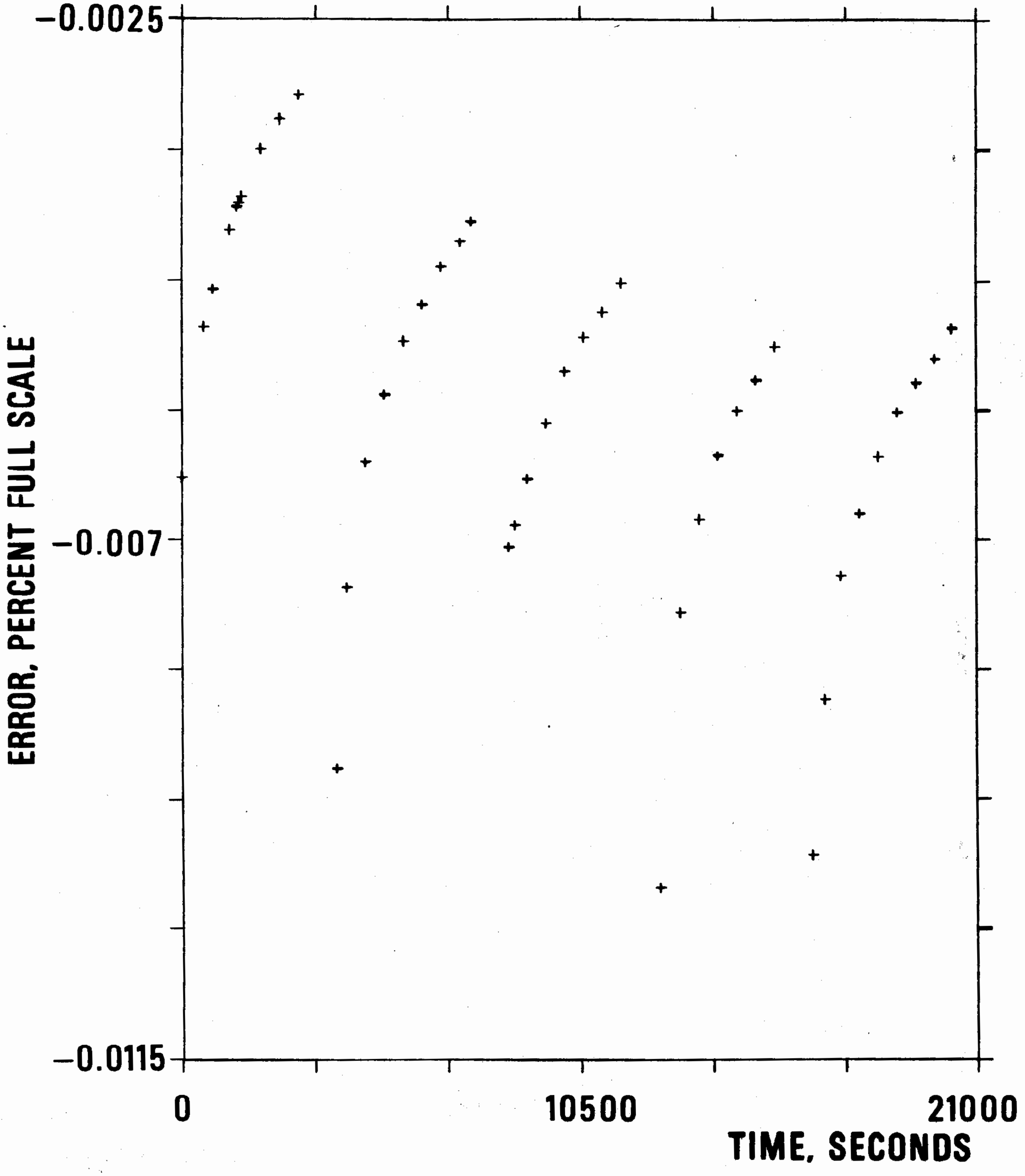

Figure 11 – Zero drift between pressure cycling.

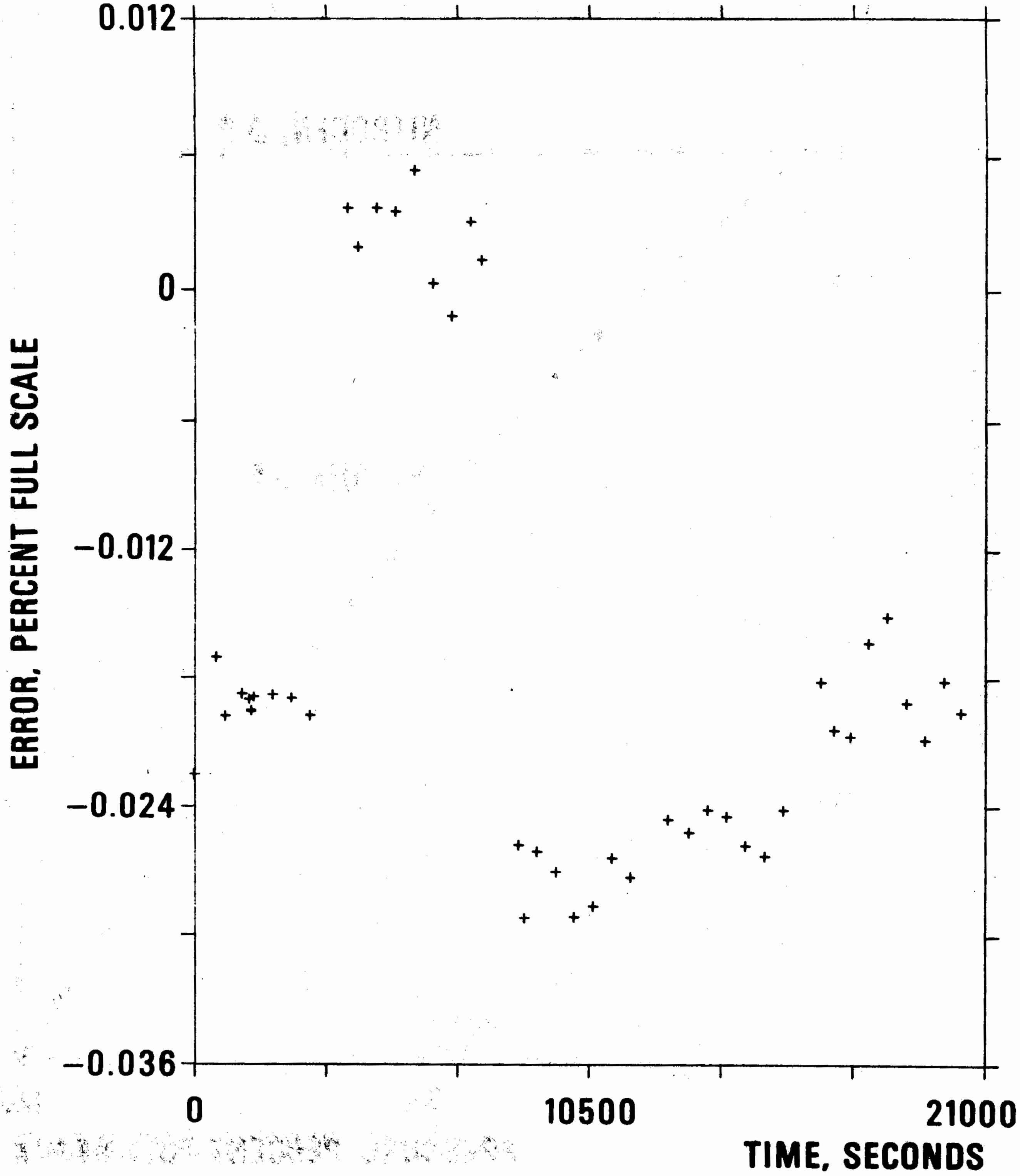

Figure 12 — Zero drift between pressure cycling.

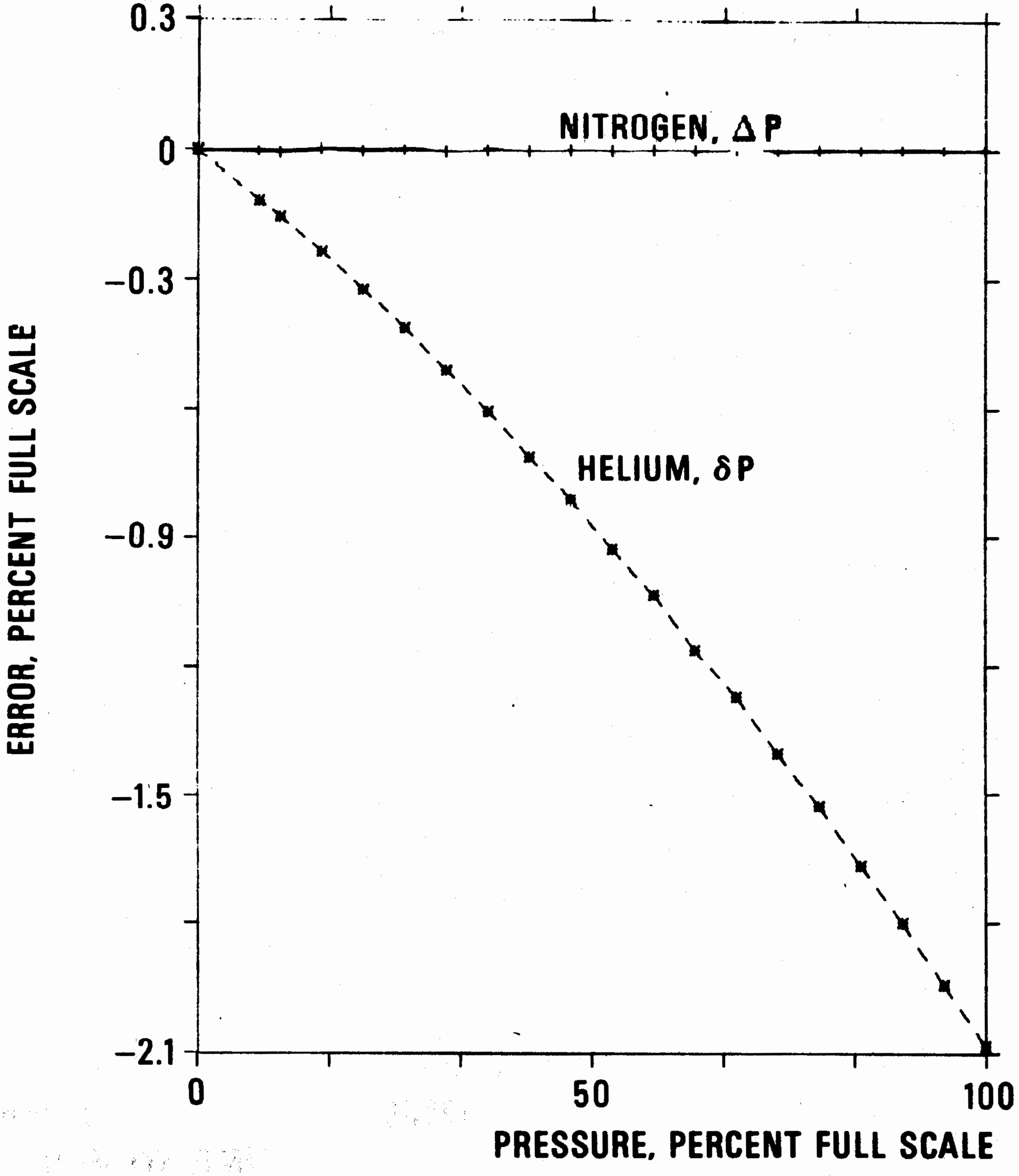

Figure 13 – Calibrations with helium and nitrogen compared.

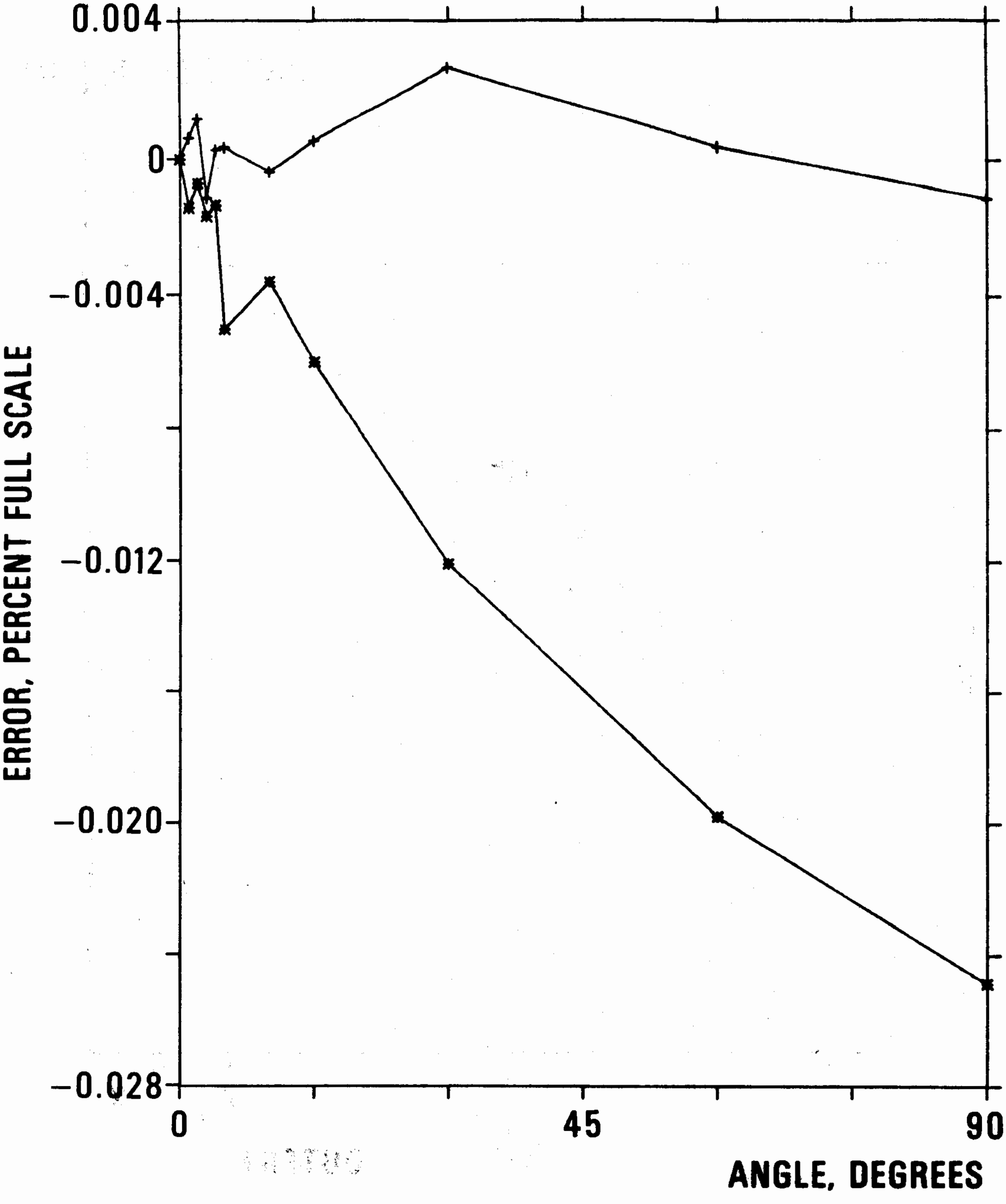

Figure 14 — Pressure change due to tilt while at full scale pressure.

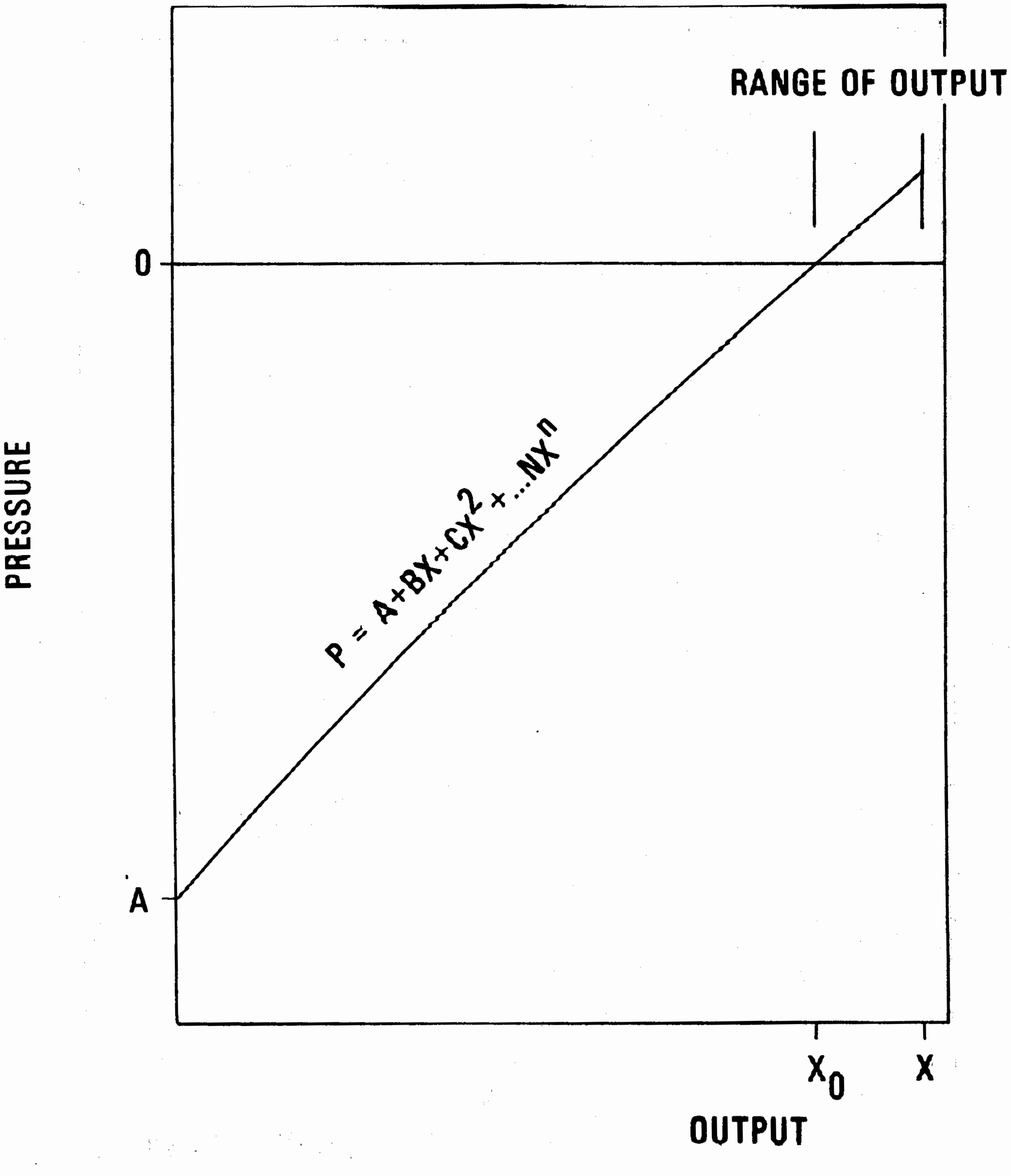

Figure 15 – An example of a transducer with nonzero output at zero applied pressure.

USE OF HOLOGRAPHY FOR FUEL DROPLET CHARACTERIZATION IN FUEL-AIR EXPLOSIVE CLOUDS

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Abstract

Holography has been used to measure droplet size, distribution, and motion in water vapor clouds for a number of years. The Naval Weapons Center applied this technology to field tests of fuel-air explosive clouds. Special design considerations incorporated in the holocamera used for this test and those design considerations to be used in following tests, enabling the holocamera to work in the explosive test environment, are described in this paper. Also included are lessons learned in the construction of a holographic reconstruction facility for particle field analysis.

USE OF HOLOGRAPHY FOR FUEL DROPLET CHARACTERIZATION IN FUEL-AIR EXPLOSIVE CLOUDS

by

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Naval Weapons Center
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INTRODUCTION

Strictly speaking, the holocamera is not a transducer; however, it can be used to obtain data that supplements and in some cases replaces transducer measurements. The Naval Weapons Center (NWC), China Lake, California, conducted a test program in which holography was used to analyze an explosively dispersed fuel-air explosive (FAE) cloud. The holocamera was used to measure particle size, particle distribution, and localized fuel-air ratios within the cloud. Although particle velocity was not measured during the tests, such an application represents a valuable capability of the system.

The use of holography for particle analysis in the field is not new. There are many examples of using this technology to study naturally occurring aerosols and particulate clouds; however, the NWC experiments represented the first application of holography to an explosive test situation. In addition, the severe natural environment of the desert test site necessitated utilization of techniques and equipment not previously used in holographic applications. This paper discusses the problems encountered and describes the equipment and methods used to obtain valid data in the explosive test environment.

HOLOCAMERA

Spectron Development Laboratories (SDL) Inc., Costa Mesa, California, was awarded a contract to produce holograms of an FAE cloud during NWC tests. Figure 1 is a sketch of the holocamera system used for the tests. Figures 2 and 3 are photographs of the two sections of the holocamera, while Figure 4 shows the holocamera assembly in place at the test site. The large shields were installed to protect the holocamera from warhead shrapnel; the aerosol cloud came through the gap between the shields. Figure 5, six photographs taken from high-speed motion picture film of an actual test, is intended to convey the severity of the test environment. The second picture in Figure 5 shows a piece of shrapnel hitting the collimator tube. Other photographs show the expanding FAE cloud from various views. The entire cloud formation process is over in less than 200 milliseconds.

Dr. J. D. Trolinger of SDL selected in-line holography rather than off-axis holography for this application. In-line holography is simpler than off-axis, thus requiring less optics, and is more likely to produce high-resolution holograms. Quoting Dr. Trolinger, "The two primary situations which call for off-axis holography in lieu of in-line holography are: (1) when the particle field is so dense that over 20% of the transmitted light is scattered, and (2) when the flow field contains many large gas density gradients which disturb the spatial regularity of the transmitted light beam."[1]

A number of problems were anticipated in this holocamera design:

1. Shrapnel from the warhead.
2. The highly flammable cloud around holography equipment.
3. Coating of the exposed lens surfaces by the fuel particles.
4. The desert environmental extremes.

To solve these problems, the following precautions were taken:

1. The main body sections of the holocamera were placed behind shields to prevent damage from the warhead shrapnel and to prevent the camera body from bearing the impact of the expanding cloud. Only the collimator probe tubes were exposed. Later, these were shielded after one tube sustained a direct hit from a piece of shrapnel and was knocked out-of-line.

2. All electrical components were either placed inside the barricade or inside a sealed box. This prevented sparks, from cooling fans and other components, from contacting the flammable fuel-air mixture. This also helped to control the dust problem that is common to desert environments.

3. The holocamera boxes were continuously purged with dry nitrogen. This served as a backup to the box seals.

4. The lens surfaces exposed to the environment were contained deep within the collimator tubes to protect them from the cloud.

Despite the precautions, the measures taken to completely protect the holocamera were sometimes inadequate. In addition, unanticipated problems were encountered that further complicated the test series. The greatest difficulty was the temperature variations and extremes reached on the desert during the tests. The holocamera was frequently out of alignment due to the temperature variations. The realignment procedure was complicated by inadequate lumination from the small alignment laser; therefore, major realignments had to be accomplished at night, thus delaying the test program. Also, the high temperature extremes were too severe for the laser head cooling system. The problem was solved for these tests by providing an ice bath for the laser cooling lines; however, a temperature-controlled refrigeration unit would have been better. A unit of this type, used to control the temperature of the entire optical system in the transmitter, would also have alleviated many of the alignment problems.

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[1] Spectron Development Laboratories. "Application of Holography to the Diagnostics of Particle Fields in Fuel-Air Explosives" by J. D. Trolinger and C. Baird. SDL Report No. 76-6059. June 1976.

The protection afforded the collimator lenses was also inadequate. They became coated with fuel during the tests, resulting in degradation of the holograms and necessitating replacement of the lenses. It is recommended that a combination of a shutter system and a purge system for the collimator tube in front of the lens be incorporated.

Components of the holocamera should be as structurally sound as possible within the constraints imposed by reasonable cost and performance criteria. For example, the beam expander/spatial filter system did not have sufficient structural integrity to withstand the shock environment, thus causing frequent misalignment. An improved design will be incorporated for follow-on work.

The desert environment of high daytime temperatures, low nighttime temperatures, moderately high winds, and even a little rain created many special problems. Dust-laden air deposited dust on uncovered optics and moderately high winds hampered efforts to cover the apparatus to reduce ambient light levels during alignment and to afford dust protection. Though not completely prepared for the extremes encountered, the system did perform well under these conditions.

HOLOGRAPHIC RECONSTRUCTION SYSTEM

The reconstruction system consisted of:

1. Spectra-Physics Inc., Model 125A He-Ne laser.
2. Tropel beam expander/spatial filter/collimator (diffraction limited).
3. Velmex 18-inch-travel YXZ traverse.
4. Lens imaging system.
5. Cohu high-resolution TV camera.
6. Conrac high-resolution TV monitor.

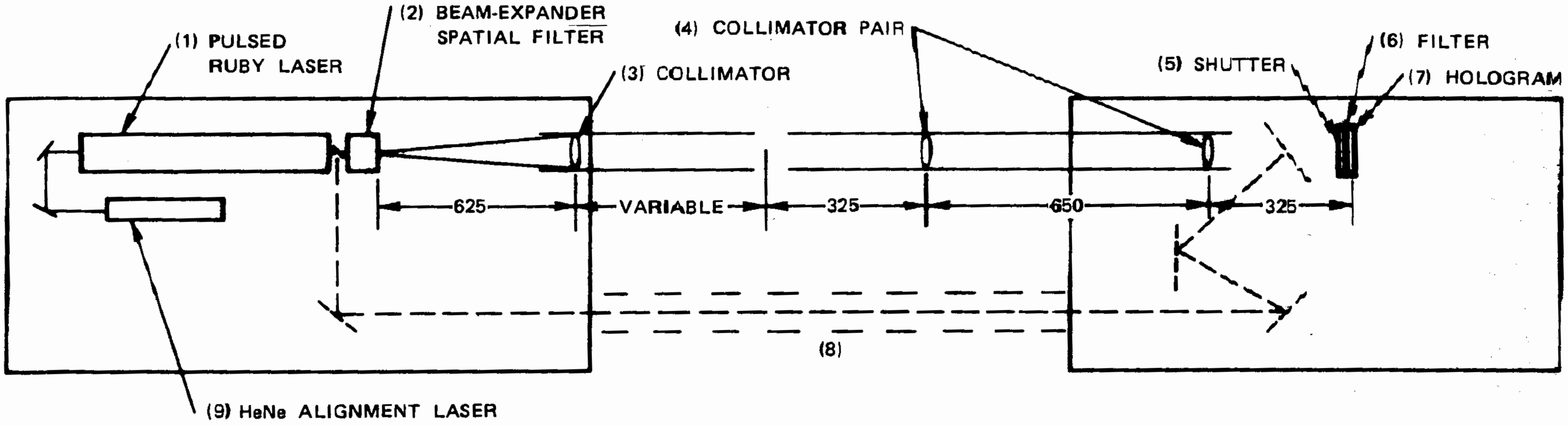
Design and construction of the facility was the responsibility of Patrick Shoals of NWC. Figure 6 shows the system. The reconstruction facility worked very well, but here too, lessons were learned. Initially, an interference pattern on the TV screen masked the cloud particle images. This was a result of the coating on the vidicon tube; the coherent light created an interference pattern between the coated surface and the vidicon tube surface. This was eliminated by placing an optical wedge on the vidicon tube. Problems were also experienced with high particle field density and overexposure of the holographic plate, both of which prevented sufficient light intensity from reaching the TV camera for proper operation. The use of a silicon vidicon is recommended to help alleviate this problem.

As anticipated, the major problem with holographic data reduction was the tedious job of particle image counting and measurement. A data reduction specialist performed this work. Her usual task was assessing motion picture film and it was felt that holography image data reduction was a similar activity. The holograms had 2,000 to 7,000 particle images each. Each hologram took from 1-2 weeks to reduce. After a few weeks, the specialist complained of eye strain, which required a reduced work schedule.

NWC has so far been unsuccessful in automating the reduction process. The main problem is separation of optical and electrical noise from actual particles. Once this problem is solved, computer-based automated data reduction is possible.

SUMMARY

The feasibility of using holography to characterize FAE clouds was demonstrated. A number of problems were identified during the effort and solutions will be incorporated into the system for a test series planned at NWC in late spring or early summer of this year. Other than this type of data gathering, it is beyond the scope of the FAE weapon development program to further refine and develop holocameras for field testing. Since holography appears to have a wide potential application to explosive field testing as a valuable diagnostic tool and research aid, it is hoped that other government agencies or industrial laboratories will develop a system specifically tailored for such an application.



(ALL DIMENSIONS IN mm)

NOTES:

(1) Holobeam Model 300, 100 mjoules, 10^{-8} sec pulse duration, .6943 microns wavelength, firing frequency 15 shots/min.

(2) 17 mm fl. single element lens, 100 micron pinhole.

(3) 75 mm dia. x 625 mm fl. corrected telescope objective.

(4) Matched pair 75 mm dia. x 325 mm fl. corrected telescope objectives mounted for one-to-one imaging.

(5) 125 mm dia., Empco, Inc.

(6) Schott RG 630, Red Filter.

(7) Agfa 8E75 Scienta plates, developed in D19 while inspecting under amber lights. Developing time ranged from 2-6 minutes. Water-washed and fixed in Kodak Rapid Fix for 3 minutes.

(8) Dashed lines represent off-axis reference beam which was not needed.

(9) Hughes 3 mw HeNe.

FIGURE 1. HOLOCAMERA CONFIGURATION

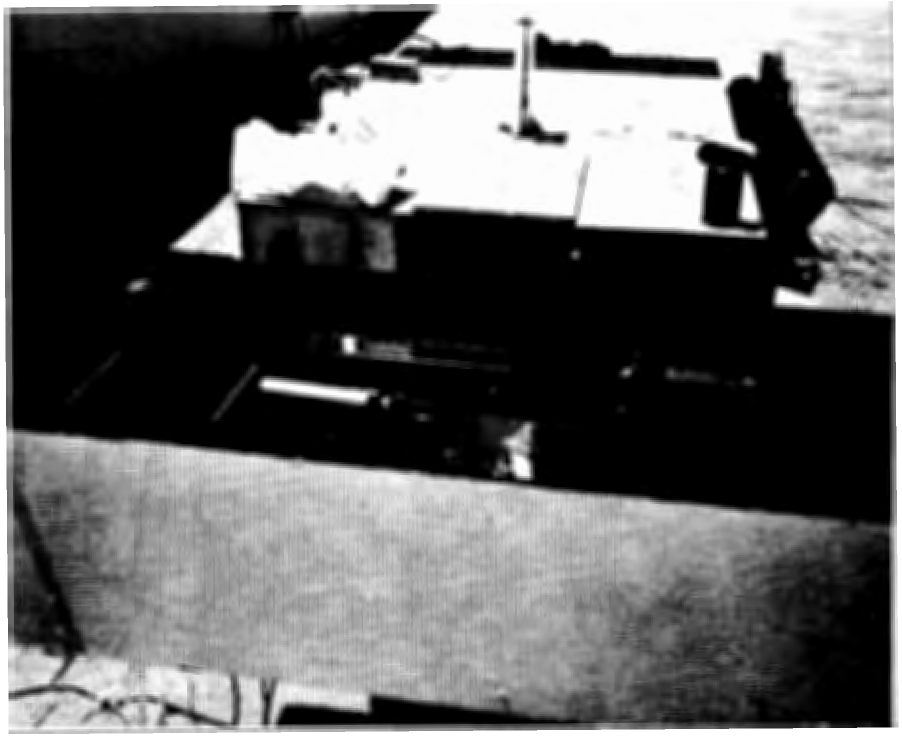


FIGURE 2. HOLOCAMERA TRANSMITTER SECTION

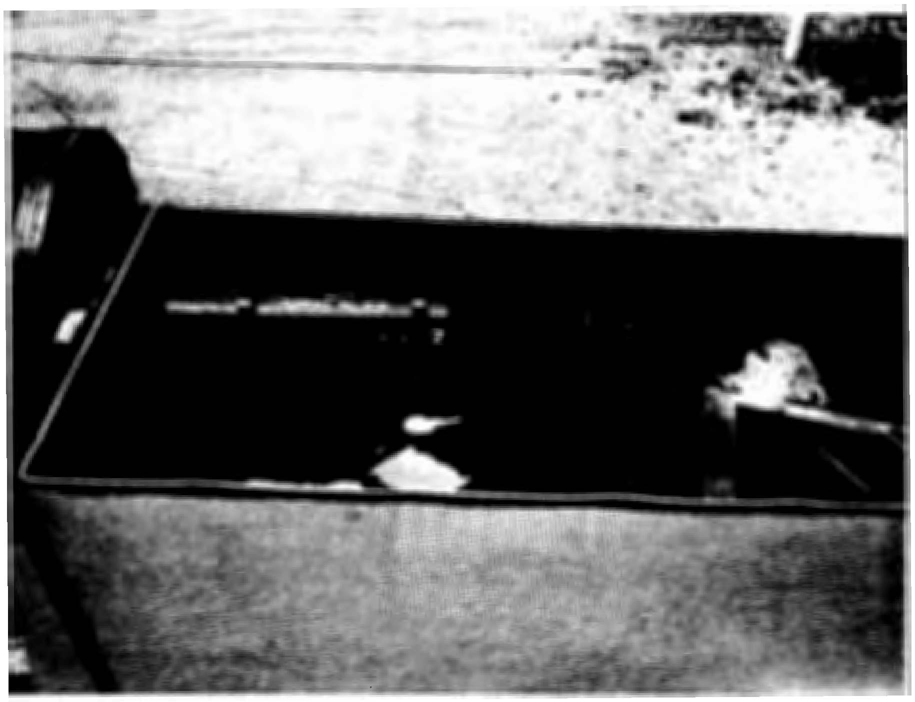


FIGURE 3. HOLOCAMERA RECEIVER SECTION

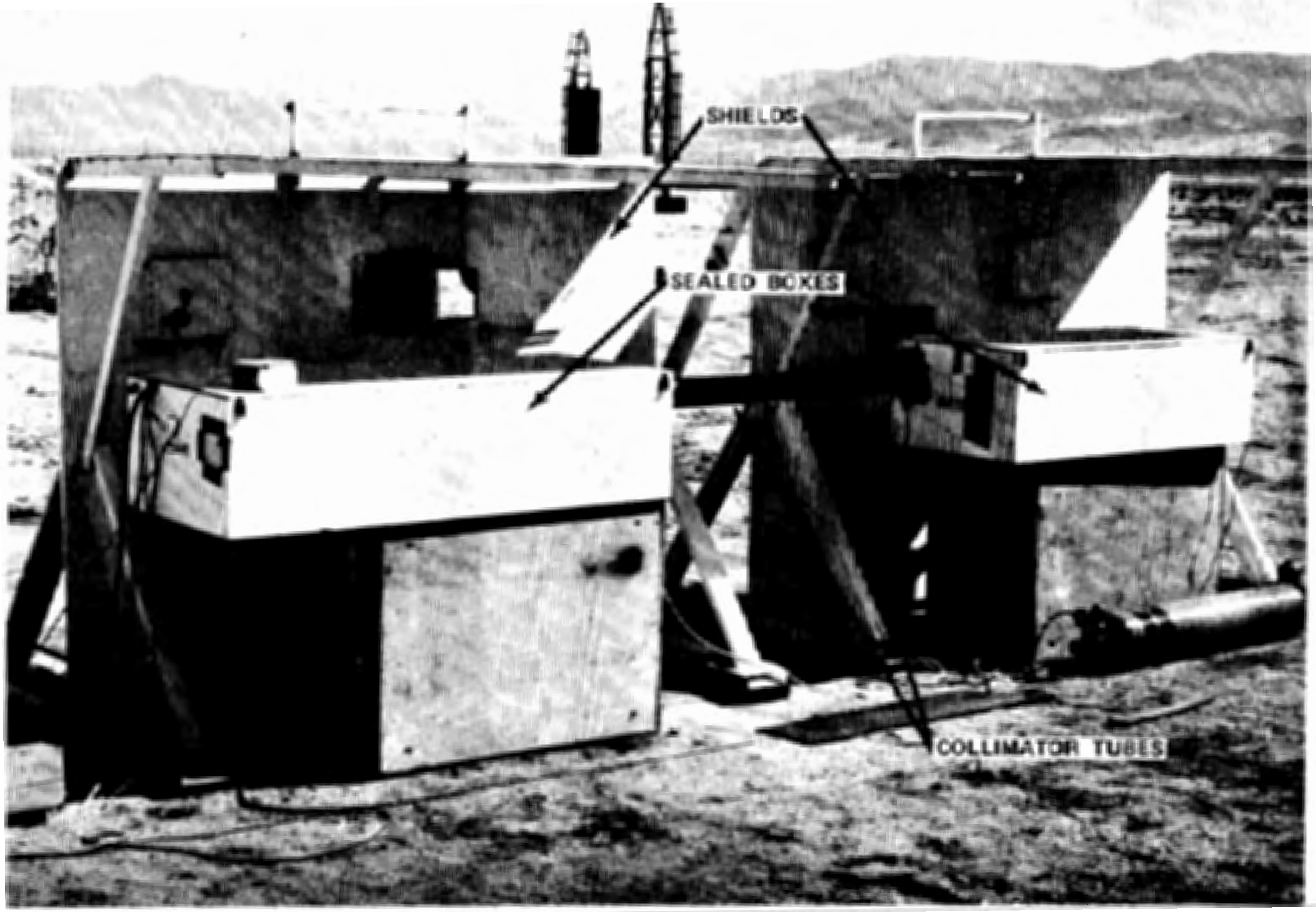


FIGURE 4. HOLOCAMERA TEST SETUP WITH BOXES SEALED

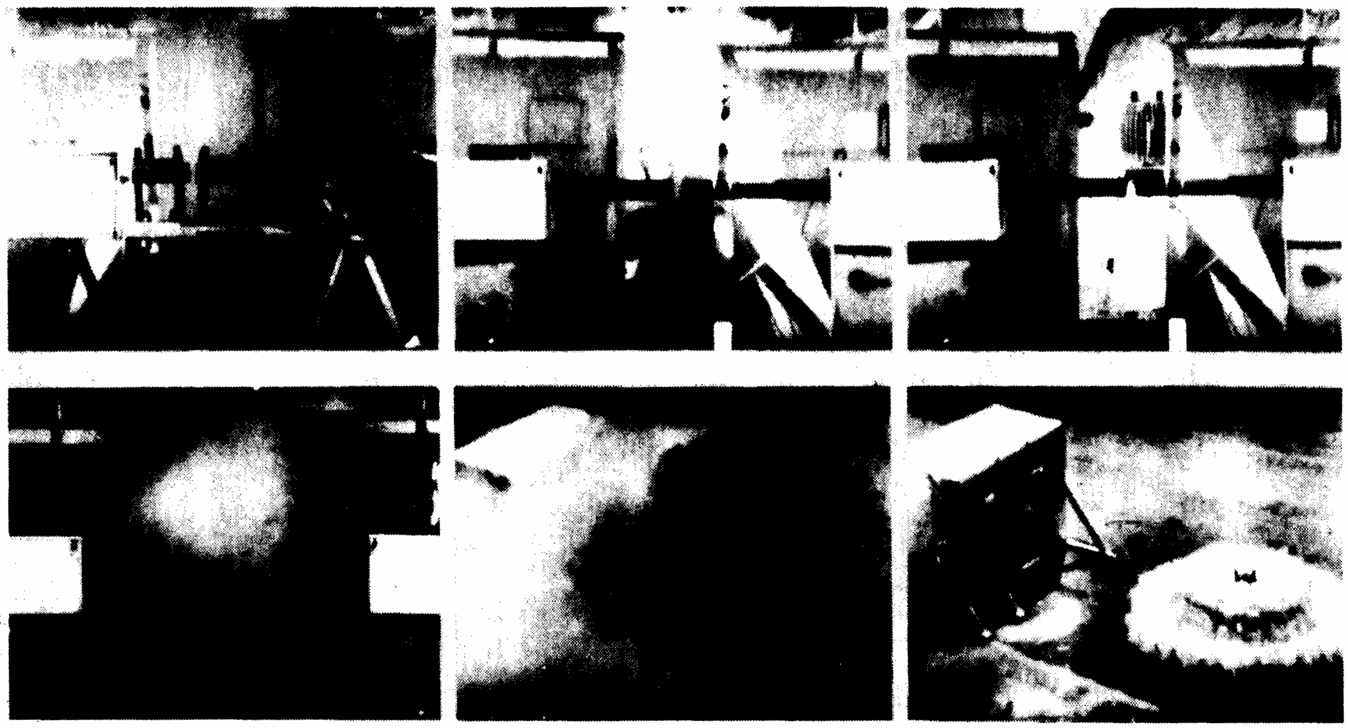


FIGURE 5. HIGH-SPEED PHOTOGRAPHS OF TEST IN PROGRESS. NOTE PIECE OF SHRAPNEL STRIKING LEFT COLLIMATOR TUBE IN TOP CENTER PHOTOGRAPH.

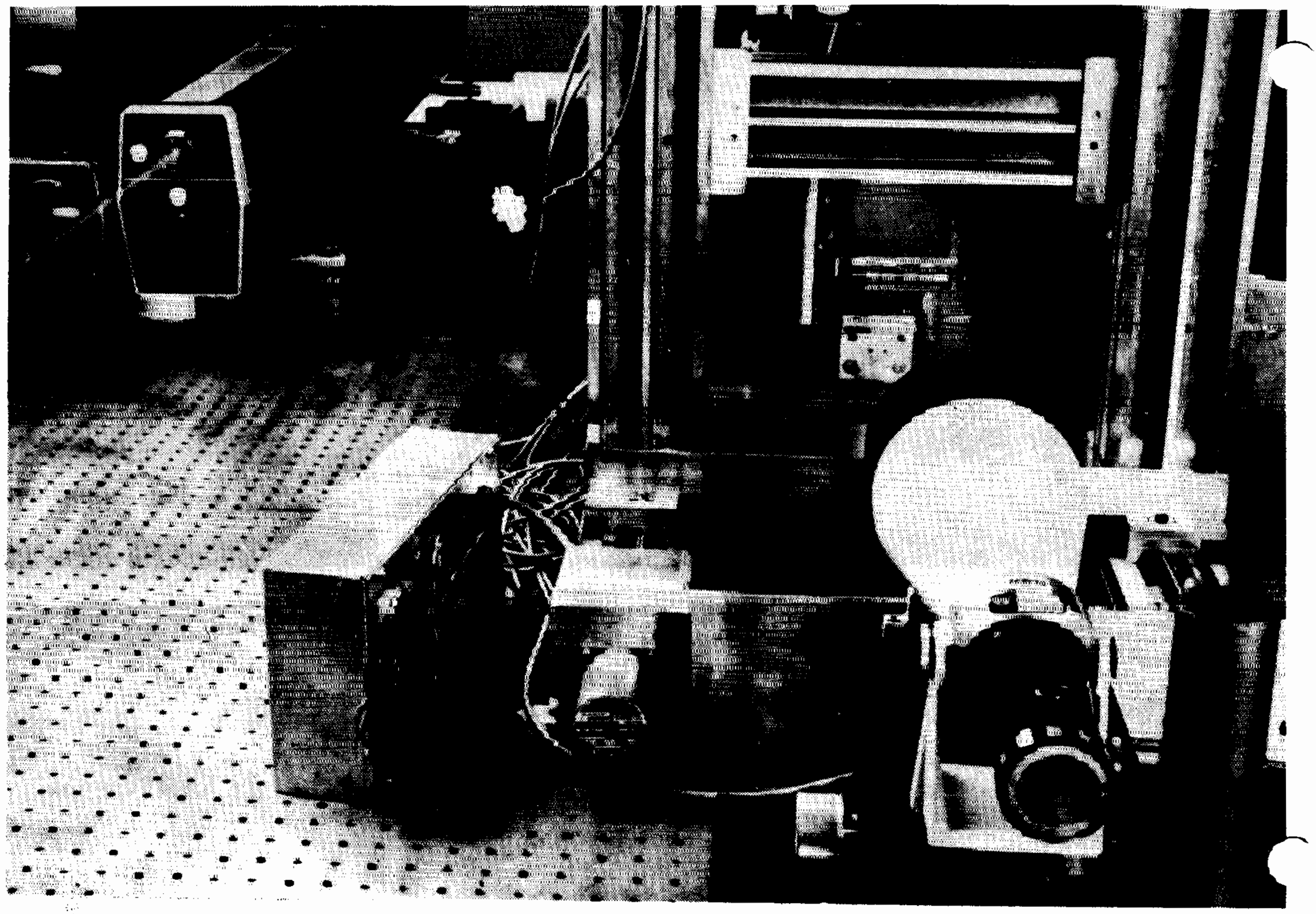


FIGURE 6. HOLOGRAPHIC RECONSTRUCTION SYSTEM

TRANSDUCER AND SIGNAL CONDITIONER PHILOSOPHY FOR LARGE PROGRAMS

By

W. T. Escue
Measuring Sensors Branch
Guidance, Control and Instrumentation Division
Electronics and Control Laboratory

ABSTRACT

The author will discuss the pros and cons of each of the following points:

- Procurement of off-the-shelf hardware versus procuring to specifications
- Commonality hardware versus hardware for the application
- Use of vendor calibration versus local calibration
- Self-checking feature versus no checkout provisions
- End-to-end system calibration versus individual component calibration

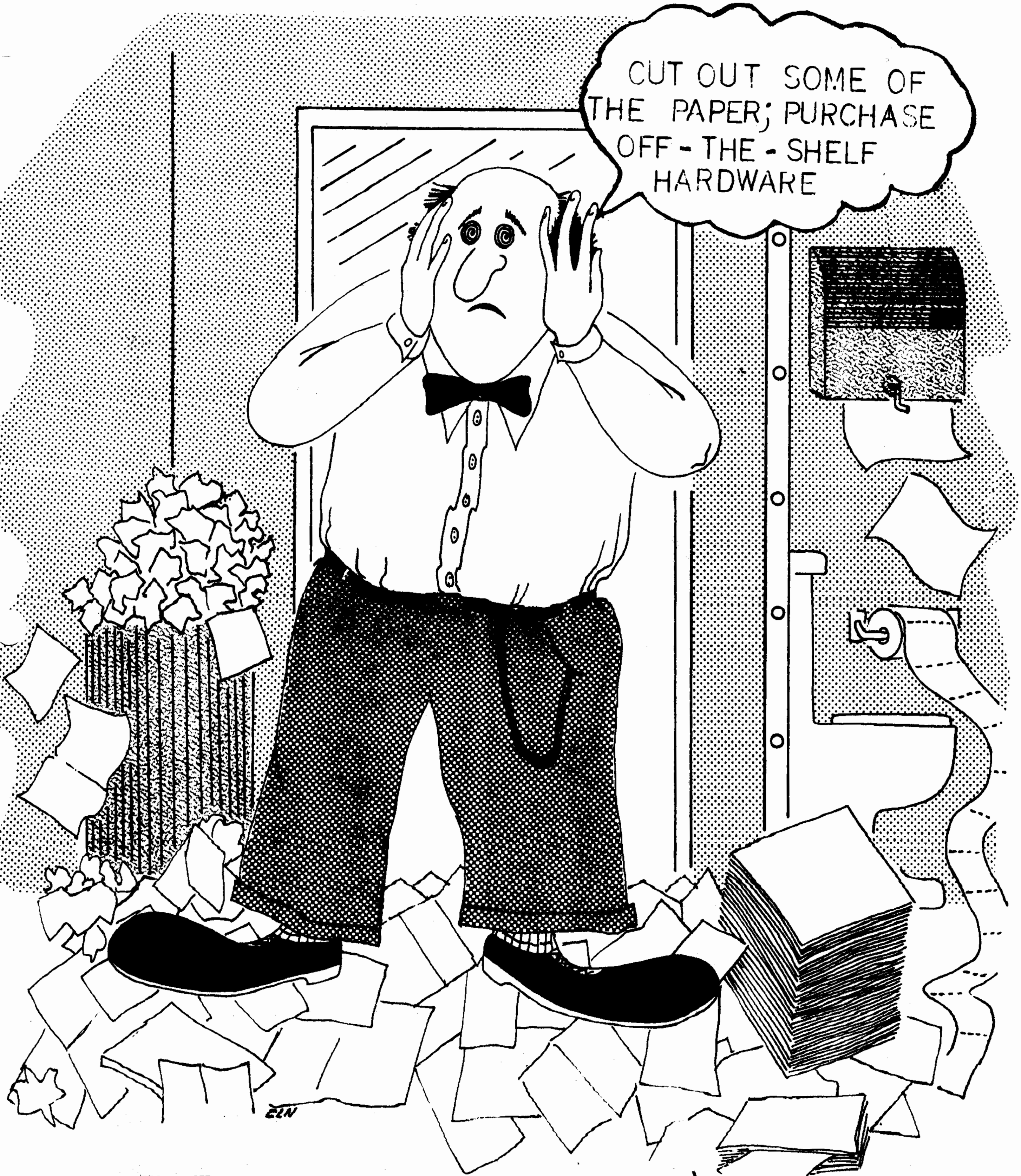
The author will discuss the problems associated with the Shuttle program concerning these points. These topics will then be offered for group discussion for others point of view.

Introduction

I began my career in Aerospace Instrumentation early in 1958. My experience is on such programs as Redstone, Jupiter, Saturn, Apollo and Shuttle. It's interesting to look back and compare the way things are done today versus how they were done in those early days.

Take for an example a pressure transducer. Today we write a very elaborate specification for a transducer. We require traceability of materials, quality control plan, use of screened parts, etc. I'm not saying that these are not good to have, but is the cost worth it? This is for so-called off-the-shelf hardware. If we specify a transducer that is beyond the state-of-the-art, then we pay an additional research and development cost.

In the early day of aerospace programs, we had transducer manufacturers standing in line to show what new developments that they had come up with and asking us to evaluate their product. We would evaluate everything on the market and select the transducer that looked best for the application. The transducer would be procured to a vendor part number. This resulted in a cost of $100 to $200 compared to a cost of $1000 to $2000 today. At that time we were launching satellites; today we are working on programs that require man-rated components and we cannot afford a failure.



OFF-THE-SHELF VS SPEC'S

Procurement of Off-the-Shelf Hardware
Versus
Procuring to Specifications

When we say off-the-shelf hardware, we really mean off-the-shelf design. Most vendors do not stock transducers, but assemble them after receipt of contract. In order to extend the state-of-the-art while procuring off-the-shelf hardware, the entire industry has to procure to existing designs rather than writing specifications. There is no incentive for the vendor to have an in-house R&D program if only a few users are procuring off-the-shelf design.

Advantages of Procuring Off-the-Shelf Design:

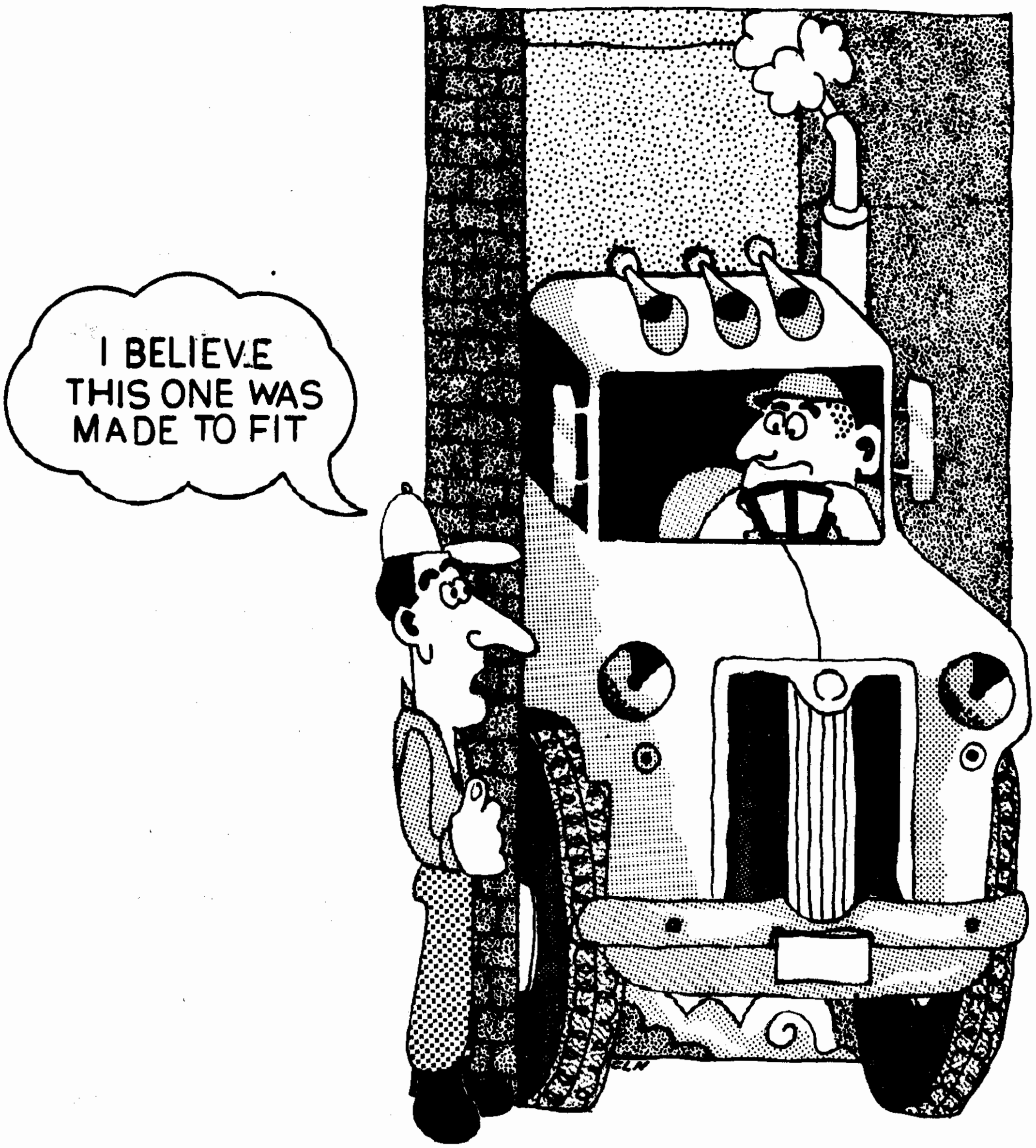
1. Reduces documentation for procurement; use vendor part number.
2. Shorter delivery times.
3. Usually has good use history.
4. Cost less.

Advantages of Procuring to Specifications:

1. Better control of the processes.
2. Better contract.
3. Better overall measuring system.

The Shuttle program procures to specifications in all cases.

This situation is brought about by the fact that the majority of the users, both government and industry, are procuring to specifications. This sometimes increases the cost of a system due to additional requirements placed upon the system. The only problem that this causes is higher cost due to added requirements that go into the specification. I think we should examine this cost growth and determine its justification.



COMMONALITY VS APPLICATION

Commonality Hardware
Versus
Hardware for a Specific Application

At first glance, you think that the more things that are common the more cost effective for the program. This is not necessarily correct. Very seldom do you ever have any two requirements that are identical; therefore, if you use a common transducer or signal conditioner for these two requirements, then you have either over-designed or under-designed on one or both applications.

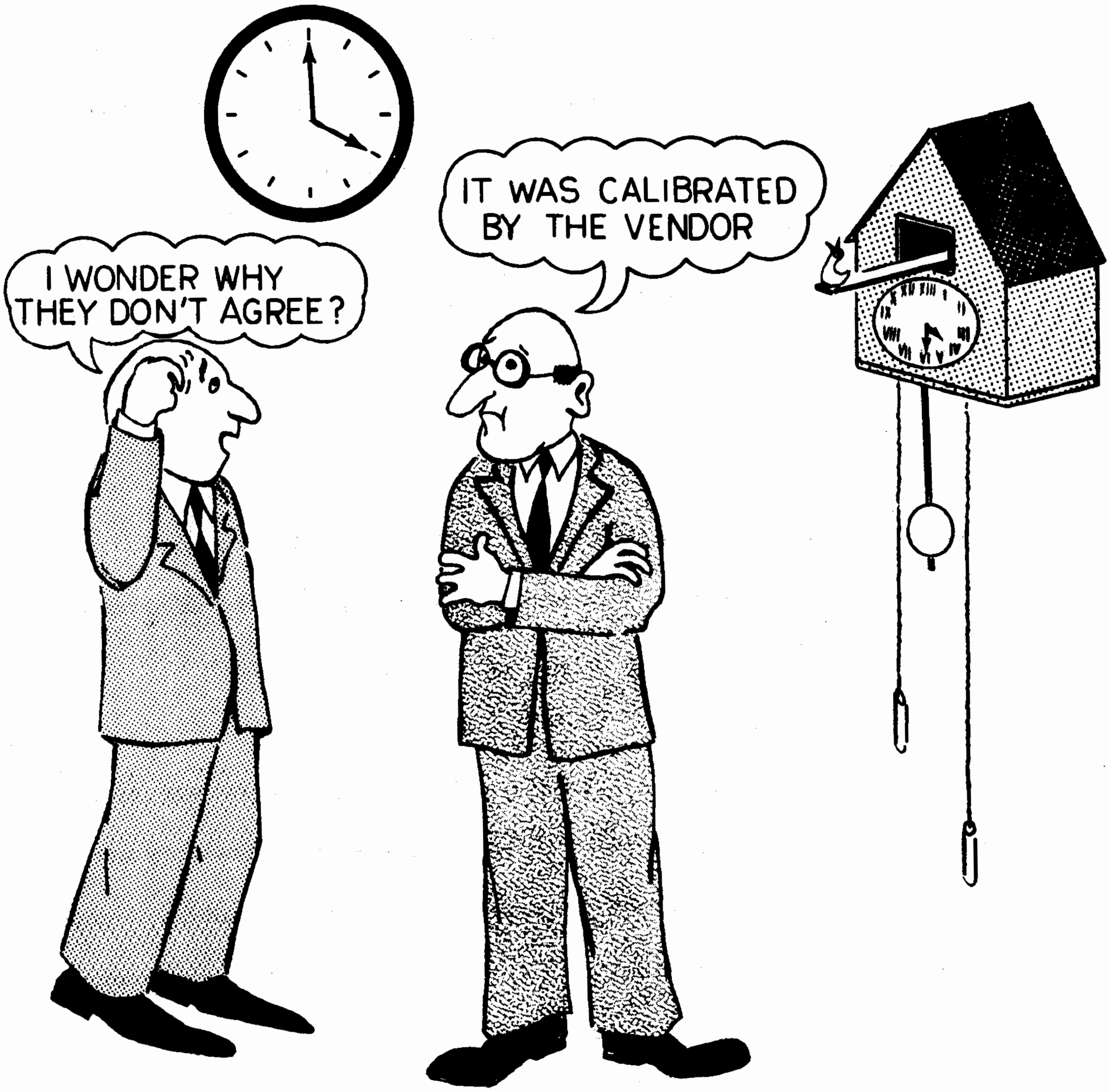
Advantages of Commonality Hardware:

1. Quantity discount.
2. One set of documentation and spares.
3. Saves procurement time.
4. Better history data.

Advantages of Hardware for a Specific Application:

1. Better accuracy; not over or under-design of hardware.
2. More flexibility.
3. Fewer interface problems.

The Shuttle consists of the Orbiter, External Tank, Solid Rocket Booster and SSME. The respective contractors involved are Rockwell, Martin, MSFC and Rocketdyne. Commonality was looked at, and it was decided that we could go common on a few items. The signal conditioner is common with the Orbiter. Some of the problems we have are interface between the transducer and signal conditioner, overloading the vendor's production facility, etc.



VENDOR CALIBRATION VS LOCAL CALIBRATION

Use of Vendor Calibration
Versus
Local Calibration

On the early programs, such as Redstone and Jupiter, we were able to calibrate each and every transducer with its mated signal conditioner just prior to installation. As the programs got larger, we started relying on vendor calibration.

Advantages of Vendor Calibration:

1. Requires less manpower.
2. Reduces size of facilities.

Advantages of Local Calibration:

1. Can make the range of measurement fit the application.
2. Gives the measurement engineer experience with the hardware.
3. Allows more flexibility in use of existing hardware.
4. Detects any damage due to shipping.

The flight measuring system relies on vendor calibrated transducers. These transducers are shipped directly to the assembly area where they are installed on the vehicle without being checked prior to assembly. I expect we will have to change out some further down stream and maybe even fly with some bad ones in non-critical areas.

For our in-house test programs at MSFC, we calibrate each and every transducer when it is received and prior to installation for use. We return many to the vendor that are out of specification. If we had no provisions for checking the calibration, we would have installed the units that are out of specification.

SELF-CHECK VS NO-CHECK

Self-Checking
Versus
No-Checking-Out Provision

We normally think of self-checking as shunt calibration of one or two points on the curve. However, self-checking may be a mechanical means of applying a stimuli to the measuring transducer. Whatever process used, it verifies that the system is functioning.

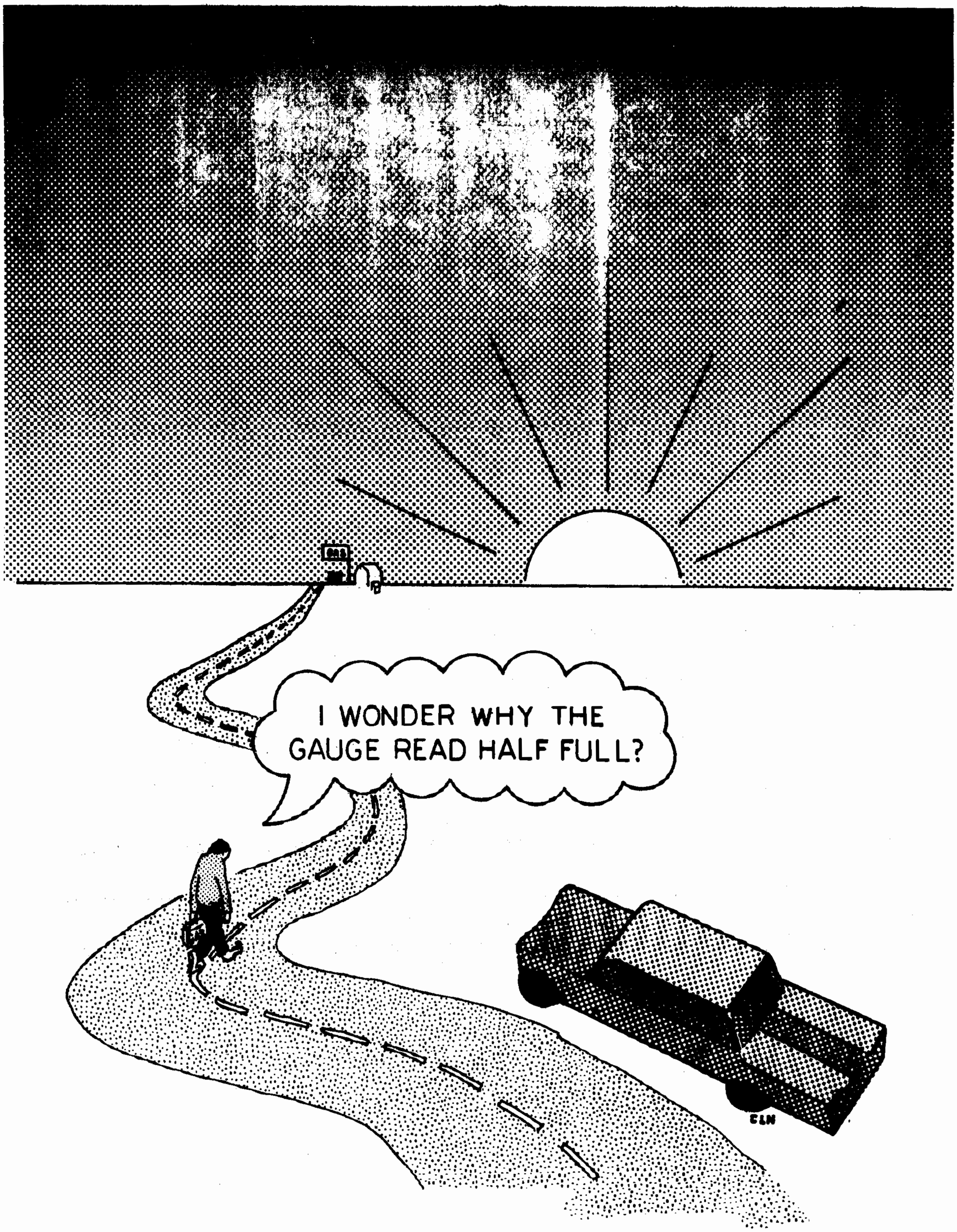
Advantages of Self-Checking Feature:

1. Adds confidence to your measuring system at launch.
2. Detects failures early.
3. May be used to update calibration curve.
4. Provides a check without a full systems test.

Advantages of No-Checkout:

1. Less complexity of the system.
2. Reduces possible failure mode.
3. Cost less.

The Shuttle program has only a few transducers in critical systems that have self-checking features. The reason is primarily due to cost and check-out time.



END-TO-END CALIBRATION
VS
COMPONENT CALIBRATION

End-to-End System Calibration
Versus
Individual Component Calibration

Theoretically, you should be able to calculate these errors due to mating.

It has been said that one picture is worth a thousand words; likewise a system test is worth many calculations.

Advantages of End-to-End Calibration:

1. Eliminates any error due to loading or impedance mismatch.
2. Gives you system level confidence.
3. Gives you a better data base for comparing your flight data.
4. Eliminates interface problems early in buildup.

Advantages of Component Calibration:

1. Usually requires fewer facilities.
2. Interchangeability between signal conditioner and sensors.
3. Requires fewer spares.

The Shuttle program does not use end-to-end calibration. Again, it is a case of cost and availability of facilities. Representative systems will be mated together and interface test run to identify any problem areas early in the program.

Summary

It is the author's opinion that for most programs, the best measuring system for the least cost can be obtained by procuring sensors and signal conditioners from an off-the-shelf design with a self-checking feature, individually selected for the application, calibrated locally as an end-to-end system just prior to installation.

Deviations can be made from this philosophy, and shortcuts can be taken that may never show up in your overall program, as long as you have a successful program. Evaluation of a system failure requires a good measuring system; how good the measuring system is usually shows up during analysis of the failed system. Evaluation can be compared to washing your car; the rock pecks don't show up until you wash the car yourself- yet the blemishes are still there.

SESSION II

DISCUSSION SUMMARY

Session Chairman: Steve Rogero

Papers: Ray, Rieger, Stein, Bean, Sires and Escue

DISCUSSION:

Paul Lederer, NBS: Would you comment on the desirability of using transducers with built-in signal conditioning, with either a frequency output or a dc voltage, in applications like the one you described?

David Ray, Kirtland AFB: I think the limit is going to be the environment the transducer sees. If you're trying to measure acceleration in high temperature environments you may have trouble. If you're trying to measure temperature in high acceleration environments you may have trouble. I have the same problem with using signal conditioners. You can use integral electronics with just about any kind of resistive bridge transducers. There are some now on the market. There are some troubles associated with that. You must filter before you a to d convert, to prevent alienating effects, etc. You want to be careful with your timing on the converting, you have to clock your conversion. You have to decide whether you want series or parallel output. Parallel output means quite a few wires and you're going to have quite a few bits coming out.

Bob George, Ames Research Center: Did you try more than one manufacturer? Did you find one essentially better than the other?

Ray: No, I think there are any number of hybrid outfits that can build that circuit. The particular one that we happened to contract with at the time was Voltran. We had them do the first few. We had some good luck with them and some bad luck. I think there are some things to be learned there. I think that a lot of thought has to go into the way the components are mechanically tied down onto the board. I have a feeling the high shock criteria might significantly affect you. I think that someone has to apply some skill to the layout and how the components are fastened.

Bill Anderson, NATC, to Pete Stein: On your carrier-based system for strain gages, what are some of the problems associated with them? You essentially are working to raise the frequency of your system up to the carrier. Other problems are line capacitance and things like that.

Pete Stein, Stein Engineering Services: In the paper there are a number of different possibilities discussed, which means you don't always need the carrier to solve the problems. If you use non-inductive thermocouples or resistive strain gages, use a special cable; and if you take all the precautions that are reasonable you can get away with straight forward instrumentation without worrying about some of these spurious effects. There are two things. One which happens very often, the customer delivers to you for testing an already instrumented unit, and you have no choice but to accept all the wrong stuff that was put in it. This happens to a lot of people. Then there is no other choice but to put in a carrier system, and then you buy the cable capacitance problems. On the other hand you may have instrumentation on the outside of a gun barrel, as it happens when you shoot the cannon, and there is a transient magnetic field that goes along with it as well as a chemical reaction. Very few people are even aware of it. All of a sudden you have these spikes of voltage coming out when you want to measure strain. You've already selected your instrumentation and got your hard wires soldered in place and covered and buried. So the trick is to select your instrumentation carefully to begin with and then to do your diagnostic check.

Alan Holmes, Lockheed, Palo Alto Research Lab: In using a carrier system you're not totally immune to the transients, are you? If there is a component in the transients that is within your filtering band you are still going to get that, right?

Comment (Ray): I'd like to comment on that. In a carrier system essentially you're trying to frequency translate this data up to a frequency where that transient hopefully doesn't cause any spurious outputs. So you've got to choose that carrier to make that kind of frequency translation. Hopefully this will get rid of your transients.

Comment (Holmes): There is one modification to the statement. If you have a transient that's say a square wave or as close to a square wave as you can get, every frequency component is there in varying degrees.

Pierre Fuselier, Lawrence Livermore Lab: I have a question for Larry Sires: What kind of quality controls did you have on those saw horses you used for the sewer pipes?

Larry Sires, Naval Weapons Center: None. What you have to realize is that what we were doing here was a trial system, to see if we could do it. A number of problems came up, such as that piece of shrapnel hitting the tube, that had to be solved on the spur of the moment. The contractor was sitting there charging us money and waiting to go home. What was available we used, we tried to solve the problems at the time.

George to Larry: I'm not really familiar with your project but it seems to me if you weren't sure you were capturing the picture couldn't you throw some low size dust particles in there and use that for calibration?

Sires: This work was done in June of last year. The new work will be done beginning June of this year. In our test plan we are going to do exactly that. We are going to put a material of known size in some of our tests. It will be incorporated in our test plan to be sure that we're capturing what we think we're capturing.

James Birdsall, Pratt & Whitney Aircraft, Fl: I was listening to Professor Stein's comment about electro magnetic pulse, and the chemical reaction. I was thinking maybe Ray's work had to deal with that problem.

Ray: Well we're dealing with transients differently. We're never really sure where some of these effects come from. When you have an air shock that big, traveling over that whole line you could very easily generate spurious voltages. In that particular test several agencies were participating and using various grounding schemes and that caused just no end of problems. I was absolutely horrified when I walked up to a shelter and found that they were burying a 440/220 V transformer inside the aircraft shelter, to run the motion picture cameras. Every agency has a right to participate in these tests. Worst off were the people who were trying to run low level signals in the midst of this very long line. They were the ones really hurting.

Birdsall: Does that phenomenon manifest itself like that suddenly, that chemical change?

Stein: The reference that I cited in the paper is a 1954 reference which goes quite a ways back. A fellow named Henderson was testing guns, I think it was in Aberdeen. Occasionally people are clever enough to run a test without anything connected to their strain gages or if you're doing thermocouple test running chromel wire in and chromel wire out, in other words you have a non-thermocouple. I was going to mention that Dave has something very interesting in his paper. We were testing rise time in a measuring system calibration circuit. Frequently, people who do that normally have it built right into their system. When you push the button to shunt calibrate you in fact are producing a resistance change and that excites all the frequency response in a system.

Ray: I think it came home to us when we were seeing overshoots in some of our calibrations. Cals start wobbling somewhere in the system. Somewhere in the system there was positive feedback or something was not quite right, but you know it.

Ray Reed, Sandia Lab: In connection with that particular question, what is the rise time of your cal step? In other words, how near a step is it really? Because in the shock domain you're talking about, there are steps and then there are steps.

Ray: Well Delta t is certainly not zero. The rise time is controlled by the slewing rate of the amplifier which drives the solid state switch. That particular amplifier is a Fairchild 74. It is one that I believe has a slewing rate of ½ V per microsec. We were talking about signal levels typically 20 mv out of the bridge and a gain of 30, so we're talking about 10's of microseconds, which for our purposes was close enough.

Wayne Whaley, Wright-Patterson AFB: I'd like to ask Prof. Stein to comment on the possibility of using FM telemetry to get around some of those noise problems.

Stein: In the 7th Transducer Workshop, they started something which is missing from this one. They asked everyone to submit a booboo as they walked in the door, and the things were read to the assembled audience. The booboos are the mistakes that you make that were honest mistakes. In the 7th Transducer Workshop booboo chapter, booboo #1 which I can quote almost verbatim, not quite, they replaced some carrier type flight telemetry systems with a new improved dc amplifier which had just come on the market. All of a sudden their stuff was full of noise levels they had never seen before and they couldn't get rid of it.

So it depends a lot on what you do before you telemeter it out. You use a voltage control or subcarrier oscillator, which you hang on the output of your thermocouple and the output of your strain gage bridge. By then it's too late, the stuff's all together, and the telemetering is the whole thing, garbage and signal. So the kind of checks I'm talking about you have to do before you get into your VCO. Everything is frequency modulated and comes out beautifully. The plea I have been making over the years is that you have to do all these checks before you start the first link in the data handling chain, otherwise you can no longer separate the stuff out.

You can't express it as a percentage of signal because the noise levels are created by a totally different physical process. It can be 10 times as large as the signal. The Rolls Royce people had these nickel lead wires producing voltages by a mechanism that still nobody understands and that were 100 times the strain gage output. With a noise level 100 times signal, no matter what you do, you are stuck. You quit using nickel lead wires in high temperature turbine work. But everybody's got to find that out for themselves. In 1977 another outfit, who has a representative here, found out that you don't do high temperature work with nickel lead wires. This work stems from about the early 1940's until the late 1970's.

Norman Muelleman, Gard Inc.: In the AM type carrier system you mentioned some problem when you initially talked about the load line or load characteristics of a line. You get around that, of course, when you're in the VCO transmission voltage mode. Any comments on that?

Stein: When you have a bridge control oscillator, (resistance control oscillator) the carrier has to go through the transducer, through the strain gages or the resistance thermometers and most of your telemetry. According to the IRIG standards, you peak a bridge with dc and then you frequency modulate the output. As far as I know there is no surviving manufacturer of resistance controlled oscillators using the resistance of the transducer itself to produce the FM.

Henry Freynik, Lawrence Livermore Lab, to Pete Stein: In Figure 4 you go through the noise checks for voltage excitation on an equal arm bridge. Do you do that same thing for current excitation of an equal arm bridge? Should you clamp the current supply when you operate switch two and should you switch to some resistance or should you just have an open circuit?

Stein: Shown at the bottom of Figure 4, Henry, for a current source you'd have to make sure that the current source has a resistance to pump its current through. You'd apply that to the bridge circuit as well.

Question to Larry Sires: What is the FAE and what is the purpose of this cloud?

Larry: Fuel Air Explosive. The cloud is dispersed and the idea is that if you have a bomb and you don't have to carry the oxidizer with you, you can have a larger bomb in a smaller package. You also have an explosion that takes over an area rather than a point. So Fuel Air Explosive disperses the fuel, put it in the air and uses the air as an oxidizer and detonator. It has a large area of detonation. You don't have a shrapnel blow-down.

Question: Is that the same thing as the concussion bombs in use?

Sires: Yes.

Muelleman to Sires: Weren't you getting into some problems on the air turbulence due to the shock of the explosion? Didn't that tend to mask the laser?

Sires: First of all we did not detonate the fuel air cloud. It was explosively dispersed. Our purpose here was to look at what the shape and size and distribution of the particles were at proper detonation time. I get a series of photos of the cloud formation, which takes less than 200 milliseconds to form. There are shock waves there, but by the time you actually snap the picture the shock wave from the initial explosion has passed. Actually the cloud is stationary and slowing down.

Larry Mertaugh, NATC: I don't know that much about lasers, but in that photograph image you get with a hologram, why do you do it with a TV camera?

Sires: You can use any number of methods. TV camera just happens to be convenient. You can take a series of still photographs. The procedure used in reducing the data is to search for a pattern throughout this volume. You know where I said a lady found 17,000 particles, she was actually looking at something less than 20% of the total volume of the picture we took. The TV image on the monitor was approximately a millimeter square. We search through the entire volume. We would go through a particular square in our search pattern and pick up any particles within that square.

Mertaugh: You mean by focusing?

Sires: Right, essentially. Our focusing here is done by physical movement of the images back and forth by moving the holographic plate.

We have a set search pattern from which hopefully we can get a statistical picture.

Mike Burger, Lawrence Livermore Lab: Did you ever think of using a counting computer system technique, like they use in hospitals for counting red and white corpuscles in a volume?

Sires: We must find a way to computerize or automate the counting process. Sizes and shapes of the particles vary between 10 and 150 microns. Surprisingly the particle shape is not spherical.

SESSION III

STRESS-FORCE AND TEMPERATURE

John S. Hilten, Chairman

DEVELOPMENT OF A PIEZOELECTRIC SOIL STRESS GAGE

Joseph D. Renick
Air Force Weapons Laboratory
Kirtland AFB, New Mexico 87115

ABSTRACT

A piezoelectric soil stress gage which incorporates a fluid coupling mechanism has been developed. The gage consists of a thin quartz sensing element supported by a nylon ring and suspended in an RTV 602/metal-oxide matrix which is contained between stainless steel diaphragms. The RTV 602/metal-oxide matrix serves as the coupling mechanism to transmit stress on the diaphragms to the sensor element while isolating the sensor element from shear and bending loads. The design is intended to provide measuring capability to 1 Kbar. Previous research with a piezoelectric polymer and its application as a stress gage is reviewed and it is shown how this experience led to the present design. Soil stress gage design criteria are discussed in general and the criteria important to the present design are identified. Results of hydrostatic calibration tests are summarized. Planned laboratory testing and future design modifications are discussed.

INTRODUCTION

The Air Force Weapons Laboratory (AFWL) under Defense Nuclear Agency (DNA) funding, has been developing a piezoelectric soil stress gage for measurements to 1 Kbar. This program was initiated after preliminary evaluation of a piezoelectric polymer, polyvinylidene-floride, which was developed by the National Bureau of Standards (NBS)[1]. AFWL experience with the polymer was summarized by Bunker[2] at the Eighth Transducer Workshop in 1975. Here, it was reported that the polymer demonstrated a high charge-pressure sensitivity, very good linearity, an acceptable temperature sensitivity and strong sensitivity to shear and bending. The shear and bending sensitivity led to considerations of packaging techniques which would permit cancellation and isolation of undesired inputs while preserving the desired pressure input. Bolt, Beranek, and Newman, Inc. (BBN) fabricated two polymer gages for the AFWL to evaluate packaging techniques. During this work BBN discovered that the polymer elements are very non-uniform in their piezoelectric characteristics from one element to the next making the task of cancelling signals generated by bending essentially impossible. Thus, at that point the AFWL concluded that the polymer was not characterized well enough to permit a meaningful evaluation of its response utilizing the new packaging technique. That is, if an unpredictable response was encountered, there might not be a way to determine if that response was due to the polymer, the packaging technique, or both. It was apparent that elimination of one of the uncertainties was required before a meaningful development and evaluation program could begin.

The approach taken by the AFWL was to develop and demonstrate the packaging technique utilizing a well-characterized piezoelectric sensing element and then perform further evaluation of the polymer in the proven package design. In the interim, it was hoped that additional work might be accomplished by NBS to provide a better understanding of the polymer response. Neither DNA nor AFWL has funded NBS work in this area.

The piezoelectric polymer, as a sensing element, has unique features of being thin and flexible as opposed to hard and brittle like crystals. There are many applications which take advantage of these features with good results, and it appears that these features are also desirable for a soil stress gage. However, it is not likely that the polymer will be successfully used as the sensor in soil stress gage until its response is better understood and a suitable packaging technique is developed.

GAGE PACKAGE DESIGN

Several factors influenced the design of the gage package. First, a well-characterized piezoelectric sensing element was required. X-cut quartz was chosen. In addition, it was felt that the design should be compatible with existing calibration facilities and AFWL field placement techniques. This dictated that the gage package have the same external dimension as the WES soil stress gage[3] since the AFWL utilizes that transducer routinely in field tests. Eventually, the piezoelectric stress gage will be evaluated side by side with a WES stress gage in an appropriate field test. A later stage in the development would be to construct and evaluate very thin gage packages.

The placement system design is described in detail elsewhere[4], however, a brief description will be provided here since this was a recent and notable advancement in stress gage placement technology by the AFWL. Some comments on stress gage design criteria are included to show how the placement technique satisfies the design criteria.

Placement Technique Description and Stress Gage Design Criteria.

The placement technique is described in Figure 1 which shows a stress gage contained in an aluminum paddle in place at the bottom

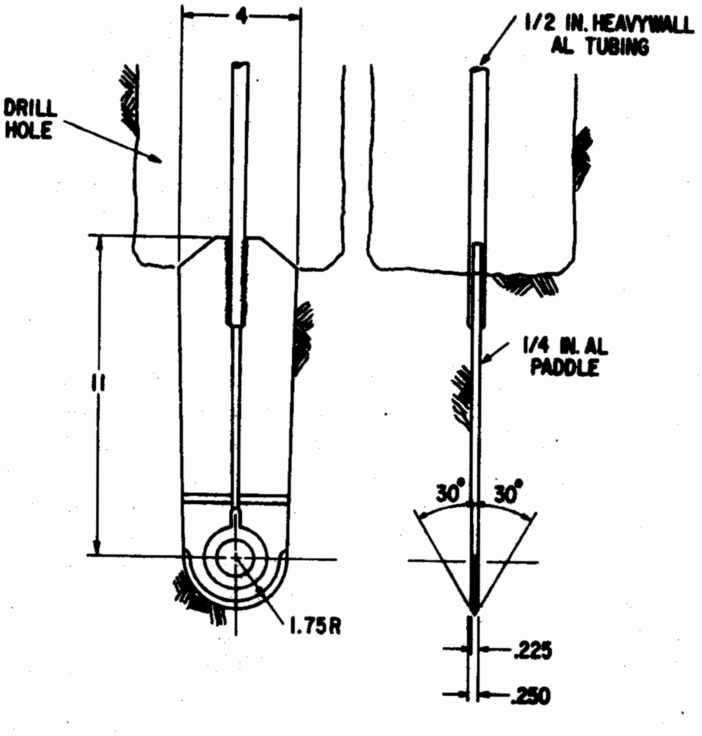


Figure 1. Details of WES Stress Gage and Paddle Assembly Shown in Place at Bottom of Drillhole

of a drill hole. First, a steel wedge is driven into the soil at the end of the drill hole (which may vary in length from 2 to 40 feet long) with a sliding hammer arrangement, as shown in Figure 2, to form a slot for the paddle. The wedge is identical to the paddle with the exception that its thickness on the forward end is .020 inch less than that of the paddle to insure that the paddle will be in intimate contact with the soil. The paddle is then driven in with a driver tube that slides over the aluminum cable exit tube, as shown in Figure 3. The driver tube is removed after the paddle is driven in. After gage installation, the drill hole is backfilled with grout. This technique works well for clay-like geologies, however, it has not been applied extensively to other less favorable geologies. Unique problems in placement are introduced for each geology type and special techniques must be developed around the basic concept.

The basic criteria for measurements of soil stress and review of the literature on the subject are summarized by Smith[5]. The general criteria for static stress measurements in an elastic material subjected to uniaxial loading are:

(1) Large diameter-to-thickness ratio of the transducer package.

(2) Gage modulus higher than that of the soil.

(3) Gage sensing area centrally located and less than 50% of total gage area.

(4) Gage in intimate contact with the soil.

These criteria apply at low pressures where the rise in soil pressure occurs slowly (on the order of a millisecond) in comparison to a shock transition (nanoseconds) so that the transducer can be assumed to be responding to static loads and must therefore meet the static design requirements. However, if the compressive yield strength of the soil is greatly exceeded, the gage will sense the hydrostatic pressure of the soil regardless of its geometric configuration. This is the case in most of the geologies and pressure ranges of interest to the AFWL, thus, the present placement technique may be expected to provide good results.

Packaging Design Considerations. The packaging concept was to place the sensing element in a thin, flat fluid-filled cavity formed by two thin stainless steel diaphragms. The diaphragms would support a negligible portion of the external pressure on the gage, providing the cavity fluid had the appropriate modulus. The fluid would experience a hydrostatic pressure due to lack of shear strength and would thus load the sensor hydrostatically. As shown by Bunker[2], the polymer

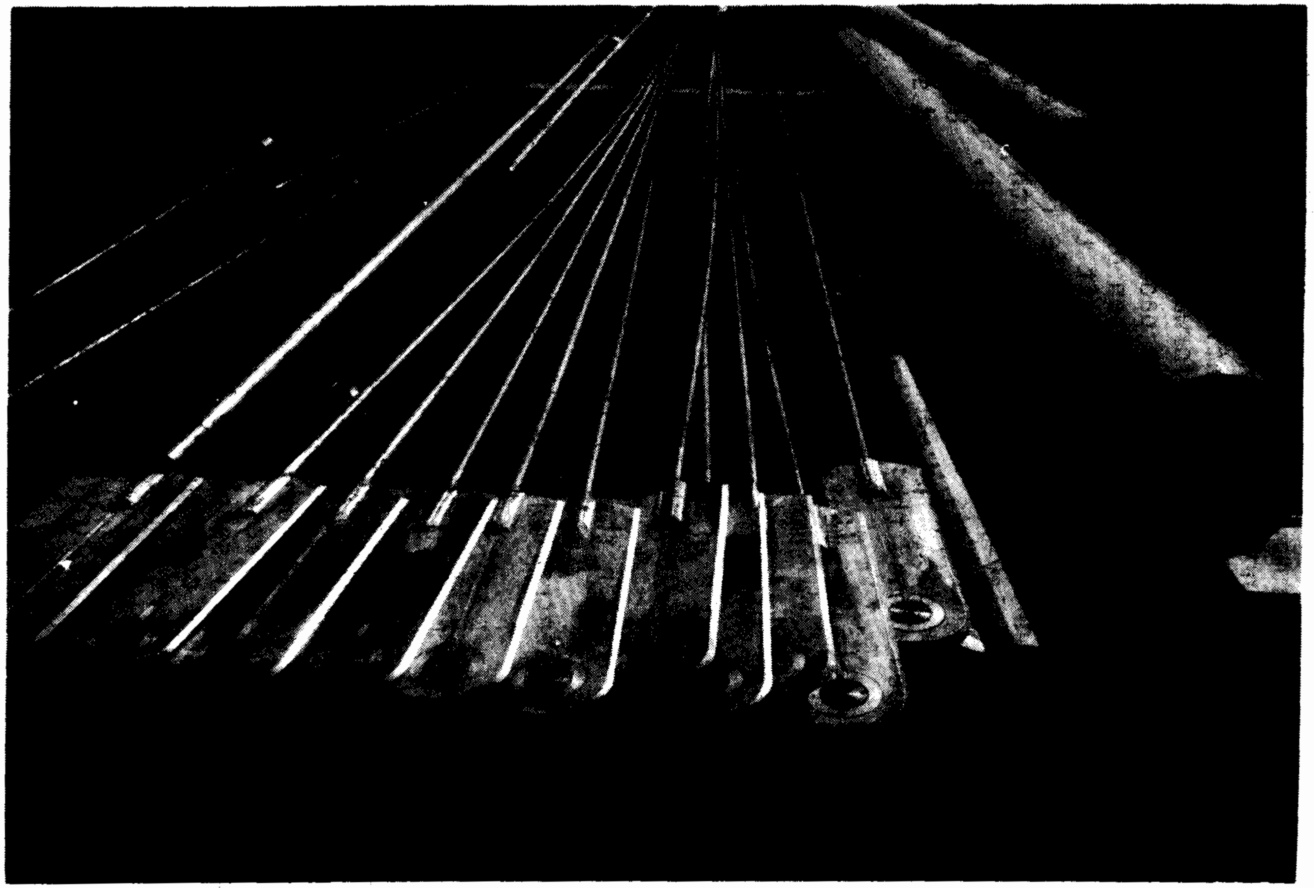


Figure 2. WES Stress Gage and Paddle Assemblies. Note Driver Tube in Place on Paddle at Far Left

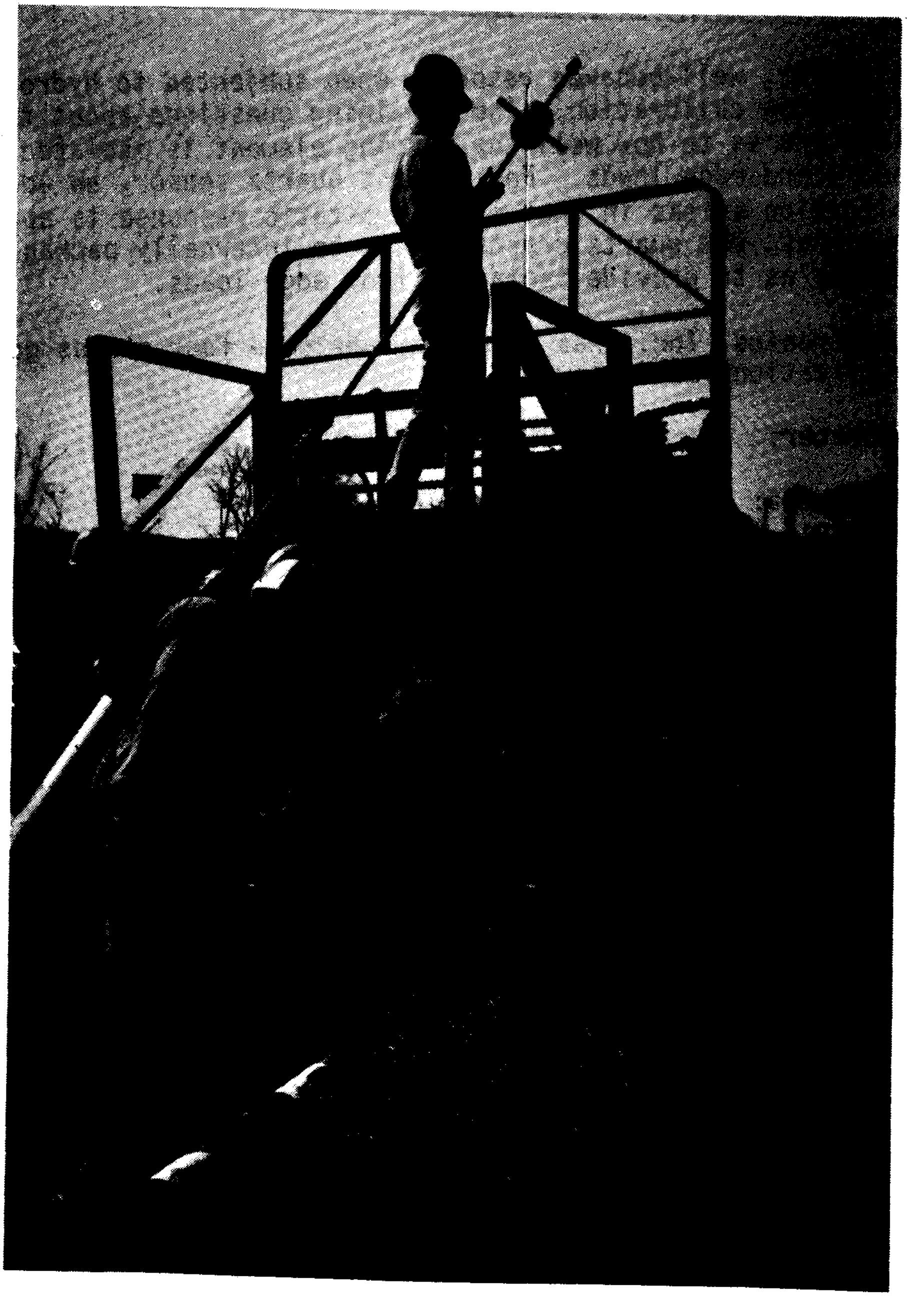


Figure 3. Placement Operations of WES [illegible]
Note Sliding Hammer Driver Assembly

indicated a linear, well-behaved response when subjected to hydrostatic loads during calibration. The remaining questions about the package design are as to how well the sensing element is isolated from bending, shear, and edge loads. For a X-cut quartz sensor, an additional complication arises in that its hydrostatic response is approximately zero. Thus, the quartz crystals must be internally packaged in such a manner as to provide isolation from edge loads.

PCB Piezotronics, Inc., was asked to fabricate four stress gages to AFWL specifications as follows:

Case Diameter:	2.000 ± .001 inch
Case Thickness:	.225 ± .001 inch
Case Material:	Stainless Steel
Sensing Element:	X-cut quartz, 2 crystals, back-to-back, center positive.
Sensing Element Support:	Low strength polymer
Amplifier:	Built-in, totally stress isolated, shock hardened to ± 10,000 g, low impedance output to drive 3,000 feet of TSP with 20 kHz response, 100 second time constant or greater.
Filler:	Aluminum-oxide/RTV 602 (2 gages) Tungstic-oxide/RTV 602 (2 gages)
Range:	0 - 5,000 psi, 2 volts full scale
Grounding:	Case isolated
Diaphragm Thickness:	.010 ± .0005 inch (2 gages) .015 ± .0005 inch (2 gages)

Two diaphragm thicknesses and two coupling materials were selected to experimentally observe the effects of variation in these parameters.

Analysis of Diaphragm and Coupling Material Loads. A simplified analysis will be performed to identify the effects of parameters such as diaphragm thickness and coupling material properties on load distribution between the diaphragm and coupling material.

The diaphragm center deflection, δ_d, is calculated per Roark[6] for a circular diaphragm with clamped edges as

$$\delta_d = \frac{3 P_d r^4(1 - \nu^2)}{16Y t_d^3} \tag{1}$$

where P_d is diaphragm pressure, psi

r is diaphragm radius, inches

ν is Poisson's ratio

Y is Young's modulus

t_d is diaphragm thickness

and

δ_d is in inches.

If we assume that the applied pressure is large when compared to the shear strength of the coupling material, then the coupling material loading will be essentially hydrostatic and an uniform pressure will be exerted on the interior face of the diaphragm. This will result in an uniform pressure differential across the diaphragm and the diaphragm center should deflect according to equation (1). Thus, the coupling material volume strain may be expressed in terms of the diaphragm center deflection. As a first approximation, it is assumed that the diaphragm deflection is linear with radius as shown in Figure 4. The change in volume, ΔV, is

$$\Delta V = 1/3\pi\ r^2\delta_d$$

For an initial volume of

$$V = \pi r^2 t_c$$

the volume strain becomes

$$\frac{\Delta V}{V} = 1/3\ \frac{\delta_d}{t_c} \tag{2}$$

where t_c is coupling material thickness.

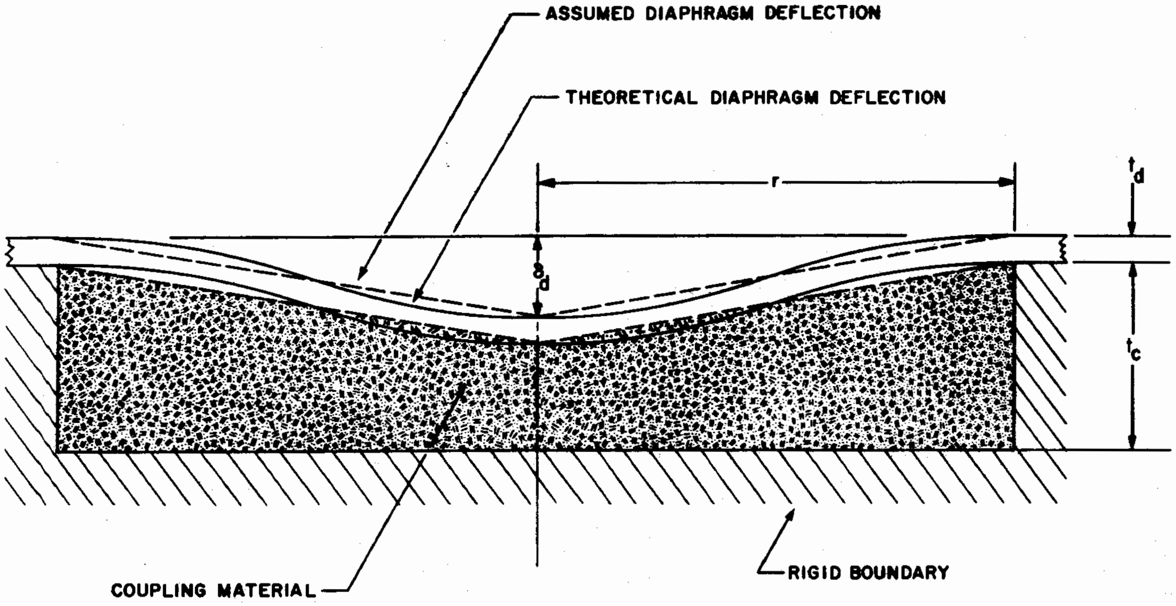


Figure 4. Diaphragm/Coupling Material Deflection/Compression Model

Now, it can be shown that

$$\frac{\Delta V}{V} = \left(\frac{m_1\rho_2K_2 + m_2\rho_1K_1}{m_1\rho_2 + m_2\rho_1} \right) \frac{P_c}{K_1K_2} \tag{3}$$

where m is mass, lbm

ρ is mass density, lbm/in^3

K is bulk modulus, psi

P_c is coupling material pressure, psi

t_c is coupling material thickness, inches

δ_c is in inches, and

subscripts 1 and 2 refer to the mixture components.

Component 1 is a low modulus carrier (RTV 602) and is loaded with component 2, a high modulus metal-oxide (Al_2O_3, WO_2), to provide an increase in effective modulus over that of component 1 alone.

Equations (1), (2) and (3) may be combined to obtain

$$\frac{P_c}{P_d} = \frac{r^4(1-\nu^2)\ K_1K_2}{16Y\ t_d^3t_c} \left(\frac{m_1\rho_2 + m_2\rho_1}{m_1\rho_2K_2 + m_2\rho_1K_1} \right) \tag{4}$$

The difference in K_1 and K_2 is such that $K_2 >> K_1$ so that equation (4) may be written as

$$\frac{P_c}{P_d} = \frac{r^4(1-\nu^2)K_1}{16Yt_d^3t_c} \left(1 + \frac{m_2\rho_1}{m_1\rho_2} \right) \tag{5}$$

or,

$$\frac{P_c}{P_d} = \frac{r^4(1-\nu^2)K_1}{16Yt_d^3t_c} \left(1 + V_2/V_1 \right) \tag{6}$$

Equation (6) is a result of the assumption that the filler material in the mixture is of infinite stiffness and its effect is to decrease the effective volume of the coupling material.

The values for the pertinent parameters in the analysis for the PCB gages are

$Y = 27 \times 10^6$ psi

$t_c = .066$ in

$r = .4375$ in

$\nu = .3$

$K_1 = 200{,}000$ psi

which reduces equation (6) to

$$\frac{P_c}{P_d} = \frac{.0002339}{t_d^3}\,(1 + V_2/V_1)$$

Figure 5 shows the relationship of P_c/P_d to variations in t_d and V_2/V_1.

The dominating effect on pressure ratio is quite obviously that of variation in diaphragm thickness. Another effect not reflected directly in Figure 5 is that of coupling material bulk modulus. Results here are based on a modulus of 200,000 psi which is fairly representative of materials with a density near that of water. It is not likely that there are many materials which have the shear strength properties of RTV 602 and a modulus an order-of-magnitude higher than that of RTV 602. Therefore, diaphragm thickness, or more correctly diaphragm radius-to-thickness ratio, is the parameter which is most controllable and has the strongest effect on pressure distribution. Generally, the criteria for load distribution, or P_c/P_d, would be for P_c/P_d to be large enough such that $P_c/(P_c+P_d) \simeq 1$, which holds for $P_c/P_d \geq 100$. However, for large applied pressures, say 10,000 psi, the diaphragm could be sustaining a pressure of up to 100 psi which could result in permanent deformation of the diaphragm if the diaphragm was very thin and the coupling material of low modulus. Thus, another constraint, diaphragm stress, must be considered.

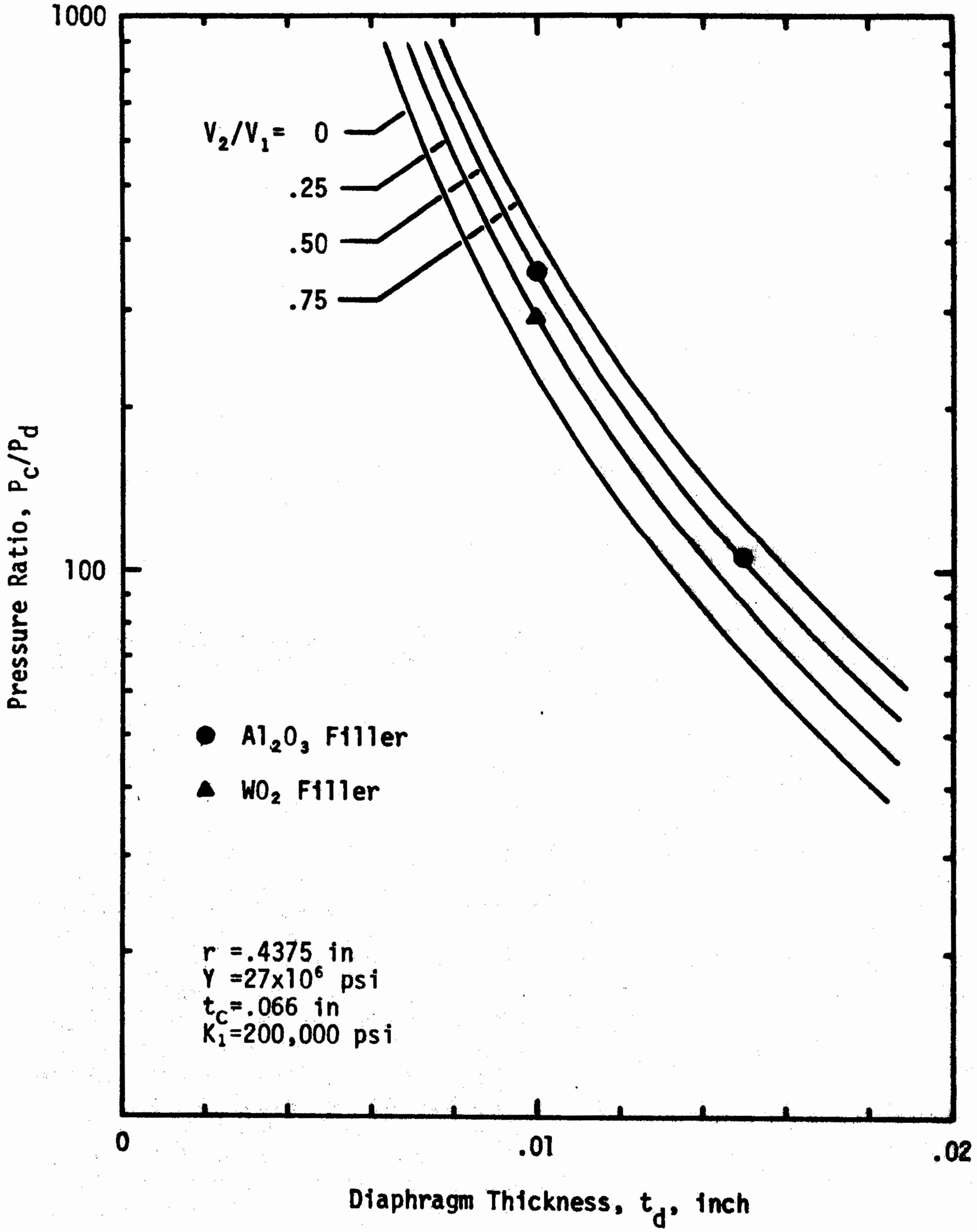


Figure 5. Effects of Variation in V_2/V_1 and t_d on P_c/P_d

The maximum tensile strain in the diaphragm occurs at the center and is given as[6]

$$\varepsilon_{max} = \frac{3P_d(1-\nu^2)r^2}{8Yt_d^2}$$

which produces a stress

$$\sigma_{max} = \varepsilon_{max} Y$$

Thus,

$$\sigma_{max} = \frac{3P_d(1-\nu^2)r^2}{8t_d^2}$$

If we do not allow σ_{max} to exceed 50% of tensile yield (σ_{yld}), then

$$P_{d_{max}} = \frac{4\,\sigma_{yld}\,t_d^2}{3r^2(1-\nu^2)} \qquad (7)$$

Equation (7) may be utilized with Equation (6) to determine the maximum permissible pressure and load distribution with the 50% yield limitation imposed.

$$P_c = \frac{r^2K_1\,(1 + V_2/V_1)\,\sigma_{yld}}{12Yt_dt_c} \qquad (8)$$

The results are shown in Figure 6 where lines of constant diaphragm stress are plotted versus coupling material pressure and diaphragm thickness for 50% yield stress. Note that the 4 PCB experimental gages all lie well above the 15,000 psi design level which means that at 15,000 psi they would all be stressed to a point less than the 50% yield stress. In Figure 5 it was seen that the volume ratio, V_2/V_1, had a small effect on load distribution. However, in Figure 6 we see the real significance in loading the coupling material with a stiff filler material in that the maximum stress experienced by the diaphragm is greatly reduced as the volume ratio is increased. The actual stress may be calculated from equation (8) (remembering that 50% σ_{yld} was utilized). Considering the gage with .010 inch diaphragm and .50 volume ratio loaded to 15,000 psi, we find a stress of 27,930 psi in the diaphragm, or a conservative 22% of yield. The stress is inversely proportional to 1 + V_2/V_1 so that at a

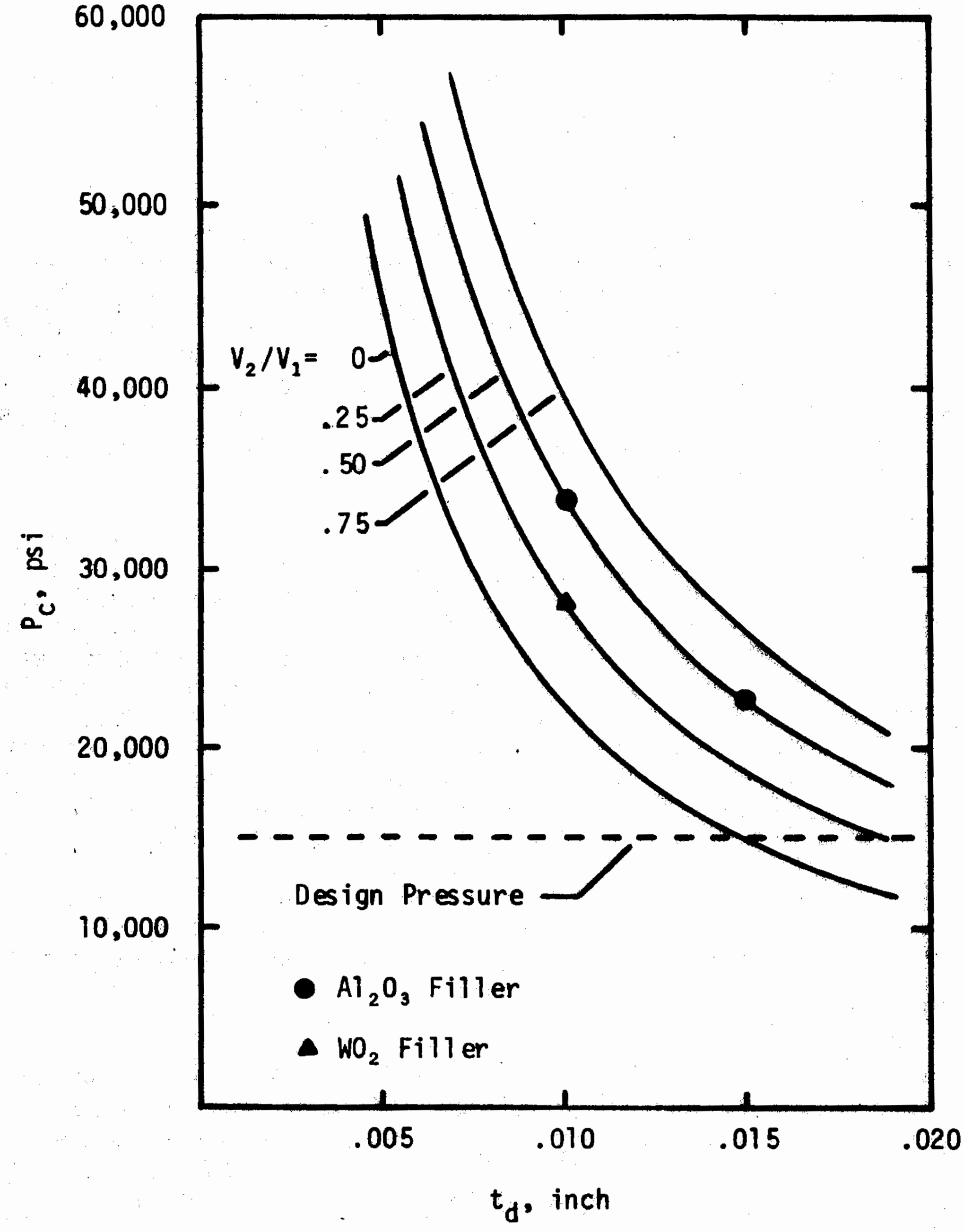


Figure 6. Curves for 50% Diaphragm Yield Stress, Coupling Material Pressure Versus Diaphragm Thickness

volume ratio of zero, the stress is 41,900 psi or a 50% increase in stress. It is also apparent in Figure 6 that a thin diaphragm also reduces stress.

The present analysis involves certain approximations as has been indicated and is far from complete. However, the approach for a rigorous and complete treatment has been established.

Gage Construction Details. Figures 7 and 8 illustrate the stress gage construction details. In Figure 7 the crystal elements and electrode are bonded with epoxy and the crystal stack clamped in the crystal housing between the element diaphragms under a slight preload. A slight lateral clearance between the crystal stack and crystal housing isolates the stack from edge loads. One gage was disassembled after testing to 5,000 psi to inspect the element diaphragm over the clearance area. Negligible deformation was observed. The element diaphragms are bonded to the crystal housing with a solder ring at 1350°F in a hydrogen-purged furnace.

In Figure 8 the electronic's housing detail is shown. The components are potted in a low modulus epoxy and are stress isolated by the element diaphragm. Figure 8 shows the remaining assembly detail. The element suspension ring is clamped in place with Delrin seal rings. Application of the coupling material is very critical in that bubbles will tend to decouple the sensing element from the diaphragms. Bubbles were removed by applying a vacuum at different times during final assembly. The aluminum-oxide/RTV mixture was quite "pourable" and few problems were encountered in assembly. However, the tungstic-oxide/RTV mixture was quite stiff and considerable difficulty was experienced. As a result, only one gage was assembled with tungstic-oxide.

A 2-pin glass seal was utilized for lead wire exit. In field design gages, an exit pipe will be utilized to provide a rugged cable/gage interface. Figure 9 is a photograph of an assembled gage.

GAGE CALIBRATION

PCB has provided the data in Figure 10 which was obtained during factory calibrations to 5000 psi. PCB has reported[7] that the gages demonstrated loading and unloading response along the same voltage-pressure path with excellent repeatability. Table 1 shows serial numbers, filler material and diaphragm thickness for each gage.

A non-linear response is noted in all gages with S/N's 396 and 401 being most nearly linear. S/N 396 is filled with WO_2 and has a .010 inch diaphragm while S/N 401 is filled with Al_2O_3 and has .015 inch

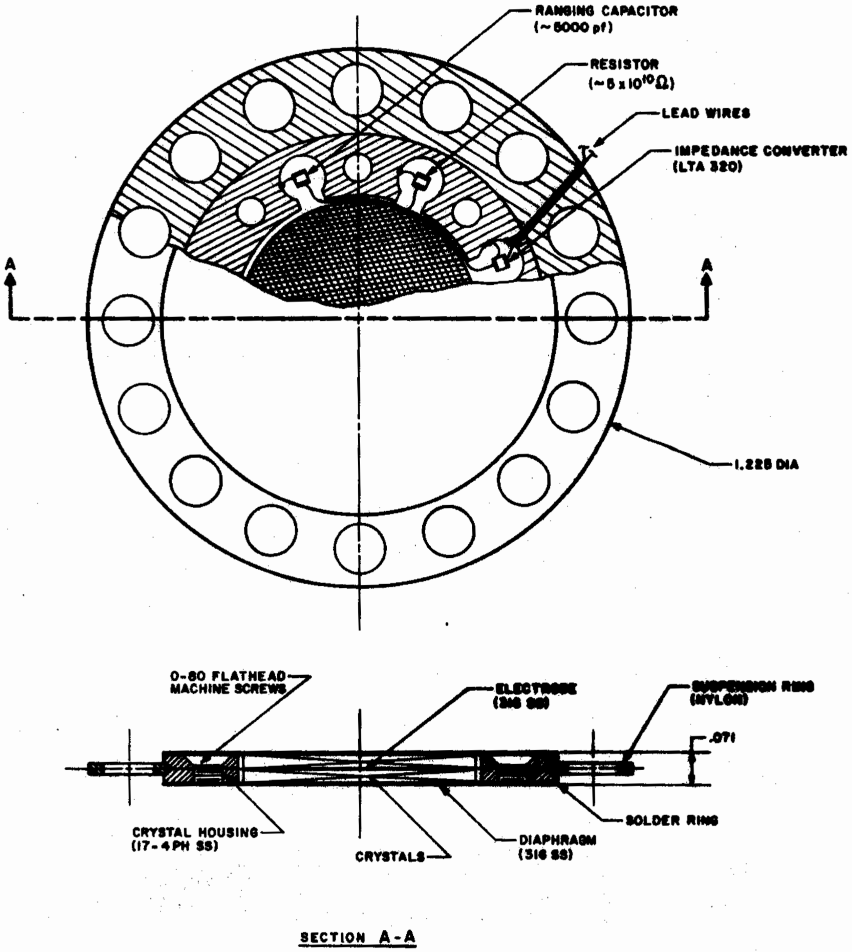


Figure 7. Sensor Element Construction Detail

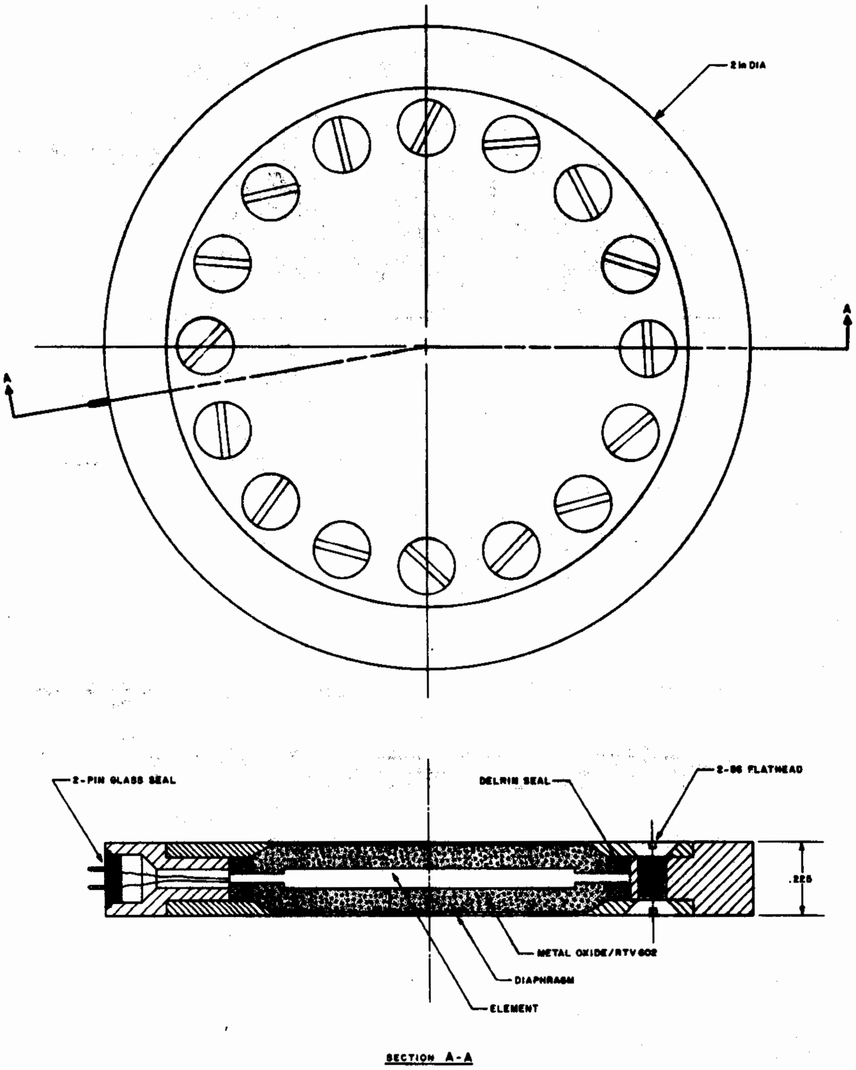


Figure 8. Stress Gage Package Construction Detail

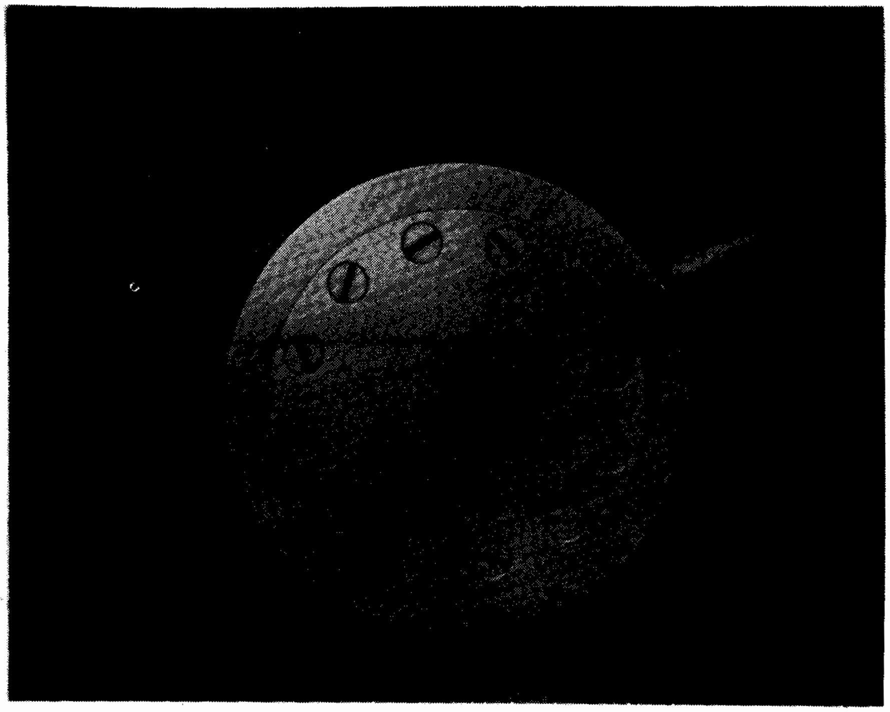


Figure 9. Photograph of Piezoelectric Soil Stress Gage Fabricated by PCB Piezotronics, Inc.

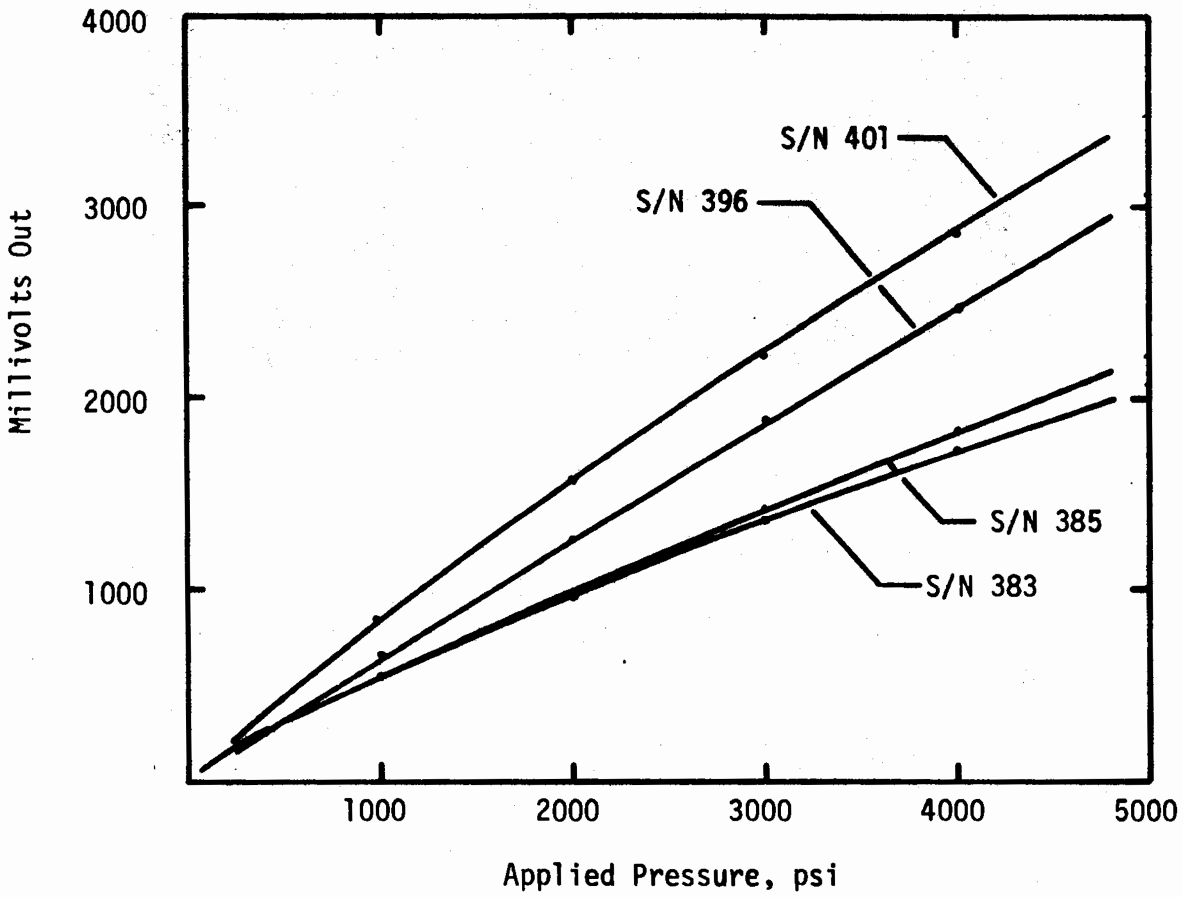


Figure 10. PCB Factory Calibration of
Piezoelectric Soil Stress Gages

TABLE 1

PCB SOIL STRESS GAGE DESIGN PARAMETERS

Gage S/N	Diaphragm Thickness (inch)	Filler Materiel
383	.010	Al_2O_3
385	.015	Al_2O_3
396	.010	WO_2
401	.015	Al_2O_3

diaphragm, thus there is no trend which might indicate the factors which affect linearity. One possible cause of non-linearity which will be investigated is bubbles in the coupling material. There has been no attempt at this time to understand or predict the non-linearity in the gage response. This will be an area for both theoretical and experimental investigation in the future.

S/N 401 was initially intended to be filled with WO_2, however, after the trouble experienced with filling S/N 396, the decision was made to use Al_2O_3.

A WES gage calibrator was utilized for initial calibrations after receipt of the gages from PCB. Considerable difficulties were encountered because of the fragile cable/gage hookup. Two gages were damaged during these simple tests and it was concluded that it would be impractical to continue the laboratory tests. Therefore, all laboratory testing was discontinued and the gages returned to PCB for repair and modification of the cable exit design. Upon receipt of the repaired gages, laboratory testing will continue. These tests will include characterization of the response of the gage to bending and shear loads, acceleration inputs, shock loading and hydrostatic calibrations to 15,000 psi. At the appropriate time, gages will be fielded in high explosive field tests.

CONCLUSIONS

A new approach to packaging of piezoelectric sensors in soil stress measuring applications has been developed. An analytical approach to the gage design has been presented which hopefully will lead to a more comprehensive understanding of the gage response. Four gages have been fabricated but minor deficiencies have been identified which must be corrected prior to continuation of the test and evaluation program. Non-linearity in the response of the gages has been observed and must be investigated to identify design factors which influence linearity.

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26-28 April 1977, Fort Walton Beach Florida

A SYSTEM FOR MEASUREMENT OF FREE-FIELD STRESS WAVES USING LITHIUM NIOBATE PIEZOELECTRIC TRANSDUCERS *

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ABSTRACT

Single crystal lithium niobate is a commercially new transducer material for stress gages useful in the measurement of stress waves in geologic media. Peak stresses of interest range from a few bars to many kilobars; waveform resolution to fractional milliseconds and for hundreds of milliseconds is required. The paper describes properties and theory of application of lithium niobate to charge-mode stress measurement, features of several gages designed for different applications, and a special piezoelectric signal conditioning unit for field operation. Some examples of stress records obtained in field tests are presented.

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A SYSTEM FOR MEASUREMENT OF FREE-FIELD STRESS WAVES USING LITHIUM NIOBATE PIEZOELECTRIC TRANSDUCERS

R. P. Reed

INTRODUCTION

Propagating stress wave phenomena in geologic media are of interest in such diverse areas as explosive containment or cratering, survival or destruction of explosively-loaded earth-buried structures, earth penetration by high velocity projectiles, and rubblization of coal and oil shale in energy recovery studies. Media range from dry, powdery alluvium to coal and kerogen-rich oil shale. Peak stresses of interest range from the order of a few bars to many kilobars. Waveform resolution to fractional milliseconds for durations of hundreds of milliseconds is required.

Single crystal lithium niobate provides a newly available transducer material for stress gages useful in such wave propagation studies. (Crystalline quartz and tourmaline and ceramic $BaTiO_3$ and PZT have previously been used). The material is sensitive, reproducible, versatile, well characterized, and readily available from several sources.[1] It is employed in a circuit for which the piezoelectric charge generated is proportional to stress.

This paper describes: 1) electro-physical properties of lithium niobate, 2) the relationships between charge output, stress, and temperature for the crystal-axis-normal wafer cuts, 3) the ruggedized and modularized piezoelectric signal conditioning unit (PESCU) developed for field use with the lithium niobate and other piezoelectric devices, 4) the special system calibration cycle applied by the PESCU to validate measurement system integrity, 5) design features of several gage packages designed for various applications, and 6) some results of field experiments using the system to monitor stress waves from explosive sources.

PROPERTIES OF LITHIUM NIOBATE

Lithium niobate ($LiNbO_3$) for stress wave transducers is a crystalline material of trigonal class 3m having three-fold symmetry about its polar or optic axis. The material is commercially grown as boules pulled from a melt by the Czochralski method. Boules of more than 6 cm diameter and 15 cm length are grown in present day production. Boules are commonly grown along the polar axis (variously designated as the z, 3, or c axis by different conventions)[2] although growth along transverse or oblique axes is also commercially performed. Transducer wafers are cut from the boule and are oriented by x-ray diffraction.

Lithium niobate is ferroelectric; it has a unique polar axis and lacks a center of symmetry so it is also pyroelectric. Piezoelectric properties are introduced by polarization, subjecting the boule to an electric field of about 5 volts per cm along the polar axis while the temperature is decreased from slightly above to slightly below the Curie Point. The exceptionally high Curie temperature (~1210°C for stoichiometric material) makes poling easy and assures that polarization of the single crystal material is uniform and very stable at room temperature. It does not depole with age or use. The piezoelectric property of transducer grade material has been shown to be very reproducible with less than 1% variation between boules of the same manufacturer and between several manufacturers.[3]

Transducer elements for stress wave measurements are typically of thin wafer form with opposite faces electroded. The element produces a charge in response to a change of temperature or stresses. The electric displacement (charge density) for a homogeneously stressed disk electroded on a face normal to a crystallographic direction is[4]

$$D_i = d_{ij} T_j + \epsilon_{ik}^{T} E_k + \pi_i \theta , \qquad (1)$$

where:

D_i = electric displacement on the electroded axis, C/m^2,

d_{ij} = piezoelectric strain coefficient, C/N,

T_j = mechanical stress component, N/m^2, *

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* The concise Brugger notation for stress employed in piezoelectric analysis designates components as $T_1 = \sigma_{xx}$, $T_2 = \sigma_{yy}$, $T_3 = \sigma_{zz}$, $T_4 = \tau_{yz}$, $T_5 = \tau_{xz}$, $T_6 = \tau_{xy}$. Also, for subscripts, i,1 = x, 2 = y, 3 = z.

ϵ_{ik}^{T} = dielectric permittivity (constant stress), F/m,

E_k = electric field, V/m,

π_i = pyroelectric coefficient, C/m^{2}K, and ($\pi_1 = \pi_2 = 0$)

θ = temperature change, K.

In transient application, a variation of charge is measured in response to changes in the forcing variables. The usual reference state is with the element charge-free at ambient conditions with stress near zero. For free-field stress measurement, no electric field is imposed and temperature is presumed to be essentially constant over the brief interval of measurement. Under these conditions, the displacement reduces to

$$D_i = d_{ij} T_j . \tag{2}$$

For all trigonal crystals of class 3m (eg: lithium niobate, lithium tantalate, tourmaline) the array of piezoelectric strain coefficients which relate charge to stress is[4]

$$\begin{vmatrix} 0 & 0 & 0 & 0 & d_{15} & -2d_{22} \\ -d_{22} & d_{22} & 0 & d_{15} & 0 & 0 \\ d_{31} & d_{31} & d_{33} & 0 & 0 & 0 \end{vmatrix} . \tag{3}$$

Thus, for the three axis-normal disk orientations,

$$D_1 = d_{15} T_5 - 2 d_{22} T_6 , \tag{4}$$

$$D_2 = - d_{22} T_1 + d_{22} T_2 + d_{15} T_4 , \text{ and} \tag{5}$$

$$D_3 = d_{31} T_1 + d_{31} T_2 + d_{33} T_3 . \tag{6}$$

The x-cut can be used only as a shear mode element, responsive both to shear stresses T_5 and T_6. The y-cut responds to stress T_2 normal to the disk face but also to transverse normal stress T_1 and shear stress T_4. The z-cut responds to T_3 normal to the disk face but also to transverse normal stresses T_1 and T_2; it has no shear stress response. The transverse and shear stress components should be recognized as potential noise sources in stress measurement. Transducer elements must be isolated from extraneous stresses.

If the disk element is uniformly loaded by a hydrostatic pressure, p, then $T_1 = T_2 = T_3 = -p$ * and $T_4 = T_5 = T_6 = 0$. Clearly, neither x-cut nor y-cut elements have charge response under hydrostatic stress. For the z-cut element

$$D_3 = -(d_{33} + 2\ d_{31})\ p$$

and the hydrostatic piezoelectric strain coefficient is

$$d_h = (d_{33} + 2\ d_{31}). \qquad (7)$$

since 1965, several determinations of the values of the low stress linear piezoelectric and pyroelectric coefficients for lithium niobate have been reported.[5-11] The d_{22} and d_{15} coefficients appear, from generally good agreement between several reports, to be well known. The d_h first and second degree coefficients for z-cut material have been very well established by Graham. The normal and transverse coefficients, d_{33} and d_{31}, for z-cut material are known less accurately. First degree values presently being used for data reduction in measurement activities are summarized below. An estimate of the accuracy of each value, based on the internal mean of the range of reported values,[5-11] is also given.

Piezoelectric Strain Coefficients for $LiNbO_3$, pC/N

d_{15}	d_{22}	d_{31}	d_{33}	d_h
70.4	20.8	-0.2	6.2	6.31
±5%	±2%	±500%	+30, -0%	±1%

Reproducibility of lithium niobate properties is better than that of polycrystalline stress gage materials. Accuracy of most linear characterization is greater than for transducer materials other than quartz. The important d_{33} coefficient requires accurate verification but the uncertainty indicated is believed to be pessimistic. The relative uncertainty of d_{31} is very large but also of small consequence. Some other physical properties important in stress gage application are tabulated below:

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* By convention, hydrostatic pressure, p, is positive, but compressive stress is negative, and tensile stress is positive. Positive coefficients correspond to output of negative polarity under compression.

Some Physical Properties of $LiNbO_3$ at 20^oC[5-11]

Density, kg/m^3	4.64
Sound speed, c_2, m/s	6838
Sound speed, c_3, m/s	7331
Melting point, T_m, ^oC	1253
Curie point, T_c, ^oC	~1210
Coefficients of expansion, $^oC^{-1}$	
α_x	16.7×10^{-6}
α_y	2.0×10^{-6}
α_z	3.18×10^{-6}
Relative dielectric permittivity	
$\epsilon^T_{22}/\epsilon_o$	84
$\epsilon^T_{33}/\epsilon_o$	30
Resistivity, Ω-cm	$>10^{11}$
Pyroelectric coefficient,	
π_3, $\mu C/m^2K$	-40
Elastic constants, N/m^2	
C_{11}	2.03×10^{11}
C_{12}	0.53×10^{11}
C_{13}	0.75×10^{11}
C_{22}	2.03×10^{11}
C_{33}	2.36×10^{11}

PIEZOELECTRIC OUTPUT

For piezoelectric measurement of abrupt(submicrosecond risetime) shock wave stresses, pulse durations must be short compared to the stress transit time through the transducer element; very thick elements are used. Charge response is proportional to the difference between stresses at the opposite electrodes. The gage element is terminated in a small resistance so that recording is in the "short-circuit" or current mode. Risetimes of submicrosecond order are easily resolved but durations are limited to a few microseconds and the stress field is in a state of one-dimensional strain.

In contrast, for in situ free-field stress wave measurements in geologic media, which we discuss here, the mechanical loading has risetimes of multimicrosecond order and persists for up to hundreds of milliseconds. The element is in constantly varying equilibrium with the surrounding medium. Recording must be in an "open-circuit" or charge mode. The sensor is commonly separated from the recorder by distances up to several hundred metres. Stress amplitudes of interest range from a few bars to tens of kilobars.

The equivalent circuit for the charge mode measurement system is shown in Fig. 1. Charge generated by stress of the transducer element, having an electrode area, A, is promptly distributed over the total capacitance of the circuit. It appears as a potential at the high impedence input of the signal conditioning amplifier. That potential is:

$$V_i = \frac{D_i A}{C_t}$$

or, from Eqn. 2,

$$V_i = \left(\frac{A d_{ij}}{C_t}\right) T_j . \qquad (8)$$

For measurement, the transducer element must be isolated from extraneous stresses and any temperature changes. Subject to these conditions, Eqn. 8 specializes to three particular cases for stress measurement using gage elements of the crystal class of lithium niobate:

Normal stress component, σ_n, y-cut crystal,

$$V_2 = \frac{A d_{33} \sigma_n}{C_t} ; \qquad (9)$$

Normal stress component, σ_n, z-cut crystal,

$$V_3 = \frac{A d_{33} \sigma_n}{C_t} ; \text{ and} \qquad (10)$$

Pressure, p, z-cut crystal,

$$V_3 = - \frac{A d_h P}{C_t} . \qquad (11)$$

The total circuit capacity, C_t, includes the charge generator capacity C_g, the distributed cable capacity C_c, and the shunt terminating capacity C_s.

Disk capacity is usually a small fraction (commonly $<1\%$) of total capacity in field applications. The lumped capacitor, C_S, is used as a standardizing capacitor to attenuate the peak pulse amplitude to a convenient value for recording. (The open circuit potential of a 2.4 mm thick z-cut disk of $5 cm^2$ area is 7.6 volts per bar.) Fig. 1 indicates typical ranges of circuit component resistances and capacitances and stray shunt resistance, R_S, experienced in field operation.

SIGNAL CONDITIONING

The piezoelectric measurement of stress wave phenomena can be complicated by induced electrical noise. The charge-mode signal conditioning requires operation into a very high input impedance. Spurious charge can be induced in the cable as well as the sensor by pyro-, piezo-, and triboelectric effects resulting from heating, crushing, and gross motion. To avoid this, miniaturized piezoelectric transducers are commonly equipped with impedance converters adjacent to the gage package. Because of the large signals available from the stress gages, the present system does not use that approach. Rather, the charge signal is conveyed to a remote signal conditioning package protected from severe stress loads and accessible for inspection, repair, and retrieval to the time of the experiment.

In some experiments the output from dummy cable systems has been recorded to document cable noise. No significant noise problems from the cable source have been experienced. To the contrary, signal-to-noise ratios have been very high.

The complete measurement system for free field stress measurement is shown schematically in Fig. 2. Stress gages of various designs are connected with cable, such as type RG22B/U, to a remote PiezoElectric Signal Conditioning Unit (PESCU). Experiment conditions sometimes require that the PESCU be located more than 300 m from the gages. Recording is performed on multichannel magnetic tape units in a recording trailer which is typically 300 to 1500 metres from the PESCU. The PESCU output signal is conveyed through multiple shielded-twisted-pair cable.

Signal conditioning consists of an operational amplifier configured as a voltage follower rather than as an integrating charge amplifier, Fig. 3. The voltage follower has proved to be stable and drift-free with open input under field conditions over periods far exceeding the time constant of the input system. Input impedance of the amplifier is typically greater than 10^{12} ohms. An additional line driver stage allows the use of output lines of several hundred metres length. The PESCU is effectively a unity gain amplifier of broad bandwidth which accomplishes high to low impedance conversion. Step response of the system allows observing pulses of a few microseconds risetime. The amplifier and associated calibrating-shorting-enabling relays and a calibrate voltage regulator for a single sensor input are provided on a printed circuit plug-in card. Ten isolated channels of input are provided in the standard PESCU. In the event of malfunction, any amplifier section is quickly replaceable in the field.

Separate channels are powered by individual DC-DC power supplies to assure isolation and avoid cross-talk in the event that one or more sensors or cables short to earth during the experiment. Operational power is furnished by rechargeable gel-cel batteries located at the PESCU package. Energy is available for at least six hours of continuous operation. There are provisions for recharging the batteries and for completely discharging them from a remote location where access to the PESCU is restricted by experiment operations.

DIAGNOSTIC CALIBRATION OF THE SYSTEM

A diagnostic sequence in the PESCU can be cycled remotely and automatically during check-out. It is cycled automatically on all channels about two minutes prior to experiment time to document circuit integrity. Each input card has a selectable calibrate voltage (0.5 v or 2.0 v).

Events of the calibrate cycle are sequenced by an electronic timer card in the PESCU. The cycle is illustrated in Fig. 4. The usual pre-experiment rest state of the PESCU has every amplifier input (and gage output) shorted. Initiation of the calibrate cycle applies a short to initialize the system to a charge-free state (1). Following a discharge period of a few seconds from initiation of the cycle, the short is opened and the calibrate voltage is applied to the input (2). This charges the input cable and gage and accurately establishes the overall system gain from amplifier input through the recorder (including transmission line losses). Several seconds later the calibrate voltage is removed without shorting the input (3). For a period of about 30 seconds the circuit is allowed to drift while any anomalous deviation from the original charged level is recorded. The drift period is terminated by reapplying the calibrate voltage for a few seconds (4). If the measurement system is in typical operational condition the drift will be, at most, a few percent of the initial value; usually it is undetectable. Decay of amplitude is usually diagnostic of excessive resistive shunting, R_s, due to gage or cable damage in installation. Increasing or irregularly varying amplitude commonly indicates a faulty amplifier component (stress and temperature being constant). As recording intervals are usually of the order of only 100 ms an exponential decay of a few percent over thirty seconds is inconsequential. In a few instances, large drifts, some exceeding the calibrate voltage, have detected faulty components, leaky cables,or gages damaged by emplacement. This allowed correction of accessible defects, reassignment of recording channels from inoperative gages, and explanation of distorted or noisy waveforms for marginal installations.

At point 5, the automatic calibrate cycle is terminated as the calibrate voltage is removed and the amplifier input is shorted for a period to discharge the input cables and sensors.

Four methods of enabling the system for recording are provided to accommodate different field experiment operations. Three are provided by different versions of printed-circuit plug-in calibrate cycle cards in the PESCU. The Manual Cycle Card allows local manual switch control, at the PESCU, of each operation in the calibrate cycle. This mode is convenient for use in system checks. The same manual control is available remotely by cable extension to the recording site.

Two automatic cycle cards differ principally in the enable operation. The Timed Enable Card provides an automatic unshorting signal timed to open the input at a synchronized interval (usually about 10 seconds) before experiment signal arrival time. The Pulsed Enable Card leaves the input shorted until an external enable pulse of greater than 10 volt amplitude and 5 microsecond duration is received (6). The input is open following a relay switching delay of about 500 microsecond duration from the enabling pulse (7). The pulsed enable mode is used where large noise signals precede the event by a predictable delay as in most explosive experiments.

GAGE DESIGNS USING LITHIUM NIOBATE

Gage design must be conducted with an awareness of two classic problems common to all free-field measurements: 1) assuring that the physical quantity to be measured is not significantly changed by the sensor, and 2) assuring that the transducer element responds only to the particular quantity of interest. Fidelity of response of stress gages embedded in arbitrary geologic media is a complex topic well beyond the scope of this paper. Some aspects have been discussed in some detail.[12] The present discussion is limited to a description of a few examples of gage designs based on lithium niobate and some sample records obtained in field experiments.

The range of in situ properties possible in stress experiments in geologic media vary widely as do the conditions of installation dictated by the experiment. To further complicate the problems, stress fields usually are significantly triaxial so that sensor elements must be isolated from several extraneous stress components. The measurement task may require the transient observation of mean stress or it may require recording one or more individual components of the stress tensor. For near-surface measurements the gage package may be placed by hand but in many experiments the sensing point may be more than a hundred metres horizontally or vertically separated from the emplacement face. A variety of gage packages is required to reliably measure stress under these diverse situations.

Some commonplace guidelines for the design of dynamic free-field stress gages recognize that

1. The bulk density of the gage should approximate that of the surrounding medium,
2. The wave transit time through the gage package should be like that of the displaced medium,
3. The effective elastic modulus of the gage package should be as great as that of the host material (the registration error tends to be less for inclusions that are relatively stiff), and
4. The aspect ratio of gage diameter to thickness should be relatively large.

Following such principles, gages of two basic geometries have been designed: paddle-form and bar-form. These are intended to satisfy a variety of measurement applications.

Three variations of paddle-form gages, Fig. 5, have been fabricated. In all of them a circular disk of lithium niobate is centered in a cavity between two high strength phenolic printed circuit boards. Three styles have been used:

1. PC Style (paddle, component resolving). The faces of the sensor disk are directly loaded by normal stress on the gage face. The annulus between the sensor element and cavity wall is empty; the perimeter of the disk is free of radial stress.

2. PF Style (paddle, fluid coupled). The sensor disk is slightly separated from the case walls and is centered in the cavity. The annulus is filled with a high modulus fluid (eg: DC 710). The disk is subject only to hydrostatic pressure.
3. PH Style (paddle, hydrostatic stress field). The disk faces are directly coupled to the case and the annulus is potted with an epoxy with properties like those of the case.

The PC style uses either y-cut (d_{22} coefficient, Eqn. 9) or z-cut (d_{33} coefficient, Eqn. 10) lithium niobate material. Styles PF and PH require z-cut material (d_h coefficient, Eqn. 11). Choice between gage styles depends on the experimental conditions and measurement objective. The PC style is used where stress amplitude is sufficiently small that the free annulus can be maintained. The PF style has been used under conditions where the stress field is severely divergent and survival is difficult. Because the brittle disk is hydraulically loaded, bending is avoided. This style utilizes the most accurately characterized, d_h, piezoelectric coefficient. The stress response is dependent on the nature of the stress field and the properties of the medium. The PH style has been employed in fluids and in saturated media of low shear strength. Under well known transient fluid loading, the response of PH style gage has agreed with alternate stress measures within less than two per cent.

The other basic geometry of gage is the Type BC (Bar, component resolving) lithium niobate gage, Fig. 6. This type has been used for the recording of the radial stress component from large explosions. The gage housing is in the form of a long cylindrical bar of about 6 cm diameter. The lithium niobate element (y-cut, d_{22} coefficient, Eqn. 9; or z-cut, d_{33} coefficient, Eqn. 10) is centered in the bar with a free annulus around the disk perimeter so the radial boundary is stress free. The bar is of a material chosen to approximate the mechanical properties of the surrounding medium (eg: aluminum-filled resin is used for experiments in tuffa; an alumina-filled epoxy has been used in oil shale).

Stress coupling of the gages to the geologic medium has been done in several ways. For measurements in oil shale, tuffa, limestone, and such competent media a "rock-matching" grout, formulated and emplaced by Waterways Experiment Station, has been used. The density and acoustic impedance of indigenous core samples has been matched by the grout at shot time within a few per cent. In alluvium, and granular materials, a moisturized tamped backfill of native soil, a slurry of native soil with a weak cement binder, or sand have been used.

FIELD EXPERIENCE WITH LITHIUM NIOBATE STRESS GAGES

The true accuracy of any single in situ stress wave measurement with any kind of gage is very difficult to assess. The reproducibility of lithium niobate transducer elements is exceedingly good so elements do not require individual calibration; however, the interaction between a sensing element, its case, and an arbitrary external stressed medium are difficult to characterize in a general sense. Fidelity of gage response and survivability are very dependent on the particulars of an application; they are not unique characteristics of the gage alone.

In practice, the plausibility of a recorded in situ result is often established by the degree of agreement between alternate measures and predictions. Laboratory calibration of a gage under controlled conditions intended to simulate expected conditions of use is sometimes performed attempting to define a "registration constant" for data reduction appropriate to that calibration. Laboratory conditions are inevitably different from the time-varying boundary conditions imposed in situ. The degree of relevance of the calibration to the actual experiment is indefinite. Two-dimensional calculations of the mutual stress fields in the sensor, case, and medium conducted with parameters and loadings varied over a range can improve confidence in the scope of validity by establishing the sensitivity of the response to variations in expected conditions.

Comparison of in situ field results with wave code calculations for the experiment can also be helpful but code calculation of stress propagation for real field situations is uncertain in accuracy because material properties used can only approximate in situ properties, actual inhomogeneities of the experiment region are neglected for lack of information or impracticality of coding, and modeling of fracture behavior is imperfect or may be ignored.

A final most important tool for measurement validation is the use of adjacent gages of different types. Dissimilarities in response clearly reveal areas of uncertainty of measurement. Where agreement is obtained in many tests conducted under similar conditions of loading and material with several gaging methods and with calculated results, a degree of confidence can be established in the gage fidelity, material modeling and code calculations (Systematic coincidence should not be discounted).

Several gages of style PH have been laboratory tested in a fluid with pressure pulses of 10 ms risetime and amplitudes to over 1 kbar. The response agreed well within 2 per cent, with the response of uncased elements and with the extensive hydrostatic calibrations of Graham.[3, 10] Stress measurements from this gage type in several field experiments in water saturated soil agreed with pre-shot predictions within several per cent. Radial stress components observed in tuffa with gages of BC type have agreed with the historic range of stresses observed by other gage types in a large number of experiments under like conditions. Fidelity of all styles to arbitrary stress fields and media is not expected to approach these favorable results.

Survival of gage packages and duration of recording has varied from excellent in most experiments (even where grouted-in-place gages were re-loaded by successive experiments) to very poor in a few instances. Indicated peak stresses ranging from a few bars to more than 10 kbar have been recorded with gages of the same size and signal conditioning. In all instances where gages survived, the records were remarkably free of high frequency noise due to their high sensitivity. Failure has not been characterized by a simple stress threshold; rather, it appears to be a function of complexity of stress field. A tendency to fail in very divergent stress fields under pulses of abrupt risetime has complicated studies of response under small scale explosive test conditions which do not well simulate the typical stress fields to which the gages are exposed in actual measurement.

Examples of stress records from gages of type PH and BC are presented in Figs. 7 and 8. The records of Fig. 7 show results from two pairs of adjacent gages of two different kinds. All four records are in response to a common explosive source in an experiment on oil shale rubblization. One record of each pair was recorded by a type PH piezoelectric lithium niobate gage. The other was sensed by a paddle-type resistive ytterbium grid gage (Pulsar SP-12). Gages of set A were adjacent at 100 m depth in a water-filled 20 cm diameter well 10 m distant from the explosive. Waveforms from the two gages are substantially in agreement. The indicated peak pressure of the lithium niobate gage is 21 per cent less than that of the ytterbium gage in the fluid environment. The gages of record set B were adjacent, embedded in rock matching grout. Under this condition the peak amplitude indicated by the lithium niobate gage was 14 per cent greater than the ytterbium gage. The two waveforms agreed for the duration of the first compressive pulse; they substantially disagreed after return to baseline. A wave-code calculation* of the predicted waveform for the location of the gages indicated a peak mean stress of 0.6 kbar followed by a tensile stress of 0.3 kbar. Because the stress gages are, at most, weakly cemented to the surrounding grout and under a slight fractional kbar, compressive prestress of the grout overburden neither could fully respond to a tensile stress of the magnitude calculated. The lithium niobate record appears to be "clipped" at a tensile level of 0.03 kbar (overburden stress was about 0.02 kbar); the ytterbium gage response indicated a compressive rather than tensile stress during this interval. These two gages, as originally grouted, were loaded again on two additional shots. The general features of the waveforms of both gages were retained; however, the lithium niobate gage showed some indication of damage on the third shot.

Figure 8 presents records from two individual BC type gages installed side-by-side with rock matching grout in a 12 in. diameter horizontal borehole radial to the explosive source. The surrounding geologic material is tuffa. Both gages used z-cut lithium niobate. The reproducibility of the record for like gages is evident.

In application, the lithium niobate gages have showed usefulness for free field stress wave measurement under a diversity of field conditions. While the lithium niobate should be reliably useable without conduction to several kilobars,[3] the distinctive feature of the gage system is its ability to reproducibly record stress levels in the range below 1 kbar to a few bars with very high signal-to-noise ratio.

SUMMARY AND CONCLUSIONS

The properties and principles of application of a newly commercial piezoelectric material, lithium niobate, to the measurement of free field stress in geologic media have been presented. The transducer material offers a number of significant benefits for stress gage application:

1. Availability - Material of excellent quality is now available from several commercial sources;
2. Reproducibility - Properties of the commercial material from several vendors are uniform, stable and essentially independent of source;

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* The calculation was performed with a two-dimensional Lagrangian-Eulerian wave code by R. R. Boade.

3. Accuracy - The physical properties and two principal piezoelectric coefficient, d_{22} and d_h, are very well known. Two other important piezoelectric properties, d_{31} and d_{33}, are known adequately for present field applications and will probably be better established in the near future.
4. Sensitivity - Gages adequate for monitoring stresses of only a few bars have been employed (gages of the same kind have been used to measure stresses of many kbar amplitude). Long cable runs can be accommodated;
5. Linearity - The sensitivity is only slightly dependent on stress amplitude and exhibits no known hysteresis;
6. Versatility - Several modes of use for normal, shear, and hydrostatic stress measurement are available;
7. Thermal Range - The transducer material retains its properties to very high temperature;
8. Gage Fidelity - The high stiffness and density of the transducer element tends to simplify the design of gage packages for faithful free field stress measurement.

Unfavorable characteristics of the lithium niobate, relative to stress gage application, are shared with other materials. Like most stable piezoelectric materials lithium niobate is brittle and may fracture under intense bending or shear loading. Gage elements tend to be thick compared to resistive elements. Charge output during failure has not been well studied. Pyroelectric output of transiently heated gages does produce substantial noise output in non-isothermal applications (this problem is much less severe than for $BaTiO_3$ and PZT ceramics). Sensitivity to transverse stress is substantial for the y-cut (it may be very small for z-cut) as it is for most piezoelectric and resistive gage elements. The adverse properties must be overcome by appropriate packaging of the sensor element and application to appropriate measurement situations. The apparent disadvantage of charge mode operation has not proved to be a practical problem under even difficult installations.

The measurement system described has been demonstrated as practical and reliably fieldable in more than a dozen full scale field experiments under adverse and varied circumstances over the past three years. The amount of care required to maintain the electrical integrity of cabling between gage and PESCU is viewed as reasonable and no greater than should be applied to any important measurement. Fidelity of measurement and survival of sensors in general triaxial stress fields is believed yet to be a problem with the gages described here as with other free-field gages in present use. Continued development to assure fidelity and improve survival is required.

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Development of the lithium niobate based system was stimulated by the recognition, by R. A. Graham, of the significance in measurement of the reproducible and useful properties of this material and by his accurate characterizations of many of its properties. Fabrication details of all the sensors were developed and construction of prototypes was conducted by R. W. Morris. The PESCU package was the product of O. J. Birdsong. J. I. Greenwoll worked out the specifics of field emplacement of gages and recording of data and guided field installation in all experiments with the assistance of C. B. Kinabrew, G. W. Burnside and many others. The author appreciates the productive development efforts of this team of coworkers, typing of this manuscript by Elaine Howard, useful discussions of free-field measurement with C. W. Smith, and the support of J. W. Wistor.

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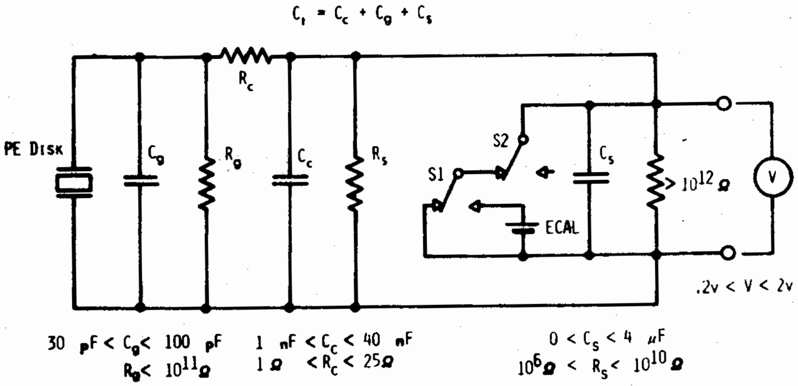

Fig. 1. Equivalent circuit of charge-mode system

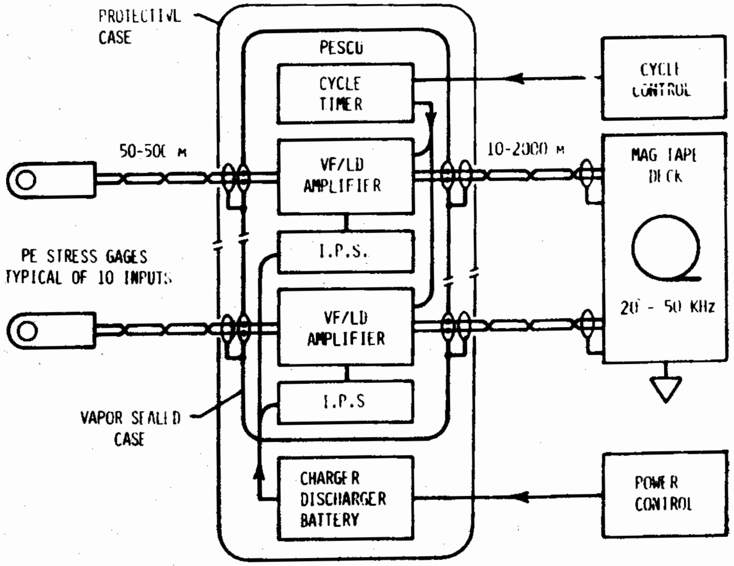

Fig. 2. Free-field stress measurement system

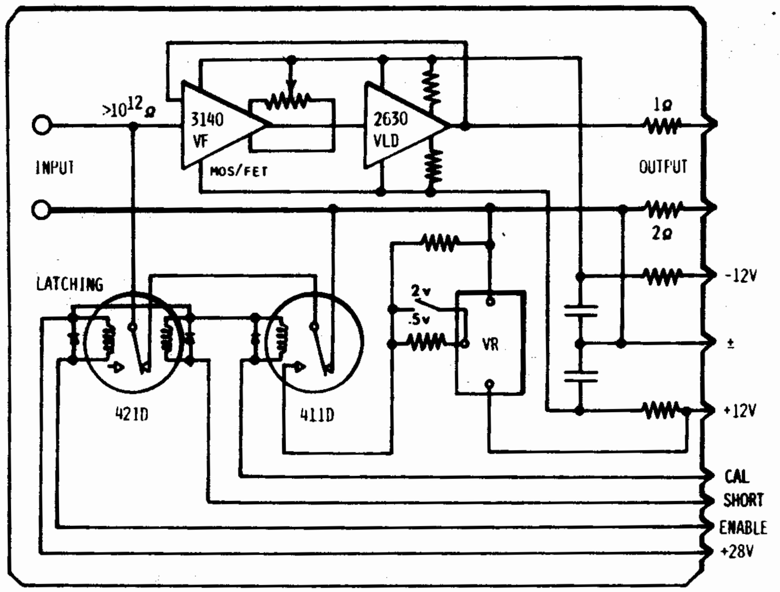

Fig. 3. Voltage follower/line driver module for piezoelectric signal conditioning

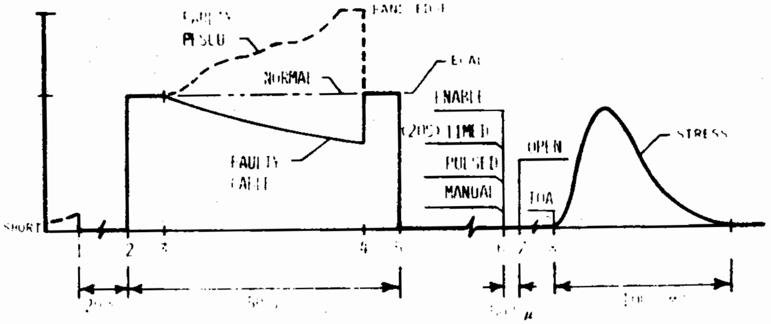

Fig. 4. Sequence for system diagnostic calibration

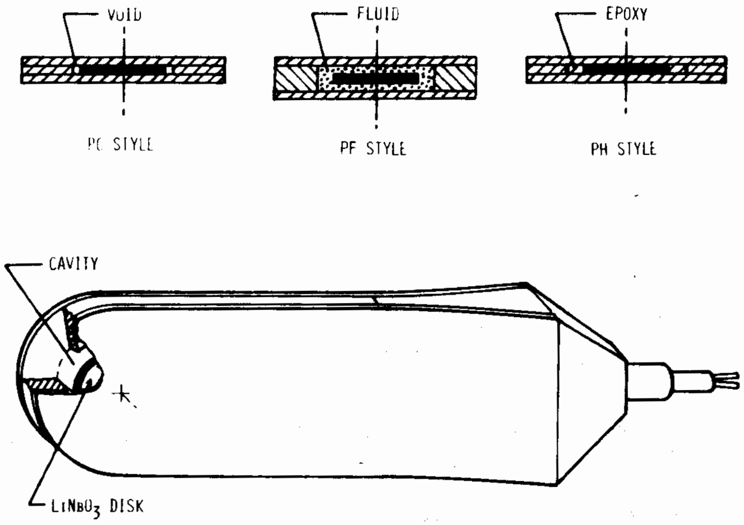

Fig. 5. Paddle-form piezoelectric stress gages

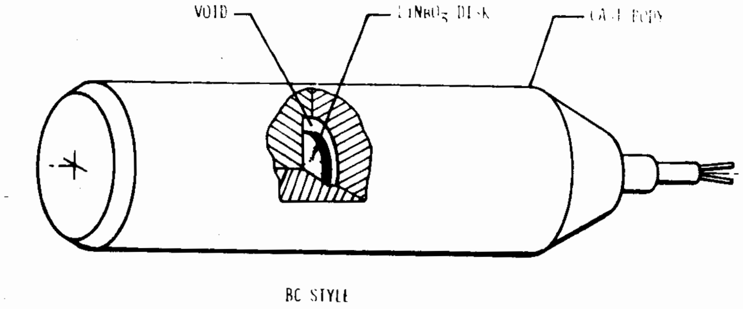

Fig. 6. Bar-form piezoelectric stress gage

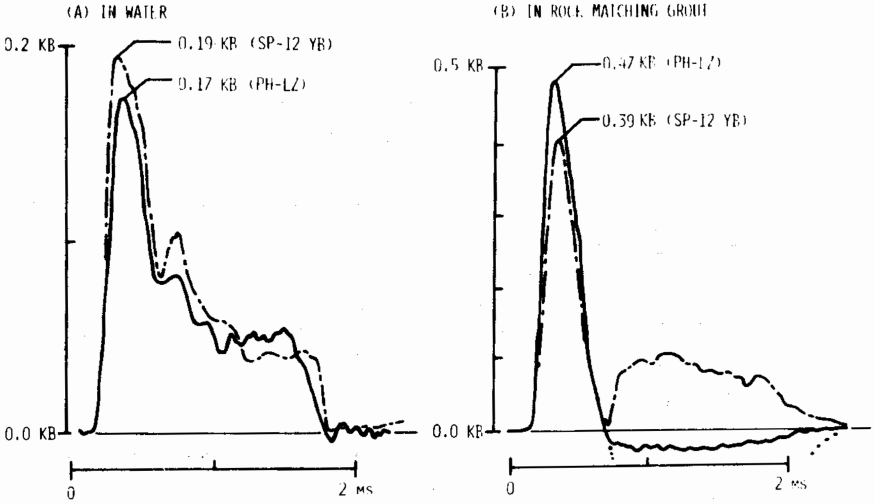

Fig. 7. Comparison between stress records from paddle-type lithium niobate and ytterbium stress gages

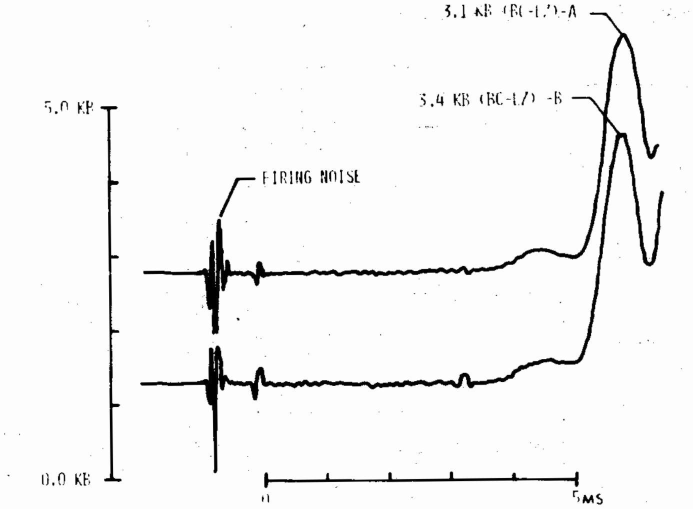

Fig. 8. Stress records from adjacent BC bar-type lithium niobate free-field stress gages

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THE INVESTIGATION OF STRAIN GAGE FORCE TRANSDUCER BEHAVIOR IN A CRYOGENIC ENVIRONMENT

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ABSTRACT

An investigation of the behavior of strain gage force balances in a cryogenic environment was conducted. The cryogenic effects on the measuring capability of conventionally designed strain gage force balances were experimentally studied. Study indicates that the balance can be used as a one percent transducer in cryogenic temperatures. A simple cooling technique for cryogenic balance calibration was devised and used successfully.

INTRODUCTION

In anticipation of future force measurement requirements in the cryogenic windtunnel of the National Transonic Facility (NTF), an investigation of the behavior of strain gage force transducers in a cryogenic environment was conducted. The cryogenic effects on the measurement capability of conventionally designed multi-component strain gage force transducers were studied experimentally. The results of laboratory calibrations as well as simple methods to achieve cryogenic temperatures for calibration are presented. The measuring capabilities of the force transducer are discussed in terms of linearity, hysteresis, interactions and sensitivities from experimental data. Study indicates that the conventional multi-component force balance can be used as a one percent transducer in cryogenic temperatures and thermal controls can be implemented as required.

CALIBRATION OF BALANCES AT LOW TEMPERATURES

The calibration results of balances HRC-01 and 2020 as well as methods to achieve low temperature environments are presented. The cryogenic balance, HRC-01, was thoroughly tested over a large temperature span; sensitivities and interactions are extracted from the experimental data. An improved and more practical cooling technique is applied successfully to the conventionally designed balance 2020.

A. Low Temperature Calibration of Balance HRC-01

Shown in figure 1 is a sketch of balance HRC-01. This balance was designed for the pilot cryogenic wind tunnel at Langley Research Center, NASA. It has five spot electrical heaters installed at various locations on the balance. This three-component (normal, pitch and axial) balance was statically calibrated at various low temperatures in the laboratory. Balance sensitivities as well as interactions are reduced from the calibrated data. Standard calibration procedures and data reduction methods used at the Instrument Research Division, Langley Research Center, are adapted from Hansen (ref. 1).

1. Test procedure. Balance HRC-01 was statically calibrated at the following temperatures: 299° K (room temperature), 255° K, 199° K, 155° K, 116° K, and 77° K (boiling temperature of liquid nitrogen). Low temperature conditions are achieved by partially submerging the balance and its loading fixture into a liquid nitrogen bath. Different temperature levels are obtained by applying various amounts of electrical power to the balance spot heaters. Data is taken only after uniform balance temperature distribution is achieved. Heaters are not used for the 77° K calibration. For room temperature calibration (299° K), neither heaters nor liquid nitrogen is used.

2. Test results. Sensitivities as well as interactions of the two remaining components, namely, normal and pitch of balance

HRC-01 at various temperatures are reduced from the calibration data.

a. Sensitivities. Figures 2 and 3 illustrate the sensitivities of normal and pitch bridge, respectively, as a function of the balance temperature. Both sensitivities are found to decrease as balance temperature is reduced. At 77° K, normal sensitivity shows a change of 4.19 percent from its room temperature value while the pitch sensitivity shows a 3.54 percent change over its room temperature value.

   The above results are compared with the data reported by Day and Sevand (ref. 2). Their data, which was obtained from a single component strain measurement formed by two strain gages mounted on a simply supported beam, are rearranged and sketched in figure 4. This time, a sensitivity decrement of 6.38 percent at 88° K is calculated. Even though the type of material, geometrical construction, and gage used in this reference are different from those used in balance HRC-01, a similar pattern of sensitivity change is observed in these three figures.

b. Linearities and hysteresis. The linearity of balance behavior at low temperatures can be examined from figures 5 and 6 where the loading curves of normal and pitch are shown. Loading curves at room temperature are also included for comparison. The degree of linearity at low temperature is seen to be inferior to those at room temperature. Hysteresis of 11 counts is observed in these two figures at 77° K. Converting the hysteresis in terms of balance full scale outputs means an 11/893 or 1.2 percent F.S. error in normal and an 11/1095 or 1.0 percent F.S. error in pitch. Furthermore, it should be noted that the laboratory calibration at 77° K indicates that the balance outputs were hunting continuously. For this reason, the data presented for 77° K is the average of a large number of samples (usually more than 30).

Hysteresis and hunting at the 116° K calibration point are less severe and the two phenomena disappear for temperatures above 155° K.

c. Interactions. Interaction data is presented in figures 7 through 10. In figure 7, the normal interaction on pitch is shown, and is found to be scattered with a maximum spread of 8 counts. At 77° K, this interaction shows a drop of 5 counts from its room temperature value. The 5-count variation corresponds to a 5/87 or 5.7 percent change of interaction. Since the full-scale pitch output is 1132 counts for an applied pitch input of 27.11 Nm, the 5-count change in normal interaction on pitch is equal to 5/1132 of .44 percent variation in the full-scale pitch output. Figure 8 shows the pitch interaction on normal. A maximum variation of 5 counts of interaction on normal is observed at 77° K. Here, the full-scale normal output is 698 counts with 667.2 Nm of normal input. Thus, the 5-count variation in pitch interaction on normal again shows a minimal effect of 5/698 or .72 percent variation in the full-scale normal output.

Figures 9 and 10 further illustrate some interesting characteristics of the balance interactions. In these figures, interactions at various temperatures are graphed together against the applied loads. For example, figure 9 shows the pitch interaction on normal at different temperature values versus the pitch moment input. Note each symbol represents interaction at a particular temperature.[1] It is seen that the interaction data is scattered randomly between two parallel boundaries. The separation of these two boundaries is 4 counts, which is consistent with the variation found in figure 7. Furthermore, the pattern shown in this figure indicates that the interaction, that is, first order pitch interaction on normal, is linear with the applied pitch moment. The slope is calculated to be -.451 ct/Nm. In terms of a mathematical relationship, it means that:

$$N_m = -.451\ m \pm\ 2.0$$

where

N_m = pitch interaction on normal, ct

m = applied pitch moment, Nm

A similar conclusion is drawn for the normal interaction on pitch shown in figure 10, that is,

$$m_N = .128\ N \pm\ 4.0$$

where

m_N = normal interaction on pitch, ct

N = applied normal force, N

From these two expressions, it is concluded that the coefficients of interactions (.127 and -.446)

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[1] Due to the proximity of interaction data, only a part of the data is plotted in these two figures for clear illustration. A complete set of interaction data is available.

remain unchanged at low temperatures. The ± 2.0 and ± 4.0 counts shown can be attributed to either experimental errors or the slight non-uniform temperature distribution in the balance.

B. Low Temperature Calibration of Balance 2020

After the specially designed cryogenic balance HRC-01 was calibrated, a conventional six-component balance 2020 was calibrated in the laboratory. Normal, pitch, and axial loads were applied successively with balance temperatures controlled at 302° K (room temperature) and 144° K. Since the balance does not have built-in heaters, a different technique was adopted to achieve the low temperature environment. Figure 11 shows a schematic diagram of the cooling system used. The balance/fixture was suspended inside a "heat exchanger" consisting of 1/4-inch copper tubes wound in a closely spaced coil. The heat exchanger is then enclosed by a styrofoam container (15 cm diam x 30 cm long) for thermal insulation. During operation, liquid nitrogen stored in the dewar is forced into the cooling coil by pressurized gas nitrogen fed into the top of the dewar. Heat transfer between the cooling coil and the balance will then lower the balance temperature. A row of injection pinholes were also drilled at the upper portion of the copper coil to allow a mixture of gas and liquid nitrogen to drip into the balance environment. This injection process was found capable of not only lowering the balance temperature but also increasing the cooling speed. With the cooling system shown, balance 2020 can be cooled to 144° K from room temperature within 30 minutes. It should be noted that once the low temperature environment is achieved, continuous regulation of the gas nitrogen pressure is needed to maintain a uniform balance temperature within experimental tolerance (± 2° K).

Calibration of balance 2020 at 144° K and 302° K is shown in figures 12 and 14 for the three components mentioned above. As before, linear input-output relationships are observed. Furthermore, sensitivities are lower at the cryogenic temperature.

The sensitivity increments are found to be 3.9 percent, 4.2

percent, and 7.5 percent respectively for normal, pitch, and axial. Interaction data was not taken for this calibration series.

C. Summary

Based on the limited amount of cryogenic balance calibrations performed in the laboratory:

1. It was found the cooling technique shown in figure 11 could be adopted for the calibration of wind tunnel balances at cryogenic conditions. Refinements such as the optimization of cooling coil configuration, flow rate control of liquid nitrogen, and insulation in general could be incorporated to improve the calibration procedure. This technique is simple to implement, convenient to use, and eliminates the need for a commercial cryostat.

2. The following measuring capabilities of the balance at cryogenic environments were observed:

   a. Linearity. Excellent linearity properties of balance sensitivity and interaction at cryogenic environments were observed.

   b. Hysteresis. This is a problem area. Errors up to 1.2 percent full-scale balance output in hysteresis were noticed.

   c. Interactions. Constant coefficients of balance interactions throughout the temperature range (229° K to 77° K) were reduced.

   d. Sensitivity. The balance sensitivities, i.e., the electric outputs over the load inputs, are found to increase as the balance temperatures are lowered. Sensitivity variations amount to a few percent over an observed 222° K temperature range.

CONCLUDING REMARKS

Cryogenic balance data obtained in the laboratory indicate that the conventionally designed strain gage force balance can be used in cryogenic temperatures; its sensitivities decrease as temperature is lowered; and its interactions and linearity are independent of temperature. Cryogenic balance calibration can be conducted routinely with a simple cooling technique in order to correct for the small changes in balance sensitivities. When sensitivity corrections are included, the conventionally designed balance can be roughly considered as a one percent transducer with hysteresis taken into account. These findings thus indicate that it is not necessary to raise the balance temperature to room temperature in wind tunnel cryogenic operations. Analysis also shows that thermal control can be applied to reduce the balance temperature differentials by installing efectrical resistance heaters at various locations on the balance (ref. 3).

Recommendations

Based on the results obtained experimentally on the balance performances and analytically on the balance heat transfer characteristics in cryogenic environments, the most appropriate areas for further work are considered to be:

1. Investigate the performances of various balance materials, strain gages, bonding agents, solders, etc., under cryogenic conditions, to establish new balance design criteria.
2. Calibrate balances of sizes comparable to those to be used in future NTF operations to fully establish the confidence level of balance performance in cryogenic environments.
3. Design an optimal thermal control system to reduce balance temperature differentials.

Acknowledgments

This research was supported by NASA under contract NAS1-11707-85. Assistance from the Force Measurement Section, Instrument Research Division, Langley Research Center, NASA, are acknowledged.

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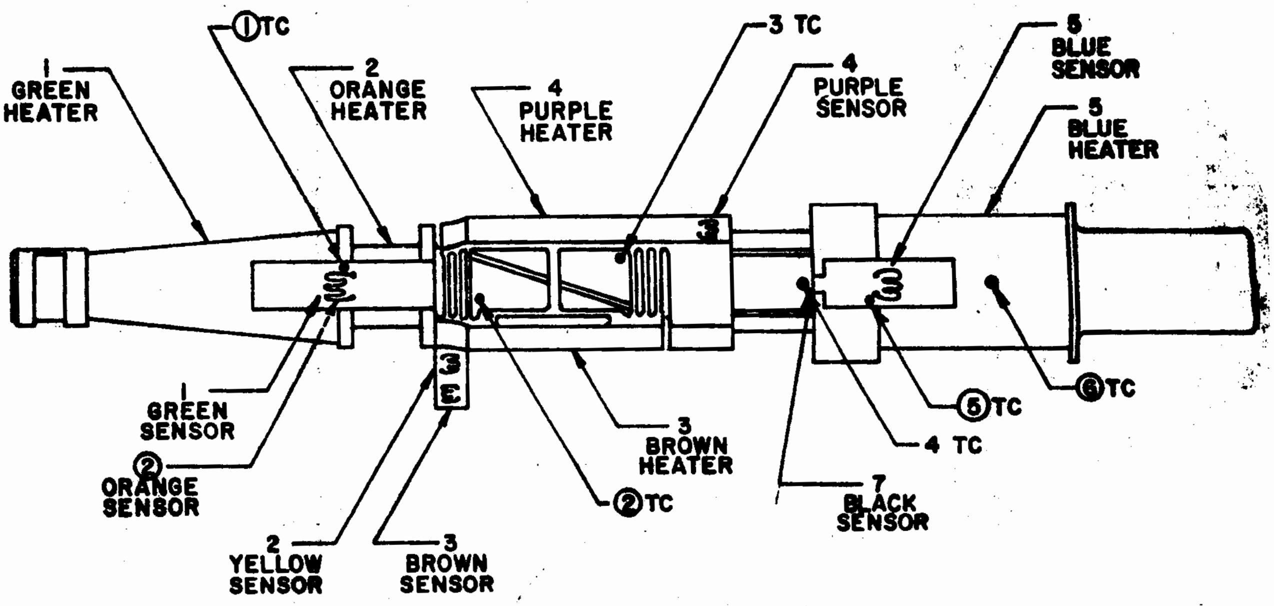
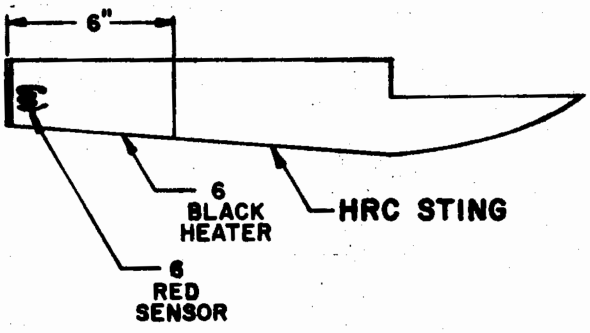
3. Tcheng, P., "The Preliminary Investigation Of Strain Gage Force Balance Behavior In A Cryogenic Environment", Technical Report 76-T12, School of Engineering, Old Dominion University. Prepared under Contract NAS1-11707-85 for Langley Research Center, NASA, June 1976.



HEATER WIRE 19/38 #26
SENSOR WIRE 7/40 #32



HEATER CODES

	BALANCE HRC-I			
	HEATER COLOR CODE	SENSOR COLOR	HEATER RESISTANCE	LEAD RESISTANCE
1	GREEN	GREEN	20.4	1.3
2	ORANGE	ORANGE/YELLOW	21.0	1.3
3	BROWN	BROWN	20.0	1.0
4	PURPLE	PURPLE	20.0	1.6
5	BLUE	BLUE/BLACK	25.0	1.4
	TUNNEL STING			
6	BLACK	RED	40.0	1.8

FIGURE I - SCHEMATIC DIAGRAM OF BALANCE HRC-OI

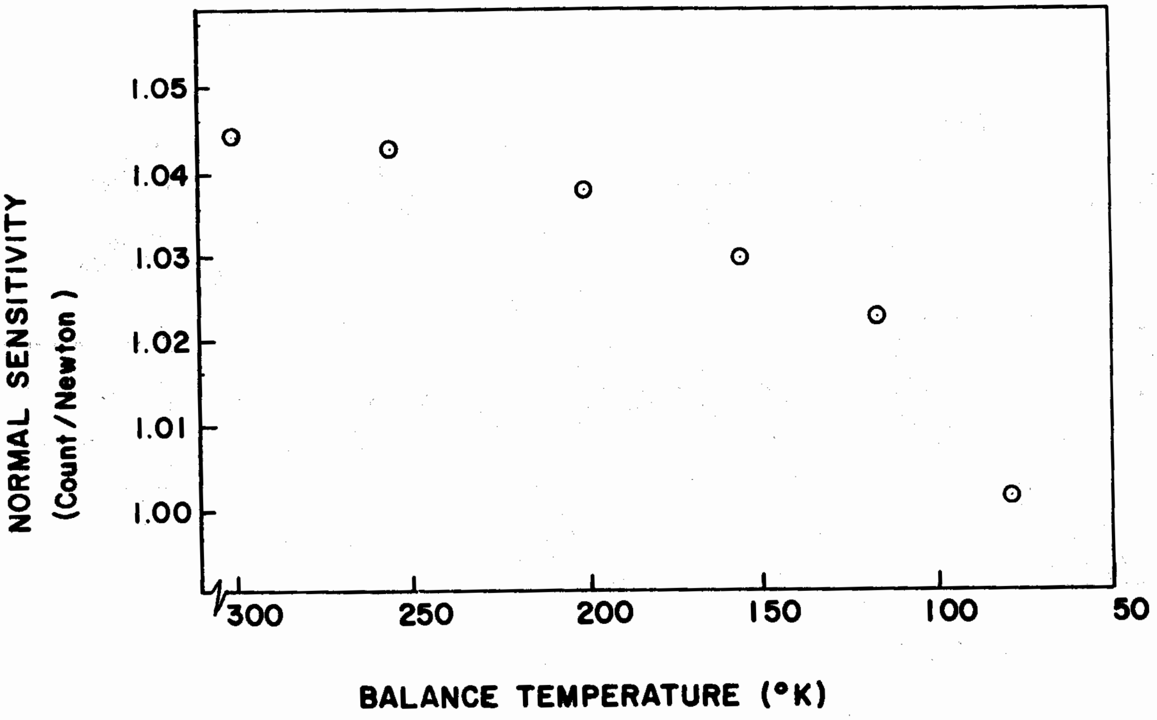


FIGURE 2 – NORMAL SENSITIVITY CHANGE WITH TEMPERATURE OF BALANCE HRC-01

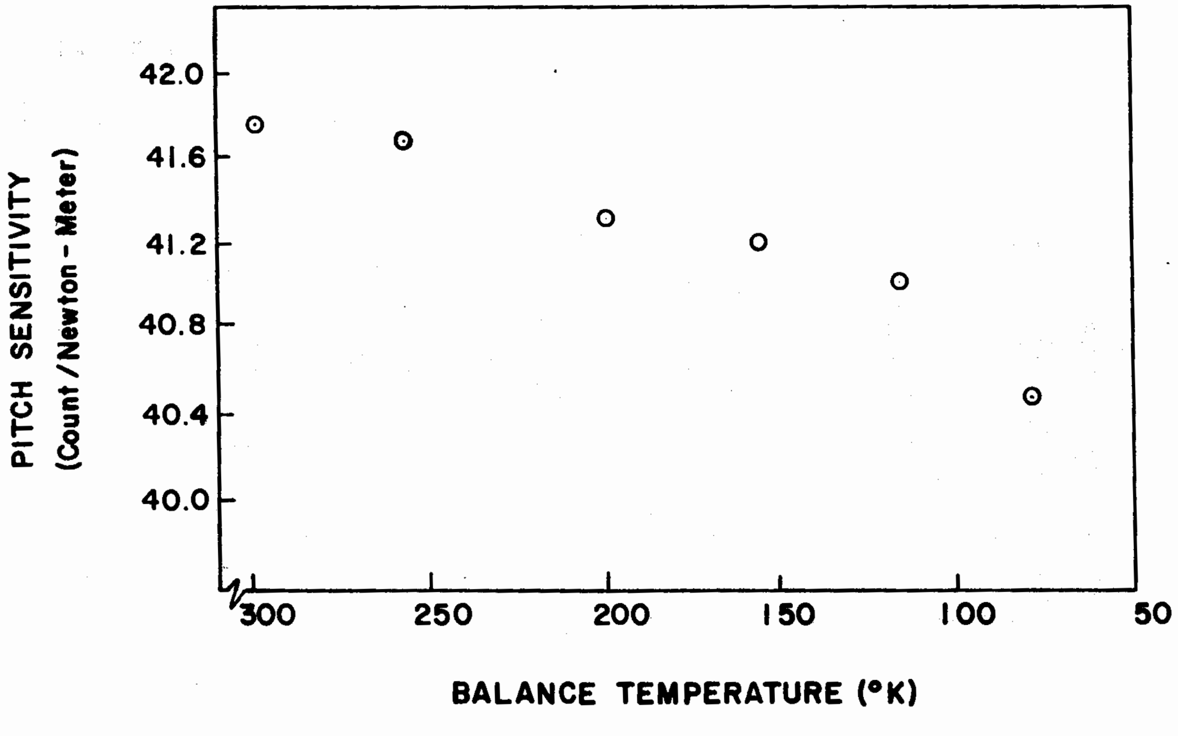


FIGURE 3 – PITCH SENSITIVITY CHANGE WITH TEMPERATURE OF BALANCE HRC-01

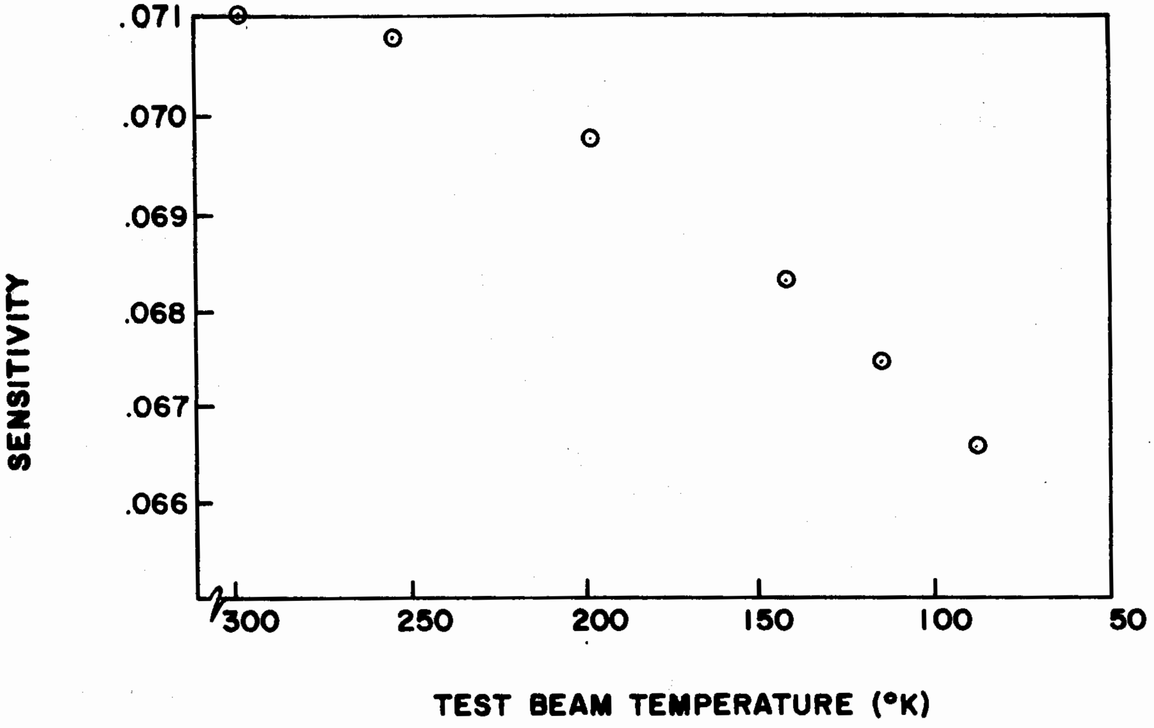


FIGURE 4 – SENSITIVITY CHANGE WITH TEMPERATURE OF TEST BEAM IN REFERENCE (11)

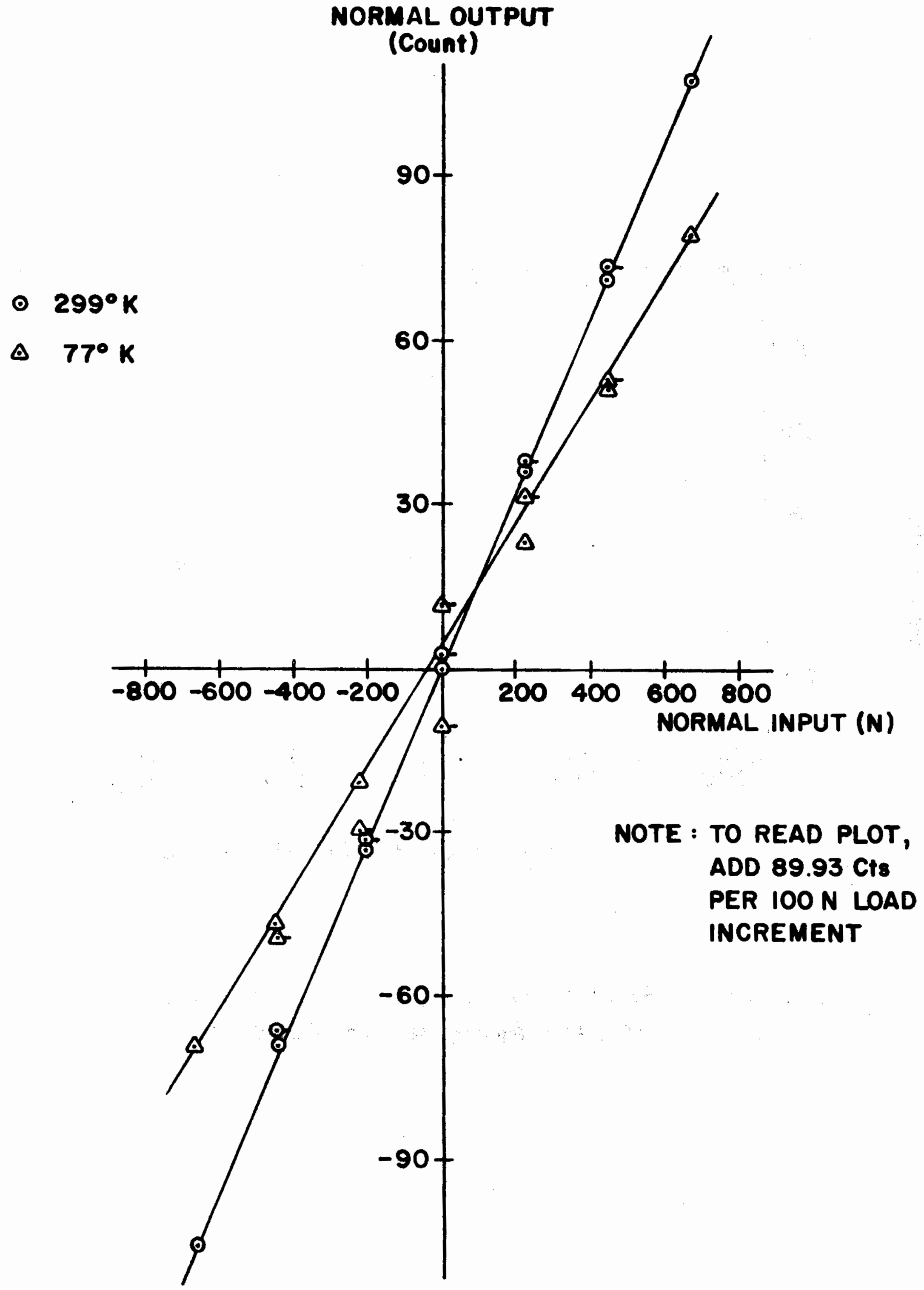
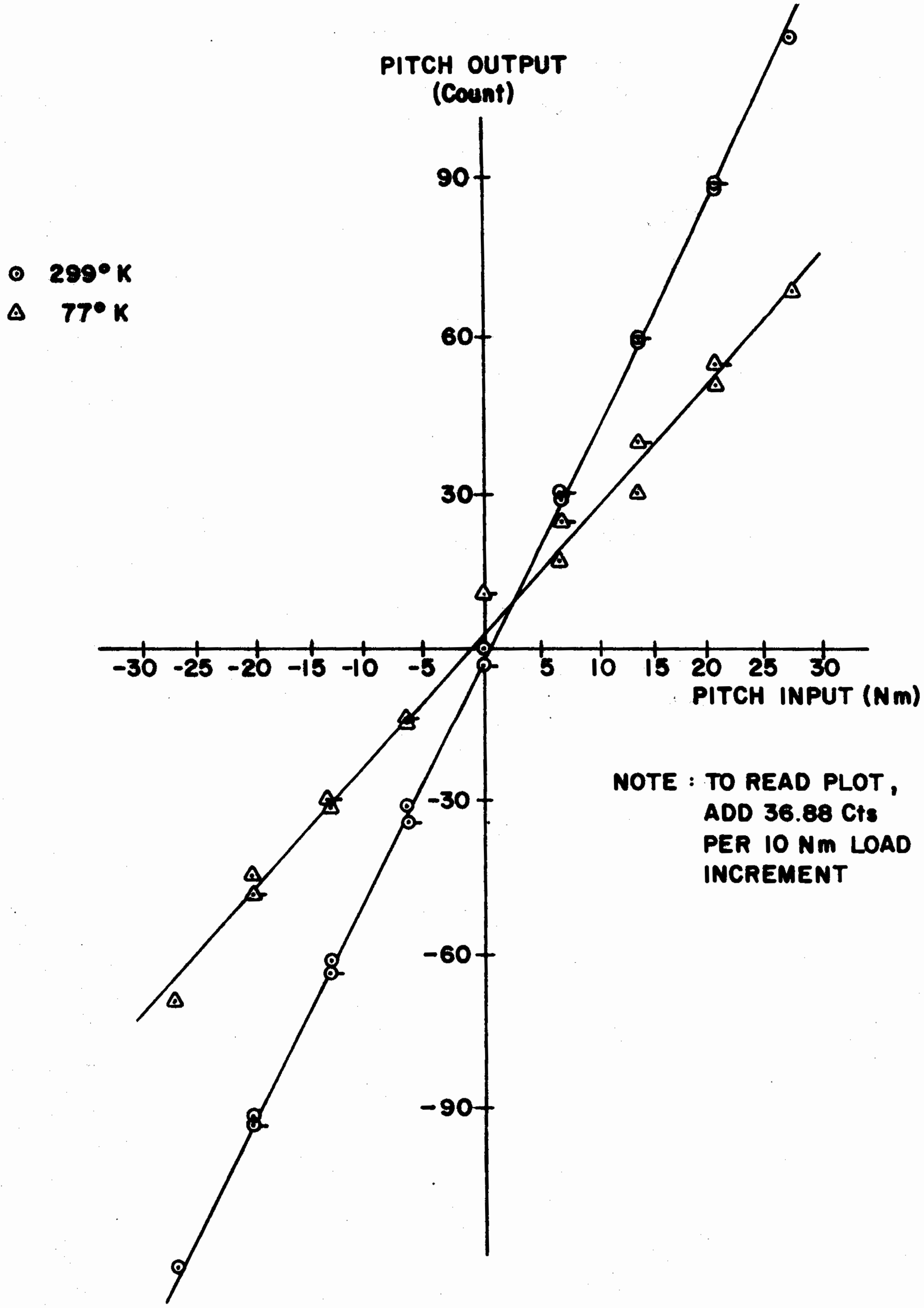


FIGURE 5 – NORMAL FORCE CALIBRATION OF BALANCE HRC-01 AT 77° K AND 299° K



NOTE : TO READ PLOT, ADD 36.88 Cts PER 10 Nm LOAD INCREMENT

FIGURE 6 - PITCH MOMENT CALIBRATION OF BALANCE HRC-01 AT 77° K AND 299° K

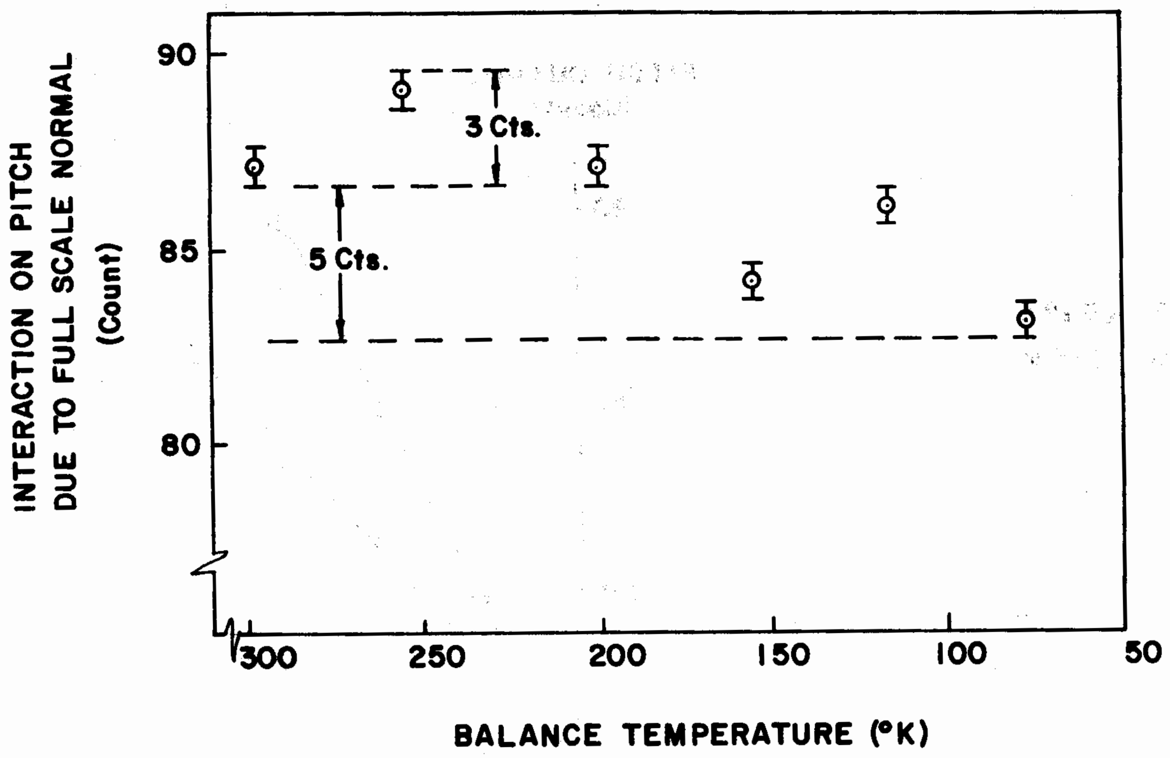


FIGURE 7 – NORMAL ON PITCH INTERACTION CHANGE WITH TEMPERATURE OF BALANCE HRC-01

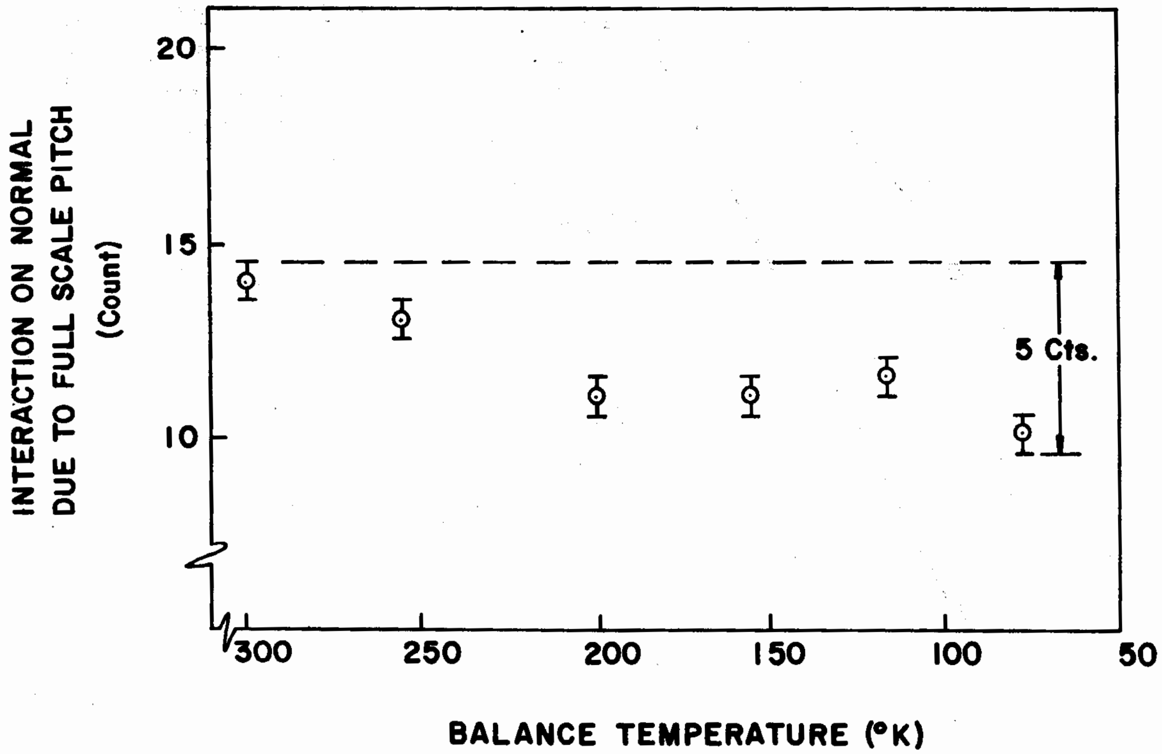


FIGURE 8 – PITCH ON NORMAL INTERACTION CHANGE WITH TEMPERATURE OF BALANCE HRC-01

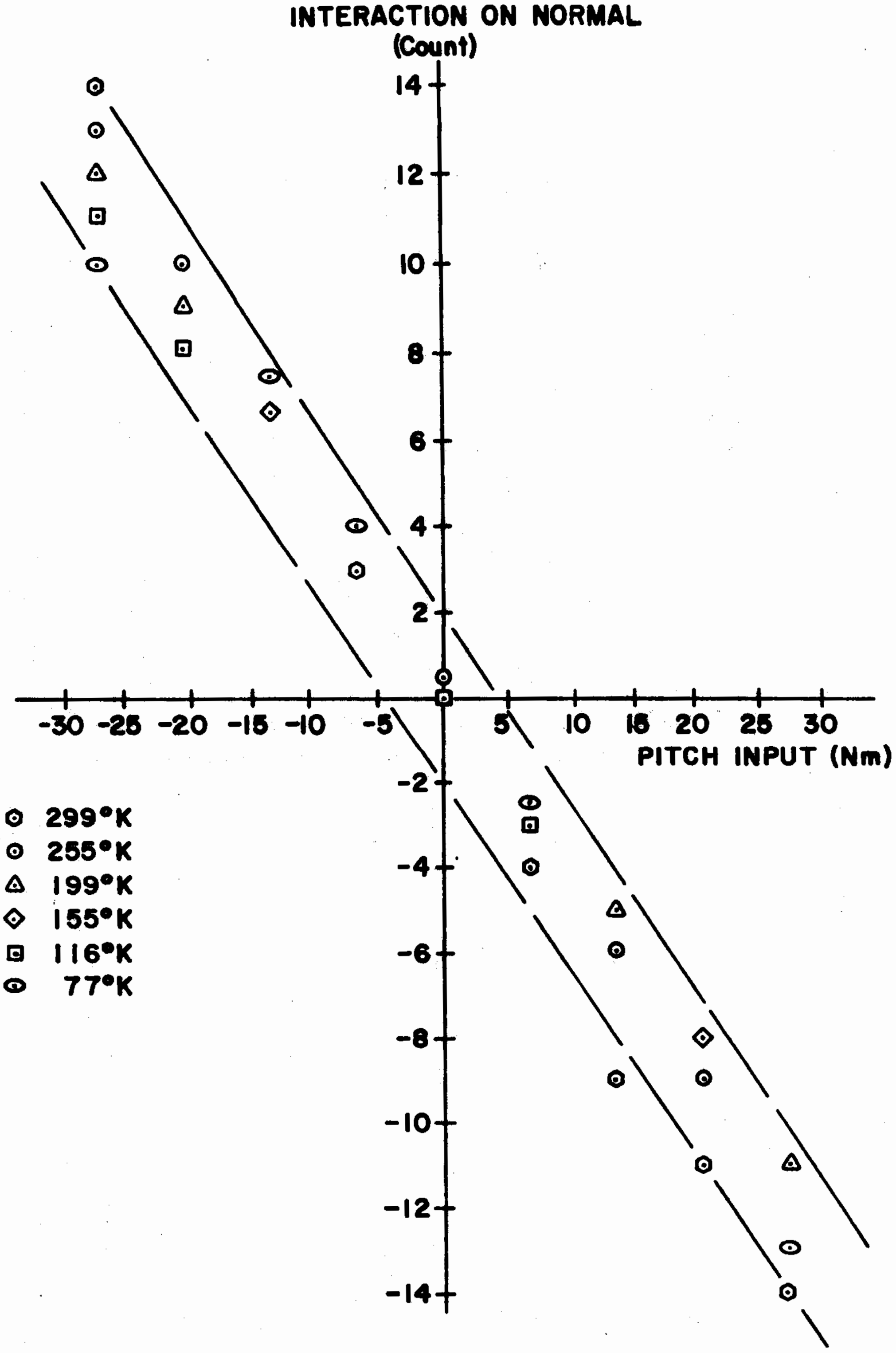


FIGURE 9 – CALIBRATION OF PITCH INTERACTION ON NORMAL OF BALANCE HRC-01 AT VARIOUS TEMPERATURES

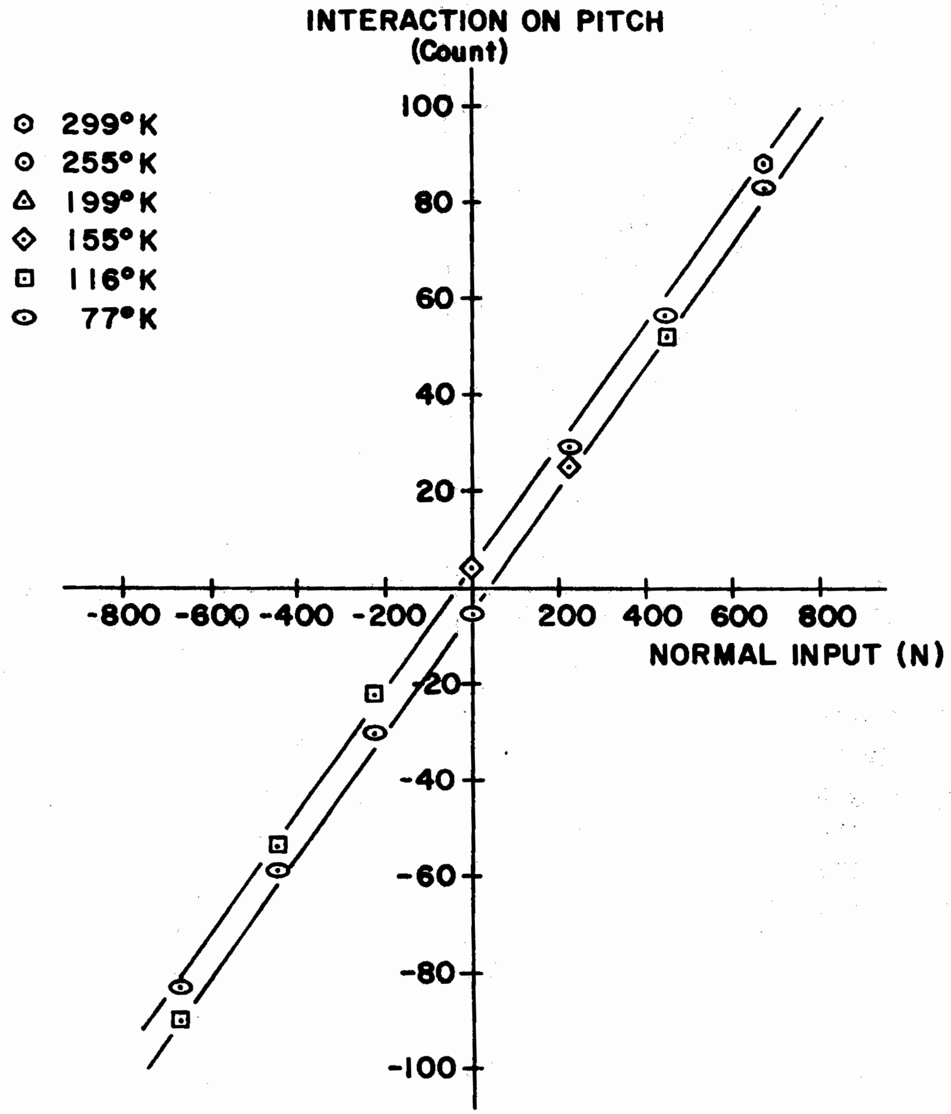


FIGURE 10 – CALIBRATION OF NORMAL INTERACTION ON PITCH OF BALANCE HRC-01 AT VARIOUS TEMPERATURES

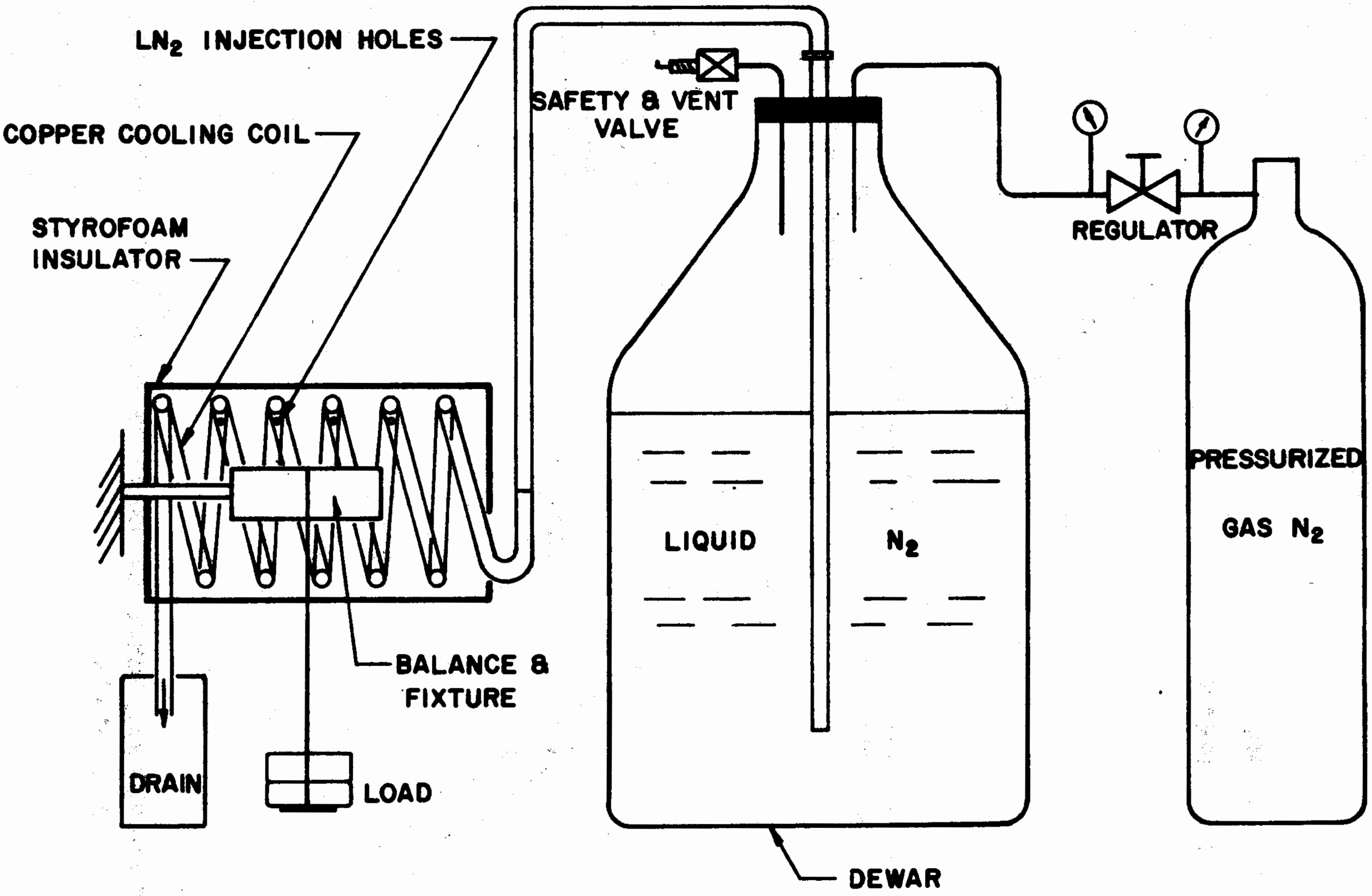


FIGURE 11 – SCHEMATIC DIAGRAM OF COOLING SYSTEM FOR CRYOGENIC BALANCE CALIBRATION

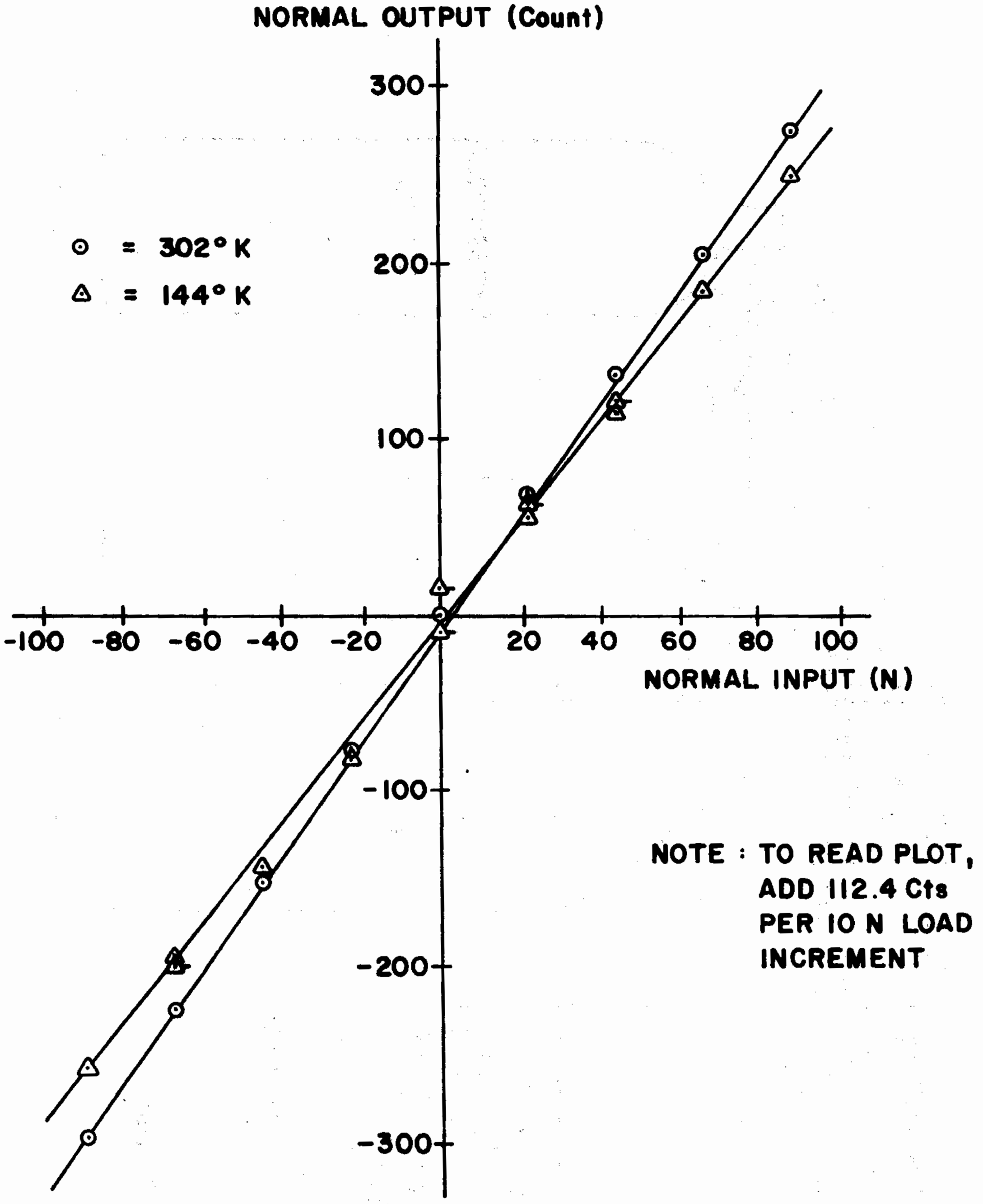


FIGURE 12- NORMAL FORCE CALIBRATION OF BALANCE 2020 AT 302°K AND 144°K

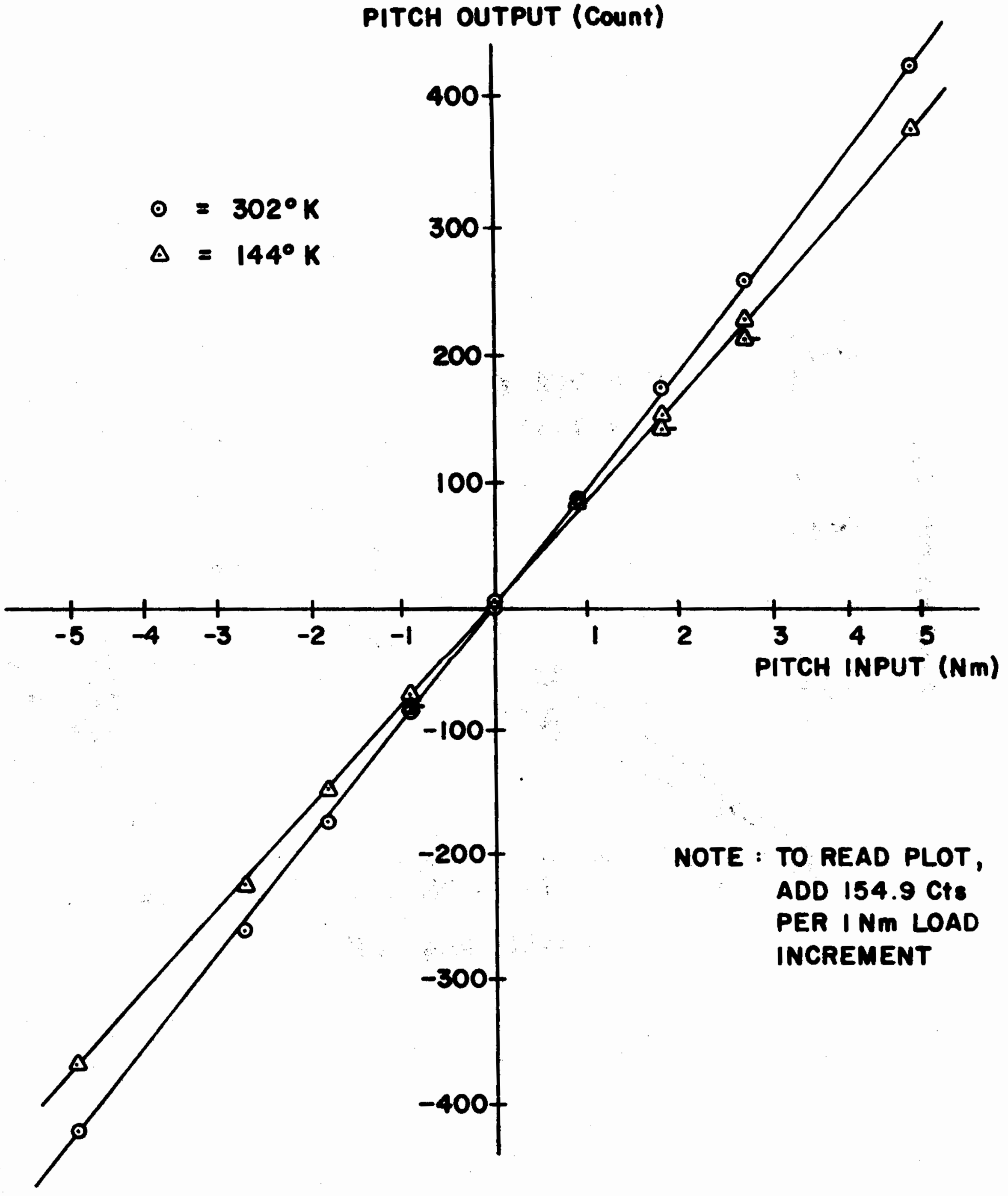


FIGURE 13- PITCH MOMENT CALIBRATION OF BALANCE 2020 AT 302° K AND 144° K

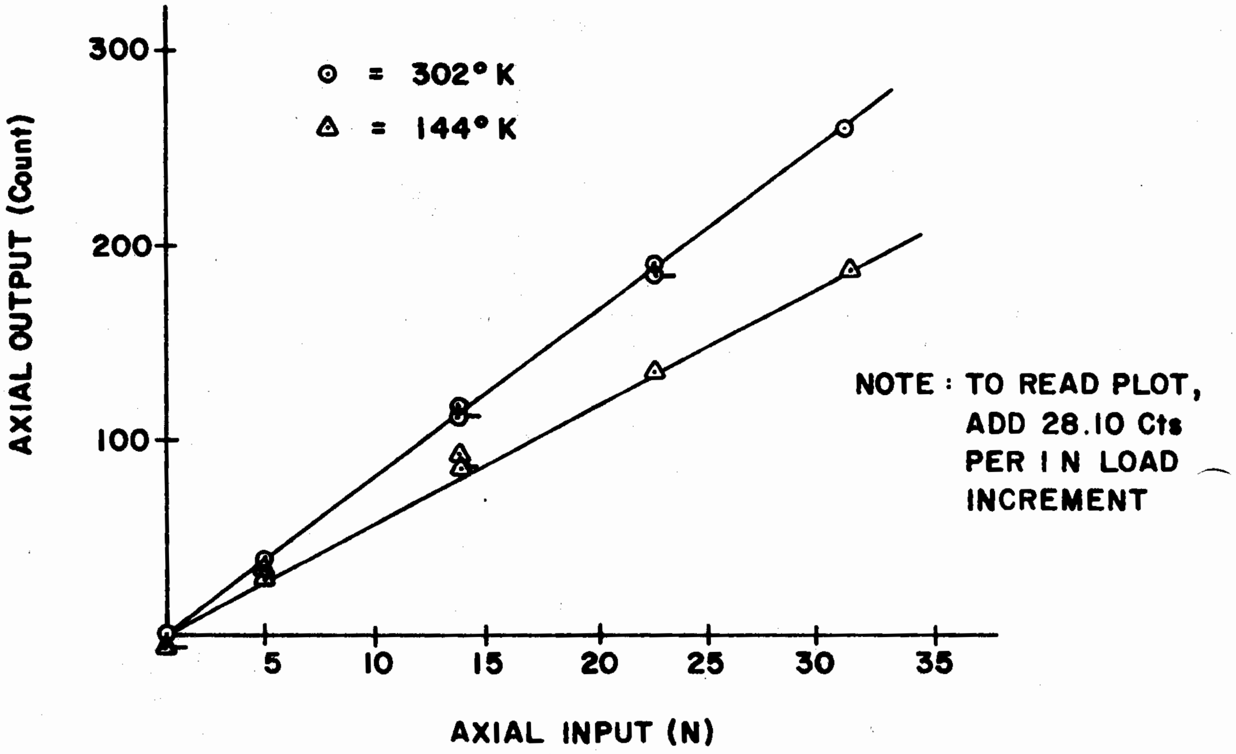


FIGURE 14– AXIAL FORCE CALIBRATION OF BALANCE 2020 AT 302°K AND 144°K

A NEW DEVICE FOR MEASUREMENT OF PARACHUTE OPENING SHOCK FORCES

Joseph E. Doerr
Arron Waldman

Maintaining the integrity of the parachute system for safety reasons of course is paramount during testing of the system. Maintaining the integrity of the parachute system as a production item during testing has always been a problem as most testing proves to be. Opening shock forces have always been a primary parameter and has been measured successfully for a great number of years. This success has always had questions associated with it due to the insertion of measuring devices normally foreign to the production system.

The method of measuring opening shock forces has always been to insert a strain gage load link in the riser harness as additional hardware in the parachute system. Each link (right and left) is approximately three inches long. The test parachute jumpers refuse to jump with them during their testing because they act as a dangerous weapon during opening. Measurements then have always been taken using torso and articulated dummies.

In an effort to satisfy the parachute test engineer and test parachute jumper a system has been devised using only existing parachute system hardware. A device known as a koch fitting is used on all personnel parachute systems for both the U. S. Navy and the U. S. Air Force. It is a small well engineered device that allows the parachute to be detached from the body harness, both for leaving the parachute in the aircraft during ground time and to disconnect the canopy after a parachute landing. None of the external surfaces of the koch fitting are large enough to allow gaging and wiring (remember the man and parachute are in a highly dynamic environment).

Use of the koch fitting pin as a force transducer became a natural choice,since the pin is subjected to the full tensile force of the parachute webbing. The pin, when mounted within the fitting, simulates a beam with its supported ends in a condition approximating that of clamped-clamped. Radial clearance between the pin ends and the koch fitting make for a beam support condition that is less rigid than clamped ends and more restrictive than simply supported. A steel sleeve surrounding the pin distributes the tensile force in the webbing to the pin, thereby simulating a uniform load condition to the pin (beam).

To physically mount the semi-conductor gages in a 4-arm active wheatstone bridge configuration, a pair of slots had to be milled on opposite sides of the cylindrical pin (beam) whose diameter is .34 inches, and whose active length is 1.75 inches. Since the strain level (load) is high, the milled slots when viewed sectionally have generous radii blending the flat surfaces and the walls.

An overall slot width of 3/32 inch accommodates the two semi-conductor gages that are bonded side by side to the flats. The gages, Kulite UGP-1000-090, have an overall length of .090 inches, an active length of .06 inches, and an overall width of .02 inches. Bonded in close proximity to the gages are subminiature terminals (tabs) of gold plated copper to which the .001 inch diameter gold wires from the gages are soldered. The terminals also provide for insulated wire attachment (by soldering) so that the semi-conductor gage network can be connected externally. To route the external leads, an axial and communicating radial hole (near the collar of the pin) were drilled, along with a transverse hole through both slots. Wires from both sides of the beam were routed through the transverse hole, to the axial hole and out the radial hole in the collar a total of 8 wires instead of the conventional 4 or 5.

The location of the strain-sensing gages on the beam was critical because of the high strain level. With a full load of 5000#, uniformly distributed along the active length of the beam, the theoretical strain at the center based on the assumption of clamped ends is calculated to be 3300 x 10^{-6} in/in. By running a series of loading experiments, a gage location was determined where the measured strain was a full scale average of 1750 x 10^{-6} in/in. A strain level of this magnitude would be considered excessive for conventional transducer design because of inherent non-linearity, however, for this unusual application it is suitable.

To conform to the 0 to 5 volt output signal requirements, and to the available 28 volt unregulated DC power supply, a signal conditioning amplifier with relatively low gain is required. The amplifier package, in addition to providing the required voltage gain also serves as the source of a regulated and stable excitation for the semi-conductor 4-arm bridge.

The average measured full scale (5000# load) for the modified koch fitting was 264 MV/V. Typical voltage regulator bridge excitation voltage is 7.5 volts DC, and the actual voltage appearing across the bridge is approximately 3 volts. A resistor in series (between the supply and the bridge) used for span (temperature) compensation drops the supply voltage. Typical voltage gain is therefore:

$$\text{Voltage Gain} = \frac{5.000}{.264 \times 3} = 6.3$$

Gains were individually tailored during loading tests to provide the 5 volt output.

THE QUADRAFLEXURE AS A STRAIN ELEMENT IN EXTENSOMETRY AND LOAD SENSING TRANSDUCERS

by

Alan M. C. Holmes[1]

and

Michael F. Duggan[2]

ABSTRACT

A stiff-hinged mechanism incorporating four equi-stiff flexures in a rectangular frame makes an ideal spring and strain element for extensometers and load cells for measuring very small loads. This is because, if suitably strain-gaged, the quadraflexure is a shear sensor rather than a bending sensor. In addition, its spring rate and performance characteristics are easy to calculate, and the device is easy to make.

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INTRODUCTION

The term "quadraflexure" is the name we have given to a stiff-hinged mechanism incorporating four equi-stiff flexures in a rectangular frame (Fig. 1). If the flexures are very stiff, the device constitutes a structure and can be analyzed as such. In most cases, however, it is advantageous to use low stiffness flexures, and in this configuration, its action is best visualized as a mechanism. The quadraflexure has been found to be extremely useful as a straining element for a sensitive load cell, where it has been used in a gram full-scale load cell. It has advantages as the strain element in deflectometers or extensometers where it is desirable to guide or mechanically limit the motion being measured to a single axis.

In both these configurations, the designer who uses it enjoys the following advantages: The spring rate, and strain at the strain gage site, can easily be calculated to within five percent accuracy. The device is relatively easy to machine, and is non-critical in all but one aspect, and this one has not been a major obstacle. As a load cell, it has the additional advantage of measuring the shear (on the element) rather than the bending caused by the applied load. This means that the position of the load relative to the device is not critical, see Fig. 2.

DESIGN OF QUADRAFLEXURES

The formulas for design of a quadraflexure (see Fig. 3) are:

$$\varepsilon = \frac{D\,t}{2\,\ell\,h} \tag{1}$$

or

$$D = \frac{2\,\varepsilon\,\ell\,h}{t} \tag{2}$$

where ε is the strain at the mid-point of the flexure

h is the distance between the mid-points of two flexures on the same leg of the frame

t is the flexure thickness

ℓ is the effective length of the flexure (i.e., the thin portion)

D is the deflection of the quadraflexure and is given in inches if the dimensions given above are in inches

The force on the quadraflexure which will cause a deflection D is given by

$$F = \frac{4\,D\,E\,I}{\ell\,h^2} \tag{3}$$

where E is the elastic modulus (lbs/in^2)

I is the moment of inertia of the flexure's cross-section (inches^4)

ℓ and h are as above

From this, the stiffness K in lbs per inch can be written as

$$K = \frac{4 E I}{\ell h^2} \tag{4}$$

By further algebraic manipulation it is possible to show that

$$\epsilon = \frac{P t h}{8 E I} \tag{5}$$

The derivations of the above formulae are by means of elementary statics and engineering mechanics and is given in the appendix. It should be emphasized that they are "exact" only if the deflections and strains are small, if the material remains elastic and if the proportions ℓ, h and t are kept "reasonable", that is to say

$$h >> \ell >> t$$

although a case where

$$h = 1.5\ \ell \qquad \text{and} \qquad \ell = 4\ t$$

resulted in a design where the performance calculated by these formulae differed by only ten percent from actual performance (on items like ϵ, K or D).

POSITION OF LOAD

It is of course desirable if the force of actuation is kept close to the axis of the translational member as in Fig. 3. But if F cannot be made to act close to the translational member (see Fig. 4), the result, which we shall call "eccentric loading" is superposition of a couple (or moment) to the bent.

The effect of a pure couple on a bent having columns (vertical members) of uniform stiffness is shown in Fig. 5. The moment F.R on the bent is resisted by the vertical forces P and the two equal moments M' (assuming the bent is symmetrical). The resulting moments on the bent are a function of the relative stiffness of the columns and cross-member and have the general character depicted in Fig. 6, which is not necessarily to scale. However, if the vertical members have low stiffness compared to the horizontal member (typically $t = .25\ b$, see Fig. 5), M' is less than $.01(F.R)$ and the moment F.R is thus resisted almost entirely by vertical forces in the columns.

The result of a grossly eccentric force as shown in Fig. 4 is then a dramatic increase in the vertical forces P (Fig. 4), but the horizontal forces remain the same, as do the relationships given in (1) through (5).

PLACEMENT OF STRAIN GAGES

For deflection or strain measurement, four strain gages are placed on two flexures on the same leg as shown in Fig. 6 and wired into a single bridge as shown in Fig. 7.

In such a configuration, the small bending in the leg due to any eccentric loading (see Fig. 5) is cancelled by the strain gage arrangement, as is any strain in the leg due to any vertical force (P) in the leg.

TOLERANCES

For the above bending cancellation to be effective, the flexures have to have the same thickness, but since this bending moment is small, the tolerance on flexure thickness (similarity) does not have to be extremely tight.

The effect of loose tolerances on the flexures is to cause inaccuracies in the predicted strains, deflections and spring rate. If one of the flexures differed by as much as 50 percent in thickness (from the rest), the quadraflexure's "gross movements" would be only slightly affected as long as

$$\ell >> h >> t$$

But strain output would be affected in proportion to the error in flexure thickness. The deflection and spring rate would be affected but about a quarter as much (percentage-wise) as the predicted strain.

Typically, on a small precision type quadraflexure with a flexure .012 inches thick, the tolerance is held to ± .001 inches, and the calculated strain in the flexure could be as much as 8 percent in error. For a similar possible error in a flexure .024 inches thick, the tolerance should be ± .002 inches. This may sound "tight", but remember that the flexures are only four small zones where the tolerance must be held. Over the remainder of the unit, the tolerance can be quite loose (± .010 to ± .030 inches, depending upon size).

QUADRAFLEXURE CHARACTERISTICS

Table 1 gives some typical calculated performance characteristics. "ℓ" has been held constant at .28", and the width (which affects the stiffness values of the table only) has been held at .25 inches. The material was assumed to be 7075-T6 aluminum and the modulus was rounded off to ten million psi (again, this only affects the stiffness values). For each value of t (thickness) and height h, the three values given in the table are:

1) The stiffness K in lbs per inch.
2) The strain for a deflection of .005 inches.
3) The deflection in inches when the strain is 4500 microstrain.

The 4500 microstrain level was chosen because it represents about 75 to 80 percent of the elastic range for 7075-T6 aluminum, and thus represents a desirable upper limit if a safety margin of 1.25 to 1.33 is acceptable (relative to undesirable permanent set).

QUADRAFLEXURES IN PRACTICE

Figures 7 through 14 are photographs of various transducers which embody quadraflexures.

Figure 7 is a high temperature strain transducer which measures strain by means of pull-rods which extend into the hot zone (see Fig. 8). This unit has been used to measure creep and stress strain curves on the Space Shuttle insulation material and has a working resolution of 5 microstrain.

The unit, including pull rods, weighs only .3 lbs which was an important consideration in design since specimens fail at values like five lbs (tension). In Fig. 9 the extensometer is shown on a calibration device which uses a quadraflexure instead of linear bearings to maintain linear motion of the calibration head.

Figures 10, 11 and 12 are also of an extensometer for use on the Space Shuttle insulation material, but at room temperature. In Figs. 10 and 11, the unit is shown on a ceramic coating tensile specimen. In Fig. 12, a spacer has been introduced to accommodate a one-inch thick insulation fiber specimen. In this unit, a third quadraflexure acts as a clamping spring to hold the left and right sensors pressed against the specimen. Squeezing the two black screws (Fig. 12) causes the sensors to move apart for easy installation and removal. Here, the purpose of the third quadraflexure is to provide spring action and to keep the sensors aligned. Working resolution of the extensometer is 5 microstrain.

Figures 13 and 14 show a load cell design used to measure load at 40 discrete points around a sample of antenna mesh. The test was to determine load-deformation characteristics of mesh under biaxial loading. The positions of the hooks connected to each of the forty load cells could be individually adjusted to produce equal load at each load cell to .0001 lb accuracy. Quadraflexures were also used below the load cells to provide rectilinear position adjustment of the load cell and its "hook".

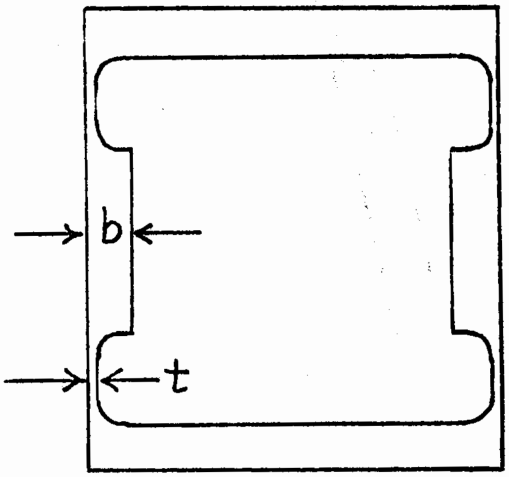


Figure 1a. Quadraflexure.

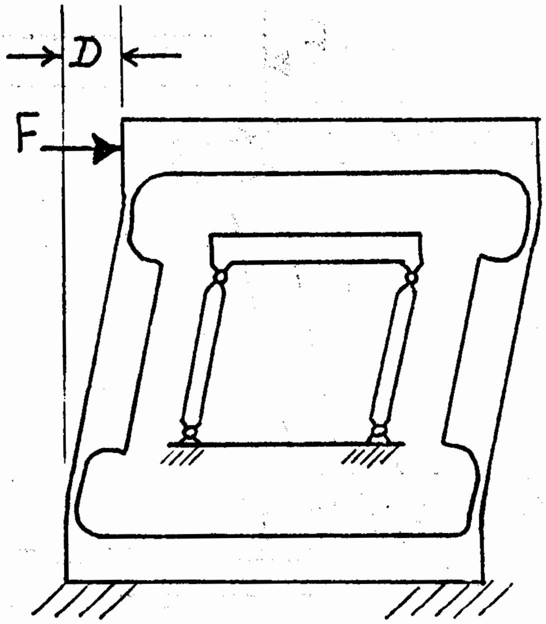


Figure 1b. Deflected Condition.

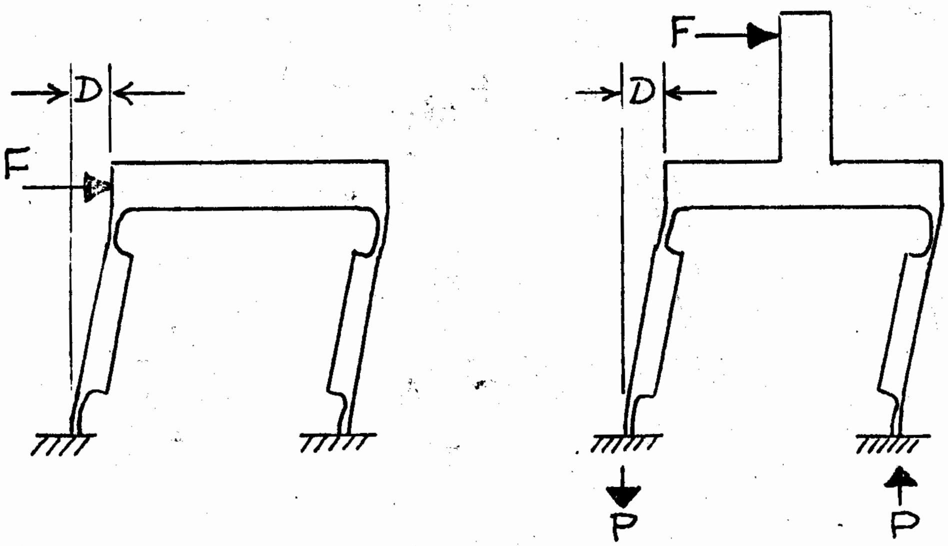


Figure 2. Insensitivity to Load Placement.

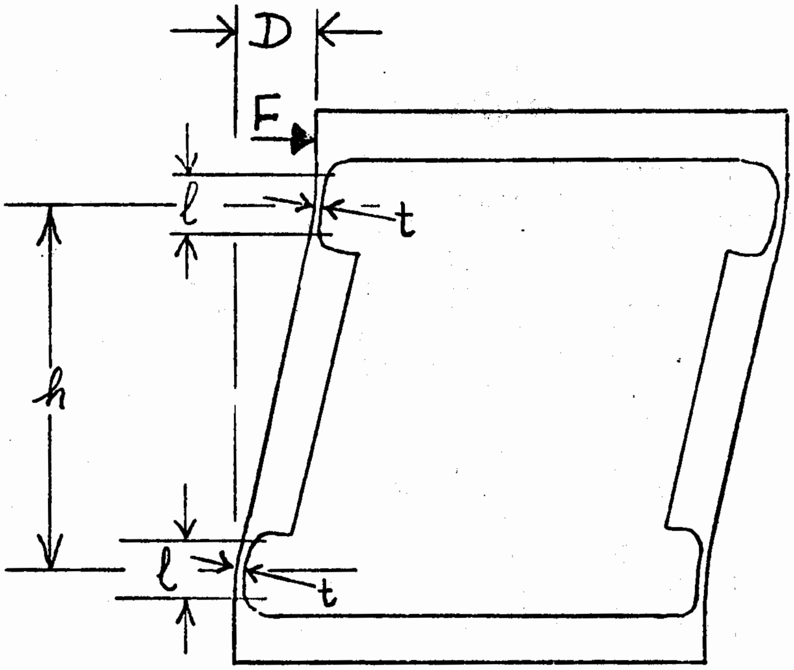


Figure 3. Quadraflexure Nomenclature.

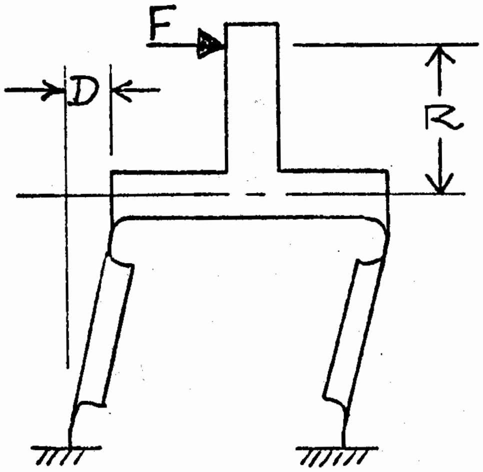


Figure 4a. Eccentric Load.

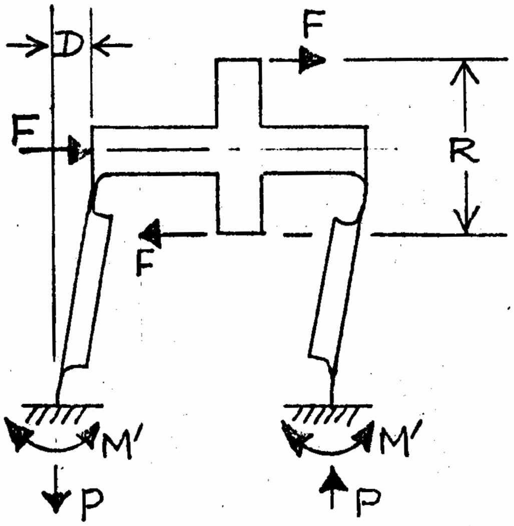


Figure 4b. Equivalent Condition.

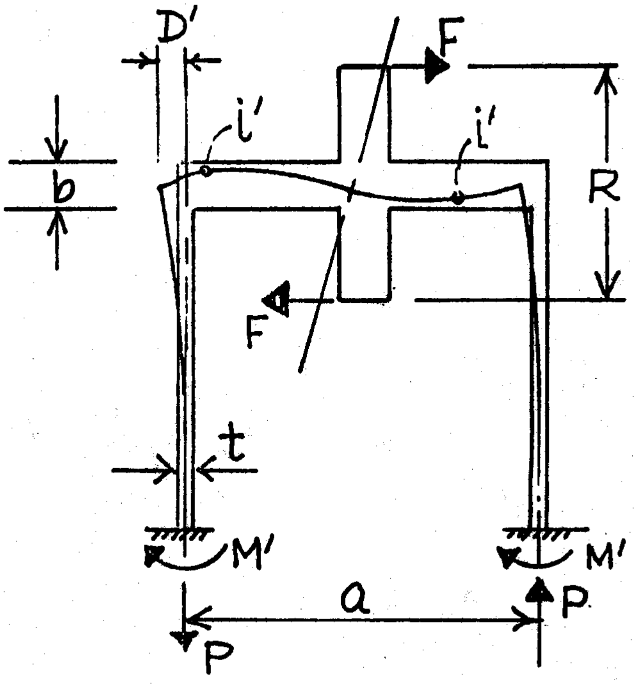


Figure 5a. Forces and Exaggerated Deflections.

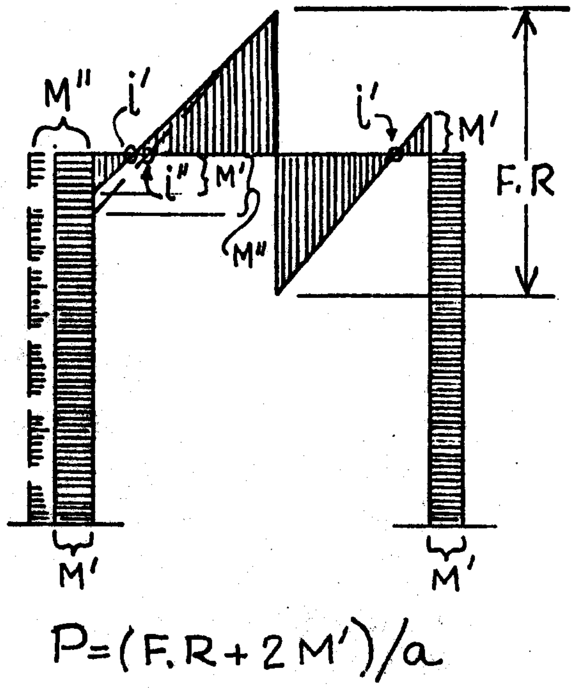


Figure 5b. Moment Diagrams.

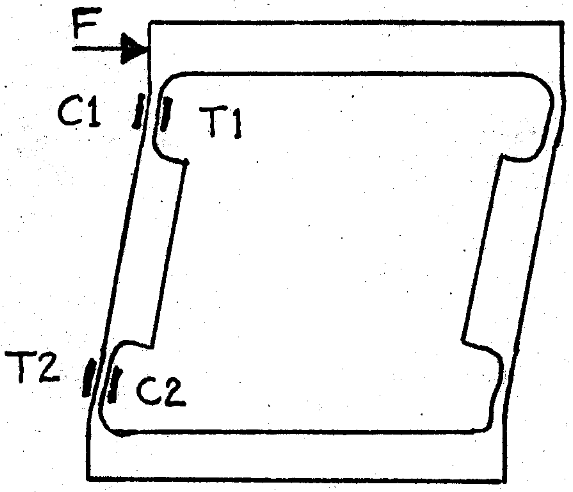


Figure 6a. Strain Gage Placement.

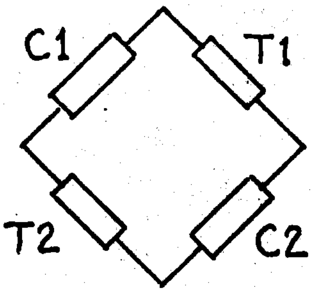


Figure 6b. Bridge Arrangement.

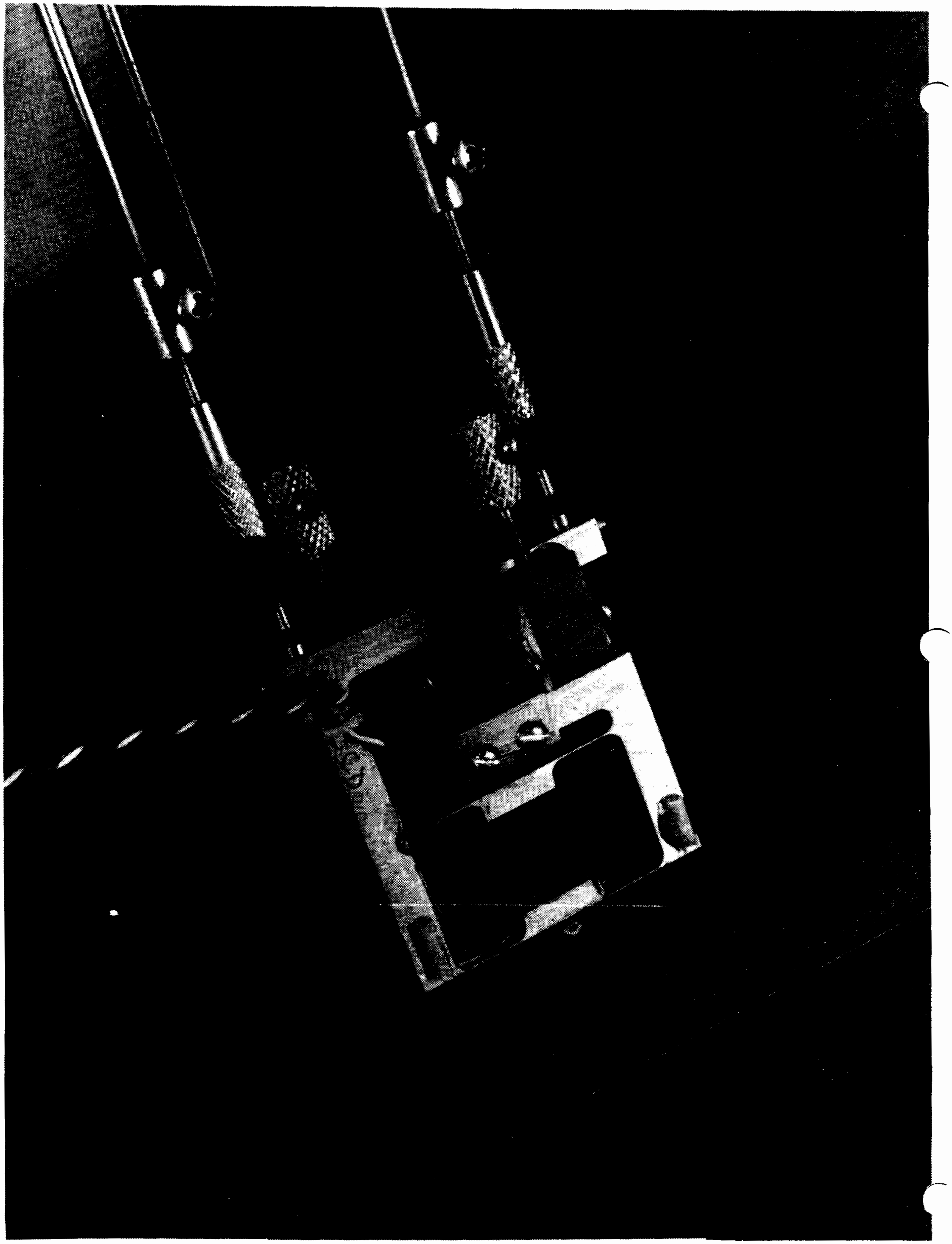


Fig. 7   Pull-rod extensometer (detail)

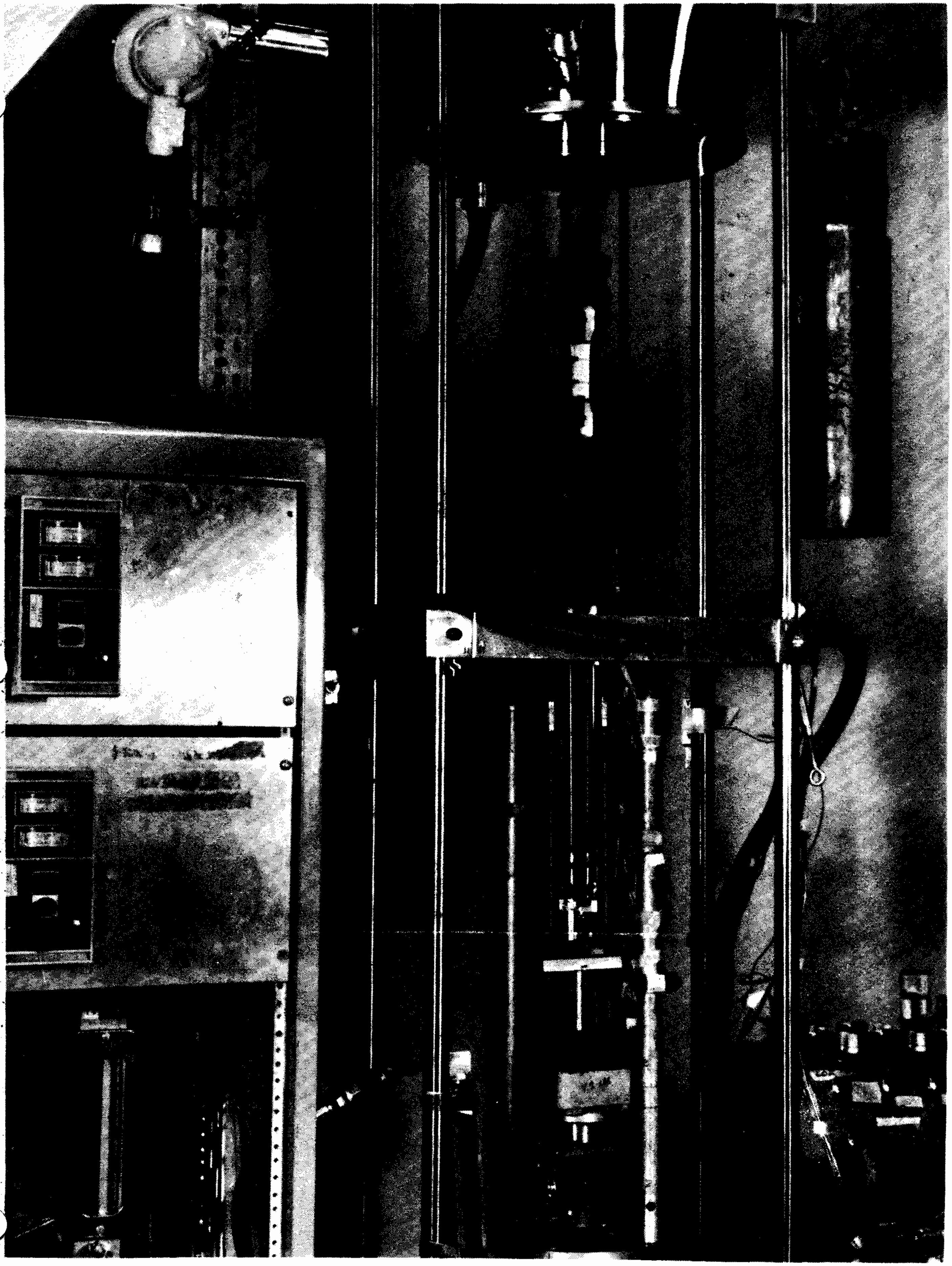


Fig. 8 Pull-rod extensometer in use

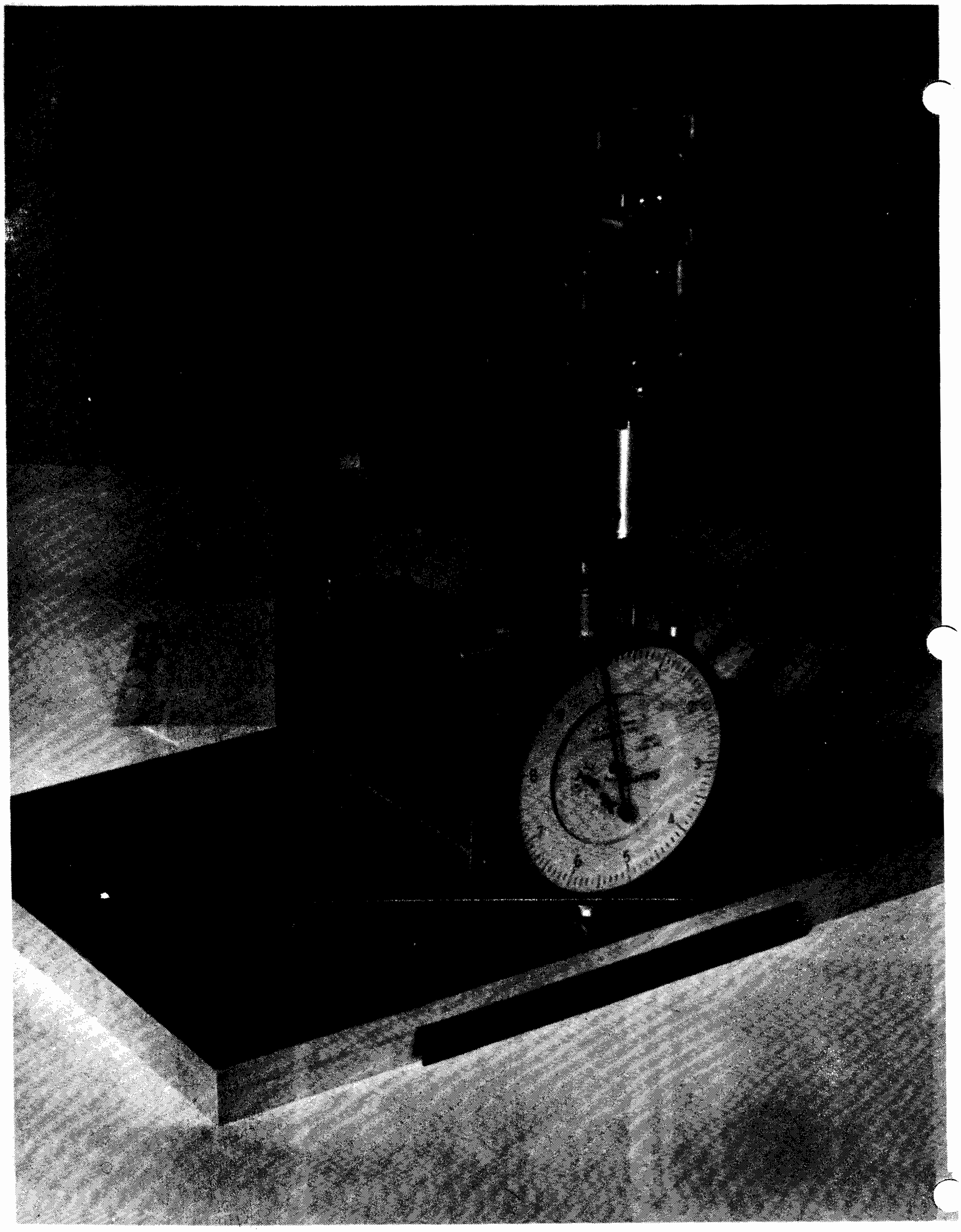


Fig. 9 Pull-rod extensometer on calibration device

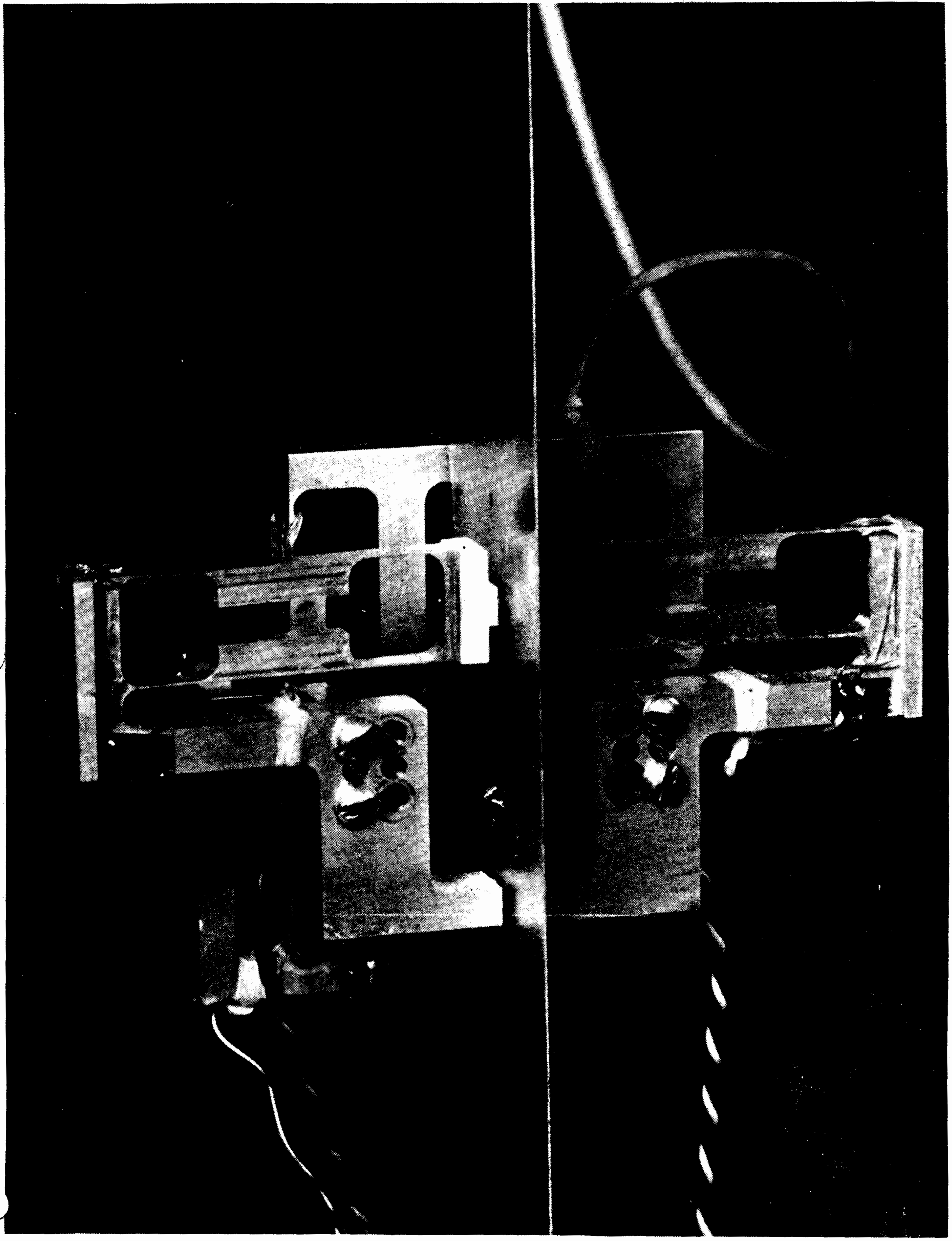


Fig. 10 Clip-on extensometer (front view)

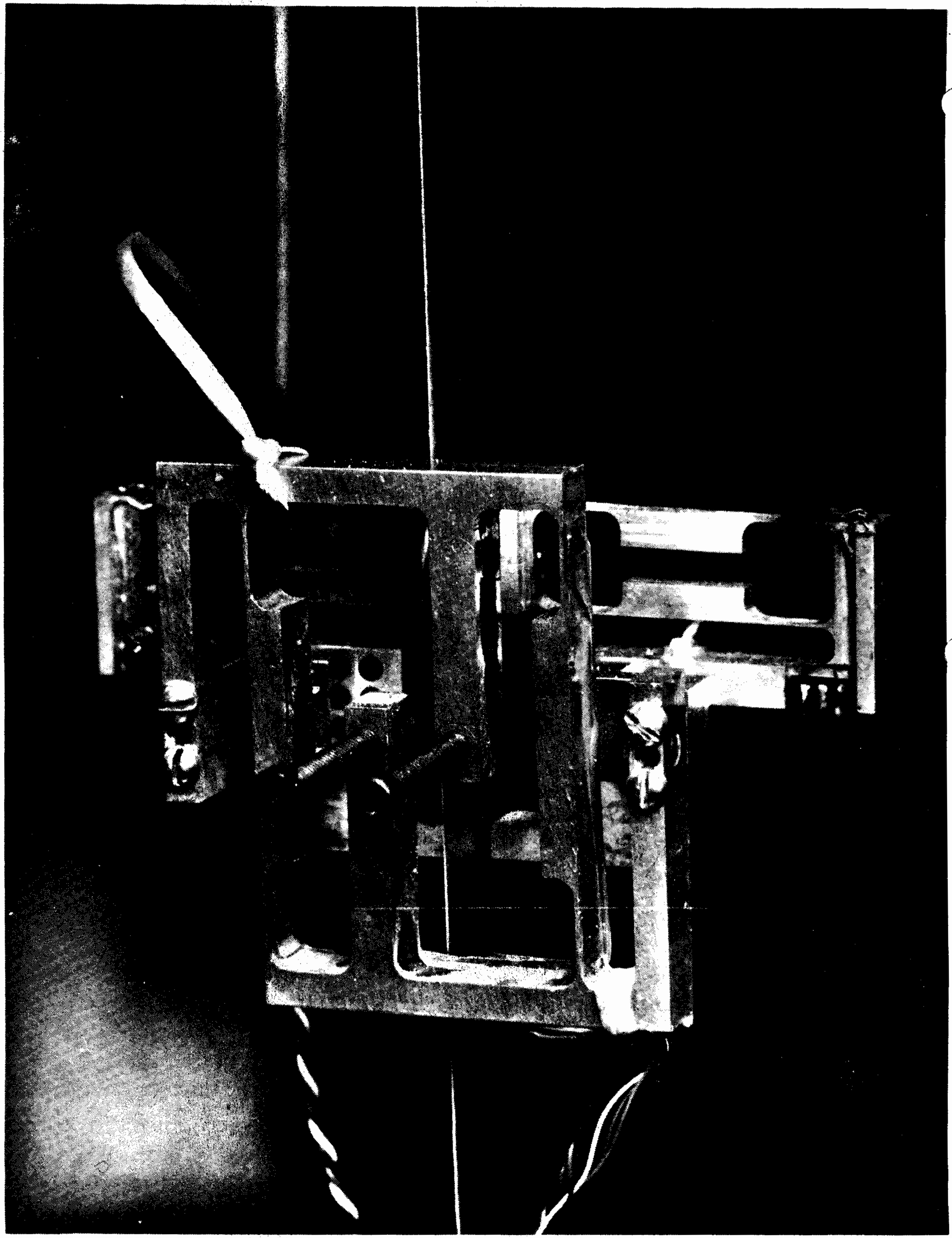


Fig. 11 Clip-on extensometer (back view)

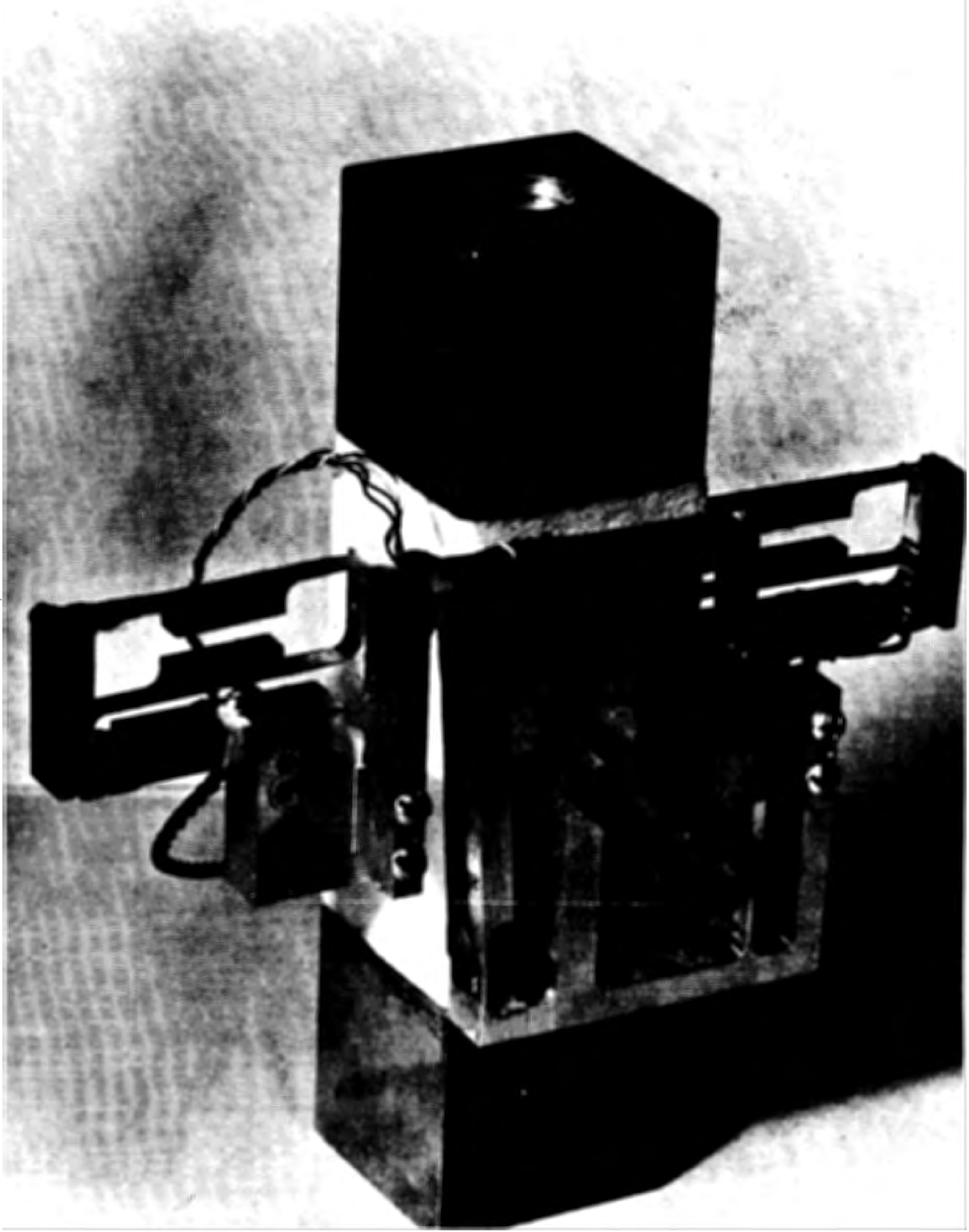


Fig. 12 Clip-on extensometer on thicker specimen

Fig. 13 High-sensitivity ad cells (biaxial mesh test)

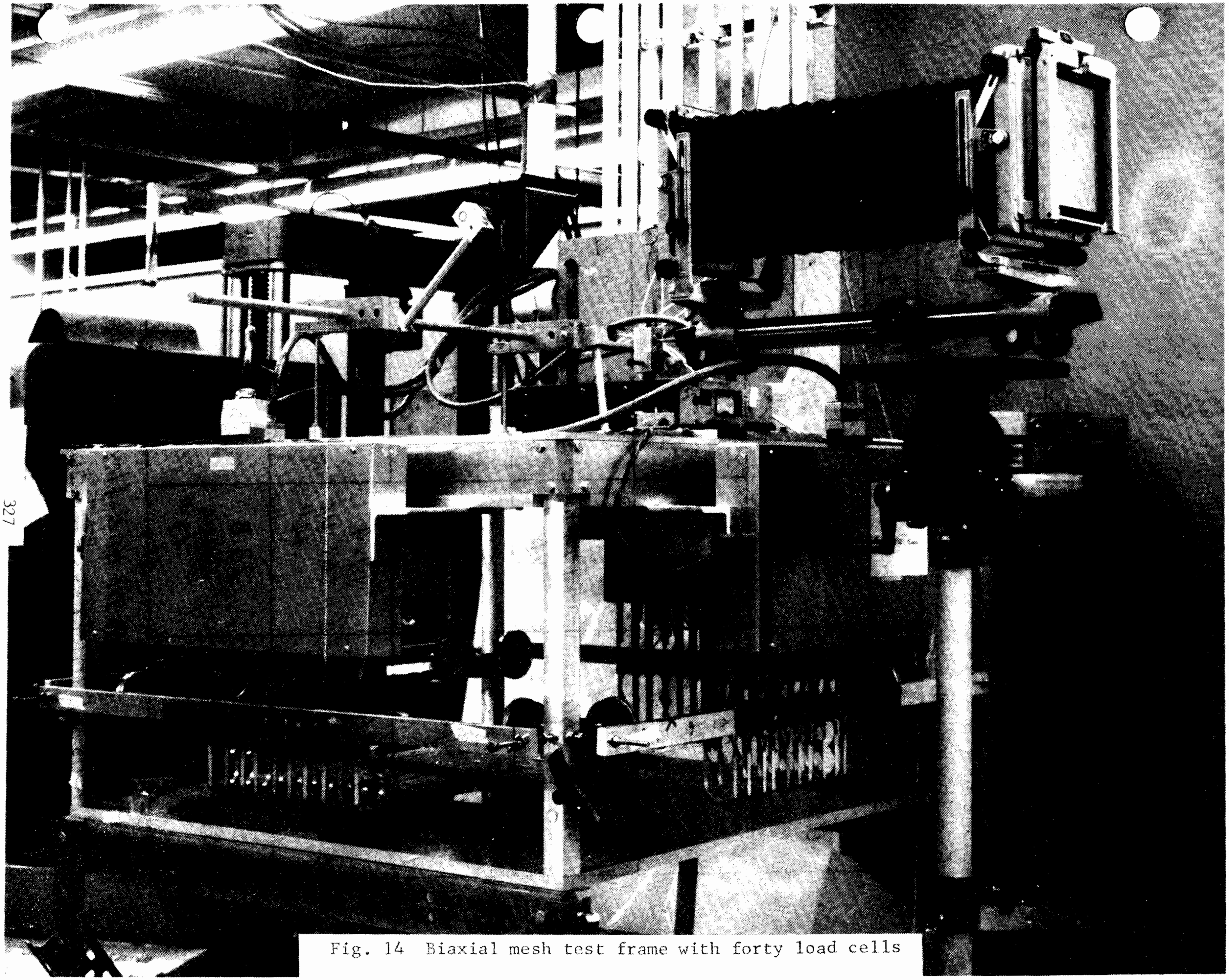


Fig. 14 Biaxial mesh test frame with forty load cells

TABLE 1

QUADRAFLEXURE CHARACTERISTICS

ASSUMES: $\ell = 0.28''$
Blade width = .25"
$E = 10 \times 10^7$ PSI

SUPPLIES: K(Stiffness) in lbs/in
Microstrain for $D = .005''$
D(inches) when $\epsilon = 4500 \times 10^{-6}$

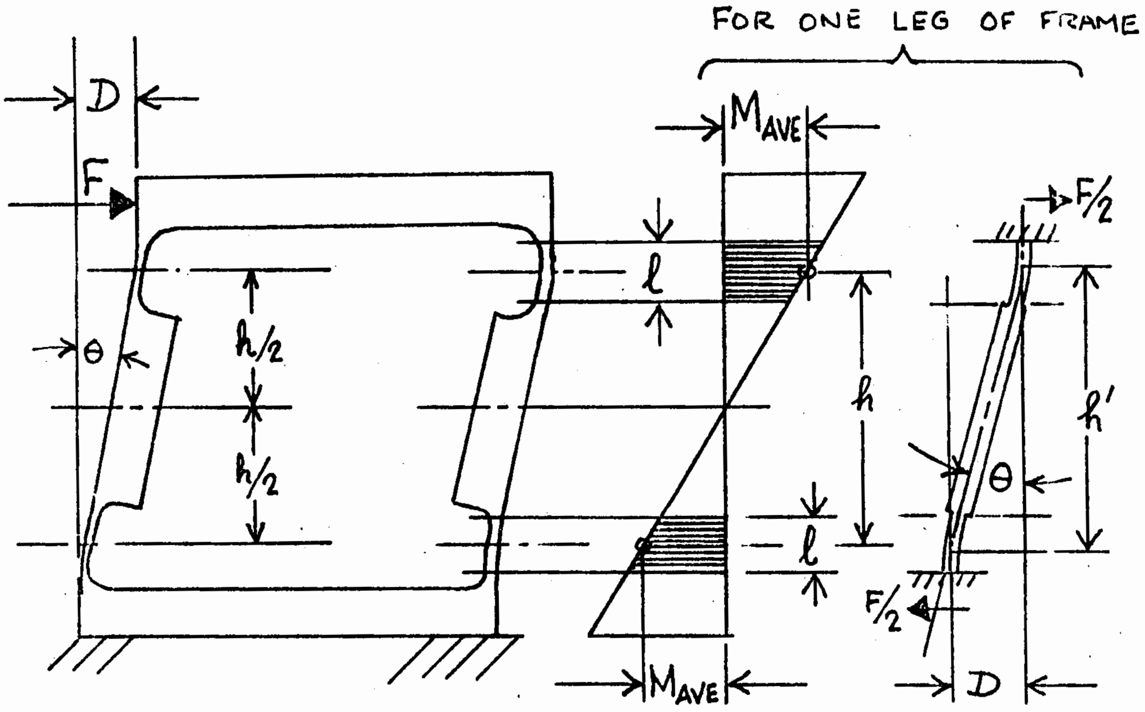
t↓ h→	.50"	1.00"	1.50"	2.00"
.010"	11.9 (lb/in) 180 μϵ .125"	2.98 90 μϵ .25"	1.32 60 μϵ .375"	.74 45 μϵ .50"
.012"	20.6 216 μϵ .104"	5.14 108 μϵ .21"	2.29 72 μϵ .31"	1.29 54 μϵ .42"
.014"	32.7 250 μϵ .090"	8.14 125 μϵ .18"	3.63 84 μϵ .27"	2.04 63 μϵ .36"
.016"	48.7 286 μϵ .078"	12.2 143 μϵ .16"	5.41 96 μϵ .23"	3.04 72 μϵ .32"
.018	69.4 322 μϵ .069"	17.4 161 μϵ .14"	7.71 108 μϵ .21"	4.34 81 μϵ .28"
.020"	95.2 360 μϵ .063"	23.8 180 μϵ .13"	10.6 120 μϵ .19"	5.95 90 μϵ .25"

Spring rate, Strain at mid-flexure, deflections as functions of thickness "t" and height "h"

APPENDIX

DERIVATION OF EQUATIONS

(SEE PAGE 311 AND FIG 3 FOR NOMENCLATURE)



$$\theta = \frac{M_{AVE}.\ell}{E.I} = \frac{\overset{\sigma}{(E.\epsilon_{AVE})} \times \overset{\text{Sect. Mod.}}{(I/\tfrac{t}{2})} \times \ell}{E.I} = \frac{2.\epsilon.\ell}{t}$$

$$D = \theta.h' = \frac{2.\epsilon.\ell.h'}{t} \cong \frac{2.\epsilon.\ell.h}{t} \quad (2)$$

$$M_{AVE} = (F/2)\times(h/2) \qquad \text{AND} \qquad \theta = \frac{F.h.\ell}{4\,E.I}$$

$$D = \frac{F.h.h'.\ell}{4\,E.I} \qquad \text{OR} \qquad D \cong \frac{F.h^2.\ell}{4\,E.I} \quad (3)$$

$$K^{*}\left(= \text{Stiffness in } \frac{\text{lbs}}{\text{in}}\right) = \frac{F}{D} = \frac{4\,E.I}{\ell.h^2} \qquad \text{OR} \qquad \frac{4\,E.I}{\ell.h.h'} \quad (4)$$

* for entire frame

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TITLE: A THERMOPILE PROBE TO MEASURE TEMPERATURE ANOMALIES IN GEOTHERMAL BOREHOLES

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A THERMOPILE PROBE TO MEASURE TEMPERATURE ANOMALIES IN GEOTHERMAL BOREHOLES

by

Bert R. Dennis
Evon L. Stephani
Billy E. Todd

ABSTRACT

The standard thermal well logging tools presently employed by oil well logging service companies use a thermistor probe as the temperature measuring device. The thermistor is normally incorporated as one arm of a Wheatstone bridge circuit. The bridge circuit must be staged for limited temperature ranges and is adequate for most well logging operations where only detection of thermal anomalies is of primary concern and the logging speed is not important.

The design of a thermopile sensor using conventional thermocouples and a downhole thermally isolated reference junction has greatly improved the temperature logging capability in a deep geothermal wellbore. The much faster response of the thermopile sensor will allow a logging rate of up to 200 ft/min in contrast to the average rate of 50 ft/min using the thermistor probe. The thermopile sensor is a low impedance device whose characteristics are very well known.

The development of a high pressure dewar chamber for use in high temperature downhole instrumentation sondes has provided a suitable environment to employ a downhole reference junction permitting the use of thermocouple measurements in the deep geothermal borehole.

I. INTRODUCTION

At sufficient depth, rock hot enough to be potentially useful as an energy source exists everywhere. In many places, hot dry rock is at depths shallow enough to be reached at moderate cost with existing drilling equipment. The Los Alamos Scientific Laboratory (LASL) has been actively investigating the potential for extracting geothermal energy in those areas of the United States that contain hot dry rock at moderate depths.[1] A man-made geothermal reservoir would be formed by drilling into an identified region of suitably hot rock and creating a very large surface area for heat transfer by use of a large-scale hydraulic fracturing technique. A circulation loop would be formed by drilling a second hole and intercepting the top of the fractured region. The heat contained in this reservoir would be brought to the surface by the buoyant circulation of water. The water in the loop would be pressurized at the surface to maintain the liquid phase, thereby increasing the rate of heat transport up the withdrawal hole.[2] Preliminary experiments and analyses indicate that thermal stresses created by cooling of the hot rock may gradually enlarge the fracture system and extend the useful lifetime of the original reservoir for many years.

On the basis of extensive studies and field experiments, the "Fenton Hill" site was selected for the development of the first hot dry rock energy experiment. This site is located about 32 km west of Los Alamos on the Jemez Plateau in that part of the Rocky Mountains extending into northern New Mexico. As a result of relatively recent volcanic activity, a large amount of heat is still retained in the rock underlying the area within a few kilometers of the surface. The primary objective of the hot dry rock geothermal energy extraction experiment is to investigate and demonstrate the techniques of drilling into the hot granitic rock, fracturing it by hydraulic pressure, producing a connected circulation loop, and then circulating water to extract the heat and transport it to the surface. The field studies include research and development in geochemistry, geophysics, heat flow, seismology, environmental effects and other areas related to employing an economical and environmentally acceptable energy extraction system. A 10 MW thermal energy extraction demonstration is planned as the first milestone for the hot dry rock geothermal program (Fig. 1).

II. DRILLING AND TESTING IN HOT DRY GRANITE

The first exploratory borehole drilled at the Fenton Hill site was designated Geothermal Test Hole No. 2 (GT-2). The Precambrian granitic surface was reached at a depth of 733 m (2404 ft).[3] Drilling continued in the granitic basement rock to a depth of 2042 m (6700 ft). Following this first drilling phase a series of experiments was conducted to study the nature and physical behavior of the hydraulic fractures created in the granitic section of the borehole. Various diagnostic logging operations were performed by well-logging service companies during this drilling and testing phase.[4] LASL instrumentation was developed to measure rock breakdown and fracture extension pressure in the borehole where the bottom-hole temperature at this depth reached 145°C.

Upon completion of the series of experiments at this intermediate depth, the borehole was drilled to a final depth of 2932 m (9619 ft) where the bottom-hole temperature reached 197°C. Again, various diagnostic logging operations were performed by the well-logging service companies. Many problems with equipment failures were experienced in the borehole due to the high temperature environment. A liner was cemented in the open borehole between 2735 m (8973 ft) and 2920 m (9581 ft) to facilitate hydraulic fracture experiments.[6] A polished bore recepticle was installed at the top of the liner to accept 10.3-cm (4-in.) diam tubing run to the surface. The liner was subsequently perforated to perform additional hydraulic fracture experiments in this region of the borehole.

Following a series of pressurization experiments to determine the permeability of the rock at the bottom of the GT-2 borehole, a small hydraulic fracture was formed. A series of hydraulic-fracture extension and pumping experiments were conducted to determine principal tectonic stress, stress variations, and the leak-off rate of the fracturing fluid. Measurements were performed to characterize the fracture and determine stability of the pressurized fracture system. The fracture at the bottom of GT-2 was eventually extended to a radius of 120 m (400 ft). It was important to obtain the dimensions and orientation of this fracture system to achieve intersection of the fracture with the second borehole. Mapping the fracture reservoir was also important to develop an understanding of the flow and heat-transfer properties.

Drilling began on the first energy extraction borehole (EE-1) approximately two months following the completion of GT-2. It was drilled to a depth of 3064 m (10,053 ft) and a measured bottom-hole temperature of 205.5°C. The downhole circulation loop was completed when directional drilling techniques were used to turn the EE-1 borehole to intercept the fracture system. The EE-1 borehole was cased to a depth of 2926 m (9600 ft) with 19.4-cm (7-5/8-in.) diam casing cemented at the bottom.

III. BOREHOLE TEMPERATURE MEASUREMENTS

The drilling program for GT-2 allowed for extensive coring in the Precambrian granitic rock followed by *in situ* temperature measurements. It was important to minimize the time interval required to extrapolate to an accurate bottom-hole temperature.[5] A continuous temperature log of the entire borehole was also necessary to detect geological anomalies such as the presence of aquifers in the granite zones that were communicating with water-bearing zones in the overlying Madera Limestone Formation. Borehole temperature measurements are now used for measuring major departures from previous temperature logs due to hydraulic fracturing and flow experiments in both boreholes. Temperature logs are presently run in the open-hole sections of both EE-1 and GT-2 during pressurization and flow experiments to determine the locations of fluid paths entering and leaving the fracture system.

IV. THERMISTOR PROBES

The initial probes used by LASL for measuring borehole temperatures employed thermistors for the sensing device. The thermistor is essentially a semiconductor that behaves as a temperature-sensitive electrical resistance which provides a high degree of resolution not available in other transducers. They are well suited for measurements exceeding 300°C. The resistance-temperature response is, however, quite nonlinear and varies somewhat from one type of thermistor to another. Each temperature probe, therefore, must be carefully calibrated.

The temperature measurements using the thermistor probes in the GT-2 and EE-1 boreholes employed two recording systems. To obtain accurate bottom hole thermal measurements, the thermistor resistance was measured with a digital ohms converter having a six digit readout with an accuracy of ±0.02% of full scale. Corrections were made for line resistance, measured as the sonde was lowered in the borehole, to compensate for thermal effects on the cable. The resistance data were converted to temperatures using a linearization equation computed for each probe (Fig. 2). To log the borehole continuously on a strip chart recorder, the thermistor was employed in one arm of a Wheatstone bridge network. The resistance span of the bridge was chosen to minimize the nonlinear characteristics of the thermistor. To maintain an accuracy of less than 4%, the temperature intervals were restricted to ranges of 50°C (Fig. 3). For logging the cooler regions of the borehole, the thermistor resistance was quite high (575 kΩ at 80°C for the nominal 10 MΩ thermistor and 66 kΩ at 80°C for the nominal 1 MΩ thermistor).

The bridge network presented a high impedance to the recording equipment. This high impedance of the source resulted in poor signal-to-noise ratio and decreased the resolution of the measuring system. The temperature sensors were Fenwal thermistor glass beads (Type GA-61P8 and GA-71P8). These glass-coated thermistors were evaluated for their stability characteristics and proved to have the best stability properties over a useful temperature range up to 316°C. The thermistor sensors were temperature cycled to improve stabilization prior to assembly in the temperature probe. The thermistor leads were Durmet wires and were silver soldered to #22 AWG Teflon insulated lead wires. The thermistor probe was then forced into contact with a copper plug that was silver brazed into the end of a stainless steel tube employed as a protective sheath. The stainless sheath, 188-mm (7-in.) long and 7.94-mm (0.312-in.) o.d. by 6.16-mm (0.242-in.) i.d., was pressure tested for an equivalent depth of 4230 m (13,880 ft) of water or 414.2 bars (6000 psi). The void volume within the sheath was filled with technical "G" copper cement up to a level of 25 mm (1 in.) below the open end of the sheath. This cement will withstand temperatures up to 500°C and provides a thermally conducting electrical insulation. After the cement cured, the remaining void section at the top of the sheath was filled with Dow Corning 92-024 high temperature aerospace sealant. The total mass of the probe was about 49 g (1.73 ounces).

The thermistor probe was assembled in a downhole sonde for logging operations. A perforated cage was incorporated into the leading end of the sonde to protect the probe from damage while descending the borehole. A block diagram of the probe is shown in Fig. 4.

The time constant* of the thermistor probe assembled in the downhole sonde was measured to be 7.63 s for a logging rate of descent of 7.62 m/min (25 ft/min). The indicated temperature lag from the true fluid temperature in the borehole would be 0.058°C (ambient temperature of 200°C). The time constant of this probe would be 7.54 s at a logging rate of 10.7 m/min (35 ft/min) corresponding to a temperature lag of 0.080°C.

V. THERMOPILE PROBE

To improve the borehole temperature measurements at increased logging rates, LASL developed a borehole thermopile transducer. The thermopile was constructed

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*Time required to reach 63% of an instantaneous temperature change.

by wiring seven chromel-alumel thermocouples in series. Each ungrounded thermocouple was sheathed with 304 stainless steel tubing filled with magnesium oxide insulation. The 1.59-mm (0.0625-in.) diam sheath had a wall thickness of 0.254-mm (0.01-in.). The cold junctions were submersed in an ice bath. The ice bath was provided by an ice filled dewar housed in a sealed pressure shell (Fig. 5). A second thermocouple was set in the dewar to measure the ice bath temperature. The reference for this second thermocouple was provided by a self-compensating electrical bridge reference junction (Consolidated Ohmic Model JR114APOC) that was also submersed in the ice bath. The reference junction was tested in an oven to insure 0°C compensation up to temperatures exceeding 80°C. This would allow measurements in the borehole after the ice had melted. The dewar, filled with crushed ice, would maintain an internal temperature of 0°C for 6 h when the downhole sonde was subjected to a borehole temperature of 200°C.

The time constant for the thermopile was measured for comparison with the thermistor probe. For a logging rate of 7.62 m/min (25 ft/min) the temperature lag was 0.002°C with a time constant of 0.310 s. A comparison of response for the thermopile and thermistor is summarized in Table I. The thermocouple characteristics are well known, and when calibrated the accuracy is greater than 0.1%. The thermopile presented a low source impedance to the surface readout equipment. Figure 6 compares the temperature readout of the thermopile at logging rates of 25 ft/min and 80 ft/min. The temperature anomalies shown in the figure are explained later in the text.

VI. SUMMARY OF FIELD MEASUREMENTS

Temperature measurements were made throughtout the Precambrian granitic rock section of GT-2 during the drilling phase. Several techniques were developed to to insure accurate bottom-hole temperatures in the fluid filled borehole.[5] Equilibrium rock temperatures calculated from relaxation data show geothermal gradients approaching 60°C/km. The extrapolated equilibrium rock temperature at the terminal depth of 2932 m (9619 ft) was 197°C in GT-2.

Bottom-hole temperature measurements were made at less frequent intervals during the drilling of EE-1 since the thermal gradient had been well established. The extrapolated equilibrium rock temperatures at the terminal depth of 3064 m (10,053 ft) in EE-1 was 205.5°C.

Prior to initiation of the hydraulic fracture experiments and extensive flow tests, a background temperature log was run in each borehole. These temperature

TABLE I

M/Min	Ft/Min	Thermistor		Thermopile	
		$\Delta\tau^a$	ΔT^b	$\Delta\tau^a$	ΔT^b
7.62	25	7.63	0.058	0.310	0.0020
10.70	35	7.54	0.080		
24.38	80	7.33	0.179	0.165	0.0034

---

[a] $\Delta\tau$ = time constant of probe in seconds

[b] ΔT = temperature lag (true-indicated) °C

measurements established a base gradient from which the effects of the hydraulic fractures and flow test could be determined. The background temperature measurements are shown in Fig. 7. The sudden change in the temperature gradient at 731.5 m (2400 ft) occurs where the Precambrian granite begins. Temperature logs were made in both EE-1 and GT-2 during the large number of flow tests which were conducted to characterize the fracture system connecting the two boreholes. Only the results of the temperature logs will be presented here.

Figure 8 describes the temperature logs run during an early low pressure flow experiment with the surface pressure not exceeding 34.45 bars (500 psi). The flow during this experiment was initiated in GT-2. The temperature anomalies measured in the borehole in the interval 2800 m (9186 ft) to 2850 m (9350 ft) and in the interval 2880 m (9448 ft) to 2920 m (9580 ft) show major departures from the original temperature log of GT-2. The background log prior to pumping shows the long term result of previous hydraulic fracturing and pressurization experiments where large quantities of cold water were forced into the fractured regions. Analysis of the time-temperature data obtained during the pumping phase indicates that 80% of the fluid leaves the borehole at the 2820-m (9200-ft) interval and 15% leaves at the 2880-m (9448-ft) interval with the rest leaving at the bottom of the borehole (see Appendix A).

A pressurization experiment in the EE-1 wellbore was run to determine the intersection of the fracture system with this borehole. The surface pressure was increased to 93.7 bars (1360 psi) during the pumping phase to insure fracture inflation. The background log made prior to pumping recorded no anomalies. The log made during the flow phase showed that the fluid was leaving the borehole in the interval from 2941 m (9650 ft) to 2957 m (9700 ft) (Fig. 9).

An important consequence of the analysis of the flow experiment data proved that the impedance to flow from the wellbores through the fracture system was too high to establish a meaningful thermal energy extraction demonstration. Plans were made to improve the wellbore impedance by employing chemical leaching techniques. A pre-leach flow experiment was conducted to record the existing fracture system parameters for comparison purposes. Prior to that start of pumping, background temperature logs were made in both GT-2 and EE-1 from a depth of 2600 m (8500 ft) to the bottom of each borehole (Fig. 10). The strange anamolies that were measured in the EE-1 borehole at this time were of concern. Later temperature logs repeated over this interval confirmed the erratic behavior of the fluid temperature and indicated that the cement bond between the wellbore and the casing had deteriorated for several hundred feet up the borehole and the fluid was circulating behind the casing in this region. During the pumping phase of this experiment, a sudden drop in the GT-2 shut-in pressure accompanied by a large increase in the GT-2 annulus flow rate indicated that the fracture system had suddenly broken through into the GT-2 borehole above the cemented liner. The point of entrance of the fluid into the annulus would appear as a temperature anomally. The post flow temperature log plotted in Fig. 11 did show a sudden rise in temperature at 2770 m (9100 ft).

VII. CONCLUSION

The temperature probe is one of the less complex logging tools and is readily fielded allowing quick data acquisition for easy analysis. The temperature measurements in the geothermal boreholes have proven to be most valuable in determining the changing conditions of the fracture system. The analysis of temperature anomalies has lead to corrective measures that have prevented major setbacks in proceeding with a 10 MW thermal extraction facility. Procedures have been implemented to re-cement the void behind the EE-1 casing and to improve the wellbore-fracture flow impedance. Specifications to construct the 10 MW thermal circulation loop are complete and fabrication has begun.

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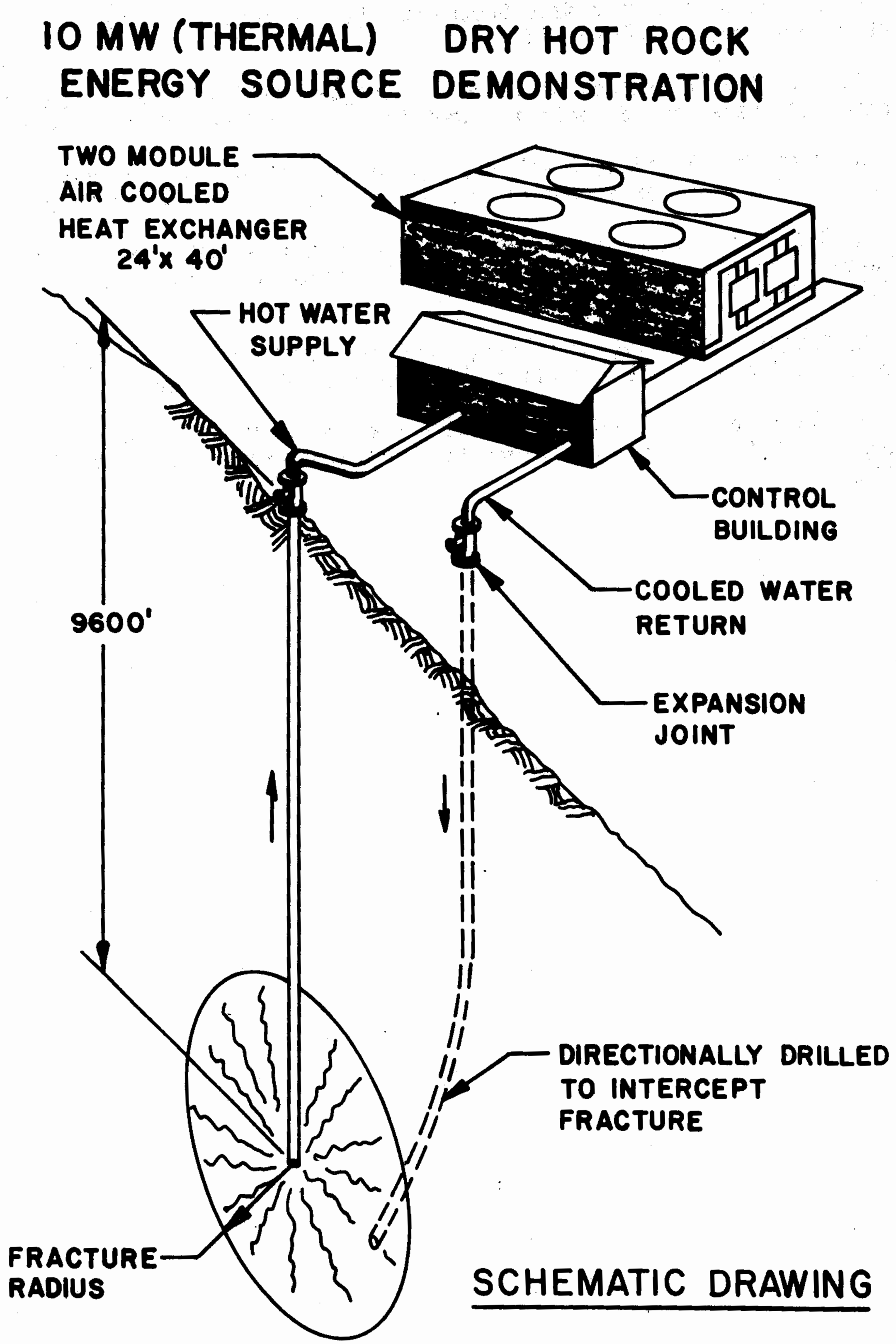

Fig. 1 - Hot Dry Rock Geothermal Energy Extraction Demonstration.

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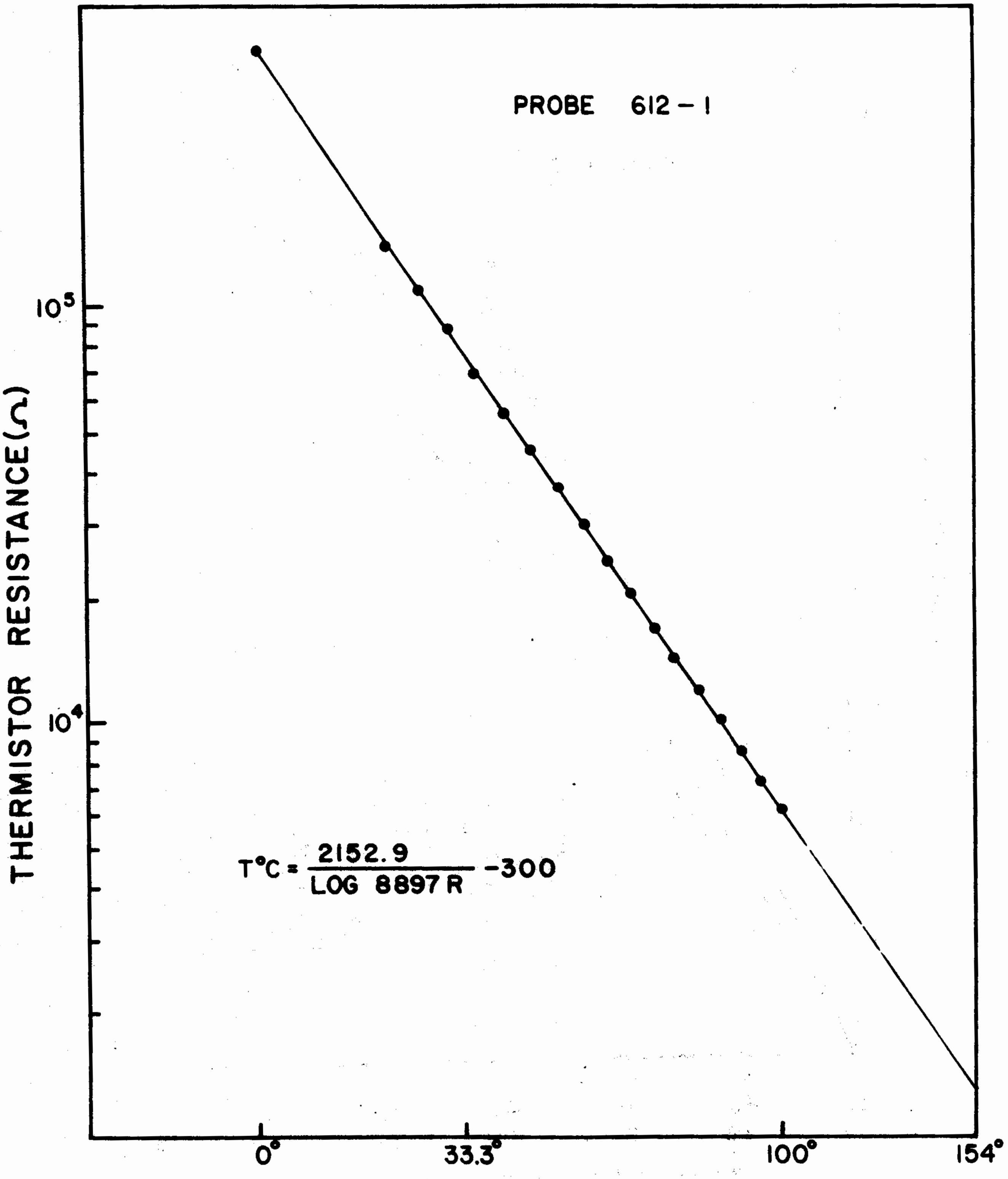


Fig. 2 - Thermistor Probe Linearization.

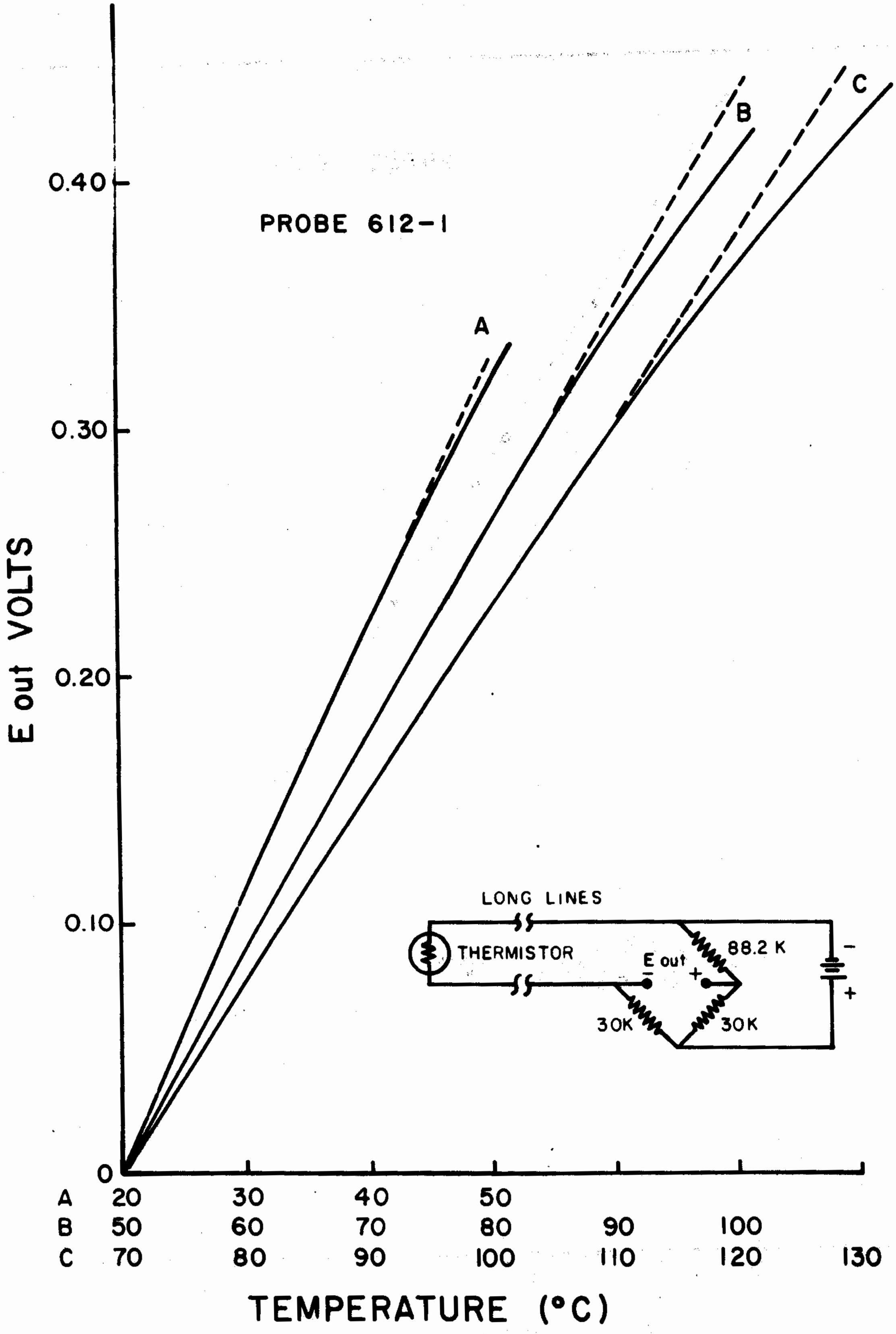
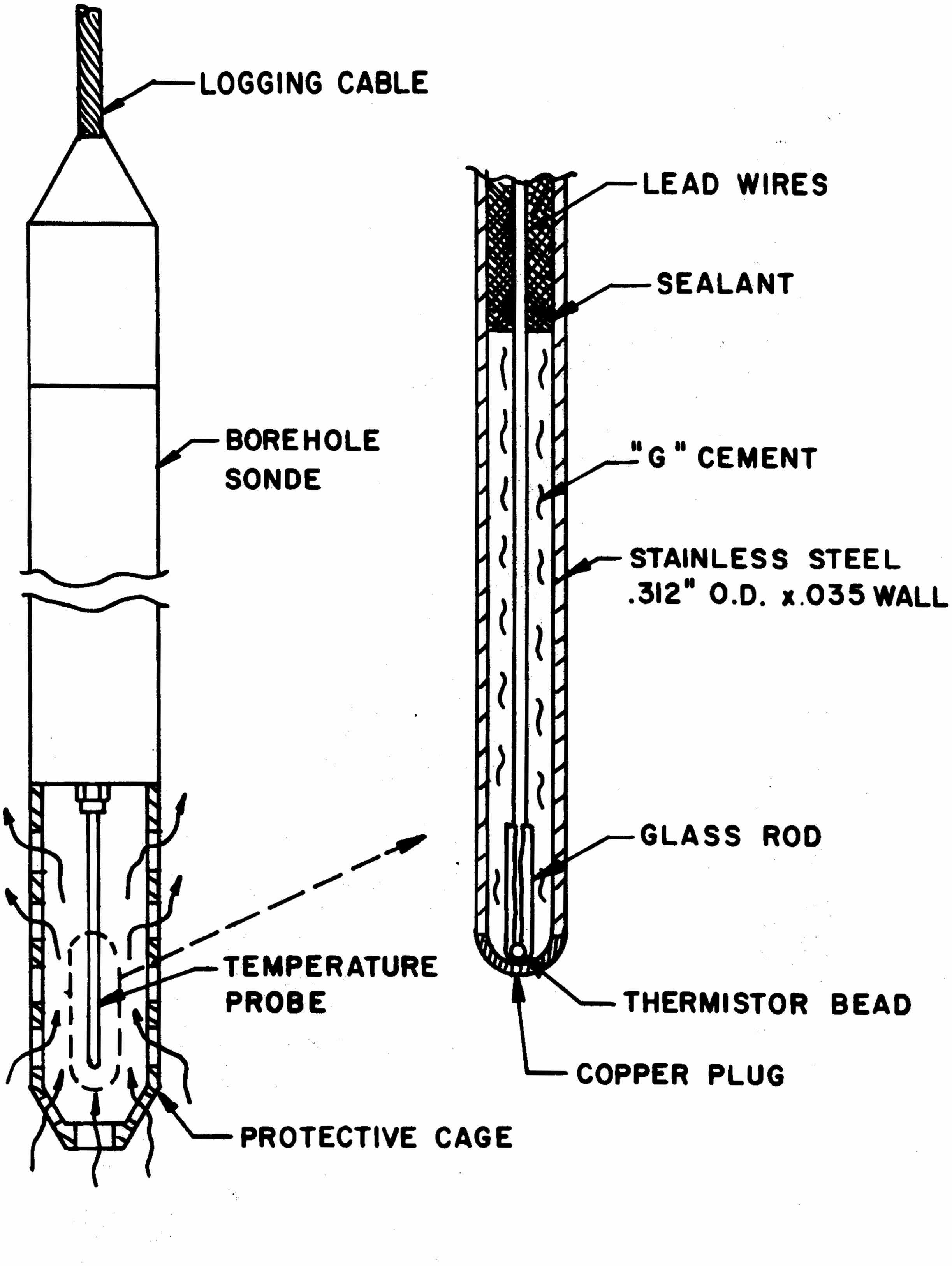


Fig. 3 - Thermistor Probe Bridge Network Calibration.



BOREHOLE TEMPERATURE SONDE

Fig. 4 - Borehole Thermistor Temperature Sonde.

343

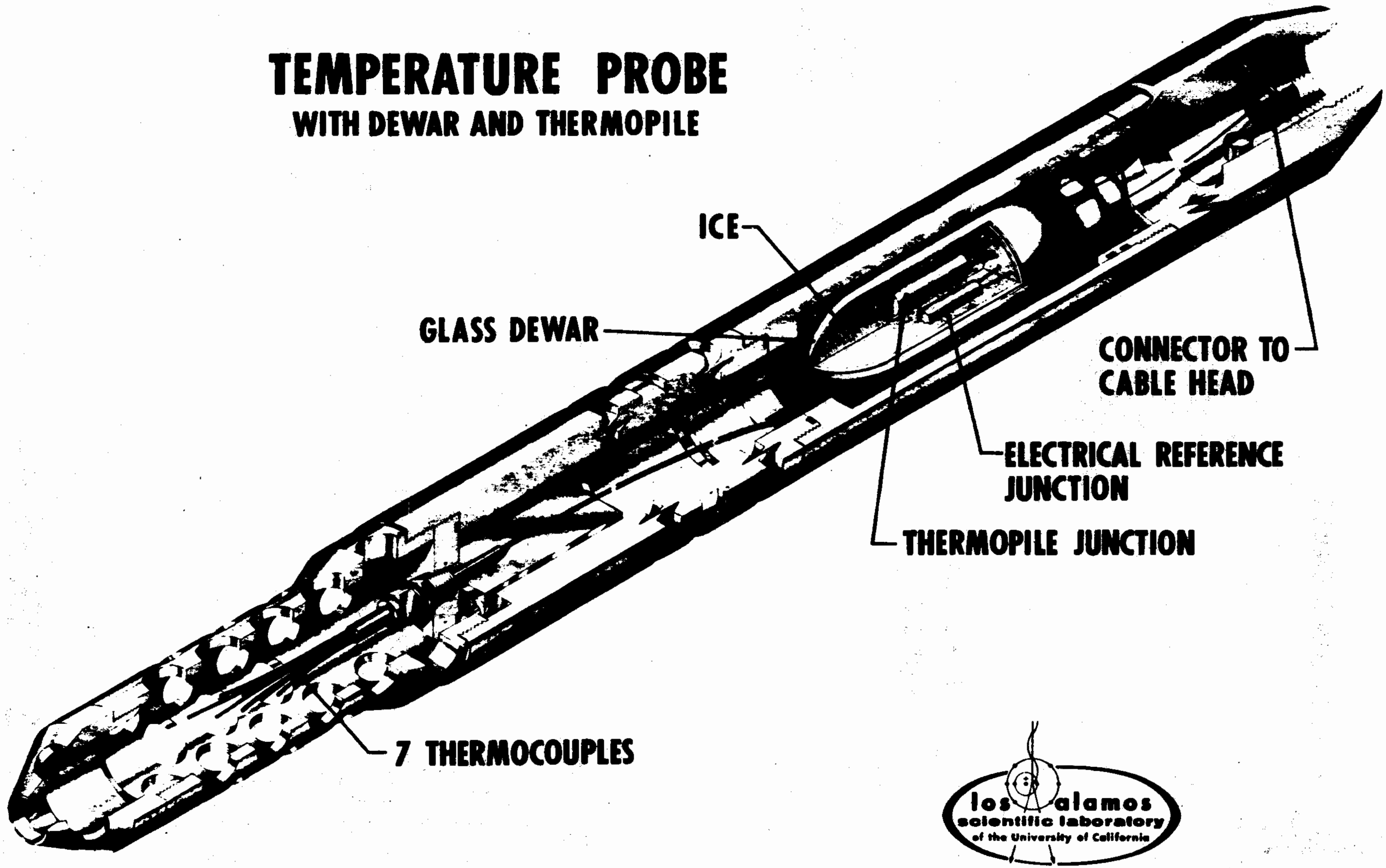


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Fig. 5 - Borehole Thermopile Temperature Sonde.

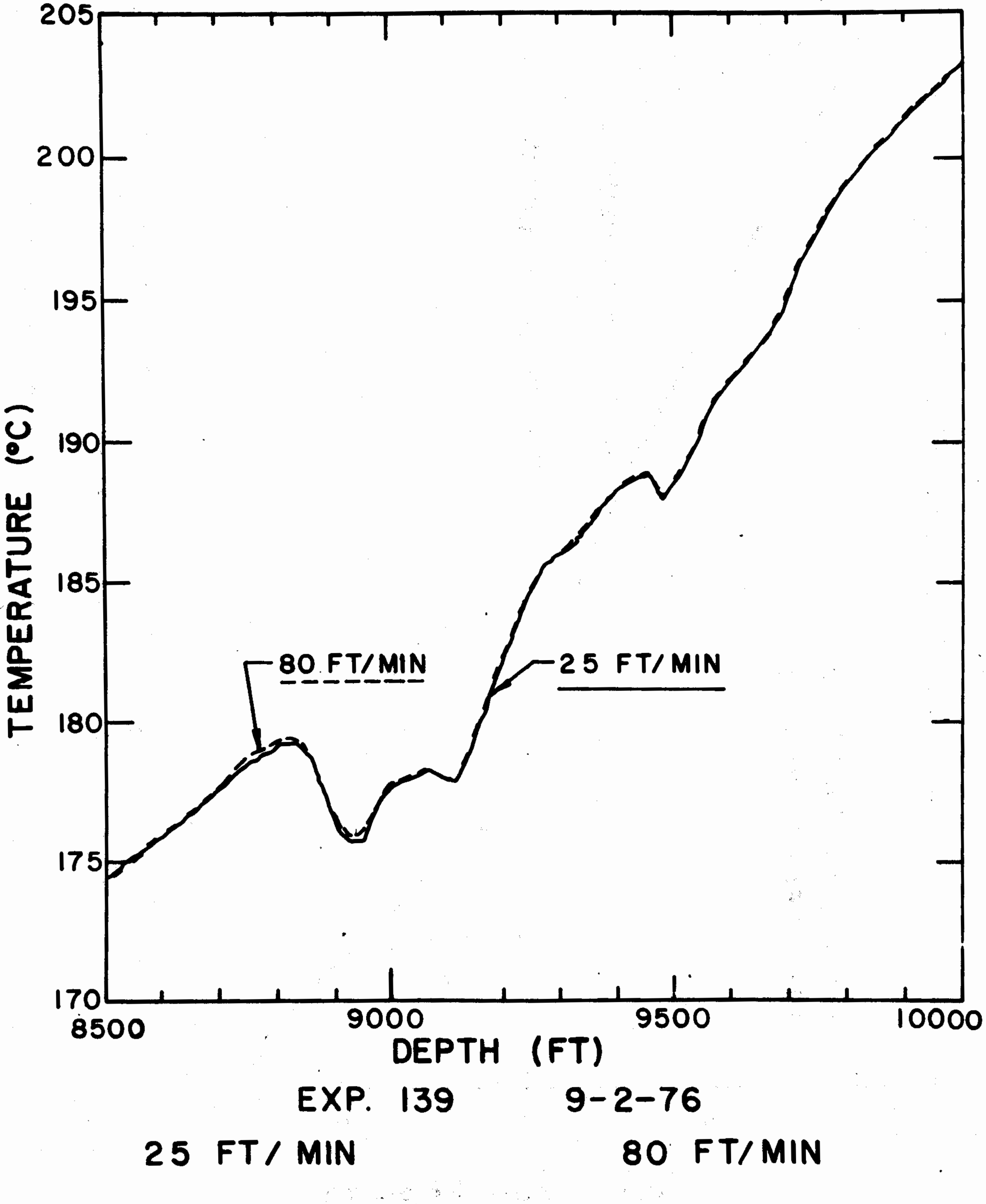

Fig. 6 - Thermopile Response vs Logging Speeds.

345

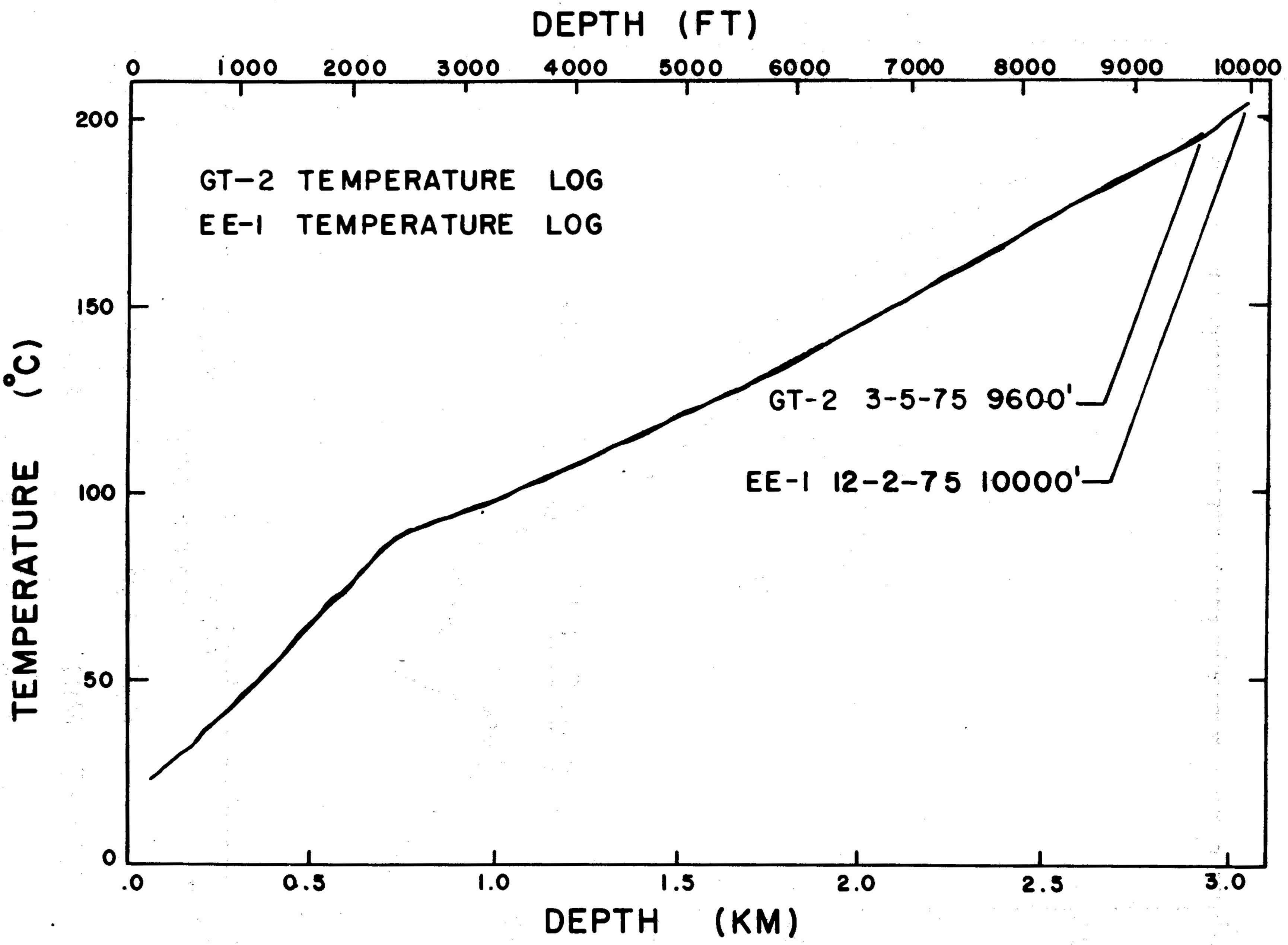

Fig. 7 - GT-2/EE-1 Borehole Background Temperature Logs.

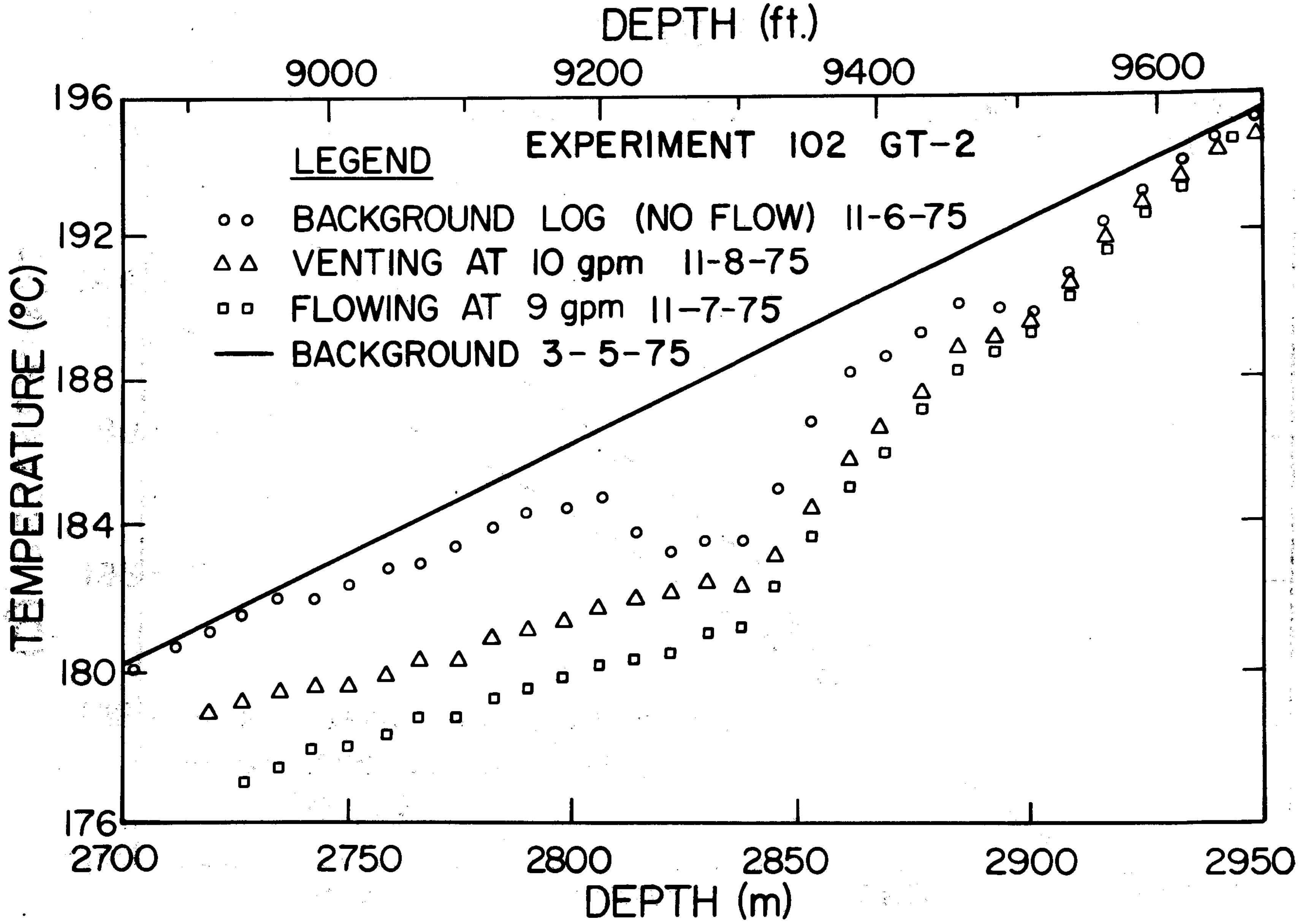

Fig. 8 - Temperature Logs During Low Pressure Flow Experiment GT-2.

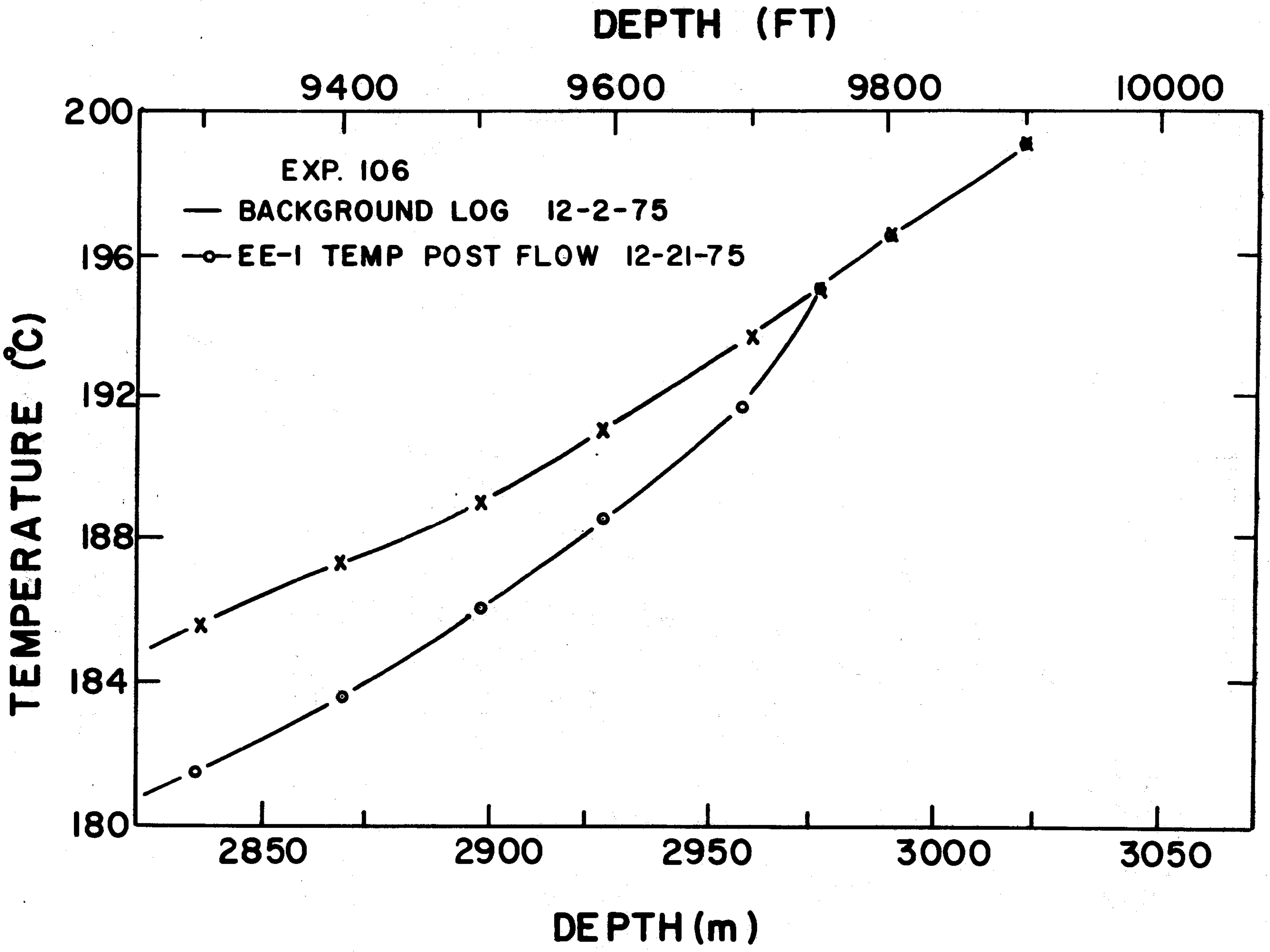

Fig. 9 - Temperature Log During High Pressure Flow Experiment EE-1.

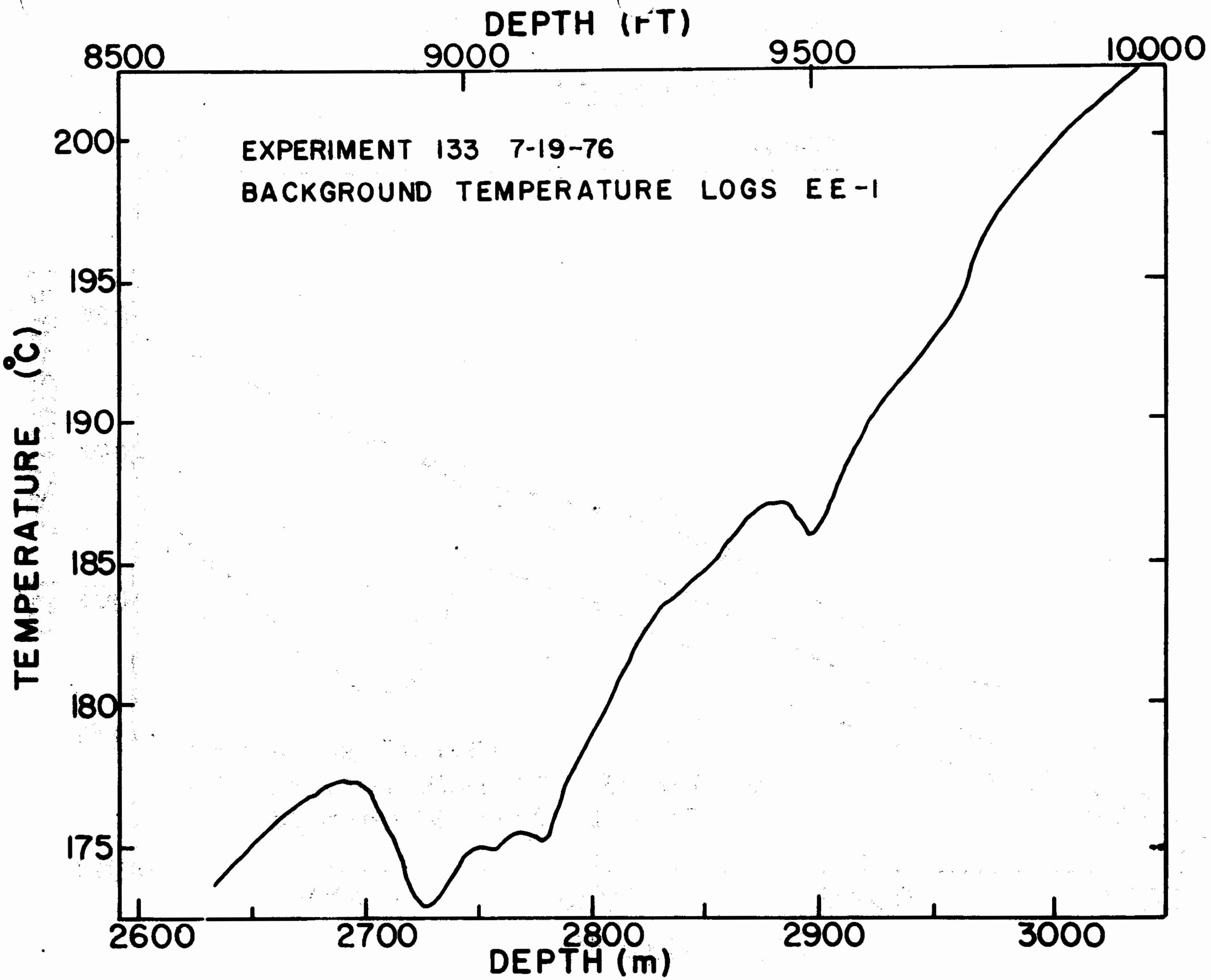

Fig. 10 - Pre-leach Temperature Log EE-1.

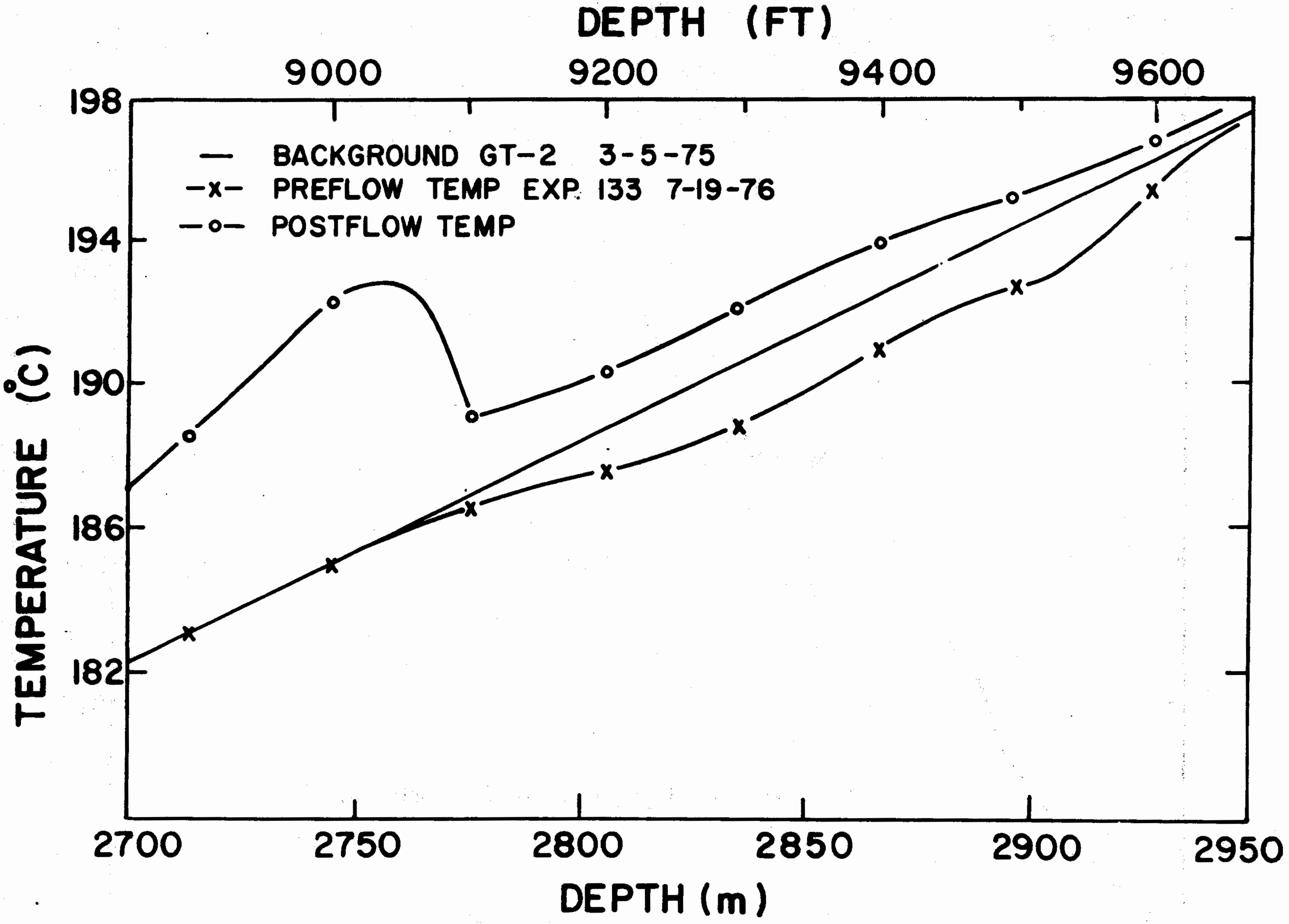

Fig. 11 - Pre-leach Temperature Log GT-2.

APPENDIX A

If the assumptions are made that the properties of the rock material surrounding the wellbore and the wellbore geometry are constant, then a relationship can be derived to describe the ratio of fluid velocities in the wellbore at different depths Z. By assuming constant rock properties and a constant wellbore radius, the ratio of water velocity U_2, at some depth Z_2 and time t to the velocity U_1, at a reference depth, Z_1, is related to the water temperature changes and water temperature gradients, $\bar{G}$, at these depths and time as

$$\frac{U_2 = T(Z_2) - To(Z_2) \quad \bar{G}(Z_2)}{U_1 = T(Z_1) - To(Z_1) \quad \bar{G}(Z_1)}$$

where the gradient $\bar{G}$ is an "effective average"[7] gradient. For short time tests with insignificant wellbore heat storage, a useful approximation for $\bar{G}$ is

$$\bar{G} = \sqrt{G(t)\ 1/t \int_0^t G(\tau)d\tau}$$

The above conditions were satisfied during the flow log of Experiment 102, which started 430 min after initiation. By choosing the reference depth Z_1 = 2790 m, the fluid velocities relative to the velocity at Z = 2790 m were calculated and are plotted in Fig. A-1. Changes in flow rate are clearly identified; about 80% of the flow leaves the wellbore in the interval between 2800 and 2820 m, about 15% leaves at 2870 m, and the rest leaves at the bottom of the hole.

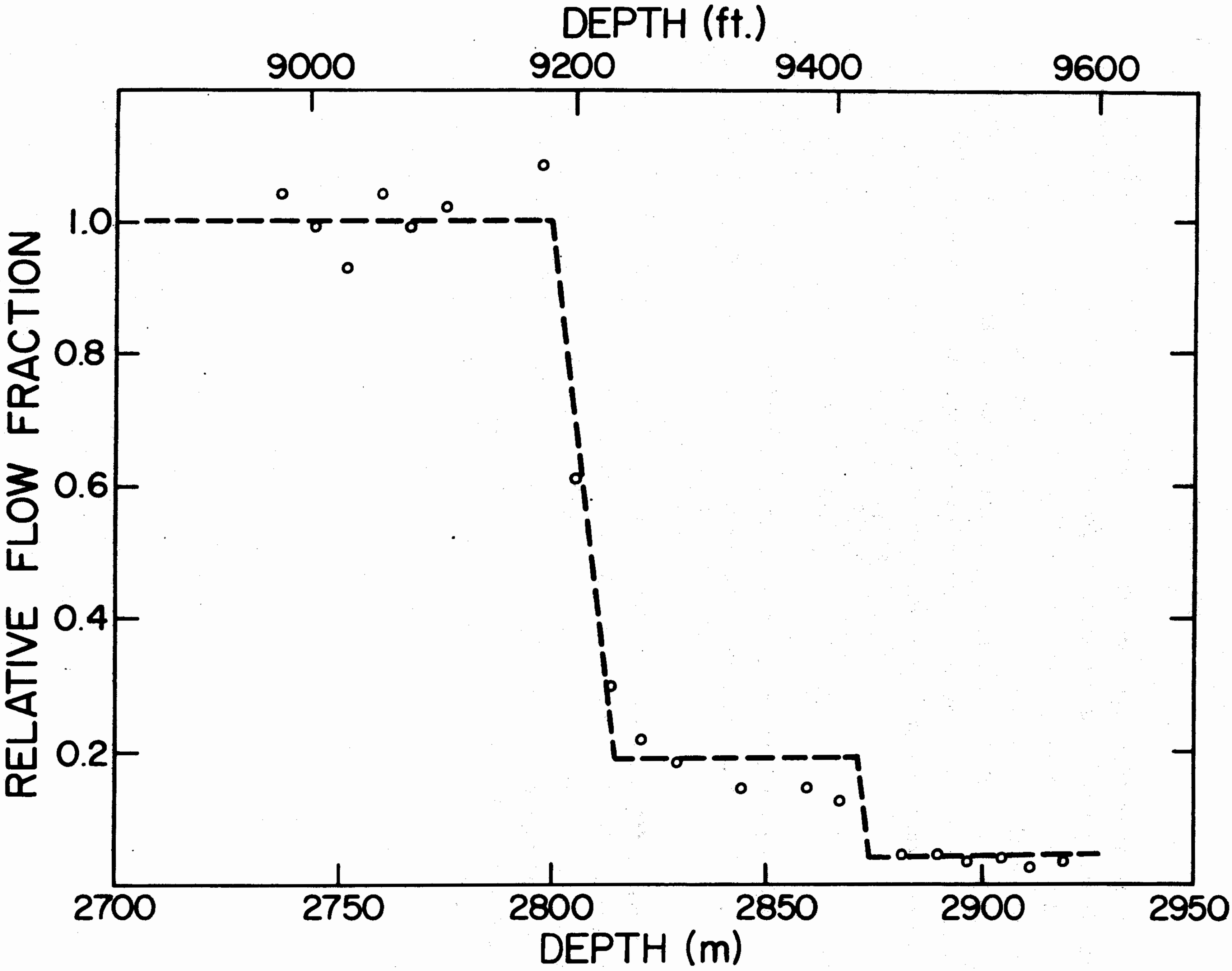

Fig. A-1 - Relative Fluid Velocities vs Wellbore Depth.

SESSION III

DISCUSSION SUMMARY

Session Chairman: John S. Hilten

Papers: Renick; Reed; Ping Tcheng; Holmes; Dennis, Stepani and Todd;
Waldman (for Doerr)

DISCUSSION:

John Hilten, National Bureau of Standards: (Announcement) We have a few copies of 3 tech notes generated since the last Transducer Workshop. I will mention their titles in case you would like to be on our mailing list to receive one of these.

1. Hydraulic sinusoidal pressure calibrator covering a frequency range of 1/10 to 100 Hz. The maximum level is 20 psi peak to peak with 100 to 8,000 psi bias.

2. Hydraulic sinusoidal pressure calibrator covering a different area, 20 Hz to 2,000 Hz, 20 psi peak to peak amplitude.

3. Technique for generating thermal transients in pressure transducers.

Paul Lederer, National Bureau of Standards: To the gentleman from the Parachute Test Range. Can you tell us something about how you calibrate the force links, dynamically or statically?

Aaron Waldman, Kulite Inc.: Yes, we calibrate at our plant statically using a regular material tension testing machine. We utilize the actual webbing of the parachute to act as a tension member. We utilize this link clamped to the tension machine. The webbing was attached exactly as it was going to be used in this chute. Tension was exerted on the webbing. We made a full calibration right up to 5,000 lbs in 1,000 lb increments. We set the gains of the individual amplifiers under test and then tested the completed system.

Lederer: How about the dynamic?

Waldman: Dynamic calibration we did not perform.

Lederer: How do you know what the thing will do during an actual parachute test?

Waldman: This is something perhaps that Joe Doerr was going to undertake at his lab. To my knowledge it has not been performed as yet.

Larry Mertaugh, Naval Air Test Center: You seem to have plenty of output. Is there any reason you went to semiconductors rather than wire gages?

Waldman: We used semiconductors because we manufacture semiconductors. That's one motivating reason. The other is the fact that the semiconductor gages are relatively small in physical size and the terminations utilize 1 mil gold wires as connections to the electrodes. It can very easily be packaged within that particular slot.

Mertaugh: Is there some reason you had to go to live people versus a dummy?

Waldman: Apparently some tests had been performed in the past utilizing dummies and there's some desire to have actual data recorded with the jumper. Unfortunately I don't know the background.

Henry Freynik, Lawrence Livermore Laboratory: Question for Bert Dennis, LASL. That background log, is that done with a dry hole or a wet hole?

Bert Dennis, Los Alamos Scientific Laboratory: Background log was done with a wet hole. The hole was always filled with water.

Mertaugh: Is that how you got the flow, by pumping the water? How did you insure flow through the fracture?

Dennis: Yes, we pumped water through the fracture system. Originally we drilled holes through a large aquifer in the volcanic sections of the hole, and that always filled the holes with water, but to circulate water in the fracture system we had to pressurize it. We used a surface pump. And that's how we are getting flow through the system.

Ken Cox, Naval Weapons Center, to Ping: You said you measured the temperature and controlled it. I was curious as to how you measured temperature in your cryogenic setup. How did you measure it and how did you control it?

Ping Tcheng, Old Dominion University: It has a temperature control device for the spot electric heaters, so it's automatic control. The temperature error is ± 2°F, that was the setting on the control device. For the other balance, using the cooling chamber, the temperature was controlled by regulating the nitrogen gas pressure manually. That requires a change about every 3 or 4 minutes. In that way we were able to control the temperature.

Cox: Did you use a thermometer?

Ping: No, we used IC thermocouples.

Lederer, to Mr. Renick: You mentioned that you had some problems in your solar stress gages with the polyvinylidene floride. Can you tell me what those problems were?

Joe Renick, Kirtland AFB: When we were looking at applications for soil stress, we did detect a very strong bending response and a very strong shear response. It also had a temperature response but you could probably live with it. So this is what really drove us into the packaging approach. There were two options. One was to go to either a very extensive characterization program which finally would have resulted in going back to NBS and saying, hey, we need you to really do some more work on helping us to understand this material and what its response is in these different modes. The other option was to go to some sort of packaging technique which would isolate the device from these particular undesired inputs.

Freynik to Dennis: Would a metal resistance thermometer have been a candidate for that temperature measurement?

Dennis: We would have to carefully measure the change in resistance of the leads along the logging cable. When you log over that gradient, that resistance change, it would affect the reading of the resistance thermometer. The thermocouple has a real fast response and was very adequate.

Freynik: Could you have used a constant current supply meter?

Dennis: Yes.

Walt Kistler, Kistler-Morse Corp., to Ping: You mentioned that the cold temperature hysteresis did increase in your measuring string. Can you tell us why the hysteresis increased? Was it due to the properties of the metal, or was it due to the strain gage, or to the bonding agent?

Ping: We don't know where it came from. We know it is there. Any mechanical/electrical device has hysteresis. It is there and we don't know how to handle it. As long as it is accountable, there's no problem.

END OF SESSION III

SESSION IV

MANUFACTURER'S PANEL DISCUSSION

Peter Stein, Chairman

(No papers given in this session)

NINTH TRANSDUCER WORKSHOP

Transducer Committee
Telemetry Group
Range Commanders' Council

APRIL 26-28, 1977

MANUFACTURERS' PANEL

Wednesday, April 27, 1977
7:30 pm - approx. 10:00 pm

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SESSION IV

MANUFACTURERS PANEL

Session Chairman: Peter K. Stein

Panel: Toy, Guthrie, Hines, Steel, Kistler, Diercks, Lally, Easton,
and Chelner (No papers presented)

DISCUSSION:

Pete Stein: If the manufacturer's representatives who were invited by Pierre Fuselier to participate in the panel would come up and present themselves as targets? Now if you would like to know who has been invited by Pierre to participate in the Third Workshop Manufacturer's Panel, there's a green sheet with everyone's name, company affiliation, address and phone number as one of the hand-outs. There have been three alterations since this green sheet has been published. Representing the Bell and Howell Co. will be Mr. Bill Winters rather than Bob Cheney, as noted on your sheet. Al Kutside from Strainsert was unable to attend and in his place Walt Kistler, President of Kistler-Morse was invited. The phone number listed for Frank Hines is incorrect, the correct number is (603) 882-5195.

Pierre Fuselier, Lawrence Livermore Laboratory: Pete, will the panel member stand up and identify himself first?

Stein: Yes, they're each going to introduce themselves. That's part of the program. I was going to give the audience just a little bit of background on these manufacturer's panels. Up until the '72 conference manufacturers were not even encouraged to attend these meetings. Is that a fair statement, Pierre?

Fuselier: That's correct.

Stein: In fact, they weren't permitted to attend. In the '72 workshop in Albuquerque the innovation was prepared that certain manufacturers would be invited to send a delegate to be a target up here, and you could throw questions at them that they would promptly respond to. Because it was thought at the time that one might develop a sort of ping-pong game back and forth, that might get out of hand, a moderator was invited to keep things under control. I'm very proud to be here for the third time as the moderator. The program is as follows: each manufacturer will introduce himself and give a brief 5- or 6-minute summary of his part in the transducer field.

I'm Herb Chelner, President of Senso-Metrics, Inc., Van Nuys, CA: Senso-Metrics is equipped to develop, manufacture, and test transducers, signal conditioners and data acquisition systems. Our 6,600 square feet of facilities are located in the San Fernando Valley area of Southern California and are well equipped for general purpose contracts. Presently in production for the low cost commercial market is our SP 65 semiconductor pressure transducer. The SP 91 is another commercially oriented pressure transducer which is available in a number of options and at a low cost. Just delivered is our SP 66 pressure transduction system containing a state-of-the-art E-Pak meeting MIL-STD-883. This high reliability, high performance and low power drain transducer operates from unregulated 28 V dc and maintains .1% F.S. accuracy and .1% F.S/100°F temperature specs. Presently being developed is a circuit board transducer capable of being soldered directly to a circuit board. This unit is activated by a ± dc power source and provides a high level signal. It has application in low range OEM equipment such as air conditioners, heater controls, etc. Senso-Metrics provides signal conditioning packages to support the transducer lines such as the two wire current transmitter. Having personnel possessing a great deal of experience in the systems measurement field, custom designs or modifications to present equipment is available to meet customer requirements for custom data acquisition, display and record systems. Being experts in the semiconductor strain gage field, gaged components may be furnished such as load links, force rods, bending beams, etc. Bending beams were designed and furnished for use in holding bearing races for making dynamic bearing friction measurements in air or vacuum. Such custom instrumentation is readily provided.

I'm Chris Toy of Bell & Howell, not Bill Winters. Bill called yesterday afternoon and said that I should be here, so I can guarantee my speech won't be anywhere near 7 minutes. Bell & Howell purchased the CEC Inst. Div. of CEC in 1960. They now enjoy a very wide transducer product. They utilize different sensing techniques for many different types of transducers which include bonded foil strain gages, unbonded wire, capacitance sensors, and sputter deposited thin film strain gage. The transducer types are absolute, gage, differential; just about anything you need. However, if you asked us to make something, you probably found out that the answer was no. As it is with most transducer manufacturers we just can't do everything that everybody needs to do and that's why you're here at this meeting now. If you have any questions I'll try and answer them. Thank you.

I'm Jim Guthrie, General Manager of Flow Technology in Phoenix, AZ. We are about 65 people who have devoted our total efforts toward the manufacture and calibration of turbin-type flowmeters and the accompanying electronics. We specialize mostly in liquid, rather than the gases; however, we do build transducers for natural gas, propane, and nitro-helium. We are presently about 75% commercial and about 25% military-aerospace. The company started some 13 years ago with 100% aerospace. As years have gone past we have been not necessarily pushed, but we have kind of found our way into the commercial market because a lot of the things we learned in the aerospace and government areas are very much applicable to the commercial field. The only difference is, in some cases, the paperwork doesn't weigh quite as much as with the military-aerospace. The products cost more to the commercial people because generally there's not any competitive bidding. We kind of like that.

My name is Frank Hines, I'm with the RdF Corporation, Hudson, NH. We've been in existence now for 25 or 30 years. Our primary product is temperature sensors of one kind or another. We have a rather extensive line of probes for general commercial and industrial purposes. We also have some mil spec probes for Navy use, nuclear submarine use. Many of our probes are special. In fact, I believe we make more specials than we do standards. The brochures I've given you are really representative of our capability more than our total product line. In that you'll also find heat flow sensors, which are basically a differential temperature measurement. They can take many forms. We also build calorimeters and have done a great deal of heat-shield instrumentation for the aerospace industries. We're still making calorimeters and low profile calorimeters, radiometers for aerospace work. We're divided, roughly I think, about 50-50 at the present time, commercial and military-aerospace type of work for which we hold full qualification of quality control procedures. Thank you.

I'm Brian Steel. I think I might need an interpreter, being a foreigner in these parts. I'm representing Schaevitz. Schaevitz is sort of world-renown for its LVDT displacement range which covers the entire spectrum from cryogenic to high temperature radiation proof subminiature to many 10 to 20 ft. strokes. In more recent years they expanded their activity into closed loop accelerometers. We also have an activity on load measurement and LVDT pressure. I'm really representing the recent facet of Schaevitz where they've gone international. Some three years ago they purchased the UK Company, of which I'm the Managing Director. Our prime activity in the UK is to design a range of strain gage pressure transducers. We also make open-loop accelerometers and closed-loop accelerometers and some displacement transducers. In terms of size, we're about 200 people in New Jersey. The UK operation is about 100 people. Schaevitz was formed in the late 40's, and the company that was purchased by Schaevitz in the UK had its birth about the same time. The pressure sensors, which is my prime activity over here in the USA, were originally started in 1971. I think that's about enough for me.

I'm Walter Kistler, President of Kistler-Morse Corporation. I used to be affiliated with a different company, as some of you may know. My company now is a small company located in Bellevue, Washington. Bellevue is a suburb of Seattle, and Seattle, for those of you who don't know, is probably the nicest city in the United States. It's surrounded by woods, by lakes, by fjords, by mountains with nice white snowy peaks.

It's a beautiful place to live, at least when it doesn't rain. As far as our products and our company go, it's a small company, about 20 people, in existence for about five or six years. The product is a line of semiconductor transducers. Many people today are in semiconductor transducers. We make or are going to make transducers for pressure, force, acceleration, and displacement. We use a special element consisting of a very small, fine steel beam with semiconducted fibers fused in glass on either side of the beam and when the beam bends one fiber is stretched, the other compressed. Right now our main item is an extensometer. An extensometer is basically a displacement transducer for very small displacements, over a thousandths of an inch down to a microinch or less. It is used to measure the strain in structures, so it's in a way a modified strain gage. You mount them on the structure and you can measure the strain and from the strain, of course, the stress existing in the structure. We use it to measure the weight of large containers and measure the contents of the containers by measuring the small compression of the structure that carries this container. That's about it. Thank you.

My name is Allen Diercks. I'm the Chief Engineer at Endevco. We're celebrating our 25th year this year. We primarily manufacture accelerometers. Our primary product is a piezoelectric accelerometer which comes in a wide variety of sizes and shapes; small devices that are less than a quarter of a gram in weight, up to relatively large sized devices that have hundreds of pico coloumbs per g output; devices that can operate at cryogenic temperatures or temperatures up to 1400°F for use in things such as sodium cooled nuclear reactors. In addition, we produce a variety, but a much limited variety, of piezoresistive accelerometers, dc responding accelerometers, using a silicon strain gage that's proprietary to Endevco. We have recently, within the last 18 months, brought out a line of piezoresistive pressure transducers that are encased in a relatively small 10-32 thread encasement. We also produce a variety of electronic signal conditioning equipment to accommodate the transducers that we manufacture.

I'm Bob Lally from PCB Electronics. We're a medium-sized company and an offshoot of the original Kistler American Company. I would like to say that Mr. Kistler here is the father of many companies, including ours, and I'm very proud to have been associated with him for many years. The products we manufacture are basically quartz stress gage transducers, and they are used to measure dynamic phenomena, pressure force, and motion. They're used to test the behavior and monitor the health of things. Those of you who were fortunate enough to take the tour at Eglin Air Force today, to see the calibration lab and the many interesting dynamic experiments that were conducted, had an opportunity to see many of our products in action. I would like to say that facility impressed me very much from several aspects. Having all that equipment operating and demonstrating gave the people here an opportunity to see the numbers and the signals, the results. It gave me a tremendously good feeling. I'm very proud of our contribution to it and I can appreciate the amount of work and effort that went into the facility. Now as far as the products go, we have a kind of a special product that has been quite successful and the brunt of many jokes, that is our electronic hammer. I don't know how many people here have used it or been involved with it, but it's basically an instrument or tool for testing the behavior of structures. As the result of this involvement and the feedback we get from customers, we've applied many of the same techniques to our own transducers' structures. We've been able to find how they function, actually how they work. We get insight into the behavior and some really detailed knowledge of the structure; the idiosyncracies of the structure. The point I'm making is as a result of this meeting, the feedback we get here, and the interaction with the customers, the PCB Company benefits tremendously and we appreciate it. Thank you.

I'm John Easton, President of Sensotec, Inc. We're from Columbus, OH. If you held this conference about the third week in January, the entire company would have been here representing. Sensotec manufactures a very broad line of strain gage transducers. We manufacture a tenth percent transducer, a quarter, a half and a one percent transducer gage, absolute or differential. In gage transducers we make a true gage as well as a standard gage, a differential, a wet-wet, a wet-dry, and an absolute and a sealed version of transducers. In addition to what you might call the more conventional type transducers, we manufacture a line of subminiature pressure transducers. These are 1/8-inch diameter types up to 3/4-inch diameter. In addition to that, we make load cells that range from 50 grams up to about 500 pounds in probably about the size of a thimble. We make the more conventional type strain gage load cells that go from 5 pounds up to several million pounds. We also make a line of signal conditioning amplifiers that basically readout the strain gage signal conditioning, provide function modules and amplifiers, in what you would call a ground-based or a base system. In addition to that, we make the internal electronics which go inside the transducers. We also manufacture a very small transducer that's about as thin as 4 or 5 postage stamps. This was an invention in the aerospace industry. We have others I will talk about later.

Pete Stein: Although Pierre didn't specifically invite me, Stein Engineering Services, Inc., is in its 27th year, incorporated in 1959, in the state of Arizona, started in 1950 at MIT. Along with the other manufacturers who have brought their literature for you to pick up, I have brought my new facilities brochure. We make no product, we offer a service; education, publication, and consulting, and it's described in the brochures. The door is now open for questions that you may wish to place to the panel, and Paul Lederer has the first question.

Paul Lederer, National Bureau of Standards: There is increasing activity in international standards at OIML. Have you people ever given thought to forming an International Trade Association for transducers manufacturers?

Diercks: To answer your direct question, the answer is no. But I have been in contact with CESTA in France which is sort of an equivalent to NBS. We will start certifying our calibrations through CESTA as we do through NBS.

Stein: Other panel members?

Easton: Well a comment that I would say without giving a commercial plug. M&D I believe started M&D magazine, which I'm sure most of you have seen. For the women, that's the one your husband brings home. He spends a great deal of time looking at the metric dimensions. But in any event, as far as I know they did try to pull together a pseudo, which you can call a manufacturer's panel. This is probably as good a representation of the transducer manufacturing group as you could get, and yet we all have such a diversity of products and uses that the commonality from a manufacturing standpoint does not exist. It almost exists from a transducer standpoint industry by industry. That is, the petrochemical transducer manufacturers would have a common reason for getting together as compared to strictly transducer manufacturers who might market to petrochemical aerospace, industrial, etc.

Steel: About three years ago, UK went into what is known as the Common Market. I thought as a manufacturer I would welcome the possible situation where we get some commonality in definition of specifications. So I looked into what had actually taken place in the EEC, and it transpired that during or since the formation up to three years ago, 20,000 different standards for test and measurement had been put forward. Only one had ever been passed and ratified and that was for the measurement of power electricity, which I think just indicates the complexity of trying to get a multitude of operations and nations all to agree on any one factor. Whether we will ever get to really worthwhile international standards on test and measurement universally, I doubt. We should all work towards that end, but I think it's a long, long way off.

Stein: Don't think we could even get 10 people to agree on how to define transducers.

Larry Sires, Naval Weapons Center: I had an opportunity to buttonhole Bob Lally on the way down here on the airplane and he explained something that was very valuable to me. I think it might be very valuable to the others, and I'd like to have it stated for the record, or at least have some comments for the record. It deals with the zero shift on piezoelectric transducers, the things that he's found out about it, what the possible causes might be, and if he cares to, offer some of the solutions for this problem.

Lally: Well it's this problem of zero shift in transducers in general that's been with us for a long time. In fact, it was in 1972 at the Albuquerque meeting there was a paper given by Sandia. A very learned paper that went into the deep technical problems in certain types of ceramic crystal transducers which exhibited gross zero shift effects. The point there was that it varies a lot with materials, different materials, and it's inherent in some materials and not in others. But the quartz crystals don't experience zero shift within the crystal at all. Actually, nature has provided us with a near perfect transduction element. So we have to mount this in a structure and we have to encase it. We're going to package it, as one of the papers pointed out today. We have to design it or build it to overcome imperfections that go into making surfaces flat, etc. We end up with a practical transducer, a commercial transducer. We find that it's highly preloaded, for one thing, and there are several other factors involved with zero shift. It is a highly stressed electromechanical structure and it's not too unlike a person who works under high stress and the strain shows. This is what happens in the transducer under shock loading. We can say take half a dozen transducers, statically calibrate them and match them identically. We can then apply a step function, say a millisecond rise time, and we'll get identical results. Then we put them in the shock tube, all at the same station, fire the tube and we get six different answers. This is the type of problem that Larry is encountering. Not only do we get six different answers, the transducer signal level will end up with six different levels, some plus, some minus, some right-on. The amount of this effect varies anywhere from less than 1% up to 15 to 20%.

It's just that under the shock loading, this highly stressed structure doesn't return to the same residual stress pattern as existed before it experienced the shock. Lately, the problem comes to light largely because of computer technology. People are starting to integrate the signal and double integrate the signal. An offshoot occurs. It goes from one extreme to the other. So when you're just looking at the waveform it's kind of an annoyance, but people accept it as state-of-the-art. Recently, we have taken a detailed look at this behavior as a result of some of the structural analysis that I mentioned previously and we find that along the axis of the instrument we have a high preload. This contributes a little bit, but not much, to the zero shift. We find that we have a number of interfaces, like a sandwich, of crystals and metal. There was another paper presented today that mentioned crystals are sensitive about many axes. In fact, one axis in the cross direction was very sensitive. We find that this is largely the culprit in zero shift of the transducer structure. We investigated various methods of lubricating that interface with different materials. In some recent results of a project with Sandia on high shock accelerometers, we have, for the first time, been able to grow a quartz structure that has so little zero shift we can't detect it in our tests. This is very encouraging to us because now we are taking this technology over into the pressure and force transducers. It's basically dealing with the structure at the interface. There are other contributing factors to zero shift, mostly in the quartz packages. If you have a large signal and you are looking at the small stuff, zero shift in accelerometers in the order of one percent or less are significant. We feel we have had a major breakthrough and we can make considerable improvements in that area. There are a lot of other contributing factors to zero shift. Often it's just a temperature response of a transducer. The transducers are sensitive to all factors and if temperature effects aren't delayed, they can cause an apparent zero shift. Another cause of zero shift is insufficient time constant. If the discharge time of the transducer is short you've got a long event; you're going to get undershoot that appears as zero shift. We've encountered problems in the electronics just at the ac coupling circuit, capacitor coupling circuit. Real capacitors can contribute significantly to zero shift. We also, if you recall the electronic circuits, the signal conditioning circuits that go with the piezoelectric crystal transducers use an insulated gate field effect transistor, a Mossfet. Now these are subject to many peculiarities. They can contribute very much to zero shift if you overdrive. If they are within the linear range, they are very good, but if you exceed the supply voltage you will get zero shift. Very often this happens and you don't even know it. There is a problem today even in the soil stress gages and certain pressure gages where the initial shock load comes very fast; faster than the transducer can respond. They have a very fast spike on the leading edge of the blast wave. You saturate the electronics and you don't even realize it. This can contribute to significant zero shift. There are many factors involved, but from the structural transducer we've made a lot of progress and these other factors, just as Professor Stein teaches, is just knowing the transducer and knowing its behavior.

Stein: Allen and Walter, do you want to tackle that?

Allen Diercks: We have identified that the polycrystaline materials, if you will, are more subject to zero shift than the single crystal materials because they are basically softer. They give you more output, but they do have the disadvantage of zero shift. There are a number of single crystal materials that look like they could be applied satisfactorily in the high shock areas. However, beyond that I'm not sure what I can contribute to what has already been stated.

Walt Kistler: I mentioned I used to be connected with piezoelectric instruments and now I'm connected with strain gages, and there is no doubt that the problem of zero shift was one of the major problems we had with piezoelectric sensors. As Bob pointed out, we couldn't quite understand it because theoretically quartz crystals should not generate zero shift and, well, it was still around. In general, I think my experience was along the lines Bob pointed out. There are many outside effects which seem to all cooperate to give you problems which makes it all the more difficult to really get down to what the problem is. My opinion, and what I usually felt was the cause, is that we have a package of many parts all heavily preloaded and if anything shakes this package, creating the conditions of a shock, it's like hitting a sensitive instrument with a hammer. You can expect something will happen, something deleterious. The parts will thus shake and end up in a very slightly different position than they were before the thing happened, and, therefore, is always on a different level after the event than it was before the event. This means that you don't know what the peak really was; should you count the peak from the zero line after the event or should you count it before the event. It's a very unpleasant and very annoying effect. We struggled for many years and we were never quite able to solve it. PCB now has made progress along this line, I hope this will prove out to be the case.

David Ray, Kirtland AFB: We had the interesting experience about a year and a half ago of hearing a paper delivered by someone from SQ, La Jolla, in which they had taken piezoresistive accelerometers and repeatedly shocked them and eliminated large portions of the zero shift. I wonder if perhaps some of the people on the panel would say if they think this thing could be done with piezoelectric, and if they know why the same thing happens with piezoresistive?

Stein: Maybe each of the panel members should get a chance to talk about zero shifts due to shock and impact. It's really a general topic of considerable interest. Allen, do you want to start on that one?

Diercks: We did a lot of work in trying to determine whether or not repeated shocks could reduce the zero shift from transducers. Our experience was if you took a transducer and repeatedly shocked it where the shocks were one shock closely followed by another, there was a decrease in zero shift. But if we put it on the shelf three days, the zero shift came back to something close to its original value. So far as any practical use, it didn't provide any. That was piezoelectric. Our experiences with piezoresistive zero shifts were primarily associated with the use of epoxies. If one stresses too highly a polymer joint, if the bond area is too small or the epoxy thickness is too great, then you'll get some wierd phenomenon, some of which show up in the output as a zero shift. But by keeping the bond area to reasonable dimensions and by keeping the epoxy thicknesses thin and by curing the epoxies properly, or better still by avoiding epoxies entirely and using something like solder, you can avoid zero shift effects in the piezoresistive type of transducers.

Ray: Have you tried repeatedly shocking piezoresistive?

Diercks: We have tried, yes. The Pixy gages we use now, if you'll let me use that brand name, the original Pixy gage had a very small bond area. It was dimensioned something like .020 by .030 of an inch. The Pixy dies we now manufacture have a bond area that is closer to .040 by .050 of an inch, although the neck of the die is the same dimensions. So as far as the resistance that is under stress is concerned, it's the same. But the bond area, the part of the die that we actually have to attach to the structure, is much larger so that the forces are distributed over a wider area. We find that very successful.

Kistler: If I may make a quick remark in the piezoresistive semiconductor strain gage instruments? We have found zero shifts also. If you exercise a newly built instrument from zero to full scale and back to zero and not repeat exactly, then you keep exercising from zero to full scale, it will come back to the same zero. But if now you go to a temperature cycle, the zero will shift back to where it was originally and again the first excursion will bring you to a new zero. Now, we do not use epoxies or organic compounds to connect our gages. We use fused-in glass. We have nothing but silicon glass and steel substances and we still find this effect. So we feel it's probably part of the quality of the semiconductor. I wonder if any of you other people have found that similar effect of semiconductor sensing elements?

Stein: Herb Chelner has a word to say.

Chelner: In accordance with my preshortened characteristics of our transducers, I'm going to kind of short circuit a little bit of this discussion. Assuming that someone knows what he is doing in making a semiconductor gage and bonding it, then zero shift, with respect to shock, is strictly a function of how much you are yielding the material you are on and the type of material you have. Shocking essentially work hardens the material and should make it more stable.

Stein: Chris, do you want to tackle that?

Chris Toy: I think Bell and Howell's answer to getting rid of zero shift is to prestabilize everything before it's even attempted in manufacture. Our best attempt to make good stability in transducers has been in going to the sputtered depositions technique where you don't have any bonding agents. I think that in 90% of the cases, zero shift has to be blamed on bonding agents and the attachments, but not necessarily the sensors themselves.

Frank Hines: I don't know if this applies to this particular application or not, but my recollection of strain gages is that if you look close enough at any elastic material, there is no elastic limit. If you can amplify the measurement well enough and observe close enough you'll find there is no such thing as elastic limit; no such thing as hysteresis-free material.

Stein: Do you want to make a comment on zero shift? I think you would be eminently qualified.

Aaron Waldman, Kulite Semiconductor: We found a tremendous improvement in zero stability by going to inorganic bonding where the space on a particular transducer permits. The utilization of organic adhesives requires a lot of patience and a lot of skill in curing and applying, and, of course, maximizing bonding areas improves stability. With respect to pure shock zero shifts, I personally have not experienced any great difficulty with our transducers to date.

Stein: Hsu also wants to say something about zero shift.

Peter Hsu, EG&G, ID: I do work with a group of people whom I call scientists and/or engineers. One of the things we do, because none of these learned gentlemen would do it for us, is build our own pressure transducers, LVDT's and whatever else you can think of. I did have a charter to digress over the last year or two, to commercialize. I have a budget of $40,000 to commercialize. We couldn't get anything that worked. We did get one pressure transducer from Kaman Sciences, and that doesn't work for another reason. Let me digress on this zero shift, we do run into this all the time. We have a most hostile environment. We go from ambient to 650°F and/or 1000°F and plunge the thing right down. That usually kills everything under the sun. The first five units we built were literally hand hewed and had no zero shift. We ran the reactor and everything worked. We need 50 of them in the next year or so or 100; whatever. Well, we can't ask this PH.D. and a scientist to build it. They said let's give it to people who know how to build them and that's where the problem started. Now we're talking about highly trained technicians still under the supervision of engineers, not a production item. Along with all these other things that's been said I think we agree. We tried cooking them and this helped some. Then when you shut the oven off and try it again it takes off again, and so cooking doesn't seem to help too much. We do believe that the warping of the strain post, if you will, or mounting structures, do cause problems. I do want to throw out to the panel: maybe these devices are so delicate that the conventional method of production doesn't work? In other words, take a well-trained, dedicated technician who can build five of these the first time around and they work. Then I have great difficulty understanding why the production people cannot make them. We have tried very hard over the last year, since I become involved, to make them repeatable and none of them work.

Stein: Tom Piper gave an excellent paper on pressure transducers at the meeting in Idaho of the Western Region Strain Gage Committee. Those proceedings, for those of you who are interested, are available from the Society for Experimental Stress Analysis. They contain some really excellent material, including the one from Jon Inskeep and a fellow from Statham. There were two papers presented at the WRSGC a few years ago that had much the same story to tell as you do. For the outer space effort they made a series of transducers where every part was tested and double checked before it went into the transducer. This made the thing impossibly expensive, but all of them worked. Your comments are very well taken. To all of the things that have been said you have to add the personality of the individual. Allen, you have another comment?

Diercks: It's very hard to generalize about a design that you have never seen, but there are two things that are critically important in the types of transducers that we make. One is surface finish. When you mate two surfaces together they have to be as flat and mated as perfectly as possible because any irregularities in the surface finish will show up as local stress concentrations, and particularly for the poly-crystaline piezoelectric materials that we use very heavily. The local stress concentrations cause local disturbances in the material and create ambiquities like zero shift. The other thing that you have to be extremely careful about is dirt, dust in the atmosphere, and cigarette ashes falling on the crystal before somebody puts the electrode on. We're very cautious about that. It's the kind of thing that does not show up when the parts go through receiving and inspection.

Stein: The comments made on zero shift so far are the same as when this topic was discussed at the last workshop, as I think some of the participants may remember, and at the workshop in Albuquerque in 1972. There is a sensor material and I believe all of the panel members agree that if it is a single crystal, the sensor material is as near perfect as you can get, which holds for the quartz and the single crystal piezoelectric material. I think the panel members agree that if the sensor element is multi-crystalline, as would be the case with the polarized ferroelectric ceramics or with the metallic strain gages, that the material itself is capable of zero shift because the shocking input changes the degree of polarization that you put on the ferroelectric ceramic, in the case of ceramics. It causes changes in the multi-crystalline structure of bonded resistance strain gages. So that takes care of the sensor. The sensor has to be attached to the transducer flexure and the adhesive. If it is a polymer adhesive, is the major cause of zero shift partly due to the cure, partly due to the area of bond? In the resistance strain gages people know enough to put big end loops on the strain gages. The pixy ends are now bigger than they were originally and all of this will help. The flexure itself is a multi-crystalline material that may locally yield, when you overstress it, then comes back looking like a different animal. This is the reason you load cycle the transducer no matter what it is; pressure transducer; accelerometer; or load cell, to get the flexural material used to working. When you put it back on the shelf it gets tired again, and when you take it out three days later or three months later it's just as tired as it was before you exercised it. The 100% people know that very well. Don't take a transducer off the shelf and put it to work without exercising. The flexure itself has to have a number of joints in it and the moment you have a joint you're in trouble, because there will be friction on that joint and it will never come back to where it was. I think Allen pointed out the importance of a smooth joint. When you stack crystals or when you have any kind of a joint in a transducer that's where you'll see the so-called zero shift to the shock inputs, even in pure single crystal transducers. Then you've got to package the joint in a case and your problem doesn't end there because the case is the outlet for the electronics and that's a different problem; they too have zero shifts. Now, can we go to a different topic, perhaps, than zero shift?

Alan Holmes, Lockheed Missiles & Space Co.: May I just linger one moment longer? Just by way of comment, I was talking about this subject to a man who specializes in stress measurements by x-ray diffraction; a technique which is not given to highly accurate and precise measurements, and he said it was visible even there. We concluded, somewhat informally, that at some level you can never just quite get away from what I call the railroad train effect where the input is the locomotive and the output is the caboose. They go backwards and forwards and, as you well know, you have to take up the slack in the coupling in each direction. For what it's worth, we decided that at some level you just can't eliminate that.

Jim Rieger, Naval Weapons Center: I had a different subject. Because this is just the opposite consideration. I buy accelerometers of the type that are the spring and dash pot variety because they're cheap. I buy them by the bushel. They're stuck in missiles that have a very short lifetime. Now all of these manufacturers have been producing things which are more and more accurate, smaller and smaller, and higher and higher in price. I don't need an accelerometer or a gyro that will give me plus or minus 1/10th% accuracy. I need something that's a couple of %, a couple of degrees accuracy, and a response of maybe 200 cycles. What I'm asking is, when are you manufacturers going to make something small and cheap for me?

Kistler: We tried to make low cost line production items, most recently pressure transducers. We felt our product should be eminently suited for the low cost pressure gage which gave reasonable performance; say $40, something like 1%. In fact, we specifically designed this for a customer who said that's what he needed. When he came out with his requirement and specifications it was anything else. His specifications, all along the line, extreme temperature differences which had to be handled, very high repeatability, no warm-up time, had to be zero, etc. Everytime we do this we find that the real requirements we seem to encounter always seem to be very tough requirements. We didn't get any place with an inexpensive, cheap transducer.

Chelner: We aren't making any accelerometers yet, and I didn't really come prepared to hand out a lot of literature. I really am technically oriented, not sales. I came prepared to contribute technically. We are presently in production of transducers in the $35 area, delivered. Now, providing that you don't need something that's way out of range, the ones that we are producing now are 1% items, that's including temperature over 100^{o}F. As far as I'm concerned accelerometers are just a diaphragm transducer with a mass on it. If you want to see me later I'll be happy to talk it over with you.

Hsu: I'd like to change the subject again. There's something that my people up there in Idaho have not found a solution for, which is germain to the transducer problem. I'd like to digress before we get onto that. I think you may find in the near future advanced energy systems, i.e., geothermal, coal gasification, nuclear fusion, and many other problems. Reactors are my business now. I think that you will find that the need for a $35 (you call them a $1.50 item) is very limited. In effect, I'm asking the learned gentlemen, with a lot of money to give away, to emphasize, perhaps a little more, the future needs, because it's not just me but a lot of other people I know who are trying to get you gentlemen involved in developing new devices. But the cost of developing these devices is astronomical. Think of the energy problem we are facing. It is kind of difficult so we need to run a lot of experiments. Along that line, I'd like to really ask if you would comment on how are you going to look into the future? Getting new devices. Anticipating higher, more difficult environments. I have a very specific problem in mind; that's the temperatures. We have found zero shift, maybe we can live with it, but transient temperature we cannot. A pressure transducer must not be a bad thermometer. We have found that pressure transducers do measure negative pressure from 2200 psi. That's very difficult to understand. Maybe the gentlemen could comment?

Stein: Paul, would you like to summarize your transducer survey for the group briefly, to tell them what the state of the industry is like and why it is less likely for individual manufacturers to develop things that have been requested. Paul has an unusual position. He is the transducer man at the National Bureau of Standards. Anybody who wants to discuss transducers at the Bureau eventually finds his way to Paul's desk. Paul ran a very interesting survey. I think it was three years ago. It's just a little bit frightening for the transducer industry and for our future.

Lederer: My memory isn't as good as it used to be. In one survey about three years ago we came to the conclusion that 300 to 400 manufacturers of transducers have a yearly market between $150 and $300 million dollars. The average company grosses about $1,000,000 a year. Under those circumstances, it is highly unlikely any one company can support very much research, other than whatever is needed to take care of an immediate crisis situation in their sales. There is no generic research work being done on transducers except in very scattered government installations. Industry cannot support this kind of thing.

Stein: Just trying to counter the idea that there are people with a lot of money to give away, the new word in Washington is "disaggregated". A disaggregated industry is one that needs help, and I can't think of any industry more disaggregated than the transducer industry. If you take an average $1,000,000 sales and allow 5% for research, which is really not unreasonable, that's $50,000. That will barely pay one guy one year with overhead to do something. That kind of development, gentlemen, is going to have to come from your end. It's you fellows up in Idaho with all the government money.

Mike Burger, Lawrence Livermore Lab: If you remember, my paper dealt with exactly that kind of circumstance. Dave over here has a real special pressure transducer problem. The way we tried to solve it was to go to you people and ask you if you would solve the problem for us and the answer was what was predicted, and what you just said. It's very difficult. Our solution was to work a little bit hand in glove with the manufacturers and try to solve that problem. That was my contribution at that time. If you try to use that sort of technique, I think that is very appropriate and I think you might be able to get a lot more done. We at LLL have a lot of scientific support which can be tapped for our application. I'm sure a lot of other agencies and government operations and non-government operations which are large could give a minimal amount of support like I did. I think that's an approach that should be used.

Stein: It sometimes creates a certain amount of laughter, but I think it is true. In the transducer industry when you come out with a new product it's very often the first customer that does your evaluation work. You both understand this and everybody benefits, but sometimes it seems a little funny to put it that way.

Easton: I would like to second that in the sense that I think the transducer industry is, in general, "solutions looking for problems". You are a very difficult customer group. Product planning is an element in marketing. If a manufacturer had the resources and could sit down and develop product plans, the transducer industry would drive him bananas. The applications to an instrument engineer in a big company are not even known until he gets out to the project engineer and finds out what the project engineer is going to do, so the manufacturer is sitting back and cannot really do product planning. The other is administrative and Pete said administrative comments were of interest. Many of you in the editorials and some of the trade press have been reading about the product liability for small manufacturers. Just as an example: last week we received our product liability insurance for Sensotec, a small manufacturer.

Last year for all our insurance we paid about $4,000. This year our bill is $27,000 and then, of course, we were lucky to get it. So as you can see from our side of the point we are not really looking for any new exciting, risky fields to go into.

Lally: One of the problems that might be encountered there would be that the insurance man just doesn't know what a transducer is or what instrumentation is. Part of the trouble here with the government and with the agencies may be of our own making. Over the past few weeks I've been in quite a bit of contact with people outside our community. I start talking about transducers, about transduction. In our transducer we have a little exciter, and we have integrating devices, and they don't know what I'm talking about. We have a very specialized language and what I would suggest is finding terminology and language that communicates much better to the people outside our community.

Stein: Sometimes I think that the insurance people know what they are talking of. Paul is working on the PIN project; that's Particle Impact Noise detection. One of the things that I read in the paper that he sent me was that $332,000,000 were wasted on some space and missile programs because there were little bits of particles inside the semiconductor cases that caused the whole thing to fail. Now maybe here a $3.00 part times a hundred million and then there's the liability that you could attach to it, so maybe those guys know what they are talking about.

Guthrie: One thing we have found has shown up considerably in the last year or year and a half. There's a communication problem between the user and the manufacturer, and the gap is getting wider. EG&G I think is a good example. We build flowmeters for EG&G which are used in testing.

It just so happened they were very close to standard because they will tolerate the high temperature and high pressure, etc. We would have liked, however, to have built some of the other instrumentation such as the special flowmeter that was used in that loop. According to some of the people that worked at EGG there had been several hundred thousand dollars spent on this device. Had a company such as Flow Technology and some of the other people known that this kind of money was available, then I think you would have probably found a lot more people knocking at the door to do this kind of work. We're building a new type of flowmeter now in which I need a displacement measurement, so I go to Schaevitz and buy the transducer. We might, on our coil winding machines, wind the transducer coil, etc., but they're the experts. We found that for $90 we would be hard pressed to get somebody fired up to do that. In looking at the real overall picture I think people in that speciality area could possibly do a little better job in designing and building specialty equipment for testing. More and more we are getting very much concerned about fixed-price type contracts. We've found that we're still getting our best response from our suppliers for new products on cost-plus contracts. I realize that those are bad words and most people aren't giving them anymore. I think it might be something that you, as users, might consider to get to those producers that supposedly have the most expertise in that area. I think you'd save a lot of manpower in your particular facilities. I know at this point we don't bid essentially on building some of the transducers for EGG because for one thing; the stack of paperwork bothers us and the other thing is that it is their design and we'd be hard pressed to guarantee it.

Chelner: I agree with the statement about communication problems. I don't think I would lay a communication problem on a user. I have been a user and now I am a seller and I remember the time when it was so much garbage being thrown my way and I couldn't tell my head from my tail. I mean confusing specs, and don't use this guide because mine's better, and you never really found out why it is better. I think we have gone through a phase where we've had salesmen representing technical companies who are just trying to sell the product to make the commission and not trying to represent the product properly. I think we do need to concentrate on opening up the communications. I found myself in that trap just recently. I put on more competent technical help in the sales area to try to make sure that we are communicating properly.

Kenny Cox, Naval Weapons Center: I have here a case history. It will only take a minute to present my case. I ran into a Research and Development situation and we were stuck with some old instrumentation, 10 years old or older. We buy transducers to match the old instrumentation. A man came along with a small project, not much money. In this case he wanted a transducer and it had to operate in cryogenic atmosphere conditions. I took out the brochures, went through them and found what he needed. They cost $700 each. He wanted three of them in six weeks. I called the company and they will deliver in six weeks. We placed the order. About 14 weeks later I got one in the mail. After half a day trying to get it to work I called the company and asked, what's the matter with this transducer? It's supposed to be a strain gage with four active arms. The company responded with, we are sorry, but we could only get two active arms in that space. The point I'm trying to make is when you print your literature, which comes first, the literature or the transducer? How much of that can I believe? I have to rely on that literature many times in many situations.

Stein: Kenny, I have a collection of catalogs where there never was a product made to fit the specs. Would anybody want to rise to the occasion of manufacturers specs and the communication problem, the art of specmanship and are there transducers that meet the specs? We're getting to the real purpose of this discussion, to clear the air once every two years.

Easton: I think that's a fair question, "Which came first, the transducer or the literature?" Since it is an honest question, I'll give you an honest answer. I think very often it's kind of both. A new design transducer will come out and we'll test it over a given length of time or over a given pressure range, but in general a manufacturer will extend it maybe up or down slightly, change it or add some characteristics that he thinks it should do based on the tests that he has run. Inevitably what happens is the user comes in and puts on a requirement like cryogenic. You have no idea whether the transducer will go down to cryogenic, but you have some idea that it might and you give it a try. I think that's where the transducer industry gets itself in trouble. I know that's where Sensotec gets itself in trouble occasionally. You have tested a design, a transducer, at cryogenic, under certain conditions. Along comes a customer who says, "Look, I want the same thing, except what I need instead of this thing being an inch and an eighth, I need it three quarters of an inch". You go to the engineering department and say, look, that's no sweat because we made it an inch and an eighth

Except, when it finally gets cut down to three quarters, you do have to internally change the design or the structure, and that's where the thing falls apart. I just don't believe transducer companies stay in business very long by purposely misleading users. I think it's a combination of the company trying to do something that it might never have done in combination before. We have in our specs about 34 listed options for pressure transducers. If you take those, there are combinations that you can't make work and, in general, a lot of users will come up with those combinations. So in defense of it I would say, very often almost everything has been designed and it probably has not been designed for every application.

Kistler: May I make a comment here, and if you permit, may I first deviate apparently from the subject? I would like to point out that I have traveled quite a lot in Europe, all over the place. Recently last summer I was even behind the Iron Curtain and had two opportunities of seeing instrument labs and calibrations, and so on. The net result is that I am more convinced than ever that here in the United States, in instruments, specifically transducers, we are way ahead of anybody else in the world. Next comes Europe and then the rest of the world, so we must not do everything completely wrong. Now to come to the subject. Users do not realize how much effort, how much design work, how much development work, how much test work it does take to develop a whole variety of transducers. It's not just one transducer of the type, you must have a whole variety. As Easton pointed out, sometimes one has to be a little smaller, another a little lower in temperature, and you get a tremendous variety when you multiply all these parameters. So there is so much testing to do. I agree that manufacturers who develop something new, a new line of instruments, has no choice but to cut corners a bit and to do certain testing, but he's not General Motors or General Electric with all their resources. He has to stop at a certain point, and then comes the results in actual life which are always more complex than what you could anticipate. You have surprises, and the surprises are very painful for the manufacturer as well as the user. That is what we are confronting here, but I don't know whether there is an easy way to get around that.

Hines: The comments amuse me somewhat because I don't know whether you realize you're dealing not with a company, but usually with an engineer. Engineers of transducer companies usually have certain creative talents and imagination and the hardest thing in the world is to stop an engineer from exercising his imagination. Once you get him going good you cannot turn him off. It's the hardest thing in the world to make him let loose of a product and give it to somebody else. He will make everyone different and everyone will be better than the last one and he will tell you exactly what he's done, but you cannot get him to make two of the same thing.

Steel: Errors in specifications or misnomers are not always in the manufacturer's favor. We, in Britain, tend to get overmodest. I've just had an example of this in the United States. We have just released a new range of wet-wet differential pressure sensors. Originally we sent over to our US offices the design spec, where we put in a fair amount of conservatism. Initial design spec was for .35% device with line pressures up to 3500 psi. I can't remember all the other parameters, but we actually built the thing. So we now cover the entire range and have done the evaluation. It turns out to be a .25% device and instead of the original status of 50 psi we're getting 10 psi with line pressures of 5,000. They just press released the thing with the design spec, whereas, we now have a full manufacturer's spec which is several times better than the press release that I've now got to face. I'm stuck with the modesty of the early design.

Stein: Let me add fuel to the fire before passing it on. Some of the problems must be laid to the door of the user. Now we've had several comments this evening about temperature transients and the horrible things they produce. In the last 40 years I'm sure people have been bugged by temperature effects on pressure transducers, accelerometers or strain gages. In the last year or so, finally, there has been an apparatus produced by Paul Lederer that will allow you to apply a standard transient temperature input to your transducer. How many of the manufacturers have heard of it and are using it to specify, and how many of the users are aware that this now finally exists. Do users realize they can write a spec for the ability of the transducer to withstand a defined, reproducible (may not be the one you have in service) temperature transient? This goes back to the communications problem and I hope Paul Lederer will speak on it tomorrow; the work that you are doing and planning to do. The stuff needs to be disseminated because it is in existence. It's just that many of us don't know that it is in existence.

Charles Thomas, Wright-Patterson AFB: About four years ago we had a problem facing us with temperature and we went out to most of the major transducer manufacturers and asked them to propose back. You wouldn't believe the fact that we got no proposals back from any of the major transducer companies, so I inquired the reason for this and got a whole range of answers. The one that came through the strongest was, we don't have the engineering staff to support this kind of development program. How are we going to get you to change your attitude of the past two or three years? Are you now open to this kind of thing?

Chelner: I don't know how the other people are approaching it from a business viewpoint, but our problem is we have a schizophrenic organization. I fight myself all the time because basically I'm an engineer and I want to do research and development and I have to worry about paying the guys working in the place, too. I have to look at it from a business viewpoint. Now one of these days I'm going to hire somebody to take my place because I don't like it, it's uncomfortable. We have broken our organization into half. We have a production outfit and we have a development outfit. Our development outfit is not unlimited but we are looking for programs that we can take on that will be within our format. I think what happens and what I saw happen in the past was when I've been involved with a large organization. You set up your organization and you set up your production. It's been pointed out that production is voluminous if you're handling it right. You have to have a complete set of drawings that are up to date. You have to control all the parts that go into it, all the processes that go into it. The first thing that happens is that somebody asks can it be a quarter inch smaller? The first thing that pops into your mind is sure. But then when you start looking at what it takes to make it a quarter of an inch shorter, you've got to change your top drawing, you've got to change your assembly drawings, you've got to change your detailed drawings, and you haven't even begun to look at whether your whole transducer characteristics will change on you. Your gage may not fit anymore because you change the size and you start looking at a whole bunch of ramifications. If it's in production, it's just too darn hard to change. But if you've got an organization that is in the development end of it, they can tackle it. That's what we're doing.

Guthrie: I basically agree with what you said. Our company being a technical type company, rather than a true production company, is primarily in the engineering aspect. We have 65 employees. We have about 15 engineers, several with masters degrees and advanced study. These people we keep busy by the fact that very few of the things that are ordered are standard off-the-shelf items. That little modification, just that small amount, as you pointed out, takes a lot of doing. We find that for instance on a $600 flowmeter if a customer says all I want is that flowmeter about 2 inches shorter. We say okay, there will be a 15% surcharge. The customer says 15% for what? Why you're saving material on this job. Then two years later, when we need to send out some spare parts, if we don't do a good job with our documenting and communicating you know what happens, we send out the spare part. From the other end we hear, you guys are crazy, this thing won't go in even if I hit on it with a hammer. We find that if you come in and you're after a product that is, within our engineering judgement, reasonably close enough to what we made in the past that we just make some reasonably simple adjustments. Maybe the temperature is a little higher or lower, flow range is extended one way or another. We are more than happy to tackle that with 10-15%, whatever is a reasonable amount of dollars. But if it's really far out we shy away from it. To go out and say we will build that transducer, and we've never built it before, but we'll get you specs for, say $10,000, that's a bad mistake. We learned a long time ago and we kind of shy away from those chiefly because $10,000 gets us about one week down the road and we couldn't get it in a week if we had to. From thereon we foot the bill, to come out with a transducer that we can't sell to anybody else. That's the reason I say that it would be nice if some people would bring back some cost-plus type contracts.

Hines: I'll reinforce that opinion just a little bit. Depending on where the inquiry comes from, it may cost us anywhere from $5,000 to $8,000, just to respond to your RFQ. In addition to that, did you know that your government will not allow us to use that cost in figuring our overhead figures? It has to come from someplace else. In addition to that, if we get the contract, we will be audited, not once but maybe five times before we get the contract finished. Those costs cannot appear in the contract in the final analysis. Therefore, we cannot afford to respond to many of your RFQ's. That I think is one of the reasons for your disappointment.

Stein: There are transducers developed, particularly with cooperation from Wright-Patterson. I can think of a number, like that mechanical recording strain gage with the instrument company up in Canada. Do you actually work with the manufacturer in developing these or how do these things come about that are in fact produced?

Thomas: Well I've worked with manufacturers before. One of the main deterrents in making a contract was the fact that after this heretofore not available transducer was built, then the government has free license to take that anyplace else and get another built. I got the distinct impression that most of you are unwilling to do that. At the same time we found that two or three of you were working in that very area in your own right. To answer your other question, we do work cooperatively with the companies involved. In this particular case it was a best effort operation. The requirements were designed very loose because our contracts people were very up-tight about this thing. You can't go out and ask somebody to do this on a fixed price operation. All you can ask for is the best that they can do. That's essentially what we did. We finally got a bite on it and we got the product. We got it from a totally unexpected source. This has happened to me twice now in succession; once with the transducers and once with signal conditioners. Out of the blue I got sources that paid off and we got what we wanted. But they weren't the people I thought were the experts. We are now considering doing a similiar thing again, and I'm wondering; am I going to be up against the same thing again?

Diercks: I think that you will find that all the companies represented on the panel have engineering staffs with finite limitations. You have to consider very carefully how you are going to apply that staff. What you probably ran afoul of was a situation where there were other alternatives for employing their engineering staffs and your alternative was less attractive to those companies that you've directed your RFQ's to than the other alternatives they had available.

Thomas: I can understand that, but I was wondering if the attitude had changed in general. Are you willing to take on that kind of specialized thing, although the market may be truly limited?

Easton: The only thing I could add to that from our standpoint is that it's very difficult to deal with the exceptions. You just don't see requests for R&D transducers come in on a regular or even an irregular basis. Therefore, you cannot staff a company and operate it productively or profitably on the basis of dealing with exceptions. If this was an industry that had a history of people coming and requesting RFQ's for research and development, then in essence you could begin to section off a percentage of the company. But I would agree with the gentlemen's comment back here, that generally speaking the orientation is either production or R&D, and then you are generally very selective. That's number 1 and number 2 is: there just aren't that many users out there looking for R&D contracts on best-efforts basis. They usually come in more or less like this gentleman down here and say can you take a standard product, modify it, and I'll pay you double or 10% or the same price, or something like that?

Guthrie: I'm sure that most of the people up here share the same thing: We are very hesitant to take on a fixed-price best-effort basis type contract because when we get to the end of the money, if we're not happy with the product we won't turn out something that's not useable. We'd rather not take the customer's money. We have done this one time that I know of in the past since I've been with the company. We have just simply asked the customer, can we cancel this contract because we don't feel like we have what you can use? He agreed and the contract was cancelled. So at this time in our company we will not or do not like to take that type of contract because we won't put out a product that's not useable.

Thomas: How then can we stimulate new development in advancing the state-of-the-art beyond what we see today? Do we have to sit and wait for you manufacturers to decide the market is big enough that it warrants the kind of operation we seek?

Guthrie: No. If the communication is like it should be and you say to us, point blank, look we have a problem. If we try to do it ourselves we're probably going to spend a whole lot of money, we might not come up with a solution. Can we work with you? Here's the contract and here's the kind of dollars we can afford to spend on this thing. If we get to the end of it and we don't have anything that's really useable, we'll see if we can't find some more dollars and go from there. That kind of communication will get your work done.

David Ray, Kirtland AFB: My question is, how many did you want? If you wanted five or six maybe you were right in going to an R&D company. That's where I see the breakdown. I see the technology available in R&D companies to make significant advances in transducers, but I don't see that technology drifting over into the people who manufacture in large numbers. Is there a lack of organization or communication ? What is the problem?

Thomas: I don't think that's the thing that really bothers me so much. I think most people will admit the need for transducers that work at higher temperatures although the need be somewhat limited. The question to me is that if we are willing in the government, at this point in time, to underwrite your research in that area, no matter what comes of it, why did you want to turn something like that down?

Hsu: I used to make money for companies, now I spend yours and mine. The direct answer to your question why would you turn it down? Because many times small companies lose their shirt and socks and everything else with it. This is true, this is a real experience. We paid for a small, triaxial magnetometer $10,000 - $25,000. Well that's a lot of money. We say we will make one ourselves. We don't know how to make magnetometers. It ended up costing us $75,000 and it was not working very well. This is precisely the point. You think you have a technology, so you make it a little smaller. It's no longer true. I can sympathize in another aspect of what you're saying. We do try. We say, here is our technology. We think we've got a handle on how to make it work. Would you see if you can make some (make 100 or 500)? Pressure transducers are a specific example. We ask RFQ either to make it to our drawing or your own, whichever you like. So it's an open-ended operation. We still get very limited response.

Lally: I'd like to make one comment on this 1000°F microphone requirement and on the 1000°F accelerometer. We're no different than most companies. I appreciate your inputs on this. One way to get this type of product is to speak up, as you've done. We're working on both of these products. In our product line now we have both microphones and accelerometers that operate at 600°F. We've had a number of companies come to us and ask us to build the 1000°F accelerometer, 1000°F microphones on a best-efforts experimental basis. We've built them, sent them out and they've put them into nuclear test programs. It's a long, slow process and we built these using available technology. Now we're not happy with the results of the products in this area. We have to compromise so much on behavior and performance that we can see it's going to take maybe even some major breakthroughs of materials, at least piezoelectric materials, to eventually come out with a 5-10% instrument at 1000°F. The piezoelectric materials that are available will operate there. You can make them work there. You get some results at those temperatures, but they are far from what we would want to offer as a standard product. Where a company needs a product and can help us on the evaluation of it, we have built and supplied a number of devices of this type; so have our competitors. I'd say that the way these things happen is really through cooperative efforts between people with the facilities to test and evaluate them and the people like ourselves with the capability to manufacture and develop. But we're still, I would say, a considerable distance from a good instrument of this type. We need financial help and cooperative help in testing and evaluating.

Larry Sires, Naval Weapons Center: I would like to bring this whole conversation back again to Kenny Cox's question. I work on an R&D project for a weapon system. We have neither the money, nor the time, nor the interest in supporting R&D in sensors. We work with off-the-shelf items. Like Kenny says, when we go out on test programs to perform a test, we deal with off-the-shelf sensors and we have to depend on the manufacturer's literature. Now I don't think Kenny was talking about changes. What we're talking about is looking at the catalog and saying, here, you've got a product that will do this, this and this. On a recent call I made to a manufacturer, just to get the availability and price, I ran into a catalog 120 pages thick with three or four products on each page. But he wasn't manufacturing and he wasn't willing to manufacture. We don't have the money, time, effort or the desire to specify something special. We have to get what you've got and like it in a fast-response type of way where we go to that literature. I think one comment here is we would like to see it be accurate, and when you put it out and you give us a catalog that you would at least be willing to manufacture it.

Two years ago when I was up here I lamented about the Microdot connector on pressure transducers, which seemed very prone to breakage. I was immediately pounced upon by two separate manufacturers who were at the meeting. They both showed me a cable that was substantially higher cost, I might add, than what I was paying, but they both turned out to be good. I think we do solve some problems here and my main problem was solved for me.

Stein: The problem you mentioned Larry is not unique to transducer manufacturers. In the academic world we revise our catalog every two years and there are beautiful courses put in that nobody has ever taught, that nobody has any intention of ever teaching. They are there to attract students to come to the university and then they are trapped. Would each panel member like to take a minute to address themselves to the problem that Kenny Cox and Larry Sires raised: how up to date are your catalogs, how knowledgeable do you think the salesmen are in terms of interpreting the specs, are they the kind of super salesmen that actually sells a product to the customer where the product hasn't even been built yet?

Chelner: I think it's a type of error that we have to address ourselves to. If it is an honest human error, then it's going to occur and it should be corrected as soon as possible. That means after the 20,000 brochures have been passed out. I don't think a vendor goes out and actually tries to promote an item that he can't build. At times you get sales types in organizations, promotional types, that get into an organization and they can see the potential. They can taste their victory and their enthusiasm and they come out with brochures. It's almost after the fact. It's usually not completely after the fact. The transducer manufacturer probably has built one in a lab someplace and it's doing fine, but they haven't gotten the production format down and often what you can do in a lab you can't do in production. I found that out myself the hard way. You come out after the fact and say okay we can do everything in the brochure except we can't put it out below this pressure and it can't go quite this range and you modify it after the fact. You do get caught in those things, and, as I say, if it's honest, if you're trying to get the product out and anticipate what you can do prior to actually getting into complete production, you get trapped in it. There are the cases where you're going to be oversold by a salesman. I don't know what to do about that. We try to select representatives that are pretty cogent in what they do. They are technically oriented and we try to give classes. I don't think a tenth of what we give them penetrates. Half the time I find they don't even read the brochures. They call me and find out what's going on. I'm not too sure what to do about it except to keep giving classes and hope it penetrates.

Toy: In defense of Bell & Howell, I think they're a very conservative company in their specmanship, if you will, and if you look at the data sheets I'm sure you can believe everything that you read in them. You'll find the transducer better than what is published. But I think as a user to get a transducer that will work in your application you need to ask one question of the salesman or whoever you're dealing with and that is: what will the transducer not do? Take the example of an unbonded wire strain gage transducer that Bell & Howell has made since 1940. The data sheet has this linearity, this temperature here, etc., but some of the information you need for your application may not be there and the transducer will not operate in your application. You need to ask questions relating to your application, especially if it is unique; not only our data sheets but every other manufacturer also.

Guthrie: We have a limited amount of flow literature here. We will be happy to send you a full set of literature. Some information we can't put on data sheets until we know how you are going to use our flowmeter. The type of fluids you will be measuring will determine the type of turbine materials to be used and the calibration procedures. So it is a communication thing and our representatives are not all engineers. Those of you who work with representatives and all the people here on the panel know that you just can't hire that many good engineers. There are supersalesmen who will tend to stretch the truth just a little bit. They like to say that their principal has the very best products going and all of us know that most of them in general are about the same. So we have to try to calm them down a little bit. We found that if we can keep them with us 10 or 12 years they usually get to know the product well. I think that type of thing is about the only cure; a lot of communication and working with people that you've known over a period of years and you learn to trust. I think that's a good way to close the gap between the user and the supplier.

Hines: I'm afraid I'll have to confess I'm short of literature and that it's not all up to date. I suspect that only about 20-30% of our products are off-the-shelf items that we can spec on sheet and normally those specs are taken by sales types. We get surprises after they are written. We find things that we didn't know before and we've got it all there in the literature sitting in the closet. I have dragged out old literature because we didn't have any new literature. It's not really wrong. It's just not complete in all cases. Certainly I don't think any transducer manufacturers that I know intentionally will mislead anyone on their literature. The fact is that sales types usually write specs and engineers give them numbers, sometimes optimistically perhaps, based on not enough information. Because somebody is pushing them, it usually results in surprises. It does happen occasionally, I don't think it's intentional. On the sales end of it we work through reps and you really run the full range when you talk to reps. There is really no way to know until you see them in operation what you can expect from them. You can change the reps; in fact it's a perpetual operation. The rep is naturally going to concentrate on sales that bring him the most gross, the most profit and he's going to push the product that he thinks will do him the most good, and that's a natural thing, but sometimes they do get carried away.

Steel: Well, our catalog, I know for a fact, isn't up to date. There must be some 20 new products which are currently being shipped to many customers which just aren't in there. Technical literature is a bore and I've been trying to find an easy solution to this boredom for years. I do it myself because I can't find anyone else to do this work for me and I have the attribute of being as thick as the thickest customer I deal with and that's a very necessary requirement. To make good literature, you have to do many processes. You always start off with the engineering spec and then you go look at the marketing aspects and see which key features you're going to latch onto. To do a two-page brochure and get it right on a typical transducer it will take upwards to two months.

which is an incredible amount of time. But in practice I find that's just about what it takes, including going to the printer. By this time everyone gets fed up with reading it. That's another point, you can never get it checked. The engineer knows what it should read and he doesn't quite notice that there was a decimal misplaced a bit and things like that. It's very difficult to produce a perfect piece of technical literature, but we can go a long way to get it the best we can. The big problem we've got is actually getting it out quick enough. When you come to our catalog, in fact the electronics for example, has all been updated last year. There are certain models shown in that catalog which we do not now offer. These have direct equivalents with improved performance at much the same price. We don't think we're deceiving the market by using this old literature until we get the new literature available.

Kistler: Well I feel we only have perfection in the industry or situation where things are static, and there is hardly any industry where things are more dynamic, more changes, more progress and improvement than in the instrument industry. Actually there is so much change, so many new products, so much improvement, that, by necessity things are in disarray a bit. Things don't agree, literature is ahead of the product or vice versa. I do think it's complex for the users and the manufacturers, but I don't think it's a bad sign. Sometimes we have some instruments in some range or some products on our product list that we have never built in a small size. We have built average size. We certainly will not say we won't make them. We certainly will consider ourselves obligated to make them and make them work, if at all possible.

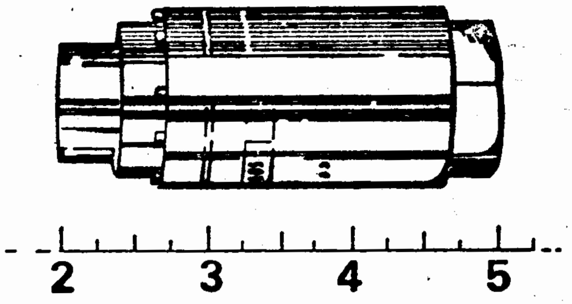
Diercks: In the products that we consider to be standard products that we package in catalog form, we try to take risks with the data sheets very infrequently. We do not include data sheets on things that we have not manufactured or produced at least in the laboratory. We do occasionally take risks with the numbers. There have been occasions when we've gotten mouse trapped. When it comes to the special product area, devices that are tailored specifically to a customer's requirements, there are a fair number of those where there are specifications available, but quite frankly the factory is not very much interested in producing them again. They were financial failures the last time we did it and we could see no way of making even a qualified financial success out of the product. These are generally products with very limited volume and very specialized orientation. I think, in general, the products you see in our data sheets, particularly if the data sheet is multicolor, are represented relatively conservatively with only rare occasions of a number being dredged from the imagination rather than having been created out of actual field trials. It takes a fair length of time to prove out a brand new product. One can create a new product in a relatively short period of time. For example, you might produce a new "widget" or create a new "widget" in a period of six months and it might take you 12 months to determine whether or not that "widget" was satisfactory for service out in the field. There are a lot of parameters to which those transducer products can respond that they should not respond. It takes a long time to really sort out the cause and effect relationship with stimuli to take corrective action and to make sure your corrective action associated with one unfortunate stimuli did not screw up the product in some other area. In general, I think Endevco is very conservative in its brochures.

Lally: Well, I can safely say that you can buy any product listed in the current PCB catalog with confidence. To be truthful, I have to tell you that it was published three years ago. The problems that have come up here I would say in long experience of writing literature, preparing literature, preparing catalogs, that with most reputable manufacturers, if you get a product well documented, you can buy it with confidence. Now if it's just a flyer or an announcement or picture or some promotional material or something of that sort, then I would be cautious.

John Easton, Sensotec: I think it's obvious that all manufacturers here have leveled with you on this topic . It comes down to a choice of wording that best represents what the manufacturer has to offer and what will also communicate with the user. For instance, the work we are presently doing is in pushing up temperature limits of our pressure transducers and increasing the accuracy at temperature. This kind of work is more evolutionary than revolutionary. Therefore, it is impractical and in fact impossible to have up-to-date literature at all times.

END OF SESSION IV

TRANDUCERS IN THE EYE OF THE BEHOLDER



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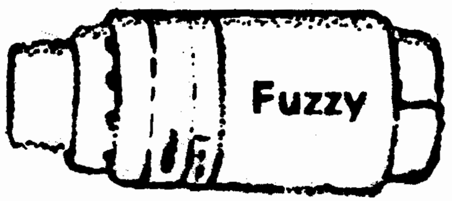

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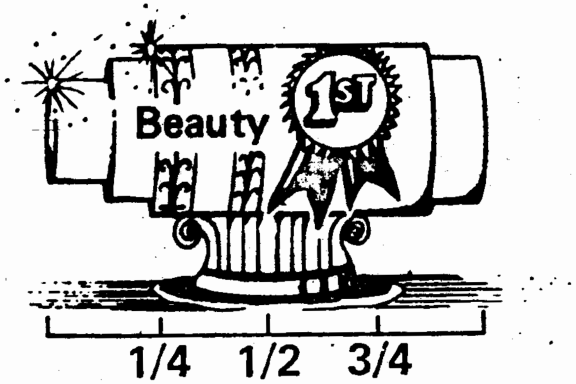

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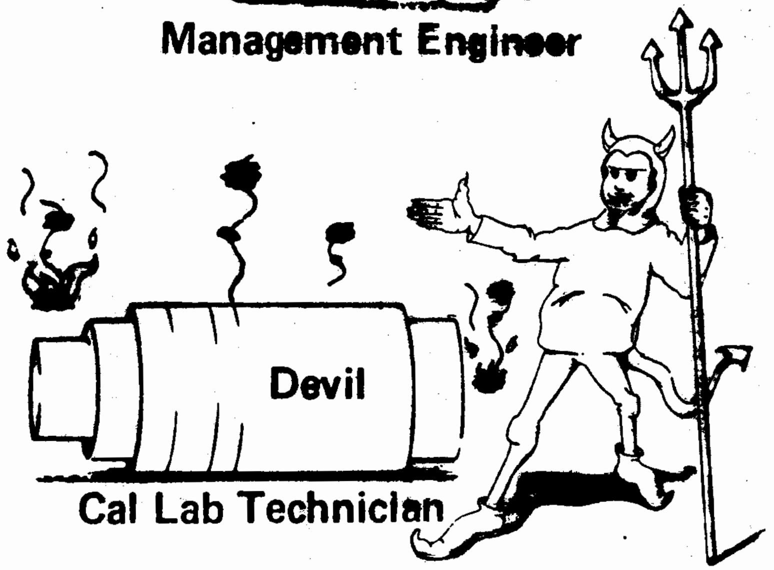

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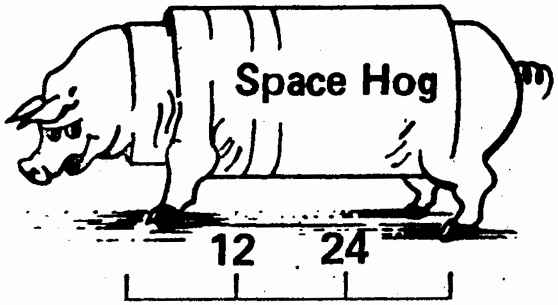

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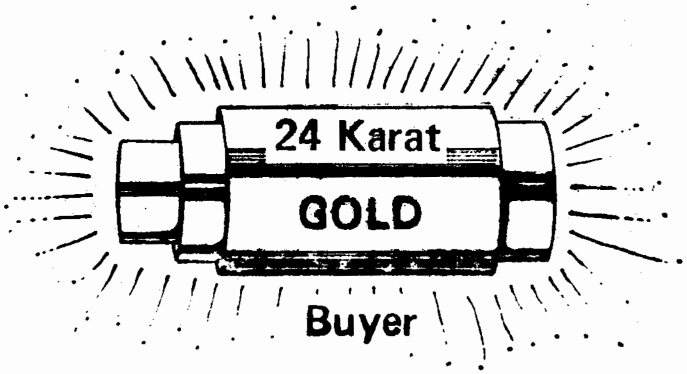

Buyer

SESSION V

DISPLACEMENT, VIBRATION AND STANDARDS

Larry Sires, Chairman

ULTRA-SENSITIVE, HIGH-FREQUENCY SURFACE MOTION TRANSDUCER*

David C. Erlich
Stanford Research Institute
Menlo Park, CA 94025

We have developed a transducer capable of measuring low-amplitude displacement histories of a flat surface over a frequency bandwidth of ≈ 1 MHz. The method requires the moving surface to be a conductor (a thin conducting foil glued to the top of a nonconducting surface will suffice), but requires no contact with this surface, and hence does not disturb the motion to be measured. A capacitance bridge circuit is used to measure the changes in capacitance between the moving surface and a stationary metallic disk positioned above the surface. The bridge is powered by RF-frequency excitation and the output of the bridge is envelope-detected to yield the desired signal. The RF circuitry produces very large signals for very small capacitance changes and greatly reduces noise due to stray capacitance. Signal sensitivities of greater than 1 volt per 0.001 inch of motion have been obtained with precisions of approximately 1 microinch. This transducer has applications in areas of low-amplitude wave phenomena, such as seismic modeling.

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* Work supported by Air Force Geophysics Laboratory

Introduction

In a recent study of techniques for measuring elastic wave response in a seismic modeling medium,[1,2] we became aware of the need for a new type of surface motion transducer.

For normal seismic measurements, a large assortment of seismometers is available for measuring surface motions. However, these transducers are not applicable to seismic modeling, in which both distances and durations are scaled down by factors as large as several orders of magnitude. What is needed is a gage that will respond to high frequencies (up to ≈ 1 MHz); will be sensitive enough to record very minute motions; will occupy a small area on the surface, so that motion variations across the surface can be measured; and will apply negligible load to the surface, so that its presence will have a negligible effect on the surface motion.

We first investigated existing transducers to determine if any had the required specifications. We found that the frequency response of piezoelectric accelerometers were too low and that even the smallest seismometers would cause unacceptably high surface loading.

We then studied the possibility of developing a gage suited especially for seismic modeling and examined three different types of surface motion gage that would have an adequate high-frequency response:

(1) Magnetic induction velocity gage, in which a conductor moves with respect to a magnetic field. This type of gage is very easy to calibrate. However, it has one serious disadvantage: if several gages of this type are to be used in close proximity, particularly to measure motions in different directions, the magnetic fields for the various gages will interact in a complicated way. Furthermore, large conductor lengths and high strength magnetic fields would be necessary to record the

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1. D. C. Erlich, "Three-Dimensional Seismic Modeling," Final Report for AFGL Contract F19628-74-C-0157, SRI Project PYU-3287, August 1975.

2. D. C. Erlich, "Three-Dimensional Seismic Modeling," Final Report for AFGL Contract F19628-76-C-0064, SRI Project PYU-4719, August 1976.

relatively low velocities produced (compared with those in high pressure shock wave experiments), resulting in high cost and complicated design to avoid surface loading.

(2) Optical surface motion gages, including both laser interferometry and light reflectivity techniques. These systems have negligible surface loading, but require complicated alignment and calibration and involve questionable sensitivity (in the case of reflectivity techniques) and high cost (in the case of interferometry, for many data channels).

(3) Capacitive displacement gage, in which the surface whose motion is to be measured acts as one plate of a parallel plate capacitor while the other plate remains fixed. This system has negligible surface loading (only a thin conducting foil need touch the surface) and simple calibration procedures. Furthermore, since the strengths of the electric fields in a parallel-plate capacitor are high only between the plates, little interaction would occur between two capacitors, provided they are separated by a distance that is large compared with the plate separation distance.

We selected the third alternative as the most promising. In the literature, we found one reference to the use of a capacitor circuit for measuring the velocity history of a plane conducting surface loaded by shock waves from a high-velocity projectile impact.[3] However, this method could not be applied directly to seismic modeling needs because it involved the measurement of changes in capacitance caused by capacitor plates (several square inches in area) whose separation changed by substantial amounts (up to 0.05 inch). We needed a system capable of

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[3] M. A. Rice, "Capacitor Technique for Measuring the Velocity of a Plane Conducting Surface," Review of Scientific Instruments, Vol. 32, No. 4, pp. 449-451, April 1961.

measuring the change in capacitance associated with smaller plates (about 0.25 square inch) whose separation changed overall by as little as 0.001 inch, with an uncertainty at least as low as 10 μin. (1 part per 100 of the expected plate motion), and perhaps as low as 1 μin. (1 part per 1000 of the plate motion).

Transducer Theory and Design

The capacitive surface motion gage we designed is shown schematically in Figure 1. At the bottom is the moving conducting plane, which could be a very thin sheet of conducting foil attached to the top of the surface whose motion is to be measured. Above this plane and parallel to it is the stationary plane, which conists of a small circular conducting plate, a nonconducting annular gap whose width (W) is large compared with the distance (D) between the two planes and a large reference conducting plane.

Both the capacitance between the large moving plate and the small circular plate (C), and that between the large moving plate and the equally large stationary reference plate (C_{ref}) vary inversely as a function of D. Now, the capacitance (C_{eff}) between the reference plate and the circular plate is equivalent to the capacitance of C and C_{ref} in series. So, $C_{eff} = (1/C + 1/C_{ref})^{-1}$, but since the area of the reference plane (> 50 sq. in.) is so much larger than that of the circular plate (≈ 0.2 sq. in.), $C_{ref} \gg C$. Therefore, C_{eff} becomes effectively equal to C, and we now have a method of measuring the capacitance to the moving plane. We simply attach wires as shown in the figure, and by measuring the changes in C_{eff}, we can determine the changes in D.

The above discussion implicitly assumes that the moving plane is a rigid body, or rather, that D at a particular time (t) is constant over the entire area of the plane. Now, let us relax that assumption by allowing D(t) to vary over the area of the plane. We can now place several small circular plates within the reference plane area and measure the capacitance between each of them and the one reference plane.

Provided the spacing between the circular plates is large compared with D(t), and that at no point actual physical contact occurs between the stationary and moving plates, we can now determine how D(t) varies as a function of position throughout the plane.

The problem now becomes one of accurately measuring C_{eff} for each circular plate. The capacitance of a 0.5-inch-diameter circular parallel plate capacitor, whose plate separation is 0.01 inch, is approximately 5 picofarads (pf). If we want to measure total plate motions as small as 0.001 inch with 1% or 0.1% precision, then we need to be able to measure total capacitance changes as small as 0.5 pf to accuracies of 0.005 pf or 0.0005 pf, respectively.

A common way to measure capacitance is to place the unknown capacitor in a bridge circuit, excite it with sinusoidal signal, and place known (calibrated) capacitors in the bridge to obtain a "null." In this case, however, the absolute value of the capacitance is not desired--only the variability of the capacitance as a function of time. The bridge components, therefore, are chosen for their nominal capacitance value, and the output voltage from the bridge is directly proportional to the change in the unknown capacitance over a certain range around the nominal null value. Where the capacitance values is very small (for example, less than 10 pf), it is customary to use a high frequency (RF) excitation and then to envelope-detect the output of the bridge circuit for conversion to a voltage analog. The higher the frequency, the lower the bridge impedance, which simplifies detection and amplification of the different signals.

To be able to measure very fast motions, the capacitor bridge and detector circuitry must have an adequate bandwidth to respond to rapid changes in capacitance. To accurately measure signals having rise times as fast as 1 μsec, we need a gage with a capacitance bridge excitation frequency greater than 5 MHz (to give an adequate sampling rate). However, because of filtering requirements (to remove the carrier wave itself) and because the carrier waves on different channels should have frequency differences greater than 1 MHz (so that the filtering process will remove crosstalk beat frequences as well) we selected carrier frequencies in the 8 to 15 MHz range.

After testing several different circuit designs for the capacitive gage electronics, we chose the configurations shown schematically in Figure 2. The five principal parts of the circuitry are an adjustable frequency RF oscillator to provide the carrier wave, a transistor amplifier to augment the carrier wave amplitude, an isolation transformer, the capacitor bridge with an adjustable reference capacitor, and a detector/amplifier to separate the signal from the RF carrier wave and boost the signal for recording purposes. Power to the circuit is provided by two 15 V dc power supplies. The reference capacitor is adjusted to be similar to the capacitance of the surface motion gage before the first motion, so that the bridge is close to its null point and therefore in its linear operating region. Subsequent change in the capacitance of the gage leads to an unbalance in the bridge circuit, which is rectified and amplified to yield a signal directly proportional to the RF signal difference across the bridge. The output signal is fed through a low-pass filter, which further diminishes the magnitude of the carrier wave, while allowing signals of frequency less than 1 MHz to pass through undiminished.

The signal bandwidth of this circuit is established by the response limit of the operational amplifier, about 500 kHz. This is adequate to meet the requirements of a 1 μsec rise time. The noise level is established by the RF oscillator and operational amplifier. The dynamic range of the system is greater than 80 dB, from the noise floor of approximatly 1 mV (assuming that the RF oscillator has an amplitude stability of better than 1%) to the amplifier output limit of 10 V.

Since the output sensitivity can be as high as 1 V per 0.001 inch motion (as determined by calibration, discussed below), the signal-to-noise ratio for a motion of 0.001 inch can be greater than 60 dB. Thus, the ultimate sensitivity of the system approaches 1 μin.

Dynamic Test and Calibration

As a first dynamic test of our capacitive surface motion gage, we performed the experiment shown schematically in Figure 3. A 2 x 2 x 1 ft block of a syntactic foam* was loaded by detonation of a small spherical explosive source cast into a syntactic foam cylinder glued to the bottom center of the block. Three of the surface motion gages were positioned on the upper surface of the block to measure vertical displacement histories. Two of these gages (C2 and C3) had their centers 0.5 inch on either side of the axis of cylindrical symmetry and a third gage (C1) had its center 4.5 inches off to one side.

The stationary plates of the three capacitive gages were thin 0.5-inch-diameter copper disks set into plastic inserts, which in turn were set into a 9-inch-diameter brass disk that acted as the stationary reference plane. The plastic inserts provided a 0.25-inch-wide gap between the gage plate and the reference plane. A 0.001-inch-thick sheet of copper foil was glued to the top of the foam block to serve as the moving conducting plane, and the brass plate with the gage inserts and circuit boards attached were suspended above the block, as shown in Figure 4, from a rigid frame connected to the table sidewalls. The reference plane would remain stationary for at least several hundred microseconds, before being affected by stress waves from the detonation. Accurate initial height adjustment was provided by three knurled knobs attached to rods with finely pitched threads, so that, with the aid of feeler gages, the stationary reference plane could be positioned very nearly parallel with the moving conducting plane at any desired height.

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*Eccofloat EF-38-A, product of Emerson & Cuming, Flotation Products Division, Canton, Mass.

We cast two of the syntactic foam cylinders containing the spherical explosive charges to permit two identical tests. For the first of these tests, we had only a very rough idea of the surface motions that would occur, so to bracket our uncertainties, we set relatively low sensitivities on our recording oscilloscopes. Posttest inspection of the oscillographs showed that the signals occurred at the expected times but were an order of magnitude or two lower than the upper limit of the values we expected, so that all the oscillograph traces were nearly horizontal lines.

For the second test, we readjusted our recording sensitivities and obtained records of excellent quality. The raw data for the surface motion gages are depicted in Figure 5, which shows a pretest baseline and the actual shot record from both fast and slow sweep oscilloscopes for each of the three gages. The delay given is that between the detonation of the explosive charge and the triggering of the scope trace. Cursory inspection of these records reveals a low-amplitude, constant frequency signal superimposed on the records from Gages C1 and C3, but not on those from Gage C2. A check of the frequencies of the RF carrier signals for the three gages yielded values of 8.75, 11.0, and 8.0 MHz, respectively, for Gages C1, C2, and C3. The frequency of the signal superimposed on the records of Gages C1 and C3 is approximately 750 kHz, which is the difference between their RF carrier frequencies. No such beat signal appears on Gage C2, because its RF carrier frequency is so different from the other RF frequencies that the beat signal is removed by the low-pass filter.

The records are quiet. Other than the beat frequency discussed above, they do not show noise of any kind. Therefore, the signal accuracy is limited only by any geometrical distortion in the oscilloscope optics, by irregularities in the width of the scope trace, or by our ability to accurately digitize the record. At these scope settings, the largest uncertainties introduced should be less than 1 mV, which corresponds to accuracies of better than 1% of the peak value for Gages C1 and C2, and better than 0.33% of the peak value for Gage C3.

To obtain calibration curves for the surface motion gages, we used the setup shown in Figure 6. The brass reference plate with the gage inserts and a 3-inch-diameter plate attached to the spindle of a very accurate micrometer were positioned in parallel on a smooth, flat table. The micrometer plate was brought into contact with the reference plate and the voltage output of the capacitive circuitry was recorded at intervals of 0.001 inch as the micrometer plate was displaced with respect to the reference plate. The resulting curves are plotted in Figure 7. Relative distance along the horizontal axis is plotted. Because of irregularities in the surface finish of the plates, we cannot set the distance between the plates to exactly 0 inch, or determine the exact distance between the plates. However, it is not at all difficult to accurately measure changes in plate separation, and this change in separation is precisely what we are measuring in the actual shot. The diamond-shaped point on each curve represents the initial voltage of the gage circuit output immediately before Shot 4719-3. The abscissa of each point thus represents the initial plate separation of each gage, and subsequent changes in output voltage will, by following the calibration curve, determine the gage motion.

The sensitivities of the gages at their initial preshot positions vary from 314 mV/0.001 inch for Gage C2 to 753 mV/0.001 inch for Gage C3. These sensitivities could, of course, be increased to better than 1 V/0.001 inch by reducing the initial plate separation. Since the baseline noise level is approximately 1 mV, we can obtain displacement accuracies ranging from 3 μin. for Gage C2 down to 1-1/3 μin. for Gage C3.

By digitizing and calibrating the oscillograph records from the surface motion gages in Shot 4719-3, we obtained the displacement histories shown in Figure 8. Gages C2 and C3, which, because of their symmetric position with respect to the explosive charge, should have observed identical loading, exhibited nearly identical measured displacement histories. Their peak upward displacements of approximately 10 μm (approximately 0.0004 inch) are within 0.5% of each other. Gage C1, located further from the source, has a peak upward displacement of approximately 6 μm. All three gages exhibit approximately 1-1/2 cycles of vertical ringing until reflections from the side of the foam block arrive (at about 260 μsec) to complicate the picture. The peak surface velocity recorded at the center of the block was determined to be about 120 cm/sec, and the peak acceleration experienced by the surface on the order of 10^4g.

Summary and Conclusions

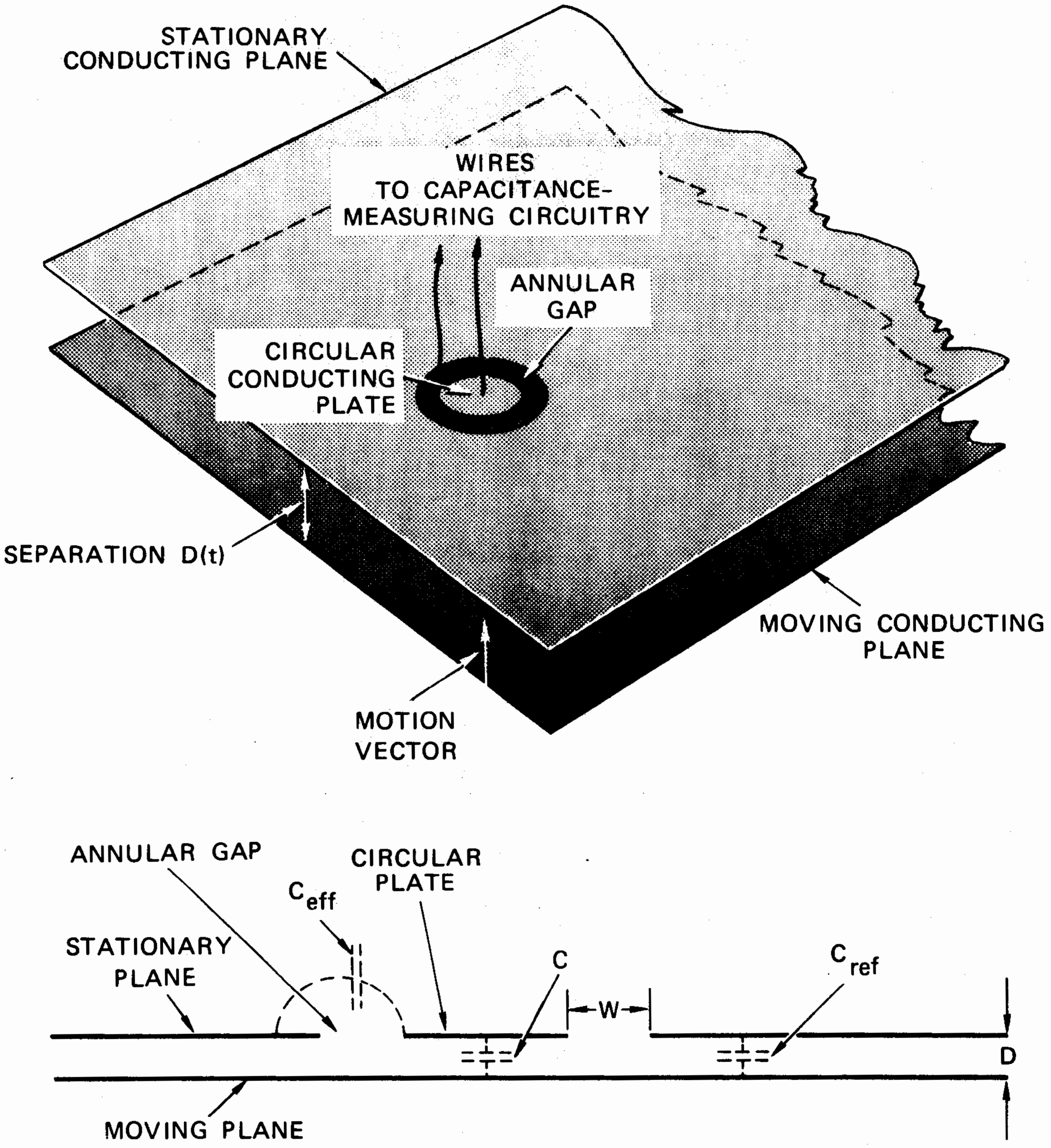
Our surface motion transducer has demonstrated its precision near the upper end of its frequency bandwidth. We have not tested its accuracy dynamically, mainly because of the lack of a suitable calibration transducer against which to compare it. However, we cannot see any reason why the accuracy should degrade dynamically, at least not within the 1 mHz bandwidth.

The capacitive surface motion gage was tested in a plane geometry: we measured the perpendicular displacement of a flat surface. Other surface geometries are also amenable to capacitor surface motion gage techniques. For example, if the motion of the inside surfaces of a cylindrical cavity is of interest, a cylindrical surface motion gage can be used to determine the radial expansion and contraction of the cylinder walls. Such a gage would be orders of magnitude more sensitive than strain gages attached to the inside walls of the cylindrical cavity in the hoop direction. The same circuitry can be used as in the planar surface motion gage. Only the geometry of the plates and the physical locations of some of the electronic components would need to be changed.

In conclusion, this surface motion transducer appears to extend our capabilities by allowing precise measurement of the displacement histories of surfaces subjected to low-amplitude, high-frequency wave propagation. The surface motion transducer should find application in various problems, including that of seismic modeling.

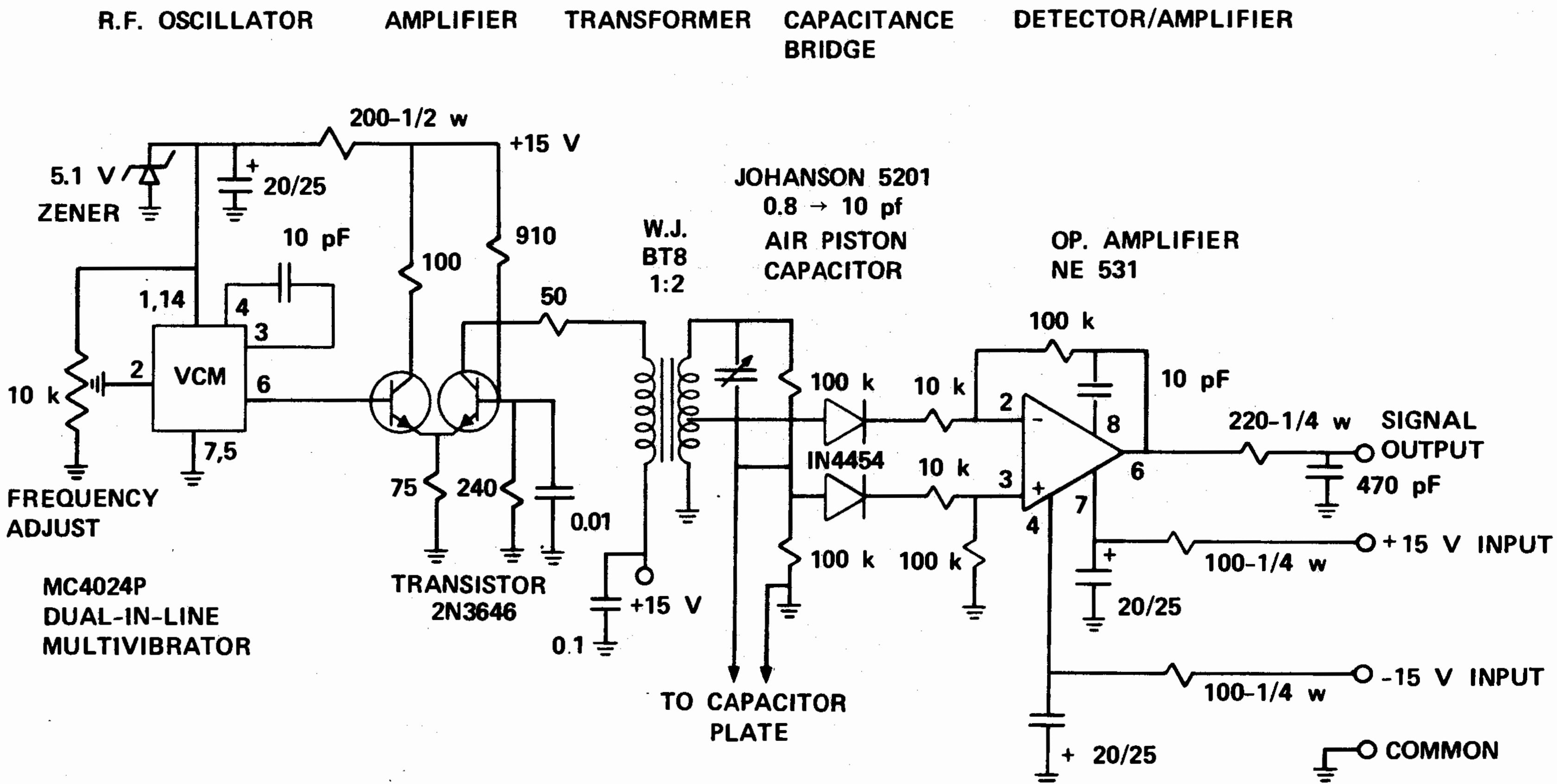
ACKNOWLEDGMENTS

We are indebted to Mr. Phil Bentley of SRI for his work in designing, building, and testing the circuitry for the capacitive surface motion gage, and to Dr. Ker Thomson of Air Force Geophysics Laboratory for giving support and direction to this program.



CROSS-SECTIONAL VIEW (Not to Scale)

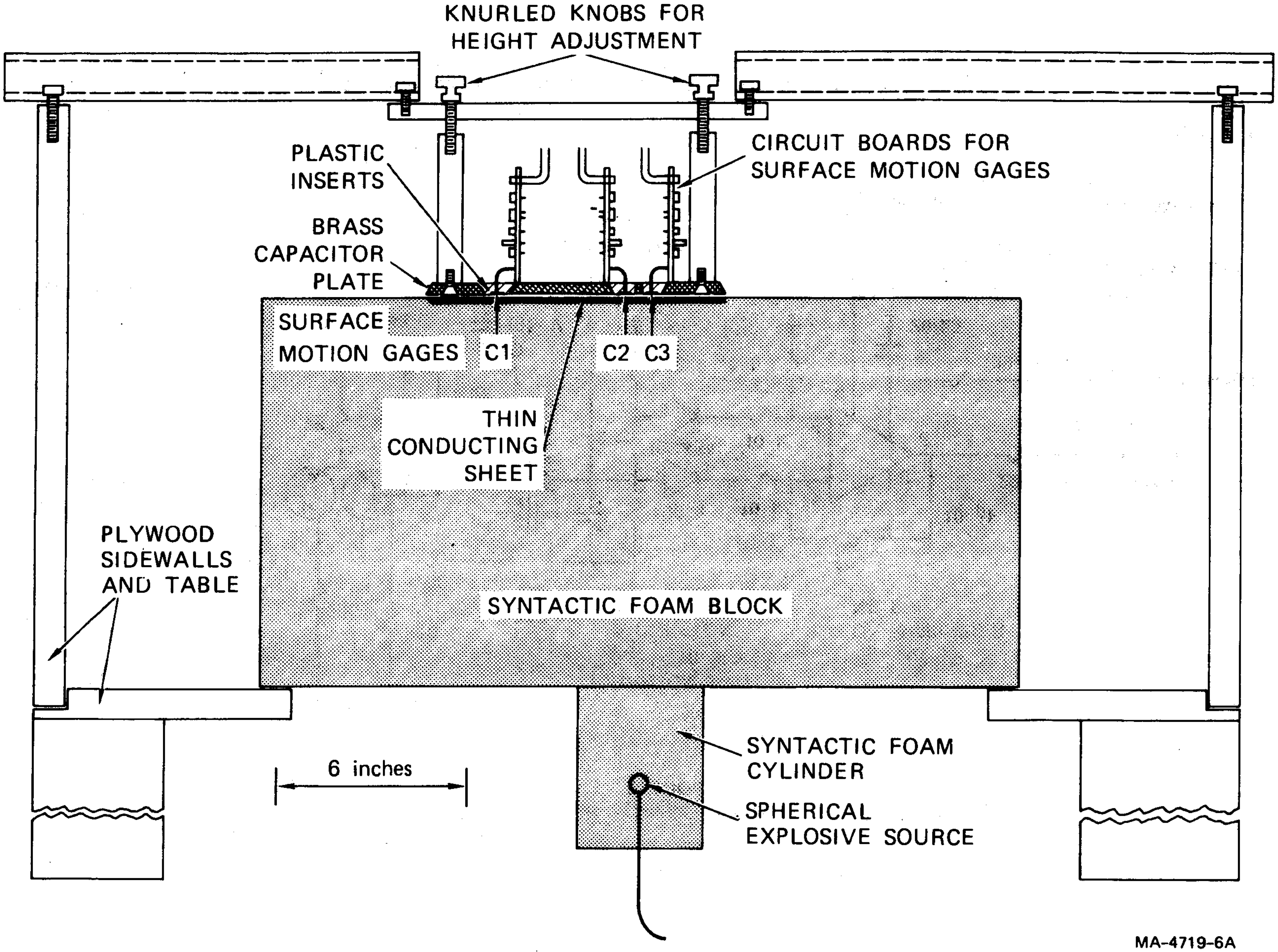
FIGURE 1 GEOMETRY OF CAPACITIVE SURFACE MOTION GAGE



NOTE: All resistors 1/8 w carbon unless shown otherwise.
All capacitors in μF unless otherwise noted.

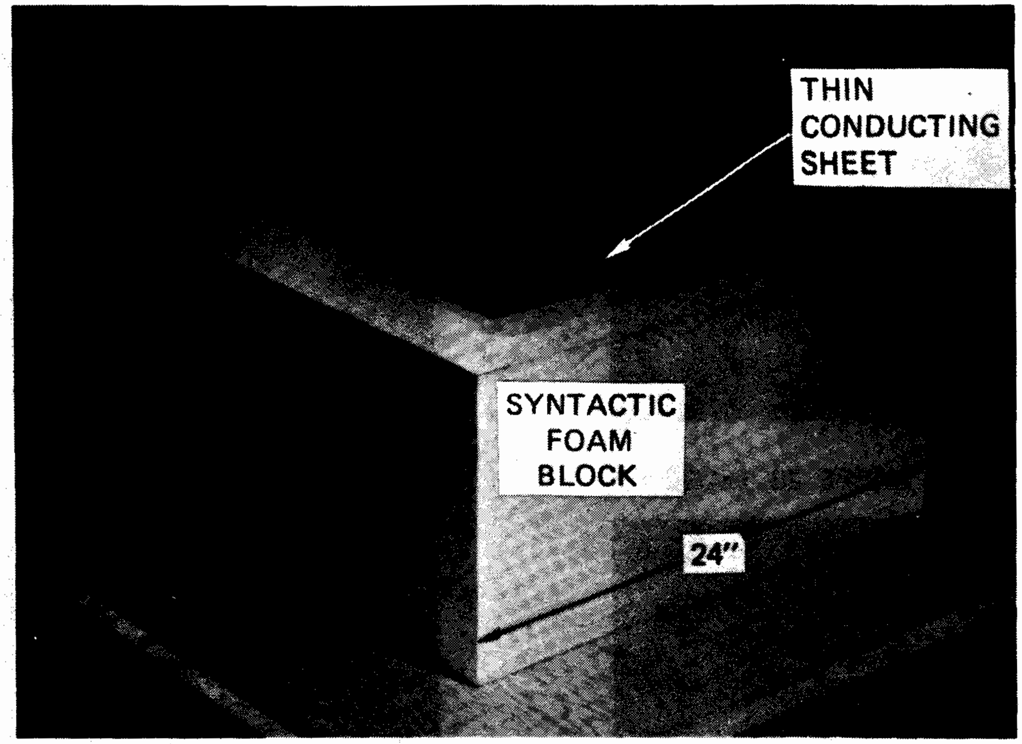
MA-4719-10

FIGURE 2 DIAGRAM FOR CAPACITIVE SURFACE MOTION GAGE ELECTRONICS

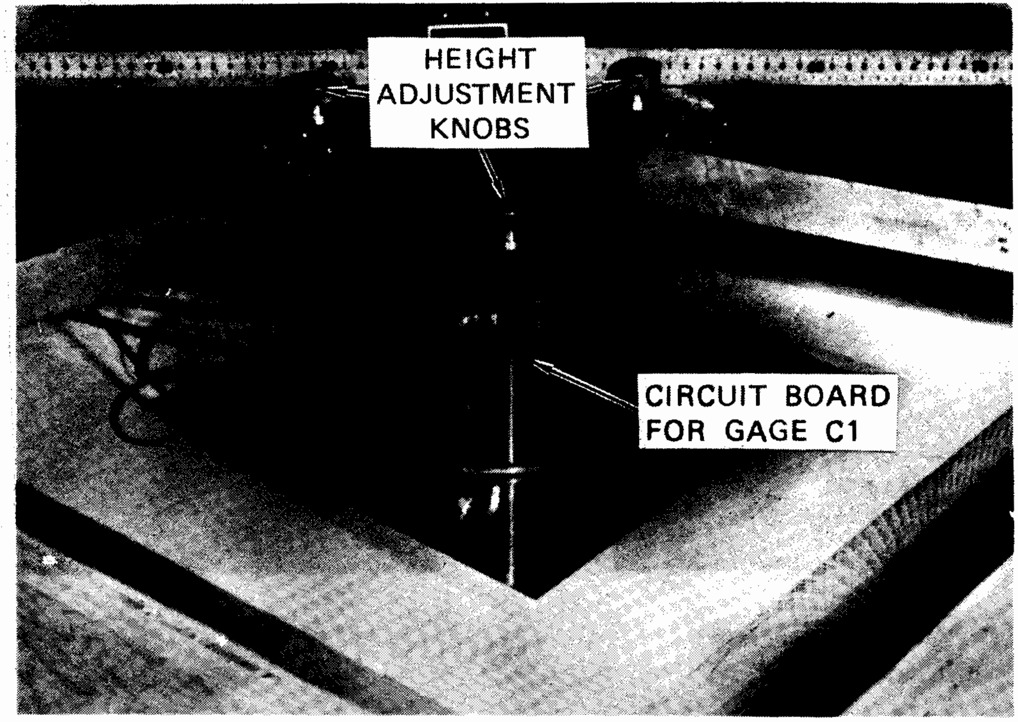


MA-4719-6A

FIGURE 3 SCHEMATIC DIAGRAM OF SEISMIC MODELING EXPERIMENTS



(a)

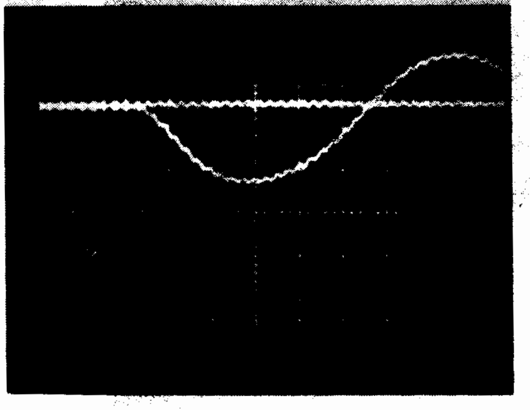


(b)

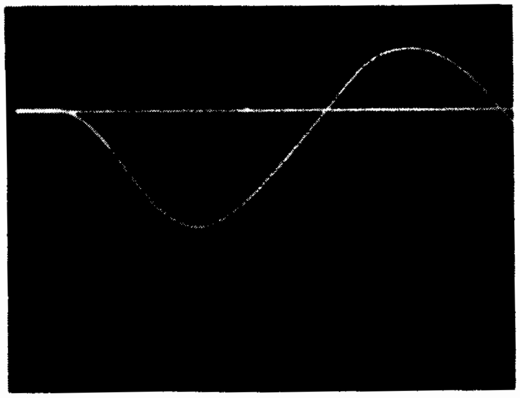
MP-4719-4A

FIGURE 4 SYNTACTIC FOAM BLOCK (a) BEFORE AND (b) AFTER EMPLACEMENT OF SURFACE MOTION GAGE ASSEMBLY

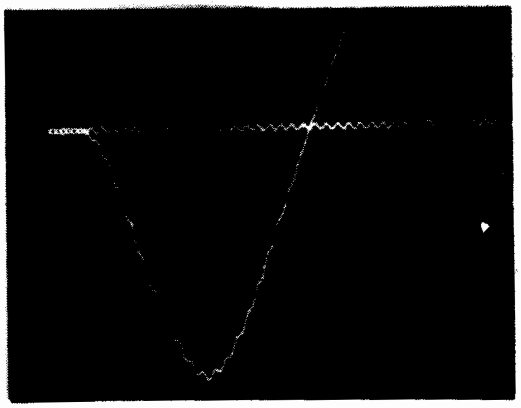
SHOT 4719-3, SCOPE 32, GAGE C1



SHOT 4719-3, SCOPE 28, GAGE C2

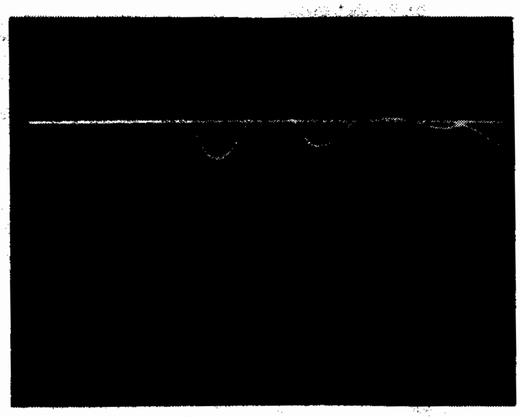


SHOT 4719-3, SCOPE 33, GAGE C3

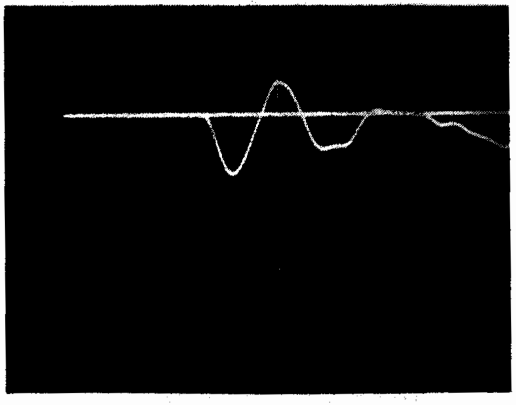


50 mV/cm 5 μsec/cm
160 μsec DELAY

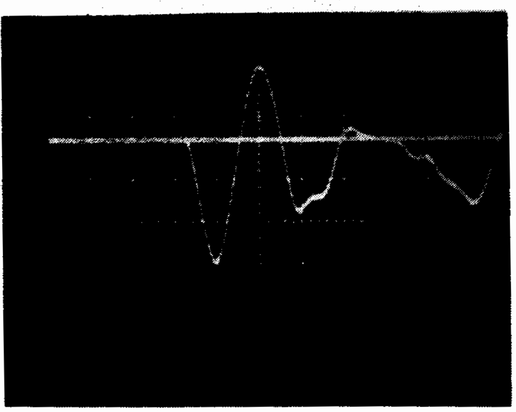
SHOT 4719-3, SCOPE 30, GAGE C1



SHOT 4719-3, SCOPE 29, GAGE C2



SHOT 4719-3, SCOPE 30, GAGE C3



100 mV/cm 20 μsec/cm
100 μsec DELAY

MP-4719-7

FIGURE 5 SURFACE MOTION GAGE OSCILLOGRAPHS

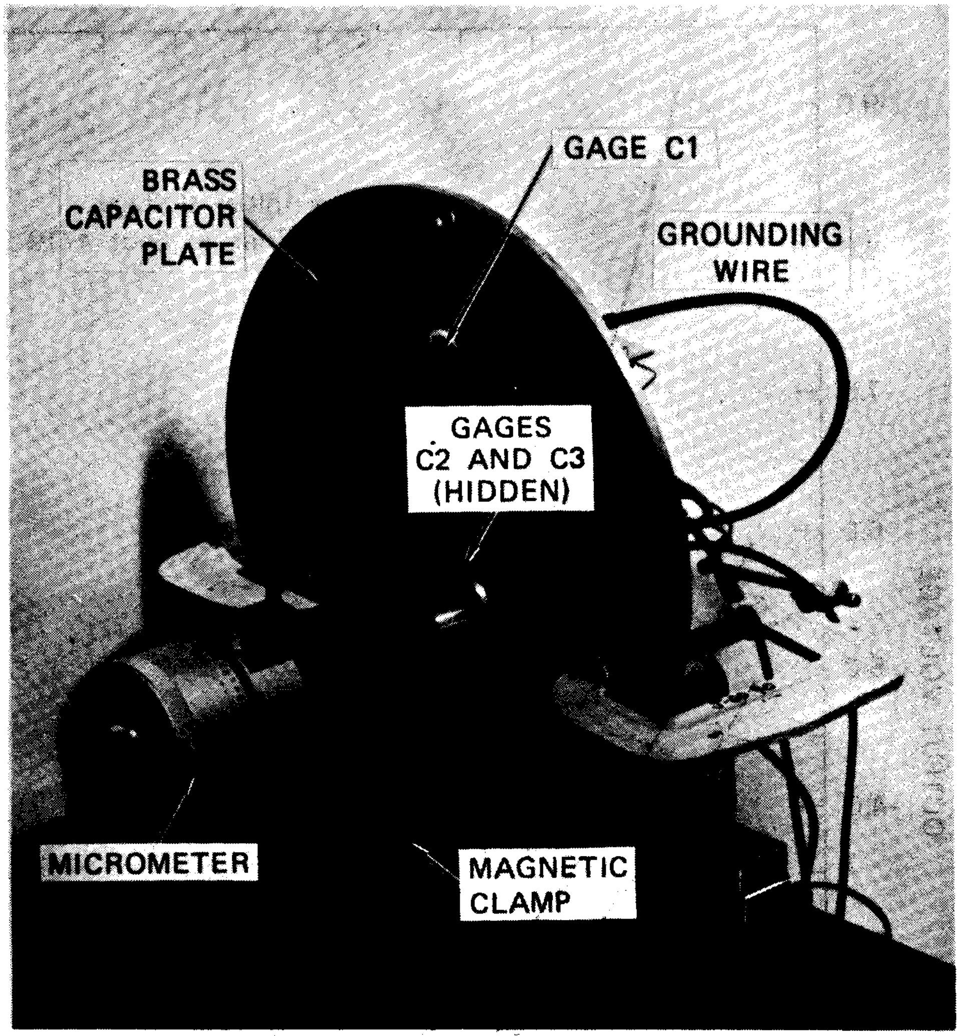


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FIGURE 6 SURFACE MOTION GAGE CALIBRATION SETUP

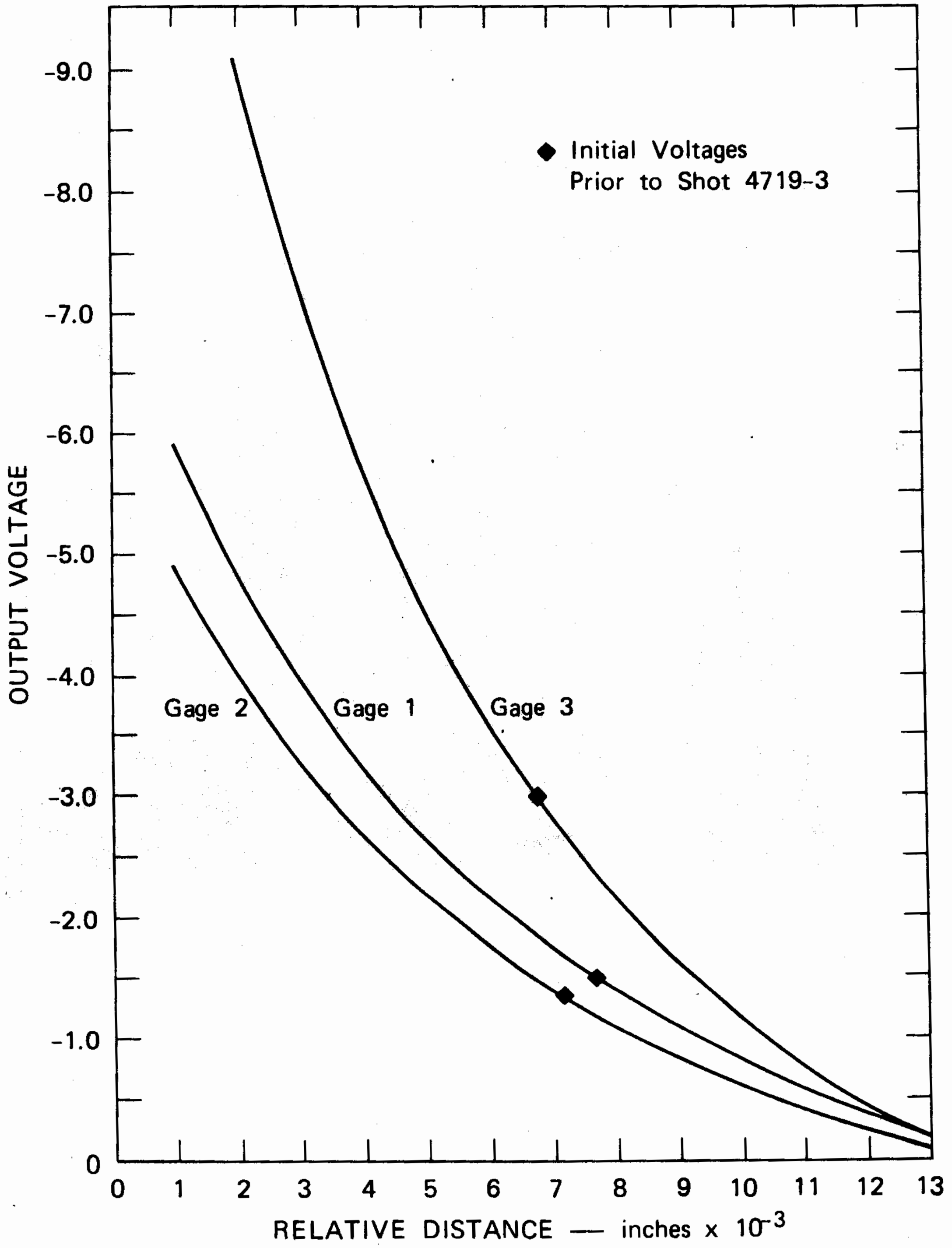
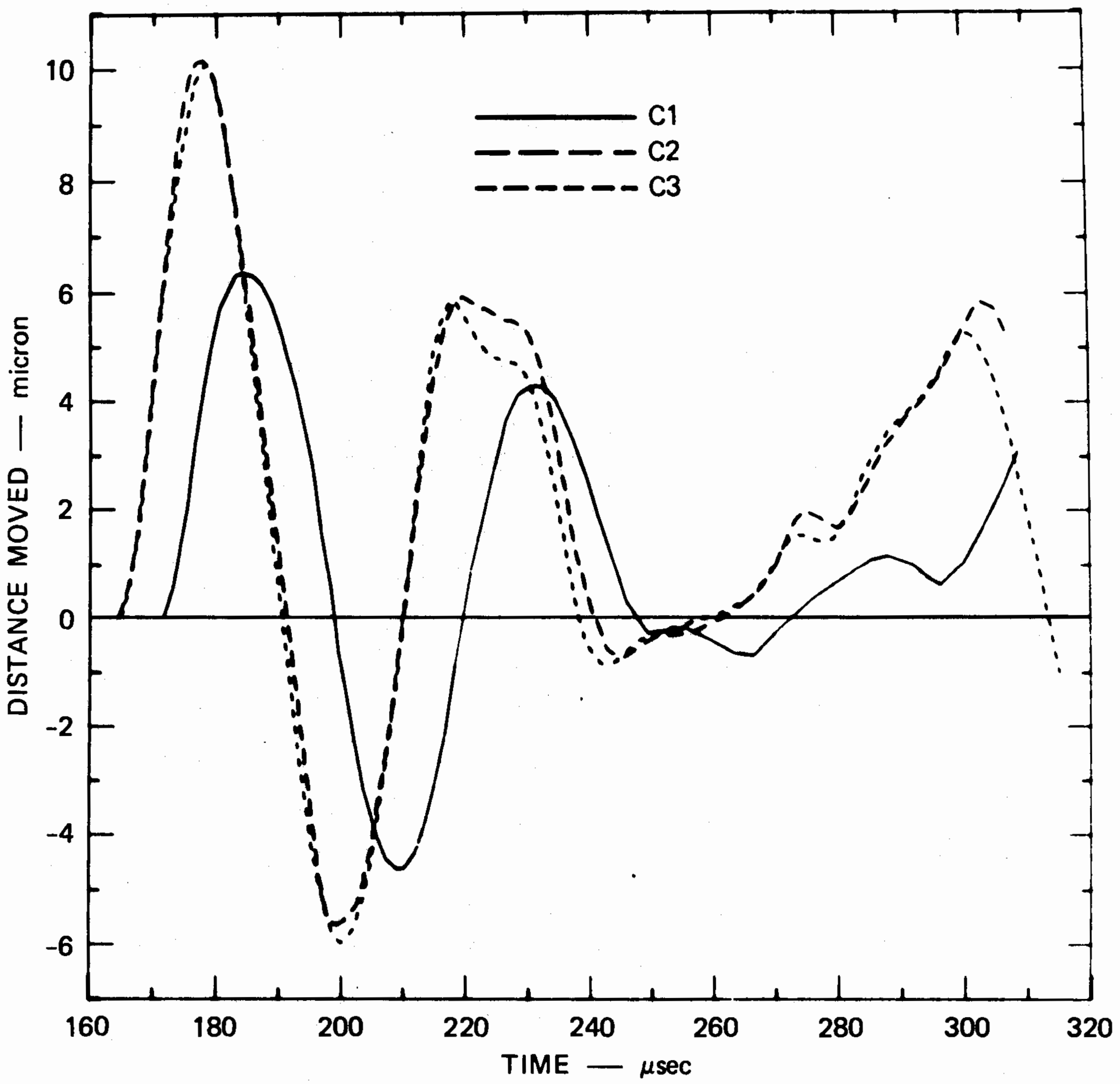


FIGURE 7 CALIBRATION CURVES FOR SURFACE MOTION GAGES



MA-4719-3

FIGURE 8 DISPLACEMENT HISTORIES FROM SURFACE MOTION GAGES

MEASUREMENT OF ANGULAR VIBRATION USING CONVENTIONAL ACCELEROMETERS

BY

P. WAYNE WHALEY AND MICHAEL W. OBAL

AIR FORCE FLIGHT DYNAMICS LABORATORY

A method is presented for measuring angular vibrations of aircraft structures using linear accelerometers. A rotary shaker system was used to examine low frequency noise and a beam experiment was conducted to examine high frequency flexure. The result is that one may tailor this method to fit a particular application. For Signal-to-Noise ratios of 5 or 10, angular vibrations of 2μrad at frequencies down to 50 hz can be readily measured for a typical structure. By specifying that the separation distance be less than one-third of a wavelength of the bending traveling wave of the structure, the maximum measurable frequency can be determined.

LIST OF SYMBOLS

θ = exact angular vibration response

$\frac{\Delta y}{\Delta x}$ = differential angular vibration response

Δx = accelerometer separation distance

ω = frequency in radians/sec

f = frequency in cycles/sec

S/N = Signal-to-Noise ratio

v = structural bending wave propagation velocity defined in Equation 11

λ = structural bending wavelength

ε = theoretical flexural error

ε_{exp} = experimental approximation to ε

I. INTRODUCTION

The performance of airborne electro-optical systems is seriously effected by the angular (rotary) as well as the linear (transverse) airframe vibrations. The angular vibration response is defined as the slope of the transverse deflection. Unlike linear vibrations, very little is known about the angular vibrations of aircraft structures and the design of airborne electro-optical equipment requires knowledge of the angular vibration response characteristics.

A technique for predicting angular vibration from linear vibration data was recently published (see Reference 1). This type of prediction scheme is a promising approach since the establishment of an angular vibration data bank is costly and there are no suitable angular transducers for such a program. However, the lack of transducers also hampers the validation of prediction schemes, so this work was undertaken to examine the measurement of angular vibration using conventional accelerometers.

A practical means for measuring angular vibration is to place two linear transducers some distance apart, subtract the outputs and divide the difference by the separation distance. The separation distance selection is an important consideration; a separation distance which is too small can result in a difference signal which is close to the transducer noise floor; on the other hand, separation distance should be chosen as small as possible to give the best approximation to the transverse deflection slope. In addition, acceleration signals are more sensitive to noise at low frequencies, therefore a small separation distance further deteriorates the low frequency response. The angle approximated by accelerometer differencing is proportional to the inverse of the separation distance, so the larger the separation distance the better the sensitivity. Thus, consideration of low frequency response and signal-to-noise ratio (S/N) yields a lower limit for separation distance. This is discussed in Section II.

Separation distance should be as large as possible for sensitivity purposes, but at some frequency and separation distance, the error due to flexure between accelerometers begins to be important. This flexural error may be defined in terms of number of nodes between accelerometers. However, node locations or spacings on a vibrating airframe are seldom known, so a different criteria is formulated here. By considering the speed of a bending traveling wave of a structure, it is possible to define a wavelength which is a function of frequency and structural properties. (See Reference 2). This wavelength relates to the node points on the structure in that standing waves form at the natural frequencies, giving rise to node points. Therefore by choosing separation distance as some fraction of a wavelength at the desired upper frequency limit, one may be assured of acceptable flexural error. The rationale for deciding on the fraction of a wavelength to use is given in Section III.

The acceptable limits of separation distance may be identified for a particular application using this technique. Those limits allow some flexibility for integration of the accelerometer locations into the hardware constraints. In this manner a method is presented for measuring angular vibrations which can supply useful data until acceptable broadband angular transducers become available.

II. NOISE CONSIDERATIONS

The technique for measuring angular vibration using linear accelerometers is based on Equation 1.

$$\Theta \simeq \frac{\Delta y}{\Delta x} \tag{1}$$

Θ = true angle
Δy = differential transverse displacement
Δx = separation distance

The lower limit for Δx based on low frequency sensitivity and S/N was examined for a wide variety of separation distances and angular levels. A rotary shaker system was used for these studies which implemented a very stiff rotary platform, thereby minimizing flexure between accelerometers. In this manner only the noise consideration is examined.

A low-powered laser with a system of mirrors and a quad cell detector provided an independent angular measurement. (See Figure 1.) The mirrors m_2 and m_3 fold back the laser beam twice to increase the effective distance between the rotary shaker platform and the quad detector. This

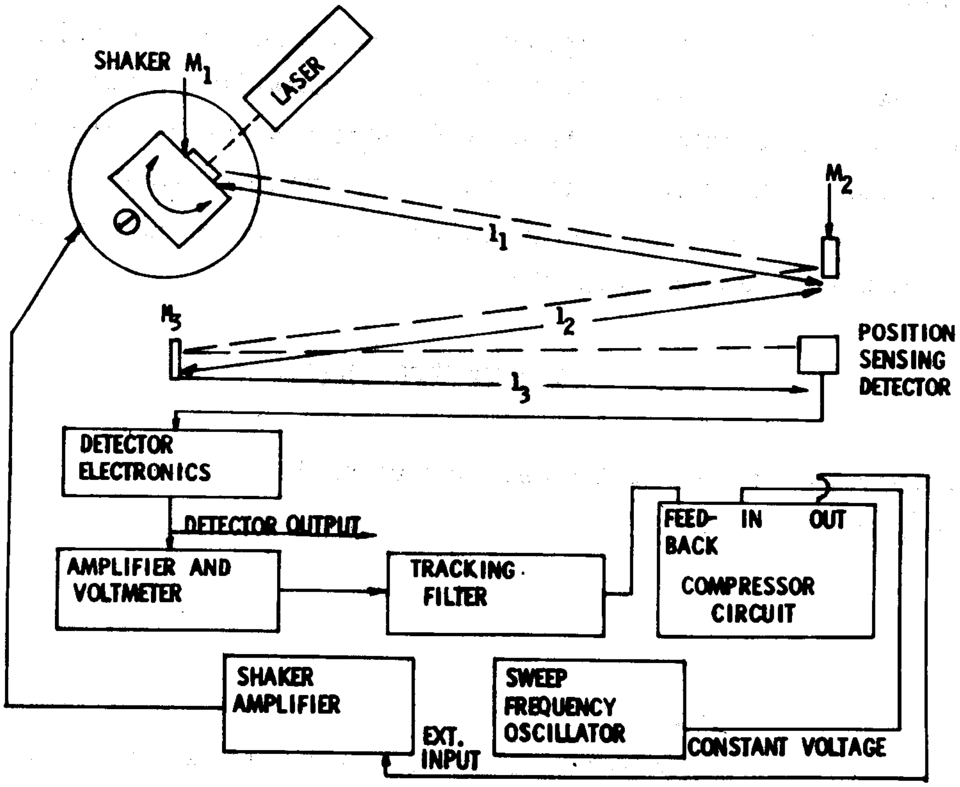


Figure 1. Rotary shaker system used in evaluating angular vibration measurement techniques.

increase in distance improves the sensitivity of the measurement system, as shown by the following sensitivity equation.

$$\frac{E_o}{\Theta} = 2\,(L_1 + L_2 + L_3)\,S_d \tag{2}$$

E_o = detector output voltage

Θ = angle of shaker platform

S_d = detector sensitivity in volts/mm

L_1 = distance between m_1 and m_2

L_2 = distance between m_2 and m_3

L_3 = distance between m_3 and quad detector.

The compressor circuit in Figure 1 was included to provide a constant angular input as the frequency was varied. Since the detector signal was noisy at low angular levels an amplifier and tracking filter were required between the detector electronics and the compressor circuit. Using this rotary shaker system, angular levels as low as 2 μrad were measured consistently.

For two accelerometers separated by Δx, the angular vibration about a point halfway between them may be approximated by Equation 1. This relationship will be exact when there is no flexure between the two points or in the limit as $\Delta x \to 0$. However, as Δx decreases the difference between the two accelerometer signals becomes contaminated with the internal noise of the accelerometers. Therefore the selection of Δx depends on desired low frequency response and sensitivity, and S/N. Angular vibration was measured using Equation 1 and compared with the results using the shaker platform of Figure 1. The relationship between the differential angular acceleration and the true angle is

$$\frac{\Delta\ddot{y}}{\Delta x}\,\frac{1}{\omega^2} = \Theta.$$

A typical commercially available accelerometer has a noise floor of $.0015g_{rms}$. Substituting that number as Δy and including a signal-to-noise

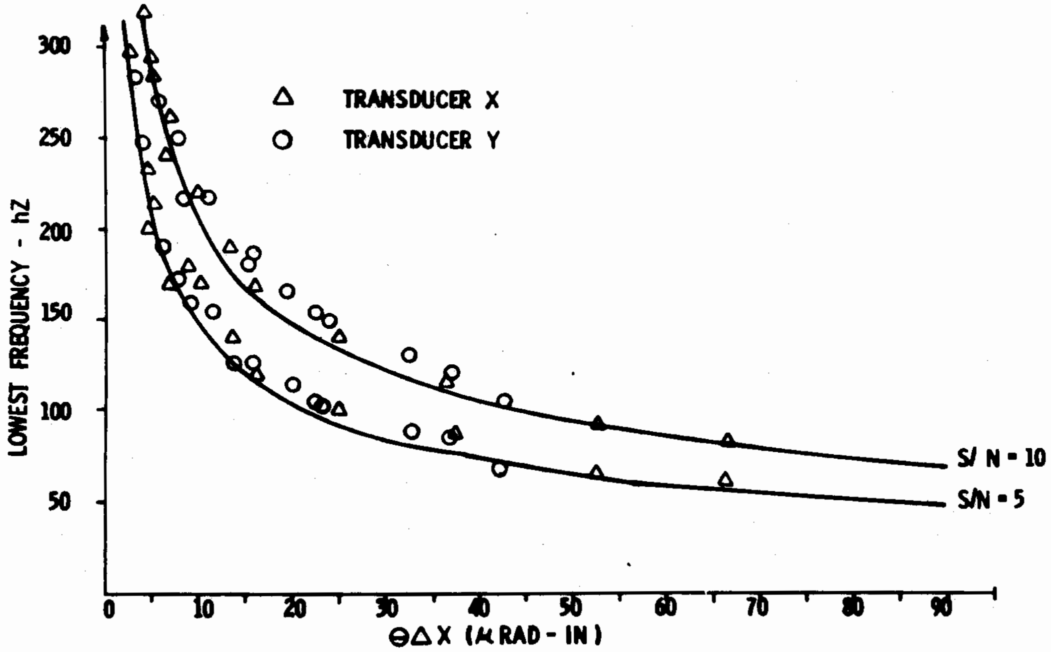


Figure 2. Noise floor for two accelerometers used in measuring angular vibration.

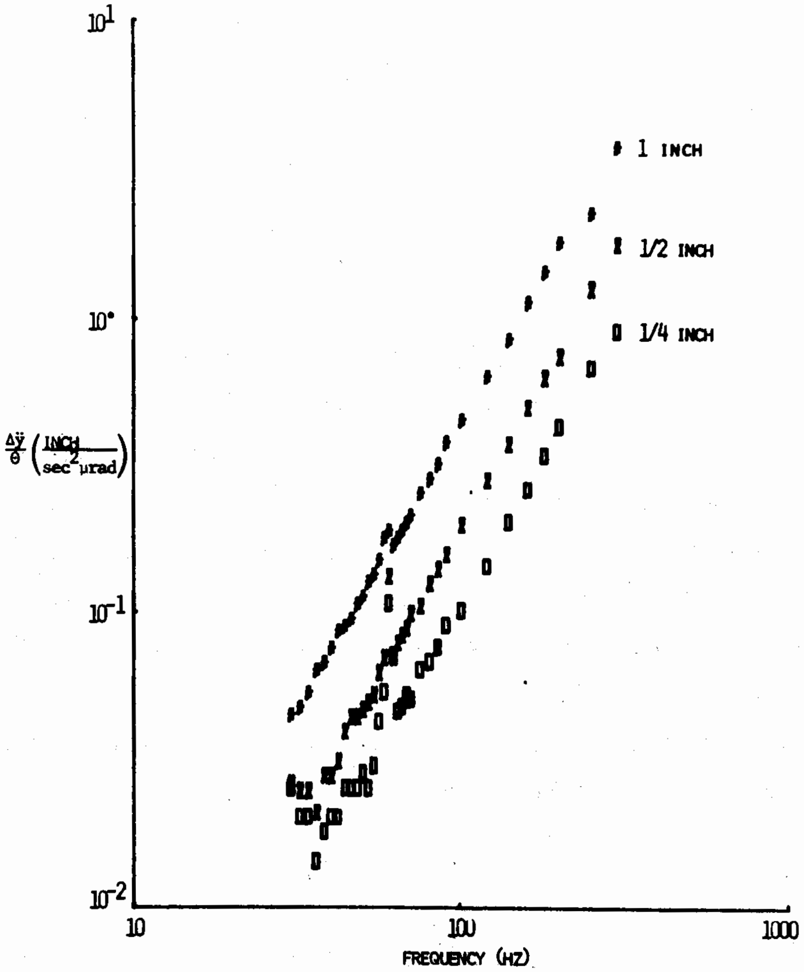


Figure 3. Angular acceleration frequency response for differential accelerometers using the rotary shaker system of Figure 1.

ratio (S/N), the above may be rewritten as

$$f^2\Theta\Delta x = .029\ S/N. \tag{3}$$

Equation 3 is plotted in Figure 2 for a S/N of 5 and 10, along with data from two different sets of accelerometers. Notice that for low angular levels and low frequencies, a large Δx is required for acceptable results. For example, to measure 2 μrad at 50 hz with a S/N of 5 requires a Δx of 42.5 inches, or to measure 4 μrad at 50 hz with S/N of 5 requires a Δx of 21.25 inches (2μrad x 42.5 inches = 85 μrad-inch).

In order to demonstrate the effects of small Δx on sensitivity, Equation 3 may be written in a slightly different form.

$$\frac{\Delta\ \ddot{y}}{\Theta} = \omega^2\Delta x \tag{4}$$

This equation was plotted along with data for varying Δx and is shown in Figure 3. For Δx = 1/4", Δy/Θ becomes larger than the theory at low frequencies, indicating that noise effects the performance. The 60 hz electrical noise peak was still present for Δx = 1/2 inches, although less prominant, but the effect of other noise contributions remains about the same; for Δx = 2 inches the noise was negligible. Doubling Δx increases Δÿ/Θ by 6 db, and for these experiments it appeared that Δx = 2" was about the best choice, with increases of Δx up to 5" having limited improvements. Another way to improve S/N is to integrate the acceleration signal to give velocity. The counterpart of Equation 3 for velocity is

$$f\Theta\Delta x = 4.67 \times 10^{-5} \tag{5}$$

Equation 5 is plotted in Figure 4, along with typical data. For Θ = 2μrad with f = 50 hz, Δx = 42.5, S/N = 10, while for acceleration it was 5. For velocity, the counterpart of Equation 4 is

$$\frac{\Delta\dot{y}}{\Theta} = \omega\Delta x. \tag{6}$$

This equation was plotted for varying Δx and is shown in Figure 5. The

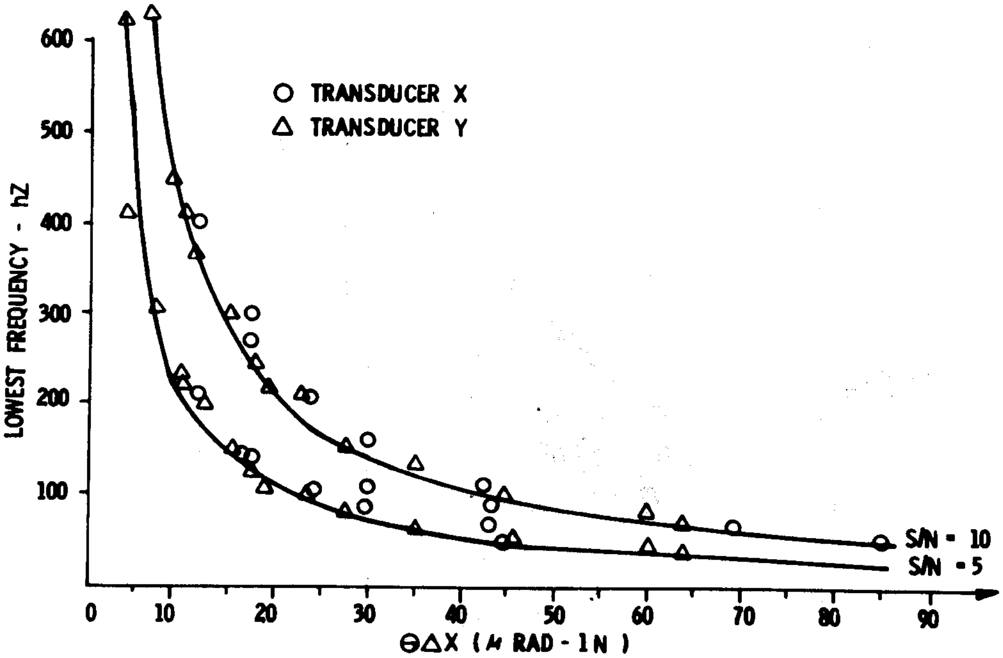


Figure 4. Noise floor of two accelerometers used in measuring angular vibration which has been integrated to give velocity.

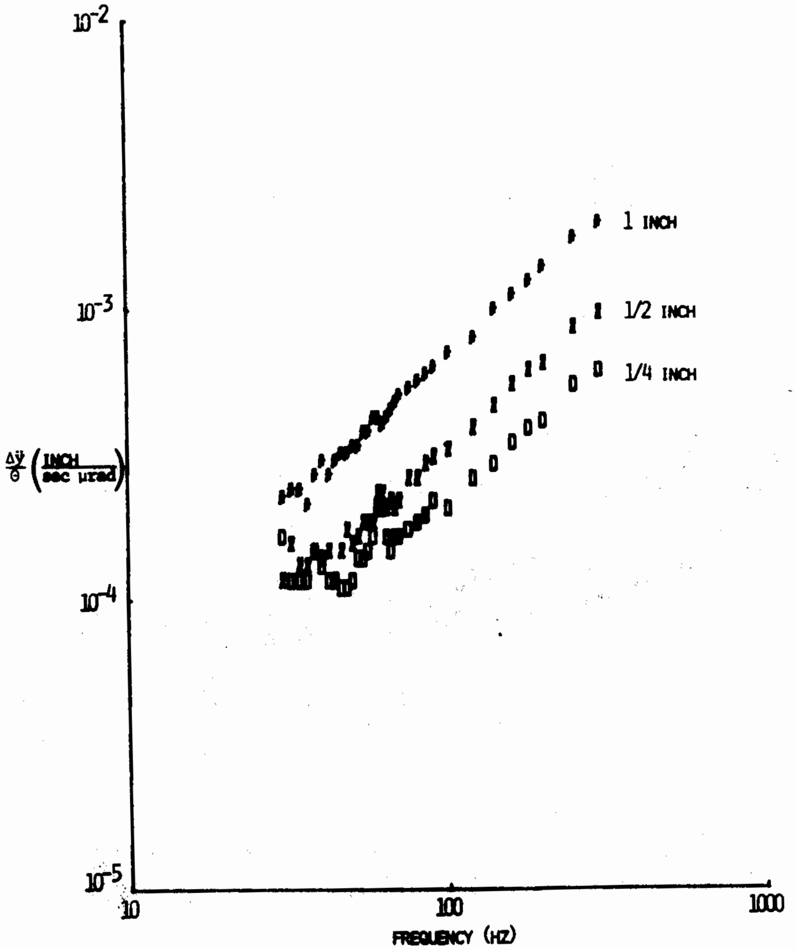


Figure 5. Angular velocity frequency response for differential accelerometers obtained by integrating the acceleration; rotary shaker system of Figure 1 was used.

sensitivity at low frequency was higher than for acceleration. Also the low frequency signal for velocity was less noisy than for acceleration. This is because the integrator has unity gain at around 100 hz; therefore, frequencies below 100 hz are amplified and the low frequency sensitivity is better. The other benefit of integrating is a 20 db improvement of dynamic range for every frequency decade.

To summarize, at a given frequency, as separation distance decreases the signal-to-noise ratio decreases. The trade-off is that as separation distance decreases at a given signal-to-noise ratio, the lower frequency limit increases. For a particular application the lower limit of separation distance will depend on the desired low frequency limit and the required signal-to-noise ratio. Therefore with a signal-to-noise ratio of between 5 and 10 it is possible to measure angles on the order of 5μrad down to about 50 hz with a lower limit of separation distance from 2 to 5 inches.

III. FLEXURAL ERROR

Angular vibration information is needed for arbitrary aircraft structures, so the concept of flexural error must be addressed in those terms. Since optical components are typically mounted on major structural components (bulkheads, spars, stringers, etc.) rather than on panels or skin structure, a beam analysis can provide qualitative flexural error insight. Therefore the rest of this section describes a simply supported beam excited with broadband random noise at mid-span.

For a uniform simply supported beam under constant amplitude forcing at mid-span, the Fourier Transform of the transverse deflection is

$$y(x,\omega) = \sum_{n=1}^{\infty} \frac{P\ell}{2m} H_n(\omega) \sin \frac{n\pi x}{\ell} \quad . \tag{7}$$

P = magnitude of forcing function

ℓ = beam length

m = mass/unit length of the beam

$H_n(\omega)$ = Frequency response function $= \dfrac{1}{\omega_n^2 - \omega^2 + j\gamma}$

γ = structural damping constant

ω_n = beam natural frequency $= \left(\dfrac{n\pi}{\ell}\right) \sqrt[2]{EI/_m}$

ω = frequency in radians/sec

E = Youngs modulus

I = beam cross section moment of inertia

The Fourier Transform of the angular deflection is found by differentiating Equation 7 to get

$$\Theta(x,\omega) = \sum_{n=1}^{\infty} \frac{n\pi}{\ell} \cos \frac{n\pi x}{\ell} \frac{P\ell}{2m} H_n(\omega) . \tag{8}$$

(For a complete derivation see Reference 1.) The differential angle defined in Equation 1 may be calculated for two transducers located at x_1 and x_2 using Equation 7.

$$\frac{\Delta y}{\Delta x} (x_1, x_2, \omega) = \frac{1}{\Delta x} \sum_{n=1}^{\infty} \frac{P\ell}{2m} H_n(\omega) \left[\sin \frac{n\pi x_1}{\ell} - \sin \frac{n\pi x_2}{\ell} \right]$$

By defining

x_c = the point where angular response is desired

Δ_x = separation distance,

x_1 = $x_c - \frac{\Delta x}{2}$

x_2 = $x_c + \frac{\Delta x}{2}$,

and utilizing a simple trigonometric identity, this may be rewritten as

$$\frac{\Delta y}{\Delta x} (x_c, \Delta x, \omega) = \frac{1}{\Delta x} \sum_{n=1}^{\infty} \frac{P}{2m} H_n(\omega) \cos \frac{n\pi x_c}{\ell} \sin \frac{n\pi \Delta x}{2\ell} . \tag{9}$$

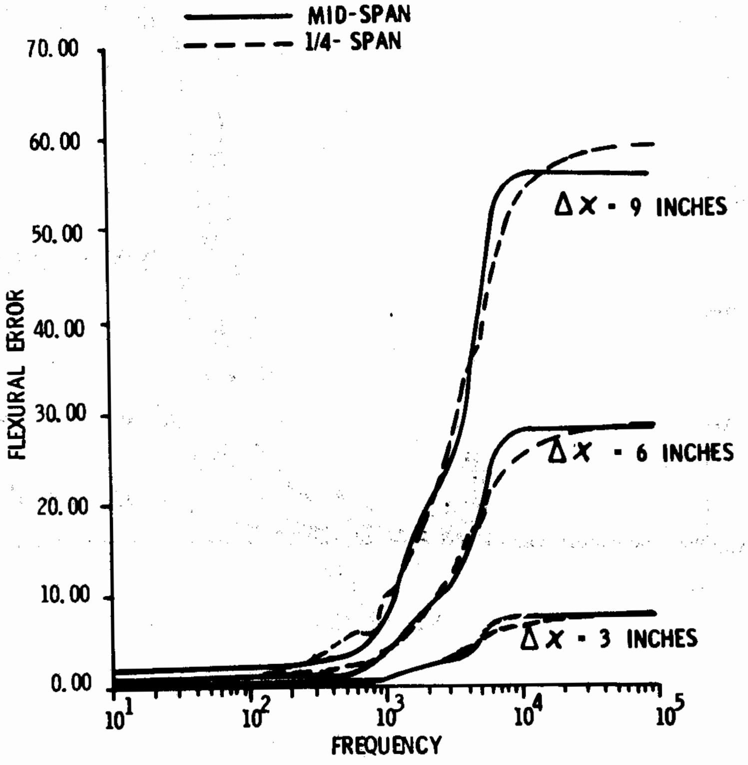


Figure 6. Theoretical flexural error between two accelerometers located on a simply supported beam centered at mid-span and 1/4-span.

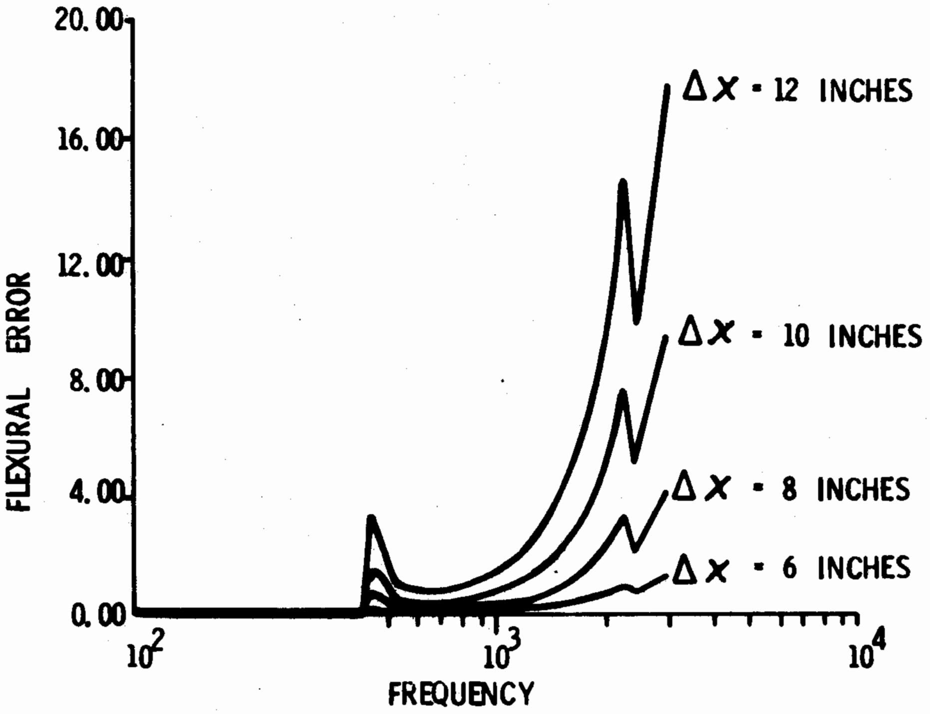


Figure 7. Theoretical flexural error between two accelerometers for various separation distance.

From Equation 9 it is seen that the differential angle is a function of x_c, Δx and ω. Also notice that in the limit as Δx goes to zero, Equation 9 becomes Equation 8. By using Equations 8 and 9, flexural error may be calculated using the definition given below.

$$\varepsilon = \frac{|\Theta(x_c,\omega)| - |\frac{\Delta y}{\Delta x}(x_c,\Delta x,\omega)|}{|\Theta(x_c,\omega)|} \tag{10}$$

From Equation 10 it is seen that although flexural error depends on x_c, Δx, and ω, x_c was shown to be an insignificant effect, as demonstrated in Figure 6. Equation 10 was plotted versus frequency for $x_c = 27$ inches and $\Delta x = 6$, 8, 10, and 12 inches in Figure 7. Note that as frequency and separation distance increase, these curves fan outward indicating more severe flexural error.

In order to determine at which frequencies and separation distances flexural error is acceptable, it is convenient to use an analogy from acoustics. Acoustical systems are sometimes modeled as lumped parameter systems whenever their physical dimensions are small compared to one fourth of the wavelength. This lumped parameter modeling concept is based on the idea that spatial distribution has a negligible effect, and time becomes the only independent variable. It is reasonable that an analogous condition holds for the differential measurement of angular vibration; that is, when the accelerometer separation distance is small compared to some fraction of a wavelength, then the spatial effects (flexure) of the beam will be negligible. That idea is the basis for choosing the upper limit of separation distance as described below.

From wave propagation theory, the wavelength is defined as the wave propagation velocity divided by frequency. The wave propagation velocity for a beam bending traveling wave is

$$v = \sqrt[4]{EI/m}\,\sqrt{2\pi f}\ . \tag{11}$$

(See Reference 2.)

Then by the above wavelength definition,

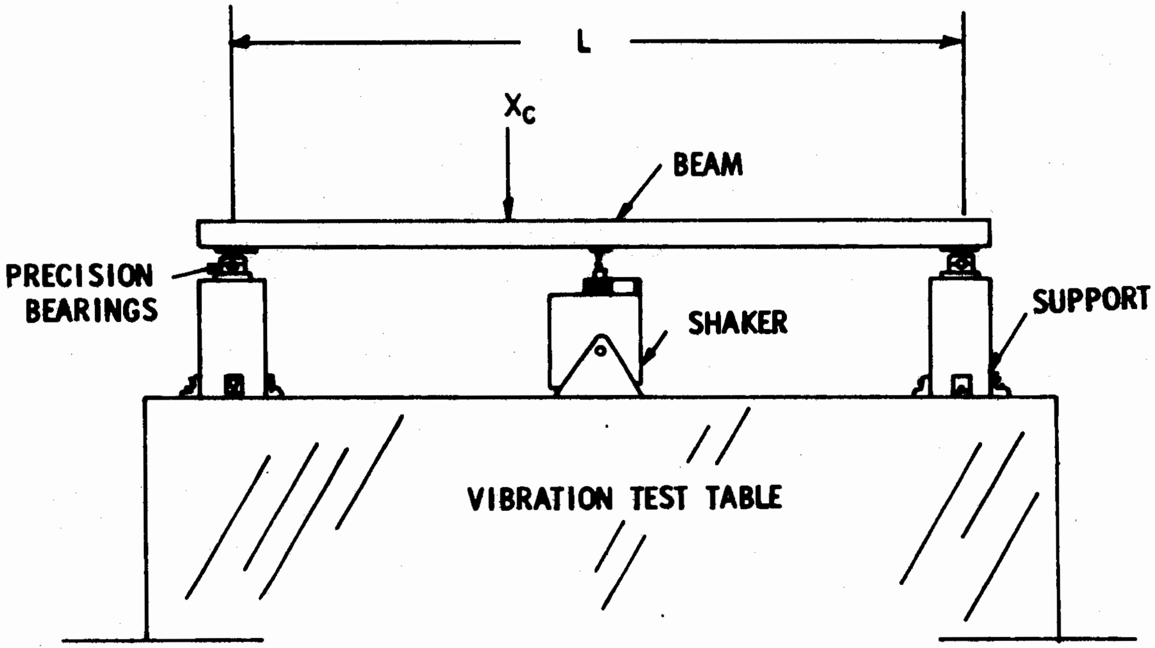


Figure 8. Experimental simply supported beam used in verifying flexural response between two accelerometers.

$$\lambda = \frac{v}{f} = 2\pi \frac{\sqrt[4]{EI/m}}{\sqrt{2\pi f}}, \tag{12}$$

Where λ = wavelength. By choosing $\Delta x \leq \lambda/2$ one would expect that there be no more than one node between accelerometers. One might expect a quarter wavelength as a reasonable choice, but the analysis summarized in Figure 7 suggests that may be too stringent. The measurement of differential angle was examined in the laboratory to verify this result.

A simply supported aluminum I-beam was assembled in the laboratory using bearing supports at the ends. (See Figure 8). The precision bearings were chosen to minimize bearing chatter and lubrication was used regularly to minimize friction during the vibration tests. A 25-pound shaker was attached at beam mid-span through a flexure and was driven by broadband random noise. The random vibration of the beam was monitored so that an accelerometer mounted on the beam above the shaker had a broad band spectrum from 50 hz to 3200 hz. (See Figure 9). Two methods of subtracting accelerometer signals to get angle were attempted;

(1) an electronic differencing circuit was utilized and

(2) the individual signals were digitized, then subtracted numerically. All transducer outputs were recorded on FM tape for later analysis.

The measurement technique described above is a critical part of this documentation. Since there are no suitable broadband angular transducers to check the differential angle, particular care was taken to assure that the angular measurement procedure was correct. By checking the individual accelerometer outputs it was possible to verify that the data was reliable. The differential angular acceleration was computed different ways giving an independent check; the difference signal computed by electronic and numerical differencing was essentially the same. The angular data was also verified analytically using Reference 1.

Though the purpose of this experiment was to validate the theoretical flexural error plotted in Figure 7, that is not directly possible since the true angle is not obtainable. However, by using the results of Section II it is possible to estimate the true angle. The conclusion of

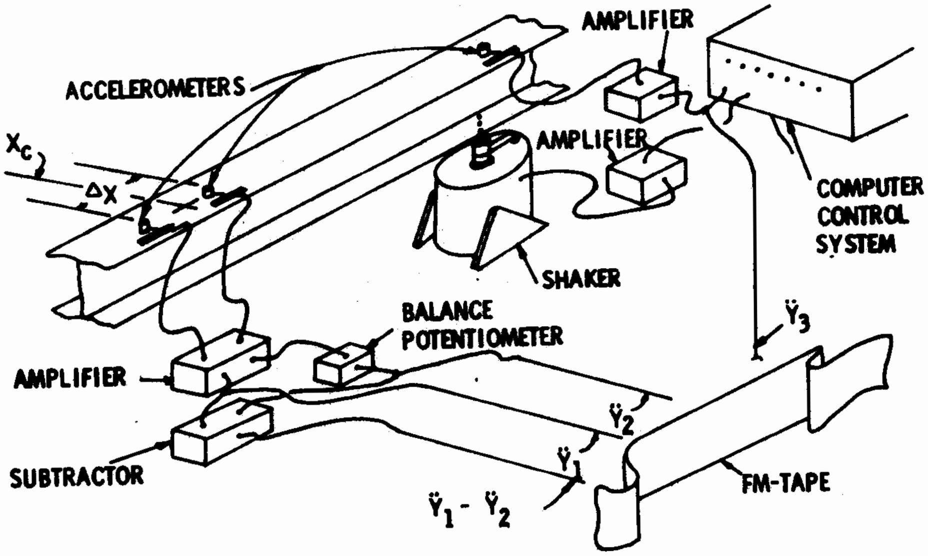


Figure 9. Instrumentation of the experimental beam shown in Figure 8.

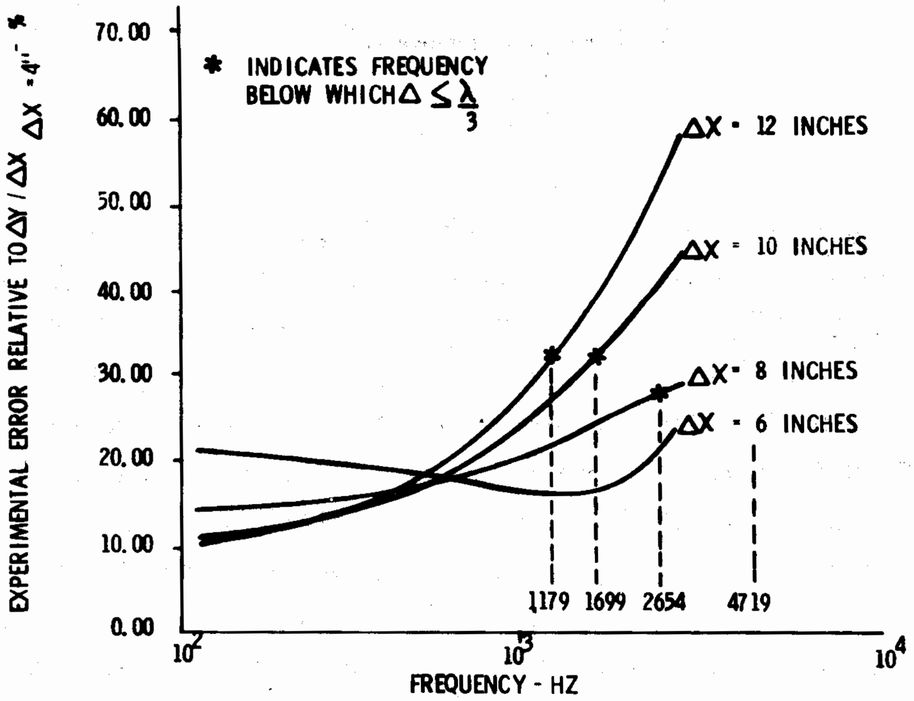


Figure 10. Experimental flexural error for the beam of Figures 8 and 9 using a least squares curve fit to random data.

Section II was that separation distances of between 2 and 5 inches give good S/N. Since low frequency sensitivity increases with separation distance, 4 inches was chosen as the best value for an accurate angular estimate. By applying Equation 13, a 4 inch separation distance gives a theoretically acceptable error up to 10.6 Khz. Even though this yields an approximation to the true angle, it is probably an acceptable reference at higher frequency and separation distance. The experimental error is

$$\varepsilon_{exp} = \frac{\left|\frac{\Delta y}{\Delta x}\right|_{4\text{ in.}} - \left|\frac{\Delta y}{\Delta x}\right|}{\left|\frac{\Delta y}{\Delta x}\right|_{4\text{ in.}}} . \qquad (14)$$

A least squares curve fit of Equation 14 is plotted in Figure 10 for four values of separation distance. Although Figure 10 shows flexural error above 10 percent in most cases as compared to flexural errors of less than 1 percent for the theoretical curves of Figure 8, Figure 10 shows the same fanning phenomenon as frequency and separation distance increase.

Further validation of Equation 13 may also be undertaken using Equation 14. By looking for the frequency at which Equation 13 holds for the experimental beam given Δx, one may observe how upper frequency limit decreases with increasing separation distance. That was done and is indicated on Figure 10. Other separation distances were examined and further substantiate the trend of Figure 10. In addition, the authors feel that use of separation distances of much more than about 12 or 14 inches will limit the upper frequency range. That fact is demonstrated by Figures 7 and 10 which show that error is increased rapidly as separation distance and frequency increase.

The error of Figure 10 below 600 hz is not reliable. One would expect the higher frequencies to be primarily effected by flexural error and that trend is verified by Figure 10. Although the error of Figure 10 appears inconsistent at lower frequencies, the individual angle measurements agree well. That is summarized below in Table I with differential angles computed at 100 hz for various separation distances with a bandwidth of 1.7 hz.

TABLE I. Angles at Various Separation Distances at 100 hz.

Δx, in	$\frac{\Delta y}{\Delta x} \times 10^{-7}$, rad
4	1.89
6	1.84
8	1.82
10	1.60
12	1.67
14	1.62

From Table I, separation distances of 6 and 8 inches give angular measurements which agree to within 1.1 percent. Therefore the low frequency error of Figure 10 is not reliable. This is because low frequency and low separation distance information has a lower S/N and sensitivity, thus making low frequency experimental error unreliable. This problem with low frequency experimental error is not indicative of actual angular measurement problems, as shown by Table I. Note that the measured angles of Table I are all below 1 μrad, the required measurement capability for angular transducers.

The important result of this section may be summarized as follows. An analytical expression for angular vibration error suggests that using an upper bound for separation distance of one third of a wavelength gives flexural error of less than 2%. This trend was verified experimentally although the experimental error was higher than the analytical error. This is because the experimental error depends on a reference approximate angle calculated with a separation distance of 4 inches.

IV. DISCUSSION

Using the technique given here, acceptable ranges of separation distance may be calculated by the engineer for any particular application. As one might expect, separation distance must not be too small or the S/N will be unacceptable. On the other hand separation distance must not be too large or flexure between accelerometers will severely hinder the upper frequency range. In general, a separation distance of between 2 and 5 inches gives a S/N of about 5 to 10 at frequencies down to about 50 hz. However, when one is primarily interested in low frequency response a larger separation distance can offer improvements.

For upper frequency limit, one must estimate the component structural properties and assure that the separation distance be less than one third of a wavelength. This criteria does not depend on boundary conditions, only on the local element on which angular vibration is to be measured. However, there are two difficulties one might encounter.

(1) For some complex structures with varying cross-sectional properties or with curved geometry, it may be difficult to accurately determine the correct equivalent structural properties to use with Equation 13. In that case, some equivalent cross section moment of inertia and mass distribution must be estimated, and the flexural error may not be acceptable. In addition, at this time there is no known technique for determining from the data when there is unacceptable flexural error.

(2) It is conceivable that for some problems where the structure is relatively flexible and where high frequency response is desired that there will be no separation distance which will yield acceptable results. In that case, obviously the differential angle method will not work. At the time of this writing differential angle measurement is scheduled for use on two flight test programs at the Air Force Flight Dynamics Laboratory but verification of this technique through that experience is not yet available.

Notice that for plates Equation 13 must be modified. Although for most structural components beams are probably a good model, it may be that one desires to measure angular vibrations on a plate element. That may be done by substituting for Equation 11,

$$v = \sqrt[4]{\frac{Et}{\sqrt{12\rho(1-\theta^2)}}}\;\sqrt{2\pi f}\;,$$

Where t = plate thickness

ρ = plate volume density

Θ = poisson's ratio.

Although there is at present no experimental data to support this approach for plates, it is known that a strong mathematical similarity exists between beams and plates so this should be acceptable until further experimental or analytical verification is available. (See References 3 and 4).

V. CONCLUSIONS

This study describes a method for measuring angular vibration using linear accelerometers by establishing a criteria for the choice of separation distance. The lower limit of separation distances is chosen based on the requirements concerning low frequency low amplitude signal-to-noise ratio. For most structures it is possible to measure angles on the order of 2μrad down to about 50 hz for S/N of 5 or 10 with a lower limit of separation distance from 2 to 5 inches. The upper limit of separation distance is chosen based on the structural properties and desired upper frequency range. By specifying the upper bound for separation distance as one-third of a wavelength, the high frequency theoretical flexural error is below 2 percent. This procedure provides an acceptable range for separation distance which will usually allow some freedom in integrating the measurement system into the hardware.

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AIRBORNE ENVIRONMENTAL AND ELECTRICAL MEASURING SYSTEM

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ABSTRACT

An instrumentation system for in-flight measurement of environmental conditions and electrical output parameters of aircraft electrical generating systems was developed and constructed by the Naval Air Test Center (NATC), Patuxent River, Maryland. The environmental measurement transducers are installed in the aircraft while the electrical generating system measurement transducers are installed in a pod which is located on an aircraft store station. Transducer outputs are recorded on a magnetic tape recorder. Environmental parameters measured are altitude, airspeed, pertinent temperatures, vibration, oil pressures, and air flow rate. Electrical parameters measured are rms voltage, rms current, power, and frequency.

Special problems encountered during the development of the system and specific transducers used are discussed in this paper. Primary emphasis is placed on in-house designed signal conditioning for frequency and rms voltage measurements.

INTRODUCTION

1. Background. The airborne environmental and electrical measuring system (E&E system) was developed at the Naval Air Test Center (NATC) for the flight test evaluation of the variable speed constant frequency (VSCF) power generating system. The test plan objective was to monitor system performance under known flight conditions. This dictated the requirements for the E&E system. The test plan also required simultaneous tests of two VSCF systems on two aircraft. Due to limited funding which existed at the time, the decision was made to use one instrumentation system on both aircraft. This lead to the housing of all practical E&E system instrumentation in a pod to facilitate aircraft installation. Funding was later made available for construction of a second E&E system, which is now 90% complete.

DISCUSSION

2. General. The parameters measured by the E&E system and required accuracies are listed in Table I. Operating temperature limits were relaxed for all equipment in the pod. The toughest accuracy requirements were placed on measurements of rms voltage and frequency. The in-house designed signal conditioning for these two parameters is discussed in depth in the following paragraphs. All transducers and conditioners used are listed in Table II. A system block diagram is shown in Figure 1. All parameters, except vibration, are multiplexed through a pulse code modulation system (PCM) before recording on the magnetic tape recorder. The overall performance of any of the multiplexed parameters is limited by the accuracy of the PCM system. The worst case error of the PCM system used is ±0.28% F.S. with resolution of 0.1% F.S. The PCM system can accept 100 channels at a sample rate of 100 samples per second per channel.

NATC DESIGNED SIGNAL CONDITIONING - VOLTAGE AND FREQUENCY

3. RMS Voltage to D.C. Converter

a. Background. The VSCF system specification required the system output voltage to remain within the limits of 115 ±1 Vrms under normal load conditions. This required instrumentation system accuracy of ±0.1 Vrms. A nominal calibrated range of 110 to 120 Vrms was chosen for the instrumentation. As noted in Table I, the operating temperature limits were relaxed for the voltage measurement because all associated instrumentation is in the pod. The relaxed limits of -40°C to +50°C still posed severe drift problems for all conventional rms to dc converters known to NATC at the time.

The first approach was to write a specification for the rms to dc converter and request proposals from industry. Only one manufacturer responded with a proposal which met specification requirements. This proposal was accepted and six units were purchased. These units were used successfully in the first E&E system. The only problem experienced was with the quality of the temperature controlling ovens in which the manufacturer housed his electronic circuitry. Two of the ovens failed over a period of one year.

When funding was made available for a second E&E system two years later, the same specification was used to request proposals. The same manufacturer again submitted the only proposal meeting specification requirments. However, the price of the units doubled, and the estimated delivery time was about 6 months. At this point the decision was made to design the units in-house. It was estimated that units could be designed and fabricated in-house for about the same cost as proposed by the manufacturer in half the calendar time. An additional factor considered was the in-house capability to readily repair units designed in-house; a capability which does not exist with the manufacturer's units.

b. Circuit Description. Referring to Figure 2, the heart of the 102175 rms to dc converter circuit is the Function Module 591. The input voltage signal is attenuated by a factor of 0.049 by the input differential amplifier A1. This attenuation factor was chosen to limit the input to the 591 converter at ±10 volts peak at a worst case crest factor of 1.7 at 120 Vrms system input. The output of the 591 is approximately 7.62 Vdc to 8.32 Vdc for system input voltages of 110 Vrms and 120 Vrms respectively. This output is fed into amplifier A2 whose gain equation is:

$$(1)\ V_{out} = -7.225\ V_{in} + 55.083$$

to yield an output of 0 to -5 Vdc for the system input range of 110 Vrms to 120 Vrms. The output of amplifier A2 is fed to the two-pole Butterworth low-pass filter network containing A3. The filter gain is -1 to provide a system output voltage of 0 to +5 Vdc for inputs of 110 Vrms and 120 Vrms respectively. The 3 db cutoff of the filter is 10 Hz insuring that any 400 Hz ripple introduced by the 591 will be attenuated by at least 30 db.

c. Design Problems. The biggest design problem to overcome in the 102175 rms to dc converter circuit was temperature drift. The specified temperature characteristics of the Function Module 591 converter are shown in Figure 3A. The 591 temperature characteristics are good in terms of its full-scale range, but it must be noted that only the region from 76.2% F.S. to 83.2% F. S. of the module's output is being utilized. Since this region is expanded by amplifier A2 in Figure 2, the effects of 591 output drift are unacceptable in terms of system outputs, as shown in Figure 3B. This lead to housing all of the circuitry shown in Figure 2 in an oven with temperature control of $65^{o}C \pm 2^{o}C$ over an ambient range of $-40^{o}C$ to $+50^{o}C$. Figure 4 shows the temperature stability of one of the 102175 rms to dc converter circuits tested at NATC after enclosing all circuit components in an oven. Figure 5 shows the linearity of one of the units tested at room ambient temperature.

4. Frequency to DC Converter

a. Background. The VSCF system specification required the system output frequency to remain within the limits of 400 ±1 Hz under normal load conditions. This required instrumentation system accuracy of ±0.1 Hz. A nominal calibrated range of 390 to 410 Hz was chosen for the instrumentation. The required operating temperature limits were the same as for the rms to dc converters, $-40^{o}C$ to $+50^{o}C$. The development history of the frequency to dc converter was identical to that of the rms to dc converter except that no satisfactory proposals were received. The frequency measurements of the first E&E system did not meet the accuracy requirements of Table I. When funding was made available for the second E&E system, it was decided to design the frequency to dc converter in-house.

b. Circuit Description. Referring to Figure 6, the heart of the 111275 frequency to dc converter is the Richard Lee 713 frequency converter. The output of the 713 is a constant amplitude, constant period pulse whose duration is directly proportional to the frequency of the 713 input. The output of the 713 is fed to the pulse shaping circuit Z1 composed of Q1 and Q2. This circuit insures that a constant amplitude pulse is available to the four-pole Butterworth filter composed of operational amplifiers A1 and A2. The filter had a 3 db cutoff of 3 Hz to insure that only the dc component of the 713 output is preserved. The second stage of the filter, A2, also provides offset. The output of the filter is fed to A3, which provides gain for range control.

c. Design Problems. Temperature stability was the primary design problem to overcome with the 111275 frequency to dc converter, as it was with the rms to dc converter discussed earlier. All components of the 111275 were housed in an oven identical to the one used for the rms to dc converter.

Referring to Figure 7, it can be shown from the Fourier expression for the 713 output that filtering out all frequency components above dc yields a dc voltage given by the expression

$$a_o = K \frac{T2}{T1}$$

where K is the peak-to-peak level of the pulse, T1 is the pulse rate, and T2 is the pulse duration. With K and T1 held constant, it is apparent that the dc voltage a_o varies directly with the pulse duration, T2. A key to the accuracy of the frequency measurement is insuring the stability of T1 and K. The stability of T1 is determined by the quality of capacitor C1 and resistor R1 in Figure 6, as their RC time constant determines the 713 output pulse repetition rate, T1. The stability of T1 was insured in the 111275 circuit design by using a polystyrene capacitor for C1 and a precision metal film resistor for R1. The stability of K was insured by incorporating the pulse shaping circuit Z1 in Figure 6.

TABLE I

PARAMETERS MEASURED BY E&E SYSTEM

PARAMETER	RANGE	REQUIRED ACCURACY
A.C. VOLTAGE	110 to 120 VOLTS RMS	±0.1 VOLTS RMS
A.C. CURRENT	0 to 100 AMPS RMS	±0.5 F.S.
FREQUENCY	390 to 410 HZ	±0.1 HZ
POWER	0 to 20 KW	±1.0% F.S.
RPM	2000 to 12000 RPM	±0.5% F.S.
*VIBRATION	±10G @ 4-2000 HZ	±5.0% F.S.
*TEMPERATURE	-55°C to +125°C	±1.0% F.S.
*DIFFERENTIAL PRESSURE	±0.5, ±1 PSID	±5% F.S.
*ABSOLUTE PRESSURE	0 to 20 PSIA	±5% F.S.

* Accuracy to be maintained over temperature range of -55°C to 125°C. All others -40°C to +50°C.

TABLE II
TRANSDUCERS & CONDITIONERS USED IN E&E SYSTEM

PARAMETER	MANUFACTURER	MODEL NUMBER	ACCURACY
PCM UNIT	BASE TEN	416-238	±0.28% F.S.
A.C. VOLTAGE	NATC/TSD *	102175	±0.5% F.S.
A.C. CURRENT	NATC/TSD *	103075	±0.5% F.S.
FREQUENCY	NATC/TSD *	111275	±0.5% F.S.
POWER	KRATOS	20.604	±0.5% F.S.
TEMPERATURE	HADES	NCA150	±1% F.S.
RPM	FOXBORO	FR322	±0.1% F.S.
DIFFERENTIAL PRESSURE	SETRA	236	±4% F.S.
ABSOLUTE PRESSURE	SETRA	202	±4% F.S.
VIBRATION	ENDEVCO	2223D TRIAXIAL ACCELEROMETER	±5% F.S.
	ENDEVCO	2640 CHARGE AMPLIFIER	

* Designed and fabricated by Naval Air Test Center.

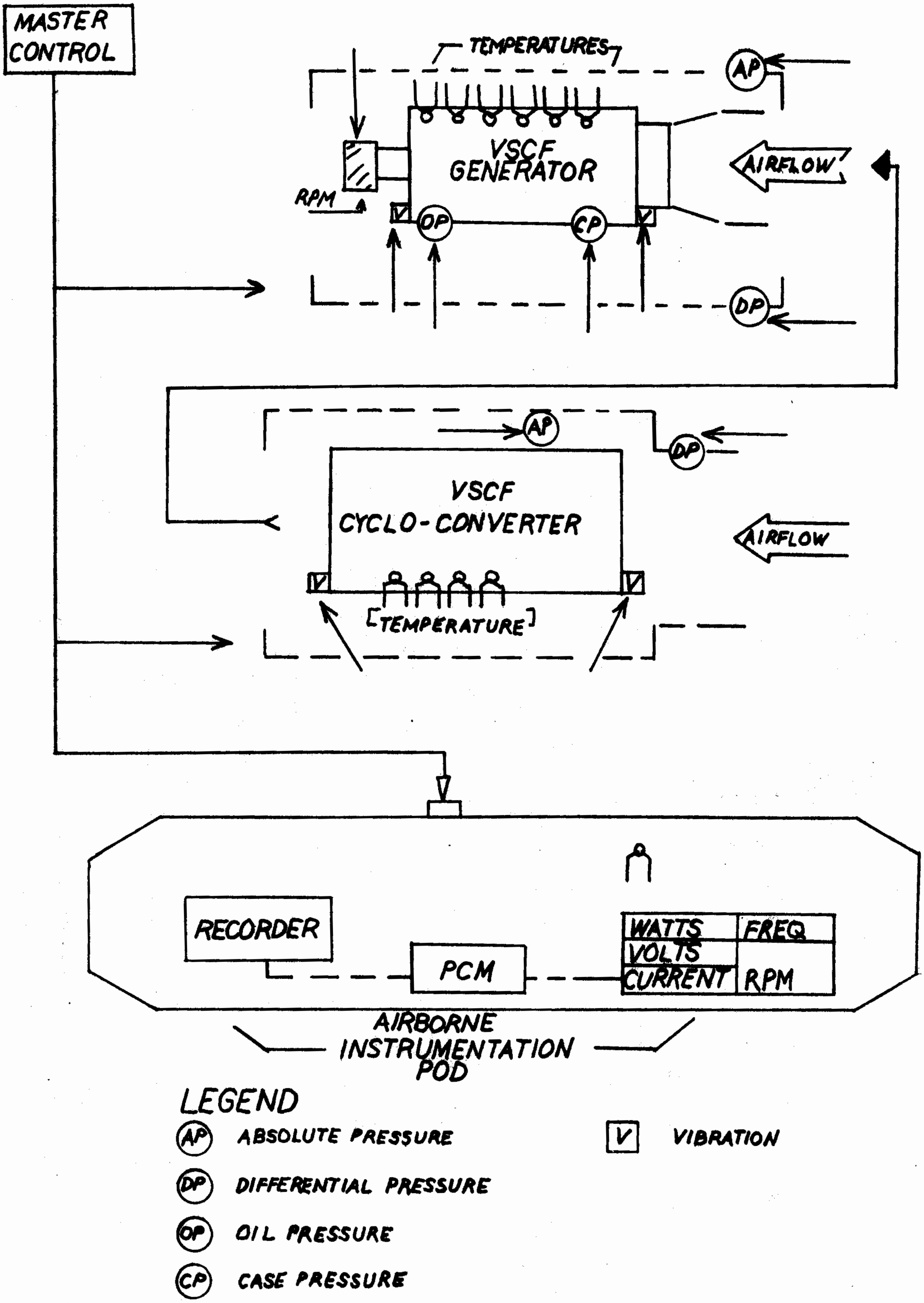


Figure 1. System Block Diagram.

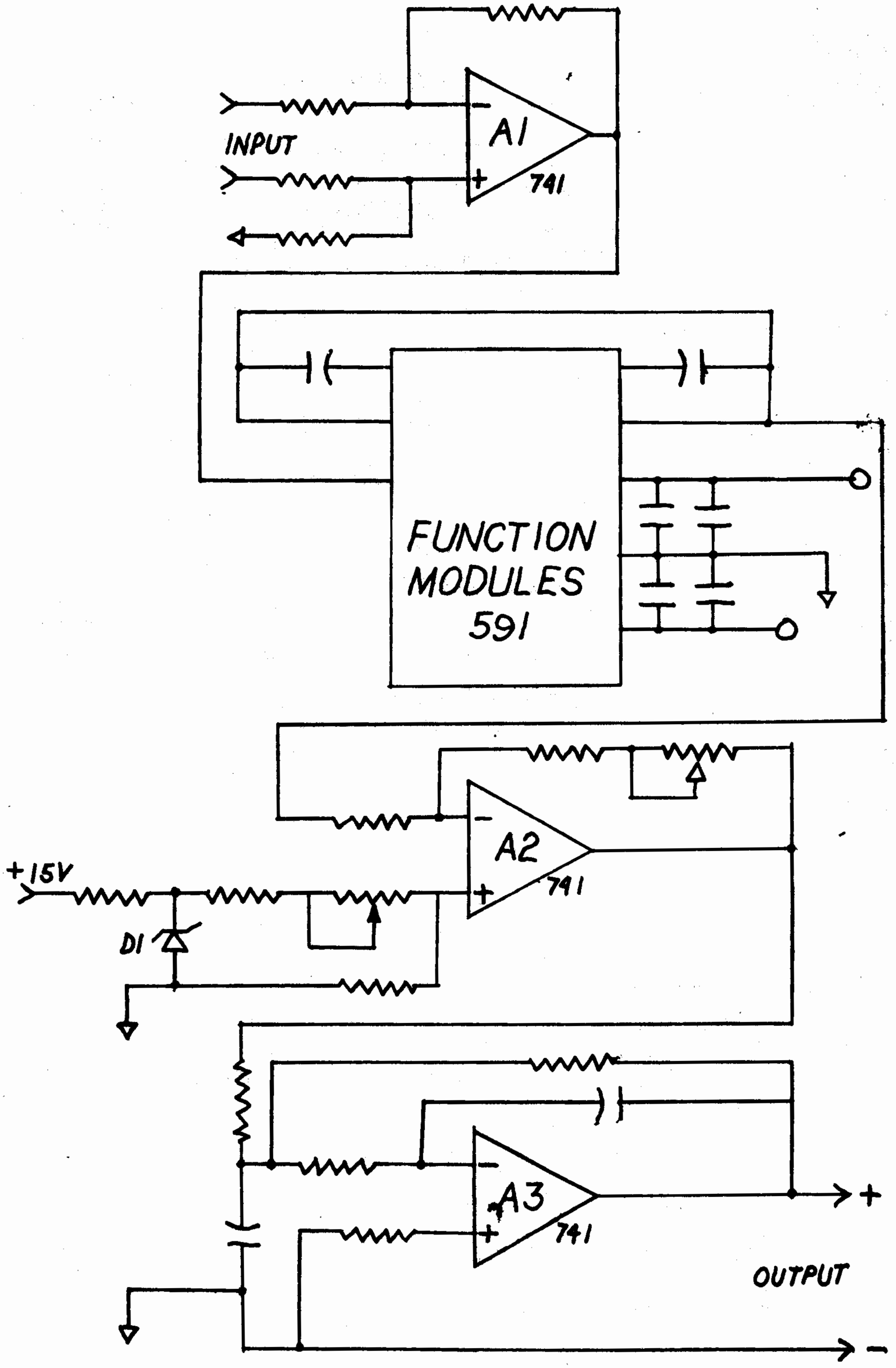


Figure 2. NATC 102175 RMS to DC Converter.

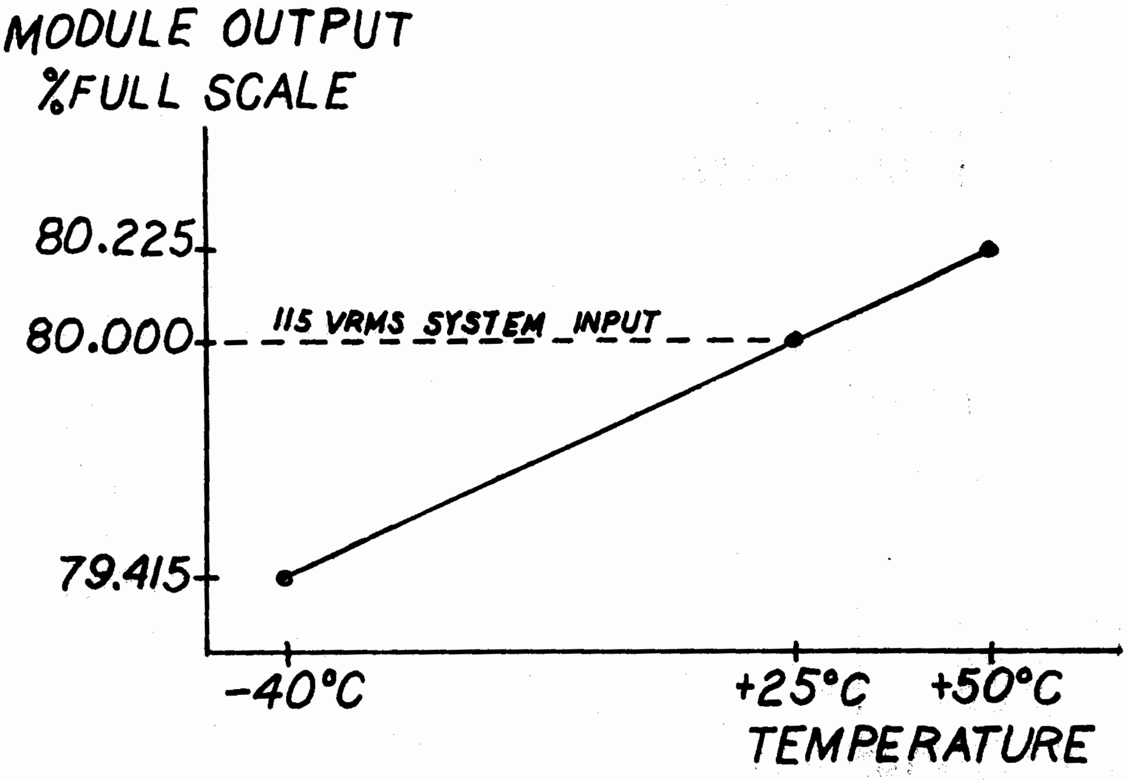


Figure 3a. Predicted 591 Module Drift.

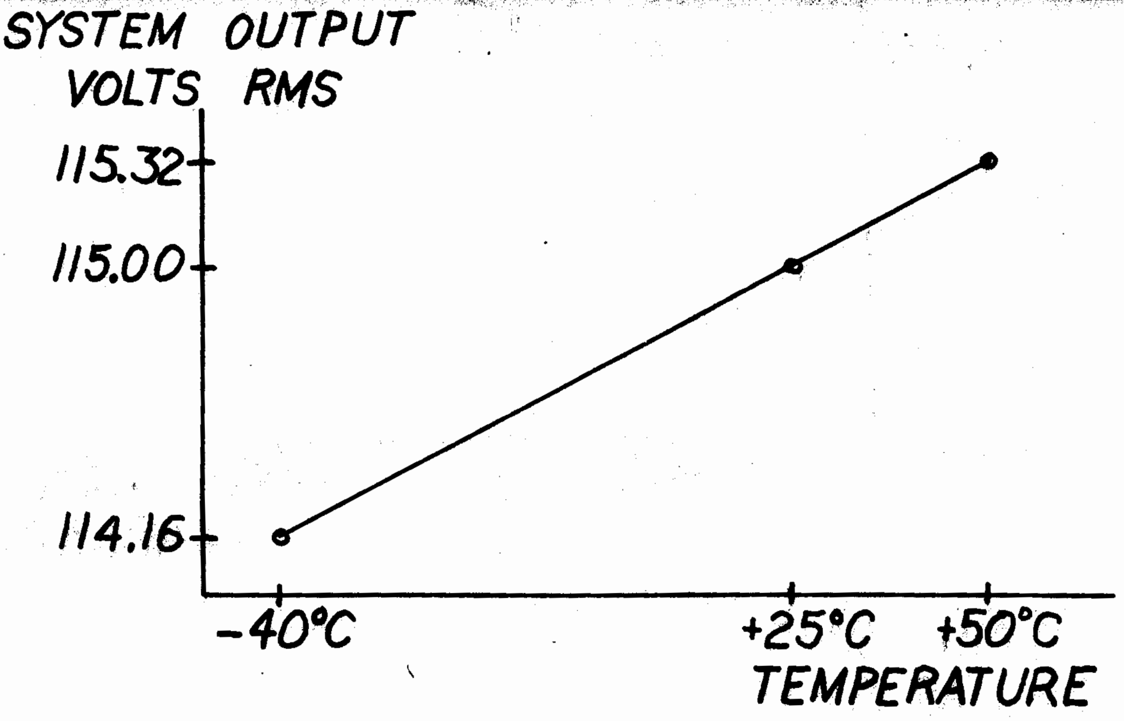


Figure 3b. Equivalent System Drift.

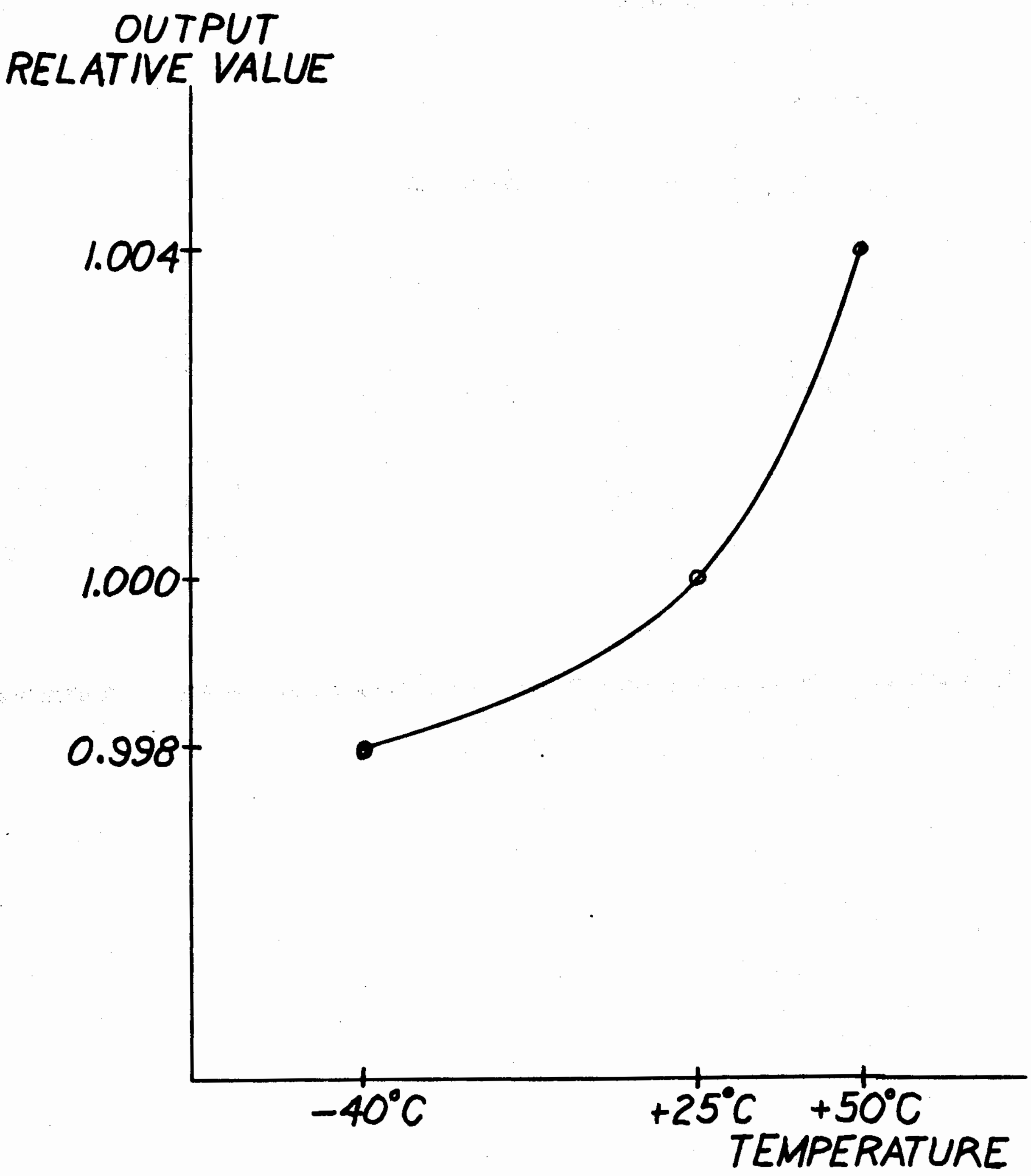


Figure 4. Temperature Stability of 102175 RMS to DC Converter.

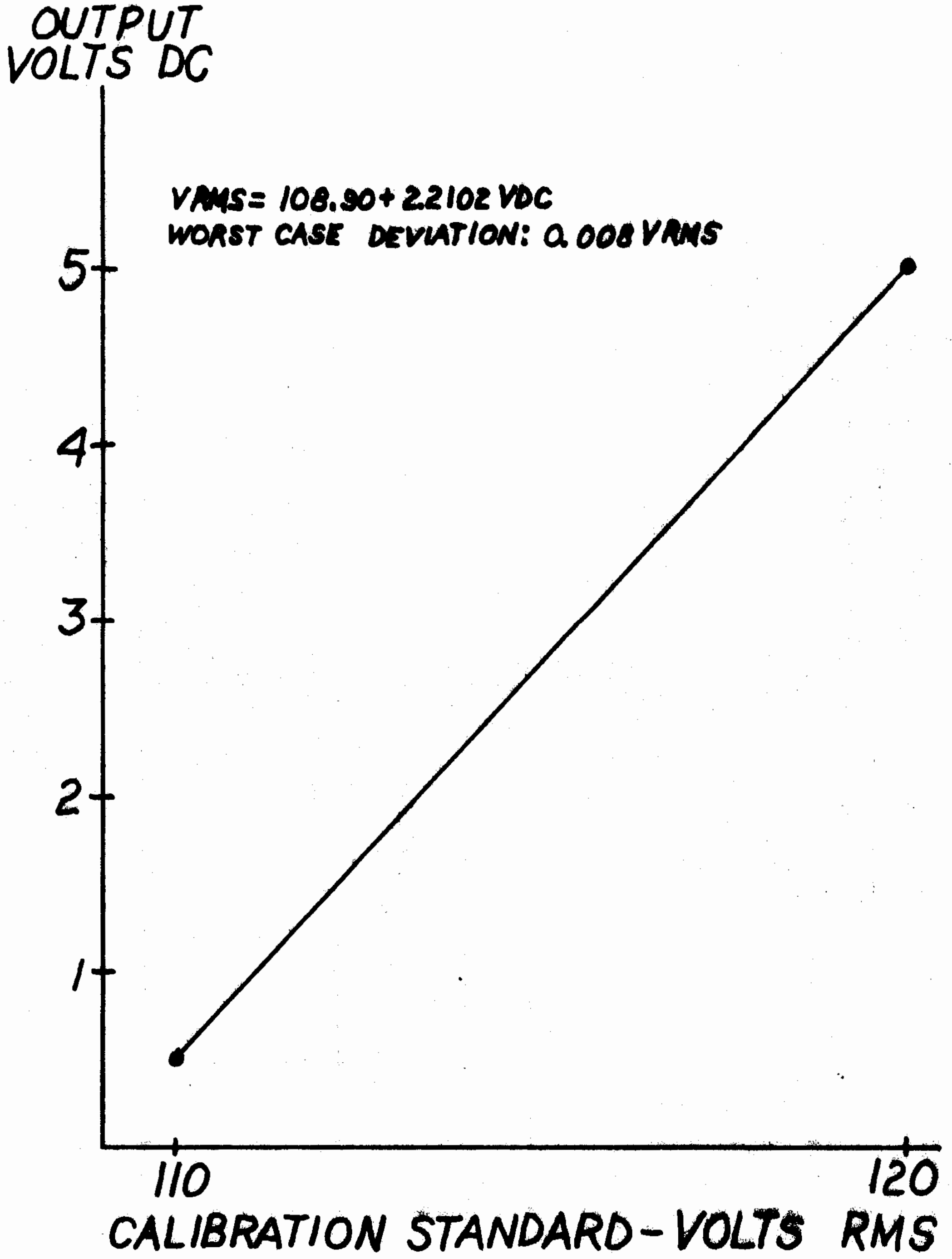


Figure 5. Linearity of 102175 RMS to DC Converter.

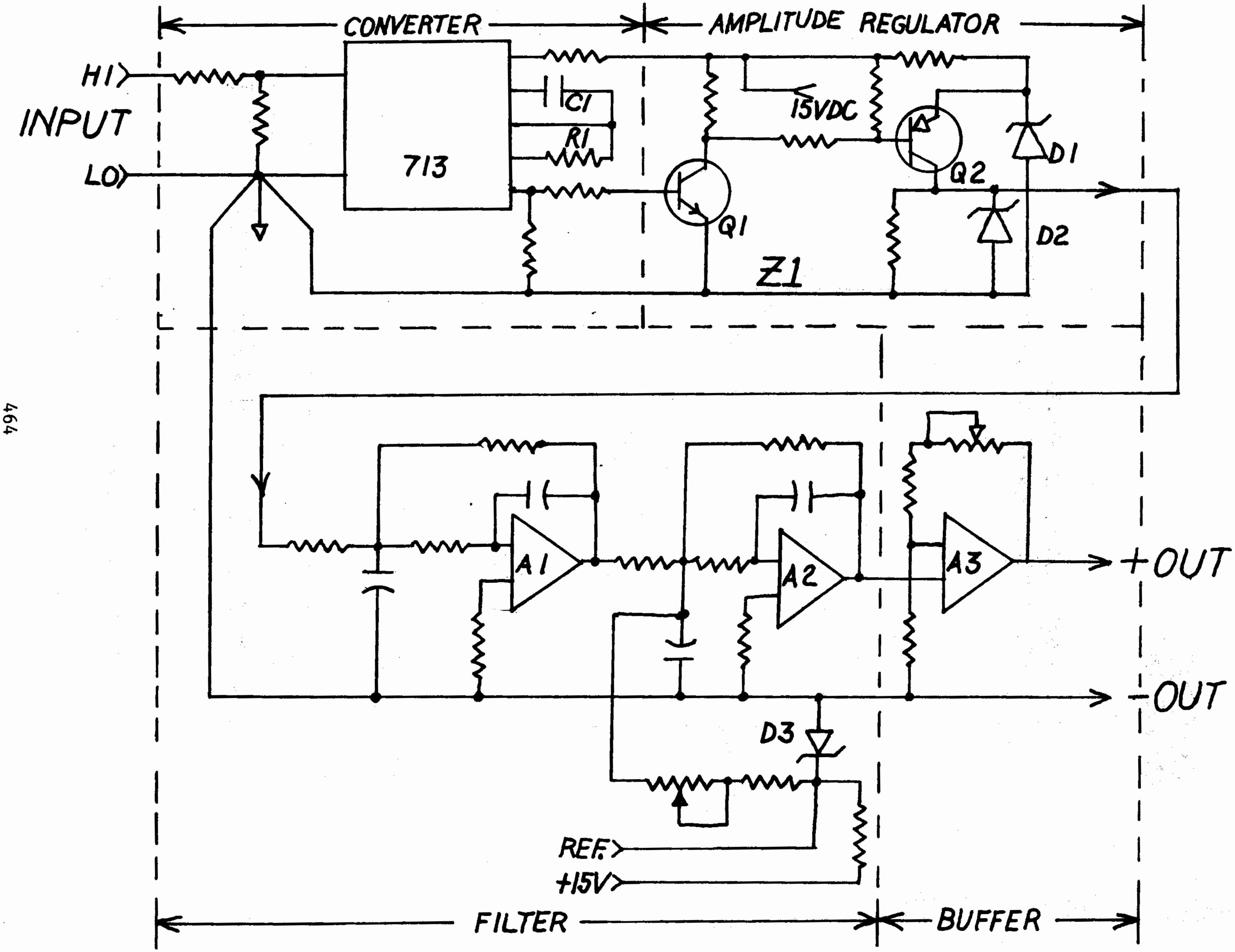
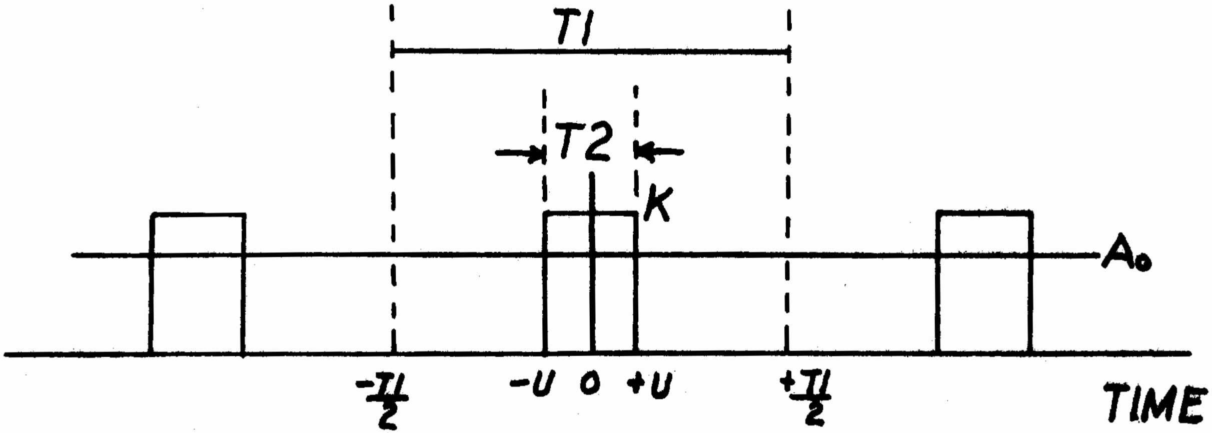


Figure 6. 111275 Frequency to DC Converter.



$$A_0 = \frac{1}{T1}\int_{-\frac{T1}{2}}^{+\frac{T1}{2}} F(T)\,dT = \frac{2}{T1}\int_{0}^{+\frac{T1}{2}} F(T)\,dT$$

$$= \frac{2}{T1}\int_{0}^{U} K\,dT = 2KU/T1$$

$$2U = T2$$

$$\boxed{A_0 = K\,\frac{T2}{T1}}$$

Figure 7. Fourier Expression for the 713 Output.

THE EFFECT OF APPLIED PRESSURE STEP RISE-TIME AND SHAPE ON THE OBSERVED RISE-TIME OF A PRESSURE TRANSDUCER

by

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Introduction

One of the most important characteristics of interest in the dynamic calibration of a pressure transducer is its undamped natural frequency, f_n. A number of methods for determining the frequency exist, many of which are discussed in some detail in references (1), (2), and (3). Shock tubes, shockless pressure-step generators or quick-opening valve devices, and sinusoidal pressure generators are the most frequently used pressure generators for dynamically calibrating pressure transducers. Unfortunately, no single device in use today seems to have all the properties necessary for achieving a complete dynamic calibration of a pressure transducer.

Although the shock tube, is perhaps, the most widely used pressure generator in the dynamic calibration of pressure transducers, it is not always operated in the reflected-shock mode, which is the best mode for determining f_n. The type, quality, and duration of the pressure step generated in that device is dependent on the design of the shock tube, the type diaphragm used, and the gases and their respective initial pressures in the high and low pressure chambers. The shape of the pressure step as sensed by a flush-diaphragm type pressure transducer is dependent on the location of the transducer within the shock tube, on its orientation, and on the shape of the sensing diaphragm. A pressure step having the shortest rise-time as sensed by a pressure transducer, approximately a rectangular step, is produced at the downstream end of a closed shock tube upon shock reflection. Step rise-times in the nanosecond range are possible under this mode of operation. To receive maximum benefit from this extremely short rise-time, the sensing end of a flush-diaphragm type transducer must be mounted flush with the back plate or reflecting surface. Unless the shock tube is operated under tailored-interface conditions, however, the amplitude of the generated pressure step will not be constant. Because shock tube operation under tailored-interface conditions places severe limitations on the operating range of that device, dynamic pressure calibrations are also performed on test transducers mounted flush in the side wall of the shock tube. With the gage in this position the sensed rise-time of the generated pressure step is dependent on the transit time of the incident shock wave across the gage diaphragm, and on the shape of the diaphragm.

If that shape is square or rectangular instead of circular, the angular orientation of one of the sides of the diaphragm with respect to the direction of shock wave propagation will affect the sensed pressure rise-time.

Because the diaphragm-type pressure transducer is basically a force-sensitive device, the force F(t) on the diaphragm having linear dimensions "a" and "b" at any instant of time is

$$F(t) = \int_0^b \int_0^a p(x,y,t)\,dxdy$$

$$= A \cdot \overline{p(t)}$$

The diaphragm area A is equal to (1)

$$A = \int_0^b \int_0^a dxdy = a \cdot b$$

The pressure $\overline{p(t)}$ in equation (1) is the average pressure on the diaphragm at time, t. If the diaphragm of a gage mounted flush in the side wall of a shock tube is square or rectangular, and one side is aligned parallel to the direction of flow, the pressure that is sensed by the transducer consists of a step with a constant-slope front. The pressure-time relationship is illustrated as curve (1) in Figure 1. This type of pressure step is the first of four pressure steps with different fronts to be discussed in this paper.

Case I - The Constant-Slope Front

The mathematical relationship expressing a pressure step with this type front is as follows:

$$\begin{aligned} \overline{p(t)} &= \frac{t \cdot p_o}{\tau} && \text{for } t \leq \tau \\ &= P_o && \text{for } t > \tau \end{aligned} \qquad (2)$$

The time, τ appearing in Figure 1 is the time when the pressure reaches its final constant value, P_o. If the side of the diaphragm corresponding to dimension "a" is aligned parallel to the direction of gas flow in the shock tube, time τ will be equal to the transit time of the shock wave across the diaphragm. In this case $\tau = \frac{a}{V_s}$, where V_s is the velocity of the incident shock wave. The rectangular pressure step sensed by a flush-mounted gage in the end plate of the shock tube is a special or limiting case of the pressure step with constant-slope front with $\tau = 0$.

But transducers having square or rectangular-shaped diaphragms are rare. The circular diaphragm is the most common configuration employed in flush-diaphragm type pressure gages. The pressure sensed by a flush-diaphragm type pressure transducer having a circular diaphragm which is mounted flush in the side wall of a shock tube is a pressure step with a non-linear rise or pressure front during the passage of the incident shock wave across its diaphragm. The pressure step sensed in this instance is the second type of pressure step to be examined and discussed in this paper. The particular type front associated with this step remains unnamed at present.

Case II

The mathematical relationship expressing a pressure step with the type front discussed above is

$$\begin{aligned} p(t) &= \frac{P_o}{2\pi}\left[2\left(\frac{2t}{\tau} - 1\right)\sqrt{\frac{t}{\tau} - \left(\frac{t}{\tau}\right)^2} + \sin^{-1}\left(\frac{2t}{\tau} - 1\right) + \frac{\pi}{2}\right] \text{for } t \leq \tau \\ &= P_o \qquad \text{for } t > \tau \end{aligned} \tag{3}$$

The non-linear rise in pressure associated with this step is shown plotted in Figure 1 as curve (2).

The rise-times and shapes of the fronts of the pressure steps produced by shockless pressure-step generators and quick-opening valve devices vary with the type and design of the device, the initial pressure difference across the quick-opening valve, the initial pressure on the gage diaphragm, and the type of working gas used.

There is no known mathematical expression which accurately describes the pressure-time relationship generated by any of these devices. Since pressure steps with versed-sine and cycloidal fronts (Ref. (4)) roughly approximate the actual steps produced, both types of pressure steps will be examined in this paper.

Case III - The Versed-Sine Pressure Front

The mathematical expression for the pressure step with a versed-sine front is

$$\begin{aligned} \overline{p(t)} &= \frac{P_o}{2}\left[1 - \cos\left(\frac{\pi t}{\tau}\right)\right] && \text{for } t \leq \tau \\ &= P_o && \text{for } t > \tau \end{aligned} \qquad (4)$$

This particular type step is shown plotted as curve (3) in Figure 1.

Case IV - The Cycloidal Pressure Front

The last of the pressure steps to be discussed in this paper is the pressure step with a cycloidal front. Its mathematical formulation is

$$\begin{aligned} \overline{p(t)} &= P_o\left[\frac{t}{\tau} - \frac{1}{2\pi}\sin\left(\frac{2\pi t}{\tau}\right)\right] && \text{for } t \leq \tau \\ &= P_o && \text{for } t > \tau \end{aligned} \qquad (5)$$

This particular step appears as curve (4) in Figure 1.

The primary purpose of this paper is to investigate the possible errors that may arise in the determination of the undamped natural frequency of a pressure transducer from measurements of the observed rise-time when the calibration procedure utilizes pressure steps having finite rise times and various-shaped fronts. Only those steps expressed mathematically in equations (2) - (5) will be examined. The investigation being reported here is entirely a mathematical one, there having been no attempt as yet to verify the results experimentally.

Approach

Most flush-diaphragm type pressure transducers in use today are highly linear devices whose response to rapid pressure fluctuations is similar to that of a mass-spring system with viscous damping. It is possible, therefore, to study the mass-spring system mathematically in some detail in order to gain insight into the response of the pressure transducer to the various pressure steps.

The familiar second-order linear differential equation which governs the response of the mass-spring system with initial conditions, $y(0) = \dot{y}(0) = 0$, is

$$m \frac{d^2y}{dt^2} + c \frac{dy}{dt} + ky = F(t) \tag{6}$$

where y represents the deflection of the mass m or the spring, c, the damping coefficient, and k, the spring constant. The externally applied force F(t) is that discussed in equation (1). As is most commonly done whenever equation (6) is studied, it is rewritten in the following convenient form:

$$\frac{d^2y}{dt^2} + 2\beta\omega_n \frac{dy}{dt} + \omega_n^2 y = \frac{1}{m} \overline{Ap(t)} \tag{7}$$

where $\omega_n = 2\pi f_n$ and $\beta = \frac{c}{2\sqrt{km}}$. β is known as the damping ratio.

Since the applied pressure step $\overline{p(t)}$ has a constant value, P_o, after time $t = \tau$, regardless of the type of pressure front under consideration, the steady-state deflection y_s which is present after the transient response of the system has disappeared must equal

$$y_s = \frac{AP_o}{\omega_n^2 m} \tag{8}$$

Because all the pressure steps being considered have the same final value P_o, and differ only during the time interval $0 < t < \tau$, the pressure functions $\overline{p(t)}$ can be expressed as

$$\overline{p(t)} = P_o\, f(t)$$

where

$$\begin{aligned} f(t) &\leq 1 \qquad \text{for } t \leq \tau \\ &= 1 \qquad \text{for } t > \tau \end{aligned} \tag{9}$$

For case I, with $t \leq \tau$

$$f(t) = f_1(t) = \frac{t}{\tau} \tag{10a}$$

For Case II with $t \leq \tau$

$$f(t) = f_2(t) = \frac{1}{2\pi}\left[2\left(\frac{2t}{\tau} - 1\right)\sqrt{\frac{t}{\tau} - \left(\frac{t}{\tau}\right)^2} + \sin^{-1}\left(\frac{2t}{\tau} - 1\right) + \frac{\pi}{2}\right] \tag{10b}$$

For Case III with $t \leq \tau$

$$f(t) = f_3(t) = \frac{1}{2}\left[1 - \cos\left(\frac{\pi t}{\tau}\right)\right] \tag{10c}$$

For Case IV with $t \leq \tau$

$$f(t) = f_4(t) = \frac{t}{\tau} - \frac{1}{2\pi}\sin\left(\frac{2\pi t}{\tau}\right) \tag{10d}$$

When equations (8) and (9) are introduced in equation (7), the following normalized differential equation results:

$$\frac{d^2\left(\frac{y}{y_s}\right)}{d(\omega_n t)^2} + \frac{2\beta d\left(\frac{y}{y_s}\right)}{d(\omega_n t)} + \frac{y}{y_s} = f(t) \tag{11}$$

where $\frac{y}{y_s} \to 1$ for $t \to \infty$.

When $f(t) = f_1(t)$, equation (11) can be solved by finding the homogeneous and particular solutions. The following equations represent the solution to equation (11) with $y(0) = \dot{y}(0) = 0$:

For $t \leq \tau$,

$$\frac{y}{y_s} = \frac{1}{\omega_n \tau}\left\{ e^{-\beta\omega_n t}\left[\frac{(2\beta^2 - 1)}{\sqrt{1 - \beta^2}}\sin\left(\omega_n\sqrt{1 - \beta^2}\, t\right) + 2\beta\cos\left(\omega_n\sqrt{1 - \beta^2}\, t\right)\right] - 2\beta + \omega_n t\right\} \tag{12}$$

For $t > \tau$,

$$\frac{y}{y_s} = 1 + \frac{e^{-\beta\omega_n t}}{\omega_n \tau}\left[A \sin(\omega_n \sqrt{1 - \beta^2 t}) + B \cos(\omega_n \sqrt{1 - \beta^2 t})\right] \qquad (13)$$

where

$$A = \frac{1}{\sqrt{1 - \beta^2}}\left\{e^{\beta\omega_n\tau}\left[\left(1 - 2\beta^2\right)\cos\left(\omega_n \sqrt{1 - \beta^2}\ \tau\right) - 2\beta\sqrt{1 - \beta^2}\ \sin\left(\omega_n \sqrt{1 - \beta^2}\ \tau\right)\right] + 2\beta^2 - 1\right\} \qquad (14)$$

and

$$B = 2\beta + e^{\beta\omega_n\tau}\left[\frac{(2\beta^2 - 1)}{\sqrt{1 - \beta^2}}\ \sin\left(\omega_n \sqrt{1 - \beta^2 \tau}\right) - 2\beta\cos\left(\omega_n \sqrt{1 - \beta^2 \tau}\right)\right] \qquad (15)$$

The definition of <u>rise-time</u> as used in this paper is the time required for the amplitude to change from 10 to 90 percent of the final steady value. This definition applies to both the applied pressure step and the response of the pressure transducer. The rise-time of an applied pressure step with a constant-slope front, according to the defnition and equation (2), must be

$$\tau_{\text{applied}} = 0.8\tau \qquad (16)$$

The rise-time of the transducer in response to that step can be found from either equation (12) or (13), whichever case applies, by setting $\frac{y}{y_s} = 0.1$, solving for $t = t_1$, and then setting $\frac{y}{y_s} = 0.9$ and solving for $t = t_2$. Finally, the indicated gage rise-time, τ_{ind}, is computed from equation (17),

$$\tau_{\text{ind}} = t_2 - t_1 \qquad (17)$$

From the forms of the differential equation (equation (11)) and the solution to the pressure-step with constant-slope front appearing in equations (12) - (15), it should be apparent that the normalized gage deflection is a function of ω_n, τ, t, and β, or more specifically, $\omega_n\tau$, $\omega_n t$, and β. This can be expressed in the form shown in equation (18)

$$\frac{y}{y_s} = G_1(\omega_n\tau,\ \omega_n t,\ \beta) \tag{18}$$

Since $\frac{y}{y_s}$, $\omega_n\tau$, $\omega_n t$, and β are interrelated, it follows that $\omega_n t$ can be expressed as a function of $\omega_n\tau$, $\frac{y}{y_s}$, and β. Thus,

$$\omega_n t = g(\omega_n\tau, \frac{y}{y_s}, \beta) \tag{19}$$

Since $\frac{y}{y_s} = 0.1$ when $t = t_1$,

$$\omega_n t_1 = g(\omega_n\tau, 0.1, \beta) \tag{20}$$

Similarly, since $\frac{y}{y_s} = 0.9$ when $t = t_2$,

$$\omega_n t_2 = g(\omega_n\tau, 0.9, \beta) \tag{21}$$

From equations (17), (20), and (21) one obtains

$$\begin{aligned} \omega_n\tau_{ind} &= g(\omega_n\tau, 0.9, \beta) - g(\omega_n\tau, 0.1, \beta) \\ &= g_1(\omega_n\tau, \beta, 0.1, 0.9) \end{aligned} \tag{22}$$

Since the definition of τ_{ind} implies the two specific values, 0.1 and 0.9, for $\frac{y}{y_s}$, these values need not appear explicitly in equation (22). Hence, that equation can be rewritten as

$$\omega_n\tau_{ind} = g_1(\omega_n\tau, \beta) \tag{23}$$

As is apparent in equation (16), $\tau_{applied}$ is some function of time τ, i.e.,

$$\tau_{applied} = h(\tau) \tag{24}$$

When equations (23) and (24) are combined the indicated gage rise-time is expressible in the form

$$\omega_n\tau_{ind} = R_1(\omega_n\tau_{applied}, \beta) \tag{25}$$

$$\frac{y}{\ } = 1 + \frac{e}{\ } \left[A \sin(\omega \sqrt{1 - \beta^2}t) + B \cos(\omega \sqrt{1 - \beta^2}t) \right] \tag{13}$$

It is evident from equations (12) - (15) that as τ is decreased, both t_1 and t_2 also decrease. Also τ_{ind} must decrease. When $\tau = 0$, τ_{ind} has its smallest value. However, with that value of τ equations (12) - (15) are indeterminant. To circumvent the mathematical problem that arises here, one can use instead the solution to differential equation (11) in which the rectangular step function is substituted for f(t), i.e.,

$$f(t) = 0 \qquad \text{for } t \leq 0$$
$$= 1 \qquad \text{for } t > 0$$

The solution in this case can be found in most books and articles dealing with second-order linear differential equations (Ref. (4)). Thus, for the special case, $\tau = 0$, the response of the transducer as a function of time with $y(0) = \dot{y}(0) = 0$ is

$$\frac{y}{y_s} = 1 - e^{-\beta\omega_n t}\left[\cos\left(\omega_n\sqrt{1 - \beta^2}\ t\right) + \frac{\beta}{\sqrt{1 - \beta^2}} \sin\left(w_n\sqrt{1 - \beta^2}\ t\right)\right] \tag{26}$$

Since τ_{ind} has its smallest value when a rectangular pressure step is sensed by the transducer, this minimum value of τ_{ind} will be referred to as the inherent gage rise-time, τ_{gage}. Because both τ and $\tau_{applied}$ are zero in this special case, from equation (25) it should be evident that τ_{gage} is a function of only one parameter, β. Hence,

$$\omega_n\tau_{gage} = C(\beta) \tag{27}$$

where $C(\beta)$ is a constant whose particular value depends only on the value of the damping ratio. The values associated with this constant can be determined from equation (26) for each value of β by setting $\frac{y}{y_s} = 0.1$ and 0.9, and finding the associated values of $\omega_n t_1$ and $\omega_n t_2$. From equations (27) and (17), it follows that

$$C(\beta) = \omega_n\tau_{gage} = \omega_n t_2 - \omega_n t_1$$

The values of $C(\beta)$ have been computed for ten different values of β. These values are listed in Table I.

TABLE I

THE CONSTANTS C(β) VS. β
FOR RECTANGULAR PRESSURE STEPS

β	C(β)
0	1.0196
.05	1.0603
.10	1.1042
.20	1.2034
.30	1.3213
.40	1.4634
.50	1.6376
.60	1.8541
.70	2.1262
.80	2.4675

Since $\omega_n = 2\pi f_n$, the natural frequency of the transducer can be determined from the tabulated values, i.e.,

$$f_n = \frac{C(\beta)}{2\pi\tau_{gage}} \tag{28}$$

With the aid of equation (27) or Table I equation (25) can be written as

$$\frac{\tau_{ind}}{\tau_{gage}} = R_1\left(\frac{\tau_{applied}}{\tau_{gage}}, \beta\right) \tag{29}$$

The same technique that was used to obtain $R_1\left(\frac{\tau_{applied}}{\tau_{gage}}, \beta\right)$ in the above equation, applicable to Case I, can be employed to find the relationships between $\frac{\tau_{ind}}{\tau_{gage}}$, $\frac{\tau_{applied}}{\tau_{gage}}$, and β, for the remaining cases. Unfortunately, the forms of the forcing functions representing the three remaining types of pressure steps are such as to make the process of obtaining closed-form solutions even more tedious than that encountered in Case I. A more simple approach is to enlist the aid of a large high-speed digital computer and solve the differential equation (11) directly. The computer program that was utilized to solve the differential equation makes use of the fifth-order accurate Fehlberg-Runge-Kutta method (Ref. (5)). To check its accuracy, the program was first applied to Case I and the computed values of $\frac{\tau_{ind}}{\tau_{gage}}$ were compared with those obtained from the closed-form solution presented in equations (12) - (15). For the same values of $\frac{\tau_{applied}}{\tau_{gage}}$ and β, the valuesof $\frac{\tau_{ind}}{\tau_{gage}}$ obtained by the two methods were found to agree to the fifth significant figure. Because of this excellent agreement the computer program was used exclusively to obtain the values of $\frac{\tau_{ind}}{\tau_{gage}}$ as functions of $\frac{\tau_{applied}}{\tau_{gage}}$ and β for the three remaining cases.

Results Obtained

For convenience the data relating $\frac{\tau_{ind}}{\tau_{gage}}$, $\frac{\tau_{applied}}{\tau_{gage}}$, and β are shown plotted in Figures 2, 3, 4, and 5. Each of these figures, which represents a different case, contains a family of curves of $\frac{\tau_{ind}}{\tau_{gage}}$ vs. $\frac{\tau_{applied}}{\tau_{gage}}$ for ten different damping ratios.

The four families of curves are very similar in shape. $\frac{\tau_{ind}}{\tau_{gage}}$, which has the value of 1.0 at $\frac{\tau_{applied}}{\tau_{gage}} = 0$, increases with both $\frac{\tau_{applied}}{\tau_{gage}}$ and β. The curves in any one family tend to bunch together as the value of β is increased. β values were limited to 0.8 because pressure transducers are seldom ever damped to a greater extent. The rate of change of $\frac{\tau_{ind}}{\tau_{gage}}$ with $\frac{\tau_{applied}}{\tau_{gage}}$ is gradual for small values of the abscissa, but increases rapidly as $\frac{\tau_{applied}}{\tau_{gage}}$ is increased. In the limit, the ratio, $\frac{\tau_{ind}}{\tau_{gage}} \bigg/ \frac{\tau_{applied}}{\tau_{gage}}$, must equal one.

The quantitative differences between the four curves representing the value of β = 0.8 for the four different applied pressure steps are very small. As the value of β is decreased the differences between curves with the same value of damping ratio increase, the largest differences occurring with β = 0. Comparing the values of $\frac{\tau_{ind}}{\tau_{gage}}$ for β = 0 at several widely different values of $\frac{\tau_{applied}}{\tau_{gage}}$ for the four cases, one finds that the smallest values occur in Case I and increase with Case II, III, and IV, in that order.

The error that arises in the determination of the natural frequency f_n from a measurement of the rise-time can be calculated with the aid of the curves plotted in Figures 2 - 5 by noting that

$$\begin{aligned} \text{Relative Frequency Error} = \varepsilon &= \frac{f_n - (f_n)_{ind}}{f_n} \\ &= 1 - \frac{(f_n)_{ind}}{f_n} \\ &= 1 - \frac{(\omega_n)_{ind}}{\omega_n} \end{aligned} \qquad (30)$$

From equation (27),

$$\omega_n = \frac{C(\beta)}{\tau_{gage}} \qquad (31)$$

However, unless the individual performing the dynamic calibration is expecting an error in the measurement of natural frequency and is able to correct for it, he must assume that the indicated rise-time is the gage rise-time. If he uses equation (31), he will be obtaining $(\omega_n)_{ind}$ instead of ω_n. Hence,

$$(\omega_n)_{ind} = \frac{C(\beta)}{\tau_{ind}} \qquad (32)$$

When equations (31) and (32) are substituted in equation (30), the following relationship between the ratio, $\frac{\tau_{ind}}{\tau_{gage}}$, and the relative frequency error is obtained:

$$\varepsilon = 1 - \frac{1}{\frac{\tau_{ind}}{\tau_{gage}}} \qquad (33)$$

Along with $\frac{\tau_{ind}}{\tau_{gage}}$, the relative frequency error was calculated as a function of $\frac{\tau_{applied}}{\tau_{gage}}$ and β in the computer program using equation (33). The dependence of ε on $\frac{\tau_{applied}}{\tau_{gage}}$ and β for the four different applied pressure steps is shown plotted in Figures 6, 7, 8, and 9. As to be expected, the four families of curves appearing in these figures have similar shapes. In all four plots the relative frequency error increases toward a maximum value of 1.0 with increasing values of $\frac{\tau_{\text{applied}}}{\tau_{\text{gage}}}$ and β. This limiting value is predictable from equation (33). The separate curves comprising each family tend to bunch together and approach a limiting set of values as the damping ratio is increased. When $\beta = 0.8$, the four ε vs. $\frac{\tau_{\text{applied}}}{\tau_{\text{gage}}}$ curves representing the four applied pressure steps are nearly identical. With decreasing β, however, the agreement between curves with the same β value tends to diminish. With $\beta = 0$ the amount of agreement is least.

Of the four types of applied pressure steps considered in this paper, the pressure-step with the constant-slope front appears to produce the smallest relative frequency error. The largest error is generated with Case IV, the pressure step with the cycloidal front. However, for relative errors of 0.1 or less, the differences between the curves from the four families having the same values of β are almost insignificant. Therefore, for frequency errors of ten percent or less, any of the four families of curves can be considered as representative of all of them. A number of values of ε, $\frac{\tau_{\text{applied}}}{\tau_{\text{gage}}}$, and β have been read from an enlarged version of the curves appearing in Figure 7. They are shown tabulated in Table II. According to that table, for a pressure transducer with a damping ratio of .05, the maximum allowable rise-time of the applied pressure step in order to limit the error to one percent is 0.265 times the rise-time of the transducer. In order to limit the error to ten percent the maximum allowable step rise-time is 0.836 times the gage rise-time.

Summary and Conclusions

The determination of the gage natural frequency from the measurement of the indicated rise-time of a pressure transducer can be in considerable error if the ratio of the rise-times of the applied pressure step and the gage is allowed to exceed a value of 1.0. The relative frequency error increases with increasing values of $\frac{\tau_{\text{applied}}}{\tau_{\text{gage}}}$ and damping ratio. Although the shape of the front associated with the applied pressure step has some influence on the frequency error, the

TABLE II

THE RELATIONSHIP BETWEEN FREQUENCY ERROR, APPLIED STEP RISE-TIME AND TRANSDUCER DAMPING RATIO

% Freq Error	Damping Ratio	$\frac{\tau_{applied}}{\tau_{gage}}$
1	0	0.275
1	.05	0.265
1	.10	0.259
1	.80	0.171
2	0	0.387
2	.05	0.375
2	.10	0.368
2	.80	0.242
5	0	0.606
5	.05	0.592
5	.10	0.572
5	.80	0.373
10	0	0.857
10	.05	0.836
10	.10	0.813
10	.80	0.529

amount of influence varies with the damping ratio, it being almost negligible at high values of β. To maintain an error in the determination of frequency under ten percent when the damping ratio is 0.80, the maximum allowable step rise-time is 0.53 times the gage rise-time. Of the four types of pressure steps considered in this paper, the step with the constant-slope front generates the smallest error in the measurement of frequency, whereas that having a cycloidal pressure front produces the largest error. Finally, it must be pointed out that the results which have been presented in this paper apply not only to pressure transducers, but also to any type of transducer whose response to a step-type measurand is similar to that of a mass-spring system with damping as characterized by a second-order linear differential equation.

Acknowledgment

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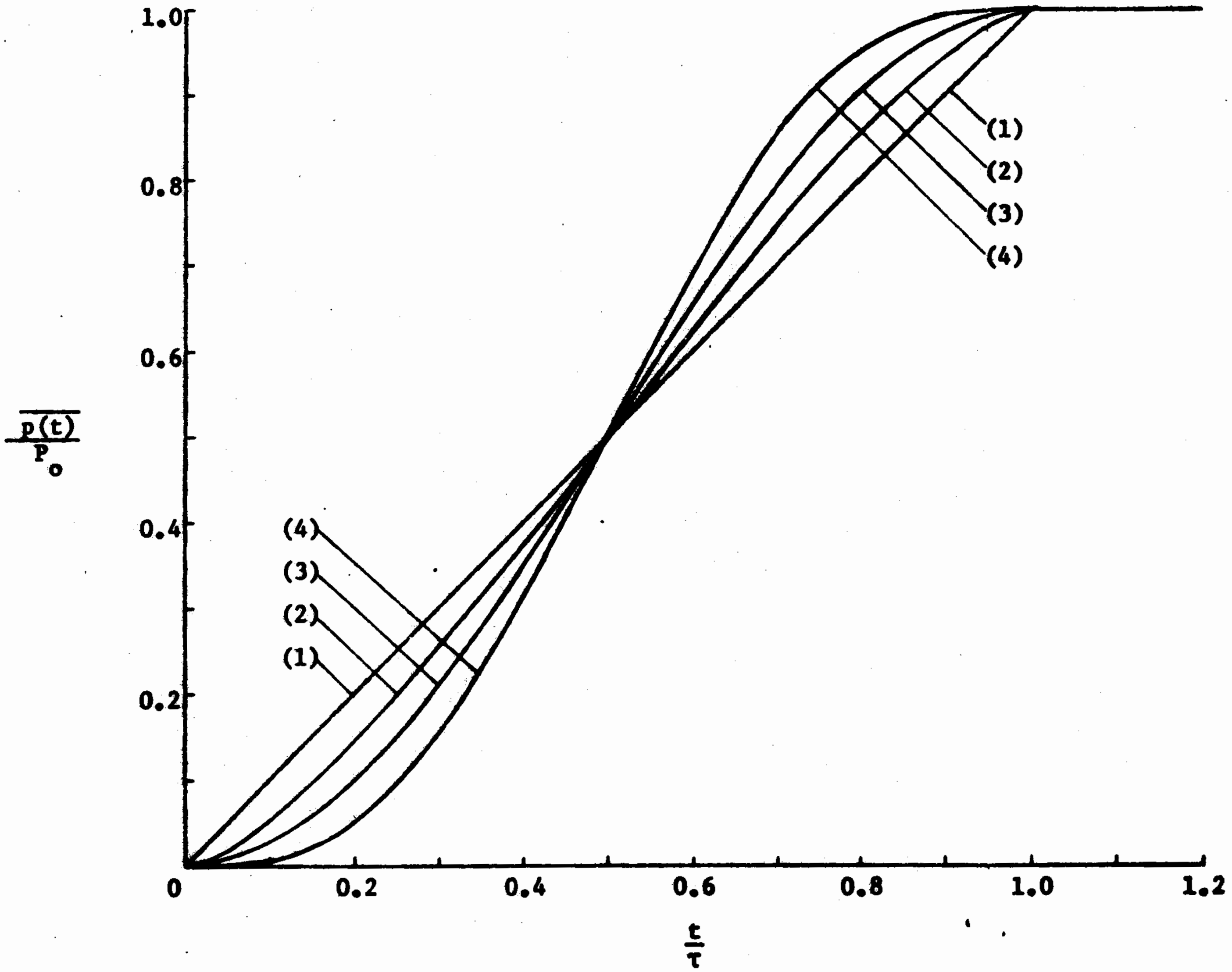


FIG. 1. DIMENSIONLESS PRESSURE VERSUS DIMENSIONLESS TIME FOR FOUR APPLIED PRESSURE STEPS HAVING DIFFERENT PRESSURE FRONTS

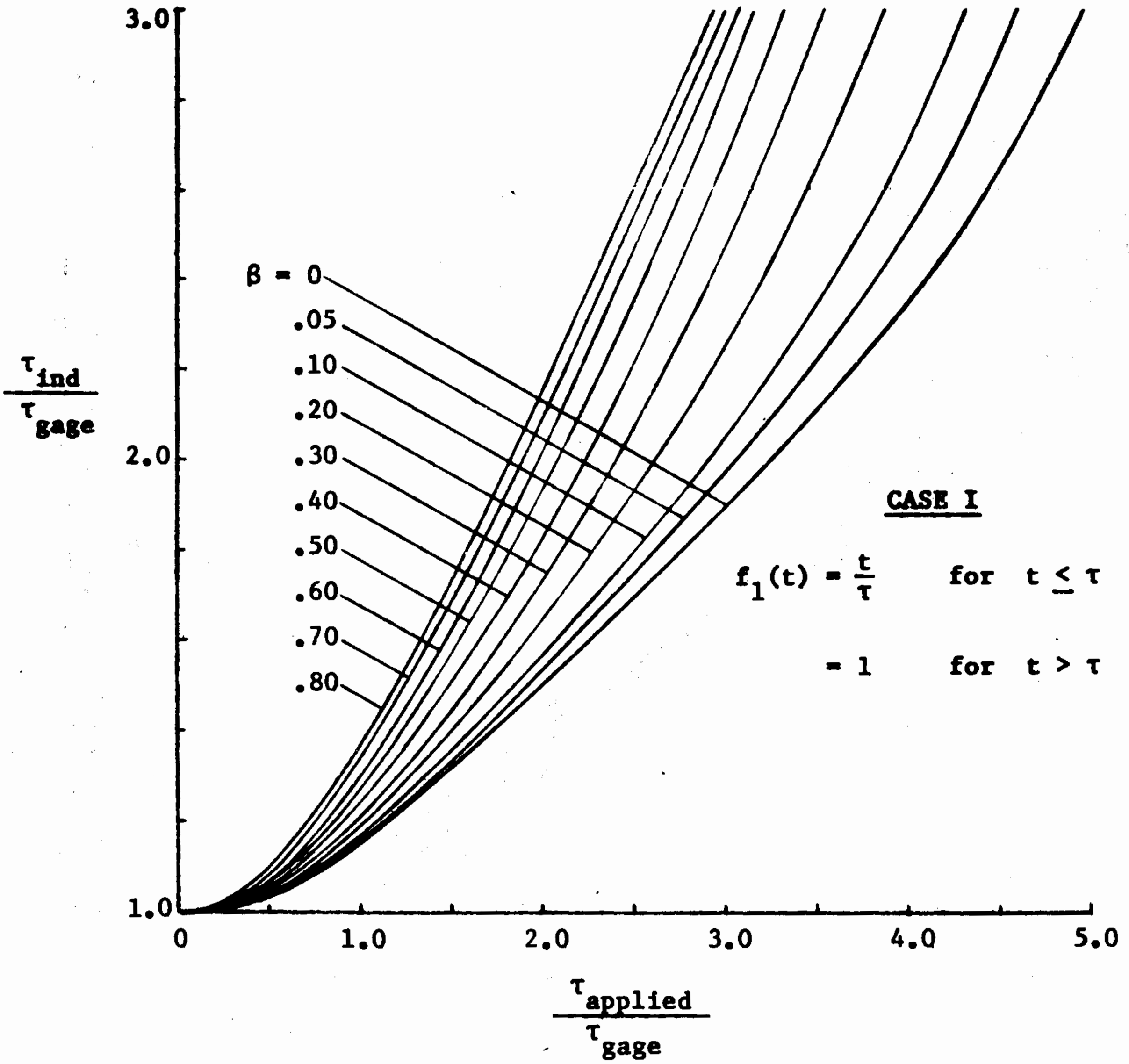
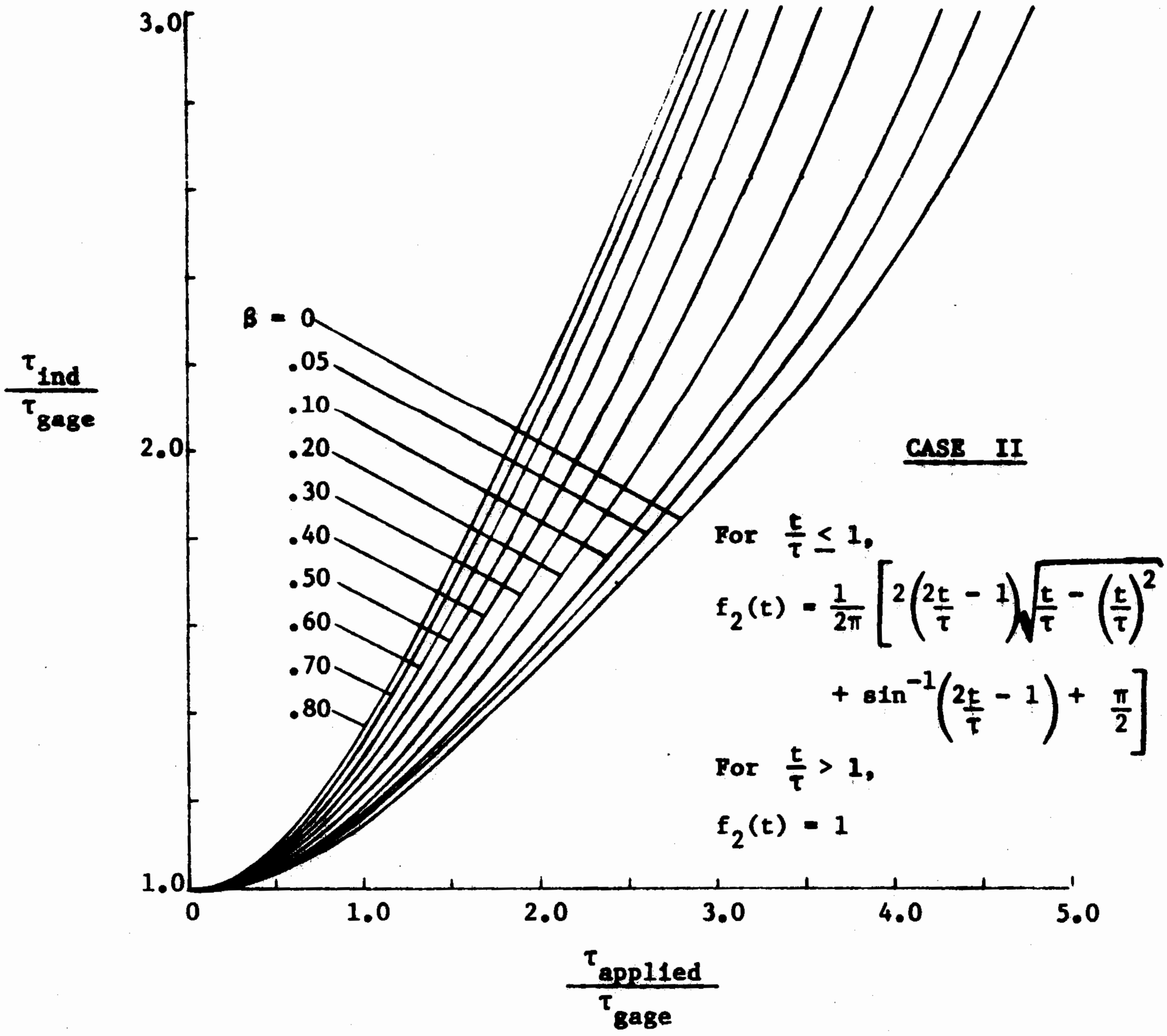


FIG. 2. THE VARIATION OF DIMENSIONLESS INDICATED RISE-TIME WITH DIMENSIONLESS APPLIED STEP RISE-TIME AND DAMPING RATIO FOR A PRESSURE STEP WITH CONSTANT-SLOPE FRONT



CASE II

For $\frac{t}{\tau} \leq 1$,

$$f_2(t) = \frac{1}{2\pi}\left[2\left(2\frac{t}{\tau} - 1\right)\sqrt{\frac{t}{\tau} - \left(\frac{t}{\tau}\right)^2} + \sin^{-1}\left(2\frac{t}{\tau} - 1\right) + \frac{\pi}{2}\right]$$

For $\frac{t}{\tau} > 1$,

$$f_2(t) = 1$$

FIG. 3. THE VARIATION OF DIMENSIONLESS INDICATED RISE-TIME WITH DIMENSIONLESS APPLIED STEP RISE-TIME AND DAMPING RATIO FOR A PRESSURE STEP CORRESPONDING TO CASE II

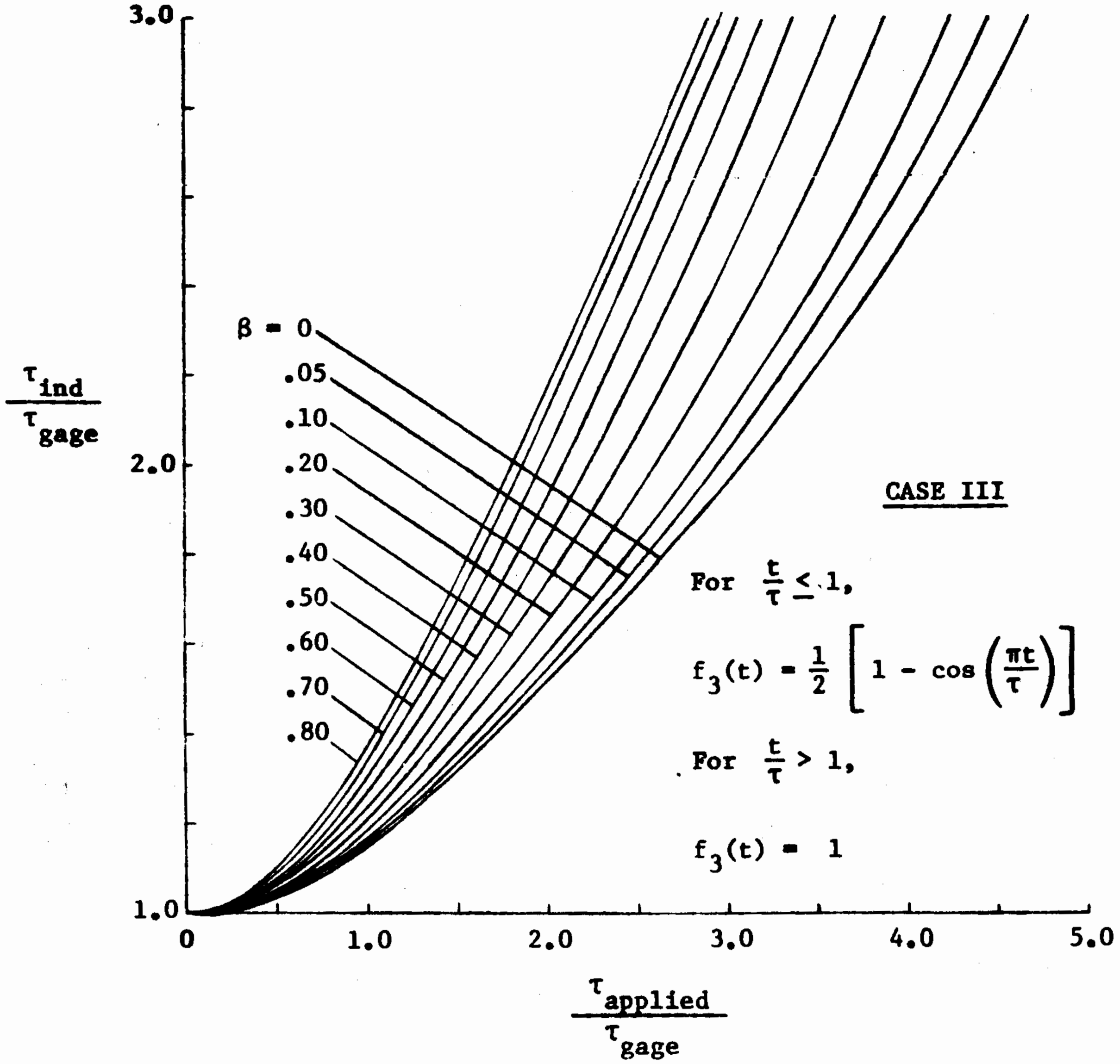


FIG. 4. THE VARIATION OF DIMENSIONLESS INDICATED RISE-TIME WITH DIMENSIONLESS APPLIED STEP RISE-TIME AND DAMPING RATIO FOR A PRESSURE STEP WITH A VERSED-SINE FRONT

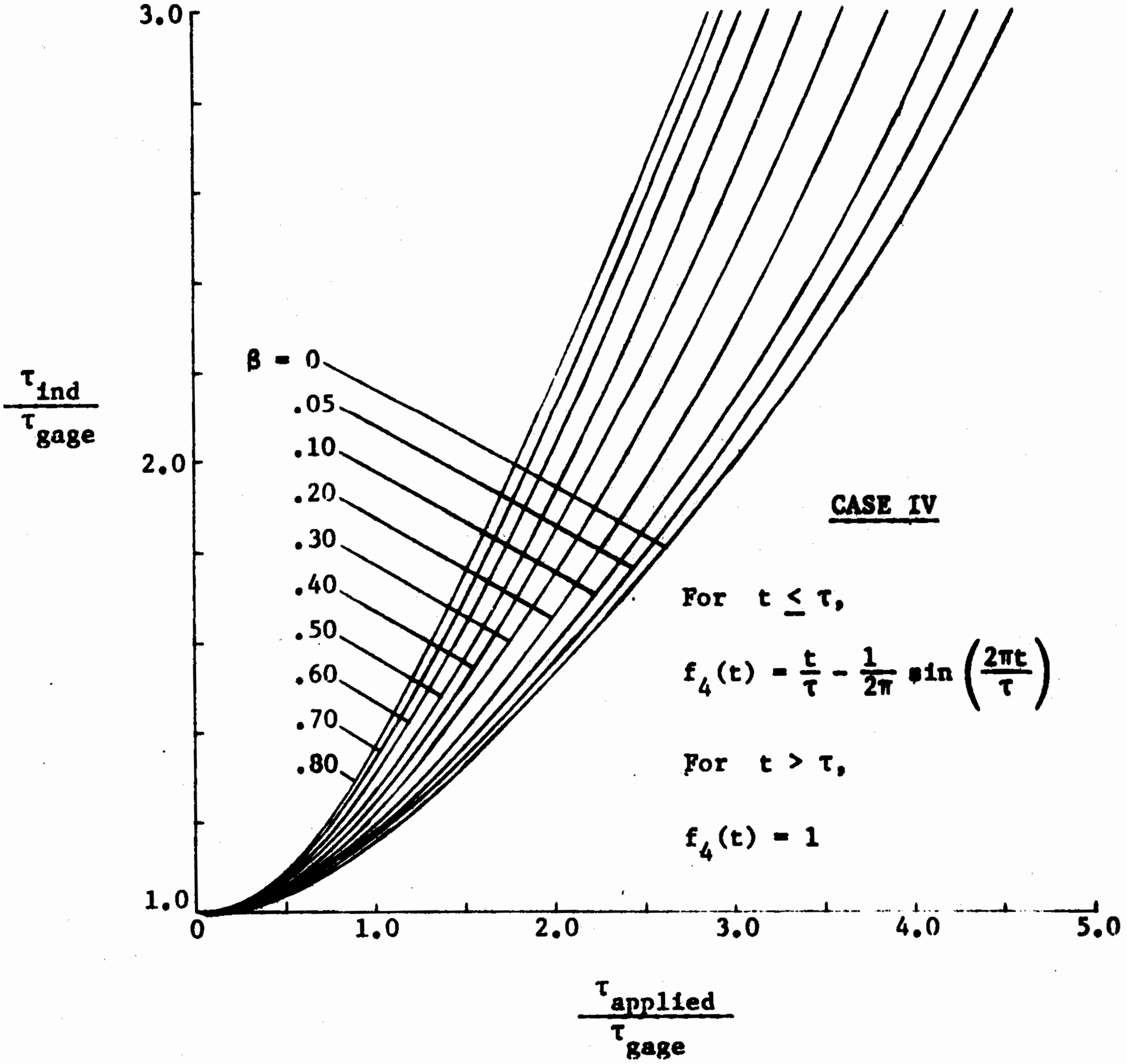


FIG. 5. THE VARIATION OF DIMENSIONLESS INDICATED RISE-TIME WITH DIMENSIONLESS APPLIED STEP RISE-TIME AND DAMPING RATIO FOR A PRESSURE STEP WITH A CYCLOIDAL FRONT

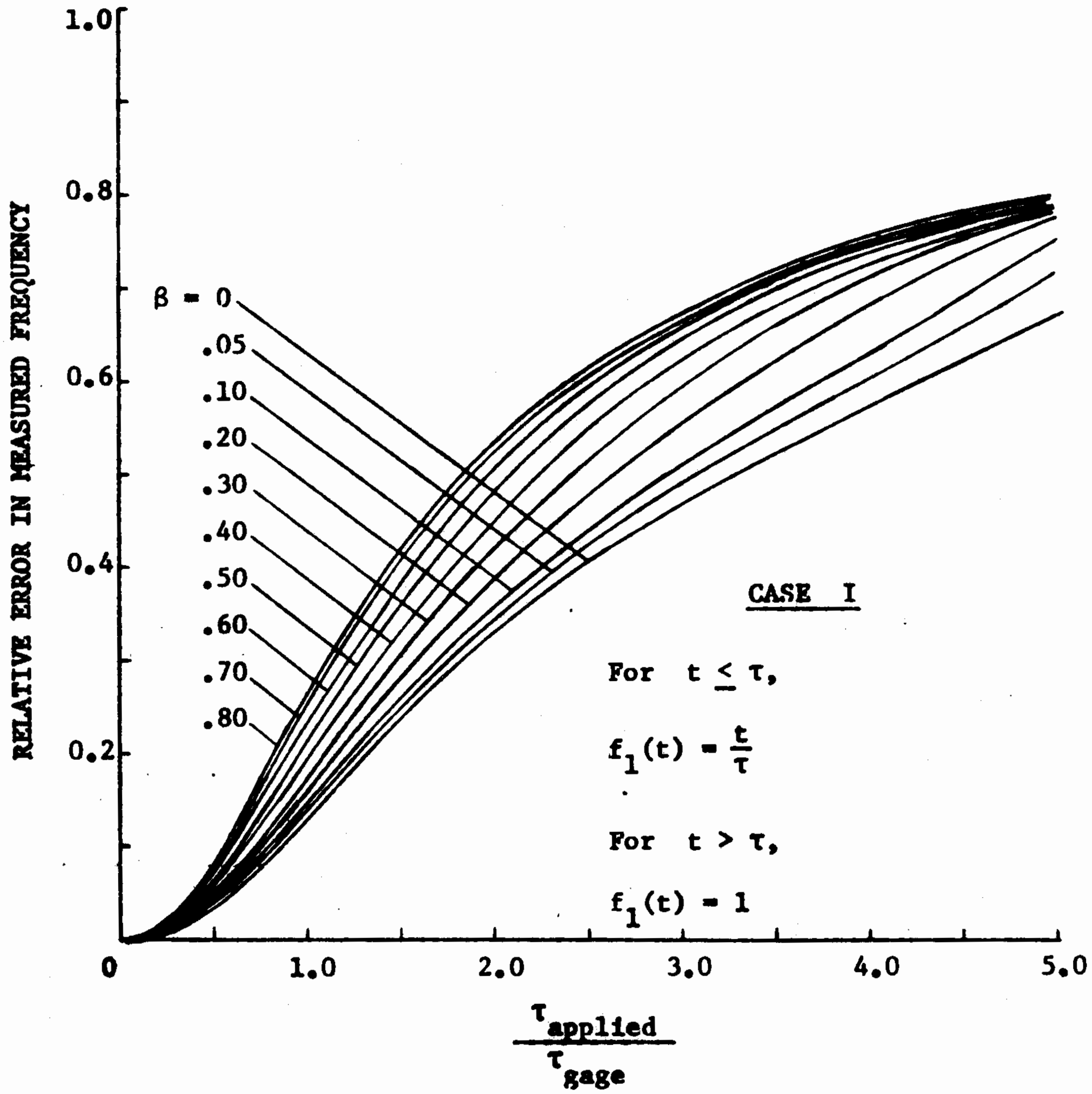


FIG. 6. RELATIVE ERROR IN THE MEASURED NATURAL FREQUENCY VERSUS DIMENSIONLESS APPLIED STEP RISE-TIME AND DAMPING RATIO FOR A PRESSURE STEP WITH A CONSTANT-SLOPE FRONT

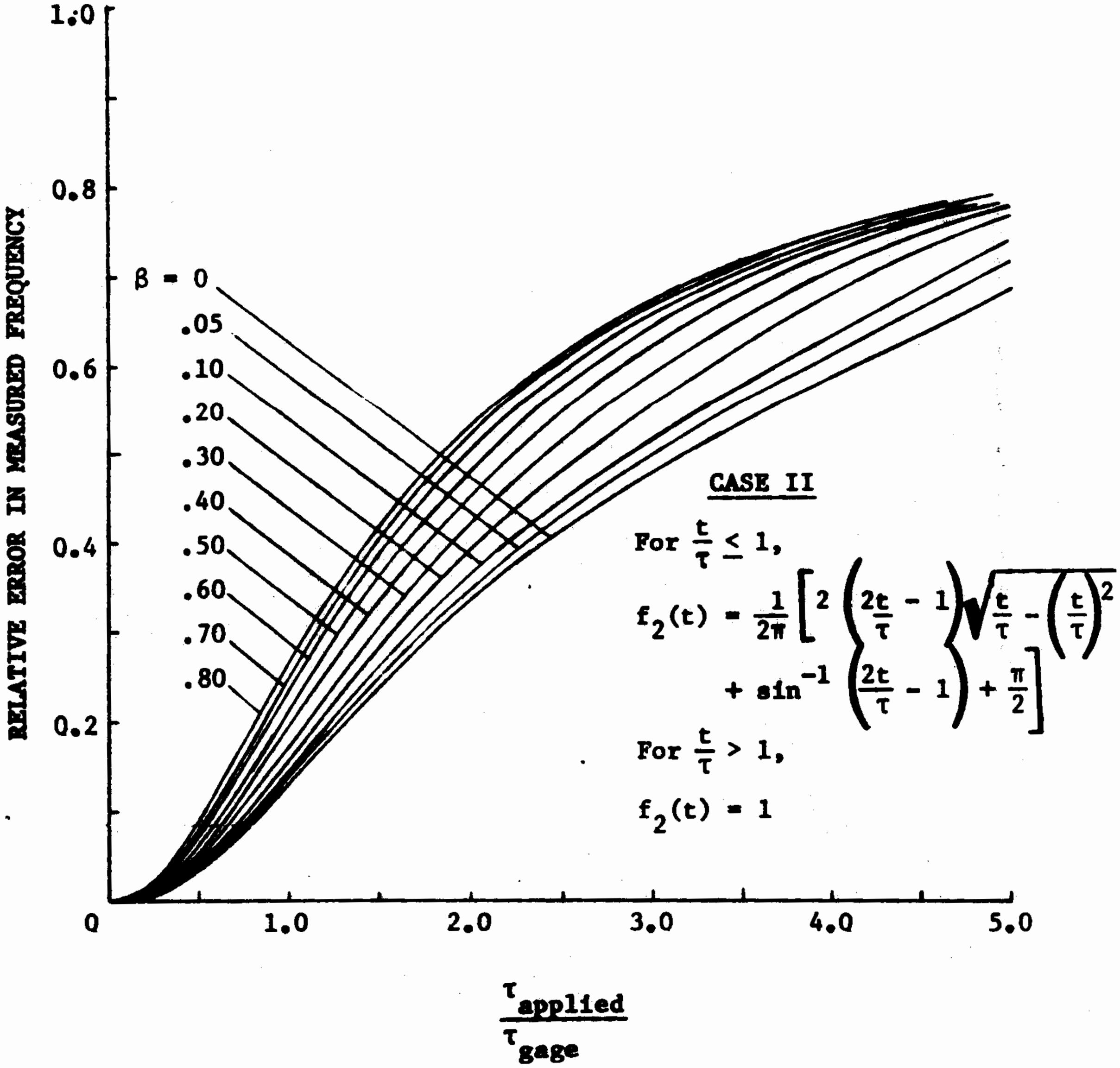


FIG. 7. RELATIVE ERROR IN THE MEASURED NATURAL FREQUENCY VERSUS DIMENSIONLESS APPLIED STEP RISE-TIME AND DAMPING RATIO FOR A PRESSURE STEP CORRESPONDING TO CASE II

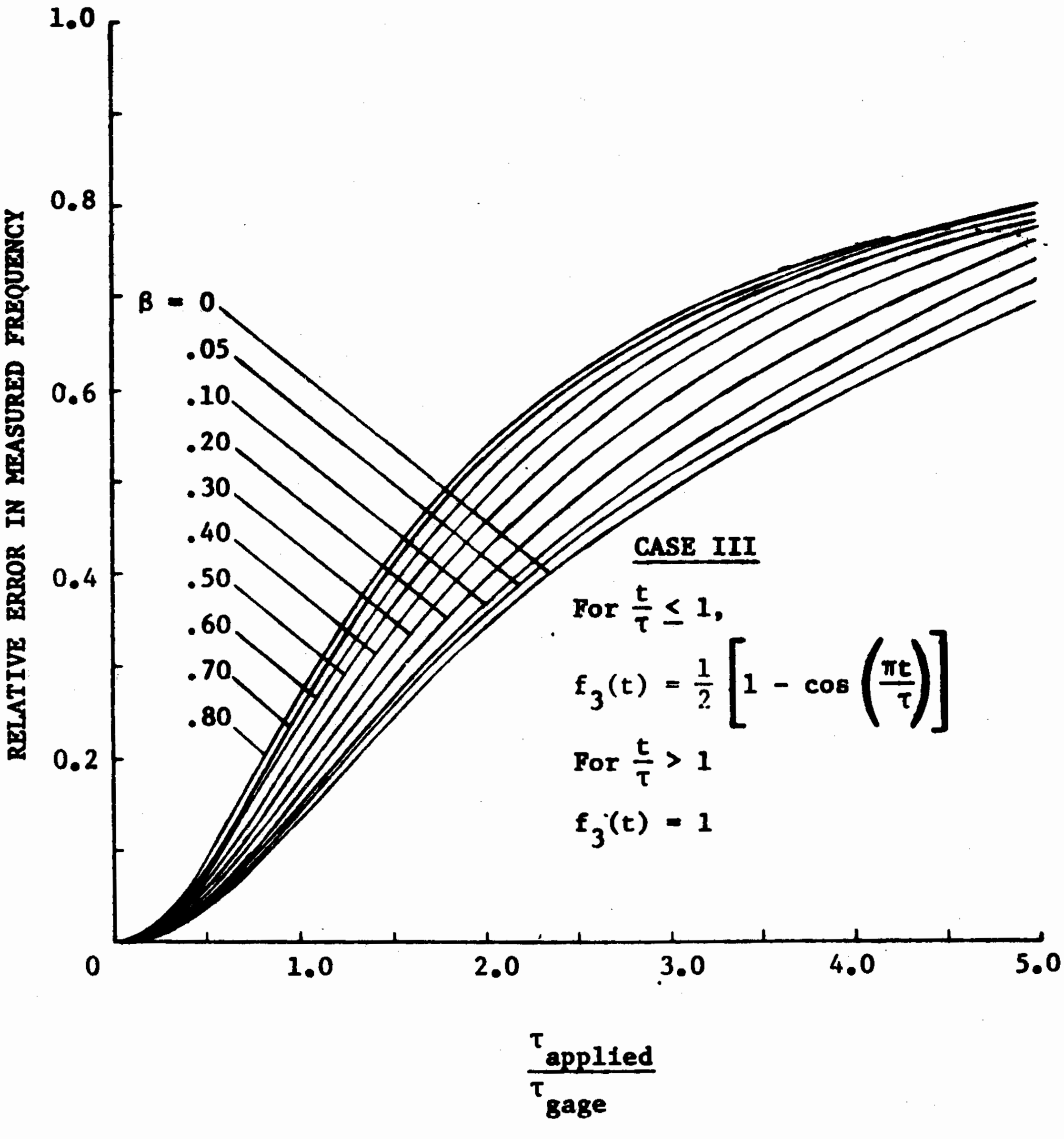


FIG. 8. RELATIVE ERROR IN THE MEASURED NATURAL FREQUENCY VERSUS DIMENSIONLESS APPLIED STEP RISE-TIME AND DAMPING RATIO FOR A PRESSURE STEP WITH A VERSED-SINE FRONT

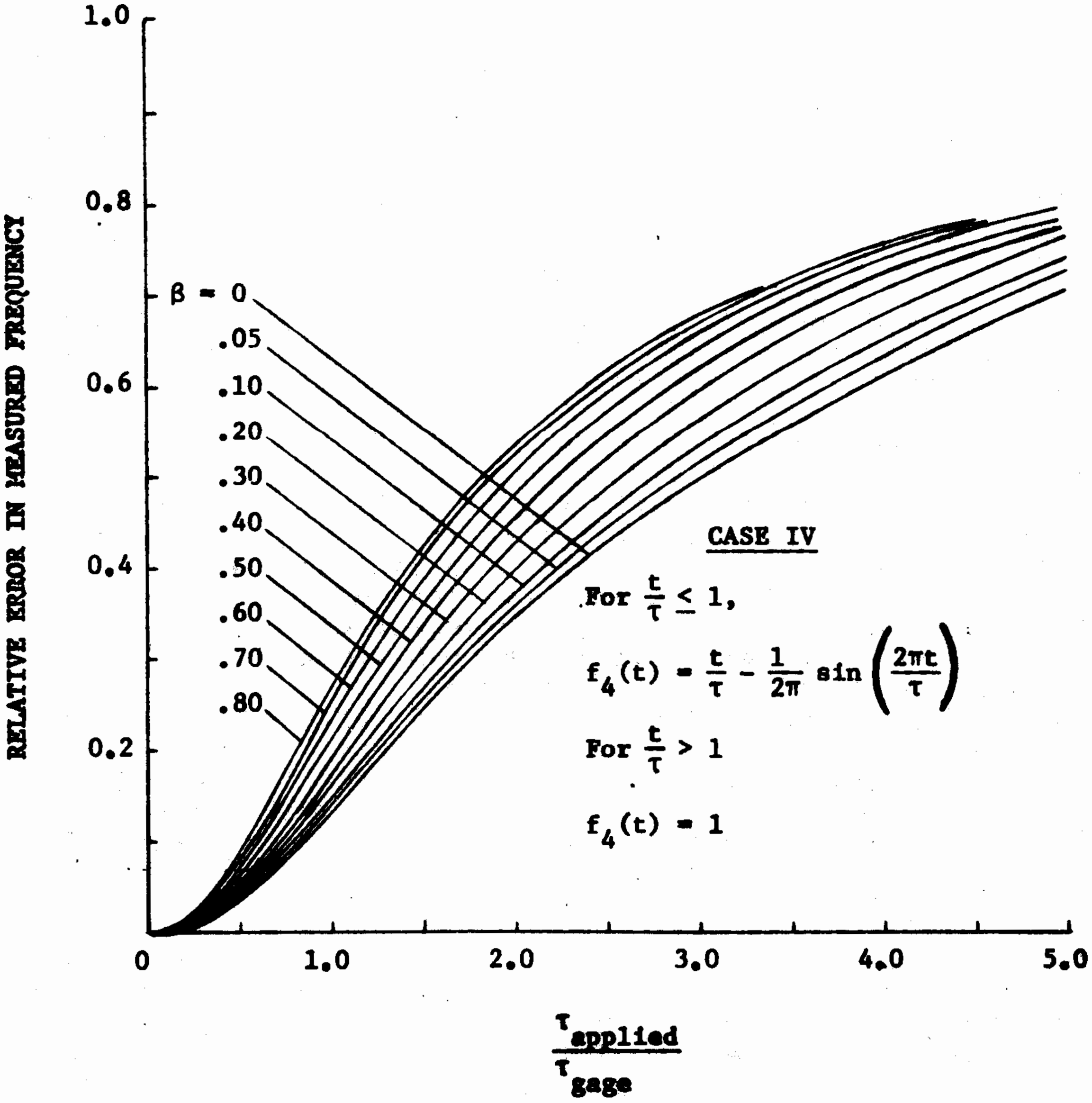


FIG. 9. RELATIVE ERROR IN THE MEASURED NATURAL FREQUENCY VERSUS DIMENSIONLESS APPLIED STEP RISE-TIME AND DAMPING RATIO FOR A PRESSURE STEP WITH A CYCLOIDAL FRONT

STANDARDIZATION AND TRANSDUCER STANDARDS

By

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Abstract

In recent years the importance of standardization documents is becoming increasingly apparent. As a result, there is an expanding interest and participation in the use and development of standards in technical areas such as the production, use, and calibration of transducers and related instrumentation. The function of American National Standards Institute (ANSI) is described and specific information is provided on the American National Standards Committee MC88 "Calibration of Instruments". Section II provides a compilation of transducer related standards.

Section I - Standardization

Standards are an essential part of modern society affecting our lives in ways never thought of by the man on the street. The building you work in, the chair you sit in, the pen in your pocket, the clothes you wear, and the car you drive are all manufactured according to standards for material, design, and performance. In recent years there has been an increasing awareness of standards and standardization through exposure in the newspapers, government activity in upgrading consumer products, metrication and international standardization activities. Engineers are taking an increased interest in the preparation and use of standards in technical areas where they were previously ignored, inadequate, or nonexistent. This increased interest in standardization is a welcome and much needed devlopment.

The word "standard" is occasionally confusing since there are two distinctly different types of standards - the standard "thing" (such as standard cells, dead weight testers, etc.), and the standard "document". This paper refers to the standard document which provides for compatibility, quality, performance, and uniformity of procedures and products.

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* Chairman of American National Standards Committee MC88 "Calibration of Instruments Used in Measurement of Pressure, Temperature, Fluid Flow, Liquid Level, Force, Mass and Density".

There are essentially four kinds of standards documents which differ primarily in the degree of agreement or consensus needed for their development and use. The Company Standard needs only to satisfy the needs of the organization. The Industry Standard is developed by a trade association to provide standards, by agreement of the members of the association, necessary to the efficient operation of that industry. A third type of standards document is the Government Standard. The fourth and last is the voluntary full Consensus Standard which is developed by representatives of all sectors having an interest in the use of the standard. Consensus Standards are prepared by such organizations as the American National Standards Institute, Instrument Society of America, and The American Society for Testing Materials.[1]

The Federal Government has also become increasingly interested in the development and use of volunteer standards as illustrated by the following examples. Two recent American National Standards have been approved for use by the General Services Administration for acquisition of federal computer systems. Research and Development Administration, and the Federal Energy Administration and the Department of Housing and Urban Development have stated that only national Consensus Standards on solar heating and cooling would be accepted in government sponsered or government regulated programs. As a result of a recent standards conference, The Occupational Safety and Health Administration (OSHA) has signed an agreement to work more closely with ANSI and a joint ANSI-OSHA working group has been established. ANSI's Nuclear Program is an excellent example of government-voluntary standards system cooperation.

Another indication of the increasing need and importance of consensus standards is the potential government intervention through the Voluntary Standards and Certification Act of 1976, S. 3555. The following summarizes an editorial which appeared in the August 1976 issue of Modern Materials Handling and reprinted in the November 19th issue of the ANSI Reporter. Bill No. S 3555 introduced into the Senate in June 1976 could throw the entire voluntary standards program into turmoil. Essentially the bill calls for government control over virtually all aspects of standardization requiring certification of standards groups/testing laboratories, and establishing procedures for standards development as well as for arbitration, agreement and withdrawl. The intent of S. 3555 is positive as it is designed to ensure participation by all sectors interested in the development of standards, to protect consumer rights and to encourage competition. Unfortunately, the presence of government control and the massive red tape would probably have a serious negative effect on the development of voluntary standards.[2]

Government intervention into the development of voluntary Consensus Standards may be unnecessary as volunteer standards organizations have been improving their efficiency and effectiveness in recent years. For example, the American National Standards Institute has made numerous changes in its procedures to streamline the development and approval of American National Standards. ANSI is the nationally recognized coordinator of voluntary standards development and the clearing house for information on national and international standards. Its federated membership includes some 180 voluntary organizations, over 1000 individual companies and with participating representatives in all levels of government. ANSI is an organization coordinating a voluntary standards system which makes use of other standards organizations; a system that has been streamlined by use and continual upgrading; a system that has gained acceptance; a system which has produced excellent results.[3]

It is appropriate to provide information on the activity of the American National Standards Committee MC88 which is responsible for development of standards of substantial interest to most attendees at this workshop. MC88 is responsible for the establishment of American National Standards for the calibration of instruments and systems used in the measurement of pressure, temperature, fluid flow, liquid level, force, mass and density. The MC88 Committee is sponsered by the American Society of Mechanical Engineers and operates under the Measurement and Automatic Control Technical Advisory Board of ANSI.

To achieve the Committee's objectives, subcommittees have been formed and are currently active in all measurement areas for which the committee is responsible. The MC88 Committee has developed two approved American National Standards: B88.1-1972 "A Guide to the Dynamic Calibration of Pressure Transducers" and B88.2-1974 "Procedure for Bench Calibration of Tank Level Gaging Tapes and Sounding Rules". The MC88 Committee has participated in the approval of numerous standards developed by other organizations as American National Standards. Numerous other transducer related standards are being developed with those in or near final draft form listed in Section II of this paper.

MC88 Standards are developed by subcommittees with highly competent members representing a wide variety of producers and consumers. The MC88 Subcommittee on Pressure is extremely active and presently has several draft standards in various stages of development. The MC88

Subcommittees on Temperature and Liquid Level are actively working on standards in their respective areas and participating in the review of existing standards. The Subcommittee on Flow has been recently reorganized and is presently identifying requirements and priorities. The Subcommittee on Force is presently being reorganized, as is the Subcommittee on Mass and Density.

MC88 is presently expanding the extent of its effort in order to expedite and increase the development of much needed standards. In order to effectively increase its activity, MC88 needs additional subcommittee and committee members. (All interested individuals can obtain further information by contacting the author, (714) 629-5111, extension 3663)

Through development of standards on calibration of transducers, instruments, and systems, MC88 plans to provide standards which are technically correct, meet existing requirements, provide uniformity of methodology and establish a common basis for both consumers and producers to determine compliance with performance specifications.

Section II of this paper provides an update of the compilation of transducer related standards presented at the 8th Transducer Workshop.[4]

Section II - Transducer Related Standards

American National Standards
available from
American National Standards Institute
1430 Broadway
New York, NY 10018

*ANSI/ISA RP31.1	Specification, Installation and Calibration of Turbine Flowmeters	
B88.1-1972	Dynamic Calibration of Pressure Transducers; Guide for	
C96.2-1973	Temperature-Electromotive Force (EMF) Tables for Thermocouples (ASTM E230-72)	
MC1.1-1975	Digital Interface for Programmable Instrumentation (IEEE Std. 488-1975)	A widely used standard for microprocessor based instrumentation used with transducers
MC6.2-1975	Strain Gage Pressure Transducers; Specifications and Tests for (ISA-S37.3-1970)	
MC6.3-1975	Strain Gage Linear Acceleration Transducers; Specifications and Tests for (ISA-S37.5-1971)	
MC6.4-1975	Piezoelectric Pressure and Sound Pressure Transducers; Specifications and Tests for (ISA-S37.10-1969)	
MC6.5-1976	Specifications and Tests for Potentiometric Pressure Transducers (ISA-S37.6-1975)	
MC88 --	Static Calibration of Pressure Transducers; Guide for	Final draft being prepared

* This standard uses ANSI's new simplified designation system which eliminates dual designators on ANSI approved American National Standards.

MC88 --	A Hydraulic Sinusoidal Calibration Method for Low Range Pressure Transducers	Final draft being prepared
MC88 --	Dynamic Pressure Transducer Calibration Using Shockless Step-Pressure Generators	Final draft being prepared
MC96.1-1975	Temperature Measuring Thermocouples	
S1.1-1960	Acoustical Terminology (Including Mechanical Shock and Vibration)	
S1.2-1962	Physical Measurement of Sound; Method for	
S1.4-1961 (R1971)	General-Purpose Sound Level Meters; Specifications for	
S1.8-1969 (R1974)	Preferred Reference Quantities for Acoustical Levels	
S1.10-1966 (R1971)	Calibration of Microphones; Method for the	Reaffirmed 1976
S1.12-1967 (R1972)	Laboratory Standard Microphones; Specifications for	
S2.2-1959 (R1971)	Calibration of Shock and Vibration Pickups; Methods for the	Reaffirmed 1976
S2.10-1971	Analysis and Presentation of Shock and Vibration Data; Methods for	Reaffirmed 1976
S2.11-1969 (R1973)	Calibrations and Tests for Electrical Transducers Used for Measuring Shock and Vibration; Selection of	
S9.1-1975	Selection of Mechanical Devices Used in Monitoring Acceleration Induced by Shock; Guide for the	New Standard

Z11.275-1975	Glass Capillary Kinematic Viscometers; Specification and Operating Instructions for (ASTM D2515-74)
Z11.299-1971	Liquid Hydrocarbons by Turbine Meter Systems; Measurement of (API 2534-1970)
Z24.21-1957 (R1971)	Specifying the Characteristics of Pickups for Shock and Vibration Measurement; Method for (ISO 2372)
Z71.1-1969	Thermometers; Specifications for (ASTM E1-68)
Z110.3-1964	Determining Relative Humidity by Wet-and-Dry Bulb Psychrometers; Method of Test for (ASTM E337-62/1972)

American Society of Mechanical Engineers
United Engineering Center
345 East 47th Street, New York, NY 10017

PTC19-2	Instrument and Apparatus: Pressure Measurement, 1965	
PTC19.3-1974	Temperature Measurement Instruments and Apparatus. Performance Test Codes	1974 Rewrite

American Society for Testing Materials
1916 Race Street
Philadelphia, PA 19103

E220-72	Calibration of Thermocouples by Comparison Techniques
E230-72E	Standard Temperature-Electromotive Force (EMF) Tables for Thermocouples

E235-73E	Specification for Thermocouples, Sheathed Type K, for Nuclear or for Other High Reliability Applications	
E344-74	Definitions of Terms Relating to Temperature Measurement	
E77-72	Verification and Calibration of Liquid-in-Glass Thermometers; Method for	Also approved as ANSI MC88.3

Instrument Society of America
400 Stanwix Street
Pittsburg, PA 15222

ISA-S31.1	Specification, Installation and Calibration of Turbine Flowmeters	1976 Standard - Also approved as ANSI/ISA RP31.1
ISA-S37.1	Electrical Transducer Nomenclature and Terminology	1975 Standard - Also approved as ANSI MC6.1
ISA-S37.2	Specifications and Tests for Piezoelectric Acceleration Transducers	1964 Standard - To be revised
ISA-S37.3	Specifications and Tests for Strain-Gage Pressure Transducers	1975 Standard - Also approved as ANSI MC6.2
ISA-S37.4	Specifications and Tests for Resistive (Platinum-Wire) Temperature Transducers	Presently in preparation
ISA-S37.5	Specifications and Tests for Strain-Gage Acceleration Transducers	1975 Standard - Also approved as ANSI MC6.3
ISA-S37.6	Specifications and Tests for Potentiometric Pressure Transducers	1975 Standard - Also approved as ANSI MC6.5

ISA-S37.8	Specifications and Tests for Strain-Gage Force Transducers (Load Cells)	1975 Standard
ISA-S37.10	Specifications and Tests for Piezoelectric Pressure and Sound Transducers	1975 Standard - Also approved as ANSI MC6.4
ISA-S37.11	Specifications and Tests for Servo Acceleration Transducers	Presently in preparation
ISA-S37.12	Specifications and Test for Potentiometric Displacement Transducers	Presently being prepared for distribution
ISA-S37.13	Specifications and Tests for High-Level DC-Output Pressure Transducers	Prepared in draft form

Range Commanders Council
White Sands Missile Range
New Mexico 88002

RCC106-73	Telemetry Standards	Revised November 1975
RCC118-73	Test Methods for Telemetry Systems and Subsystems	Revised July 1975

Scientific Apparatus Makers Association
1140 Connecticut Avenue, N. W.
Washington, D. C. 20036

MTI 1 & 2	Load Cell Terminology and Recommended Test Procedures, 2nd Edition, 1964	
RC5	Resistance Thermometers, 2nd Edition, 1963	Presently being revised

RC8	Thermocouple Thermometers (Pyrometers), 2nd Edition, 1963
RC20	Measurement and Control Terminology, 1973
Z236.1	General Purpose Glass Laboratory Thermometers

REFERENCES

1. "The What and Why of Standards", ASTM Standardization News, February 1977

2. "Handcuffing Standards", ANSI Reporter, November 19, 1976

3. Landis, J. W., "Strengthening the Federated Standards System in the United States", ASTM Standardization News, May 1976

4. Rockwell, D. W. "A Review of Transducer Related Standards", Minutes of Eighth Transducer Workshop, April 22-24, 1975

SESSION V

DISCUSSION SUMMARY

Session Chairman: Lawrence Sires

Papers: Erlich; White; Aronson; Whaley and Obal; Lederer (for Rockwell)

DISCUSSION:

Jim Rieger, Naval Weapons Center, to Erlich: Basically you're talking to me, because I tried to do the same thing and it didn't work. But I see what you did and I'm going to try that. The multi-vibrator that you're using for your local oscillator, instead of using a crystal, makes you free running. I found that thing drifts all over the place. Not with just the temperature but apparently also with the tides and the moon. And I'm wondering if that has any effect on the thing you are trying to do.

David Erlich, Stanford Research Institute: Well, I tried it for several days in advance and also right before the shot. We checked the function of temperature and it didn't seem to cause any problem.

Larry Mertaugh, Naval Air Test Center, to Whaley: On yours I was a little confused. Are you not interested in the low frequency end?

Wayne Whaley, Wright-Patterson AFB: In general we are interested in broad band. We try to get as low as possible. It's the limitations of the technique that causes us not to be able to get low.

Mertaugh: Did you look at velocity transducers at the low end?

Whaley: No we didn't. We have a card that we can stick in our amplifier that integrates that acceleration signal to get velocity. That helps some.

Mertaugh: But you're still limited to the signal-to-noise ratio of the accelerometer ?

Whaley: Right. There is a tendency for the theoretical error to flatten out above a certain frequency.

Mertaugh: Is that just because the beam is responding only to its own natural frequencies above a certain frequency? As I recall your curves came up and then went flat.

Whaley: I really don't know why it does that.

Mertaugh: Then also, you're using a laser just to give you an input. What kind of a positional sensor do you use for the beam, a series of little detectors that are triggered?

Whaley: It's a quad cell detector.

Mertaugh: Was there a range you could operate on?

Whaley: Yes, it's about 1½ in. square so you're limited to that play, that travel.

Mike Obal, Wright-Patterson AFB: I'd still like to give a little background. Wayne mentioned there are two angular sensors on the market. One was never built for aircraft ; it's only good up to 100 Hz. It has pretty good low frequency response. It works on a magneto hydrodynamic principle. The other one is a velocity sensor called ADA by General Electric. But the thing is so big if you put it on any particular part of the aircraft you will be measuring the response of that particular area of the structure.

Obal to Mertaugh: Did you talk to the dynamic lab people there at Wright-Patterson? They do a lot of work on optical sensors. They also use rate gyros. You're only going to get the low frequency with them.

Whaley: We're really only talking about micro radians and sub-micro radians sometimes. So we're usually way down into the noise.

Walt Kistler, Kistler-Morse Corp., to Whaley: You mentioned the low frequency cut off of your accelerometers was 3-5 cycles. So I assume you thought of dynamic piezoelectric accelerometers. Now did you also consider that there are other accelerometers, like semiconductors or servo accelerometers, which do go all the way to zero frequency response? Did you ever consider those or is there any specific reason that you could not make use of those?

Whaley: Well, in general the low frequency end is limited by how far apart you can place the two accelerometers. Usually this technique will be limited by that rather than the accelerometer itself.

Mertaugh: The signal-to-noise is certainly a function of the accelerometer.

Whaley: That is true, but still we tried this technique on an F4 and got down to about 20 or 30 Hz. That was as low as we could get. Below that it's just obvious noise. Again it depends on the separation distance and how stiff the structure is.

Charles Thomas, Wright-Patterson AFB: I think one thing we ought to clear up here is that the drive behind much of the work that Mike and Wayne are doing is the application of the laser to a weapons program which we are participating in rather heavily. They already have several systems that can take care quite nicely of much of the motion below 100 Hz. You have to look at that low frequency end. It is not as important as the higher frequency. We are driving for these higher frequencies and that is where we run into a great deal of trouble with the commercially available transducers for measuring angular vibration. They simply can't resolve an angle down as low as the ones we need to look at if we're going to give the people we work for the kinds of accuracy they need to design their systems. That's part of it, a major part of the problem. We have tried one other system that they didn't mention. We had hoped it would show some promise. It was built by Honeywell. We've had 2 strikeouts in a row there. We're not yet sure why it failed; whether it was mechanical or electrical. We are in the process of trying to find that out right now.

Bob Lally, PCB Piezotronics Inc.: What again is the importance of angular measurements related to the structural behavior of aircraft?

Whaley: Of course when we talk about the angle you can kind of break it up into two. The beam has a rigid body angle, the difference between two points, and also would have a bending or slope. We are looking at both of those. Of course if you want to hang certain things on certain parts of the aircraft to direct optical waves, or whatever, you don't want angular deformations between any two reference points on the aircraft. You want to keep those in line, or at least you want to know what's happening, so you can put passive or active isolators at different points of the aircraft in line with each other.

Mertaugh: We have projects which are familiar with the Wright-Patterson dynamic analyzer associated with optical installations in pods. Angular vibrations are obviously the dominant thing to affect camera resolution. That's why we are interested in it.

Norm Muelleman, Gard Inc.: I wondered if any of you have considered using laser interferometer techniques for generating displacements and vibrations, using sampling techniques?

Whaley: Yes we have thought of that. But frankly we haven't had the time to pursue that possibility. Right off the top of my head I would say that most laser interferometer techniques require an awful lot of instrumentation that you probably won't want to put on an airplane. You want to keep it as simple as possible. We want sensors that we go in and glue onto someplace on the airplane; land the airplane and take the sensor off and put it somewhere else. For the kinds of programs we are talking about, we may not have the time to use a full blown system like that.

Bill Anderson, Naval Air Test Center: I'd like to ask Rudy White why he mentioned the requirement for accuracy in the signal conditioning for that pod, and would like for him to discuss why that accuracy was needed in the system he used?

Rudy White, Naval Air Test Center: The accuracy was a requirement from the specifications on the generating system which was being evaluated. The requirement on this particular system was to maintain a voltage accuracy of 115 V rms ± V over normal operating conditions. This dictated the 1/10 V rms accuracy we were looking for.

END OF DISCUSSION FOR SESSION V

Ken Cox, Naval Weapons Center, Chairman, Ninth Transducer Workshop: The issue I want to talk about directly affects all of us. I'm going to ask a question. There's a letter by Pete Stein, who had to leave for a previous speaking engagement, explaining the situation at NBS. How many of you are aware of this letter? The letter mentions some interesting situations that have developed at NBS, directly affecting transducer calibrations. We are very fortunate to have Dr. David Goldman, Deputy Director of the Institute for Basic Standards, National Bureau of Standards, Washington, DC, present to enlighten us on this subject. I now present Dr. Goldman!

Address
by

Dr. David Goldman
Deputy Director of
Basic Standards Institute
National Bureau of Standards
Washington, DC

to the attendees of the
Ninth Transducer Workshop

(Question and answer session follows the address.)

Dr. David Goldman: I take it that it's going to be all uphill from now on. I think I'd like to talk a little bit in general about what the Bureau of Standards does or should do. But I could put in context what has happened, or what I think is going to happen in the future. The function of the Bureau of Standards, as I see it, is to make sure that measurements that have to be done in physical science and engineering can be done to the accuracy required at reasonable cost. We do that by a variety of techniques. First of all we examine the system of all measurements and decide whether or not the Bureau of Standards really has to be part of the system. Sometimes we look at the system and decide not, and find out later that we made a mistake. Sometimes we decide we should be in it and after a while it becomes clear that we shouldn't. One of the ways that we assure that the measurement requirements are met is through the concept of traceability, that is, to assure that when you make a measurement in the laboratory or in the field, somehow by some chain, which is usually very ill defined, it is traceable back ultimately to the highest accuracy measurements that can be made and are made at the Bureau of Standards. The trivial example that I can give on this subject that you all are familiar with is our calibration service. Laboratories send us instruments. We calibrate them. We tell them what the present uncertainity in the instrument is and, if we do our job properly, how long it will hold this calibration. It sounds like a great idea but in actuality it's only a poor substitute for our real job, which is to make sure that the measurements that are done actually on site are being done correctly. We do that by another technique, something which I trust you have all heard about, called the measurements assurance program. We might send out an instrument to a variety of laboratories who voluntarily participate in this program, and examine some of the measurements that they make on this instrument. They send us back the data and we will evaluate it. Sometimes we send standard reference material, for example, a radiopharmaceutical and we have to determine whether the measurements of radioactivity or specific activity are done correctly. We analyze these things and send a report back. Now one of the things that we have learned, that everybody has learned, is that the better procedure, the more correct procedure of measurement assurance, is very much more expensive. Calibration services can be done reasonably well in our laboratory and it's fairly easy to demonstrate how much resources one has to put into it. Measurement assurance programs on the other hand are much more difficult to do, require a great deal more coordination, and in fact, the analysis thereof sometimes takes a fairly long time. Why do I mention these things? Because internally the Bureau of Standards has new demands on it. For example, as we shift from a calibration-base-traceability chain to a measurement-assurance-based chain it costs us more resources, more professional people, and additional automation equipment. I can tell you right now the Bureau of Standards has not had a budget increase in the 5 years I have been Deputy Director of the Institute of Basic Standards. In addition the Bureau of Standards is regarded as a natural resource by Congress. Whenever they think of a technological problem or a problem that might have a technological solution, not something very big like energy, because there you obviously have to set up a new agency, but for instance what standards should one have for recycled oil, this is dumped at Bureau of Standards and they say, go solve it. Examples are: testing consumer products and durability of energy efficiency of air conditioners. All of this is assigned to the Bureau of Standards and somehow or other people usually forget to assign us additional resources, additional money or additional people. Therefore, we have in essence a continuing reprogramming effort within the Bureau of Standards and certainly within the Institute for Basic Standards. I think we average approximately 6% reprogramming effort each year as we move out of the more traditional type services to the services which we feel necessary for the future. That's factor #1. Factor #2 is that for the first time since I have been at the Bureau of Standards, beginning in October which is our new FY, there has been a reduction in the base of the Bureau of Standards. That reduction in base is translated into 8% reduction in our Institute. That means that we really have to drop some services, or if you like, we have to raise our charges to the users of our services, and we look into both of these possibilities. When Peter Stein visited the Bureau of Standards last year, shortly before he wrote his letter, we had just received the budget figure from the outgoing Ford administration. At the time it was before election so it could have been that Ford was still the President and we had to reduce our budget, our people, by approximately 8%. One of the remarkable things you find out about Washington politics is that even with a new administration, nobody puts money back in just because it was taken out by a previous administration. Thus the budget that went to Congress, that is presently before Congress, and presumably therefore will be approved, includes in it this base reduction of 8% which starts in September. In order to meet these double-barreled blows of enforced internal reprogramming, as well as reduction bases, a variety of measurement services were proposed for abandonment, and indeed several groups of people were transferred from our Institute into other areas for example, recycled oil and consumer product testing. With this transfer it looked like some competencies would be at least temporarily and probably permanently lost to the Bureau of Standards. I think that at the time Pete visited we were really at a low point and were doing our best to figure out how to meet the needs and what we could do about the whole thing. In fact the National Conference of Standards Laboratories met a few weeks after this I'm sure many of you are members of organizations that are participants in the National Conference of Standards Labs. I gave a talk very similar to this one to the NCSL last October. Of course I got lots and lots of questions about concerns and complaints which I trust I will also hear this morning. In fact, a colleague of mine, Mr. Birmingham, prepared a talk before the executive board of the NCSL. It was written up and appears *in toto* in the NCSL Newsletter of March 77. If you don't have a copy of that you can get it. In this letter he specifically lists those measurement services which we have reduced or eliminated. Now I can tell you, that of the services that we thought we would have to eliminate last October, we have mitigated those effects and in fact have reduced far fewer services. I would like to tell you roughly the number of services that we have eliminated and then you can ask me specific questions. Before I do that though, the question comes up: how can you propose eliminating something because it wasn't necessary, and then add it back in? How do you justify doing that, and isn't this a little bit more of bureaucratic fumbling? To that I obviously have to accept some of the responsibility, but I'd also like to deflect some of the responsibility to the users of our services. One of the remarkable things that we find is that until a service is cut off, not even proposed to be cut off, but actually cut off, one doesn't find out who uses our services and why. I'm not quite sure I know the reason for that. To some extent the interaction at the level of the provider to the user of our service, who frequently are our calibration facilities and the calibration laboratories of other organizations, industrial and governmental, have such good relationships that they never talk about why they need the services. When our director quotes the vice president of research of a large industrial corporation, he says, I'm sure the NBS is necessary, but I'm not sure I really know why. So we have a selling problem to do, to convince the people that our services are cost efficient and the replacement thereof in the diverse private or governmental sector of our economy will cost considerably more in order to assure the same reliability of measurements. What I actually have just done is summarize the most recent exercise that we're going through, called zero-based budgeting, which means to examine from the ground up what would happen if we cancelled each one of the various services that we have. I'm convinced that the country would suffer. For historical reasons I will tell a little bit about what happened to those that we announced we were going to cut off, and instead are retaining. The largest group of people who will be transferred are those who used to be included in our engineering mechanics section, which at one time was about 40 people. The engineering mechanics section had the custody of our 12,000,000 lb testing machine, one of our glorious white elephants. Other responsibilities are for vibration measurements and ultrasonic measurements. As far as I know the 12,000,000 lb testing machine will not be used for traceability; it's really not needed. Instead, where traceability in national standards will be required, we will utilize one of the testing machines in private industry or at a university which we can go in and check by transferring back to our million lb. deadweight machine and a variety of other deadweight machines, which we still maintain with the Institute of Basic Standards. The ultrasonics work is continuing and has been transferred temporarily to my direct jurisdiction. We will be announcing soon a service for providing calibration of ultrasonic transducers and also the sale of standard reference materials so that people can self-calibrate in their own laboratories. The vibrations work was scheduled for total elimination. In actuality, we are only eliminating the shock work. We will probably reintroduce the shock activity sometime in the future. Over 90% of our work referred to vibrations in the normal range above 50 Hz and I suppose below 10K Hz. We are continuing that service. One of the things we did, to provide that service with an actual reduction in funds or personnel, was to automate the service and to assign it as a part time responsibility to a more senior person, so that actually a reduced number of people provided the same work, and that is available. The flow measurements program was cut, and I remember the number to be about $580,000. This essentially would have eliminated totally the water flow facilities. That was not a proposal made by the Institute of Basic Standards. When we examined the real implications of such a cut, we reprogrammed it back into the flow measurements program. I found enough other governmental money and some industrial money so that at least for the present all of our flow facilities will remain open and our calibration services will not be changed. The other large area where we will reduce services is in that portion of the frequency spectrum that we call electromagnetics, which in actuality covers from about 30K Hz on up into the giga Hz range. We will not in any way and never have proposed reducing traceability, direct traceability, to the national volt standard which we maintained in Gaithersburg. In actuality we hope that soon everybody will be able to purchase his own national volt standard based on the ac Josephson effect and for a few tens of thousands of dollars you will never have to refer back to us except to make sure that you are doing the measurements properly. On the other hand, in the radio spectrum we are reducing services. We're reducing them primarily because we think that the user laboratories can provide most of the traceability chain without reference back to the Bureau of Standards. We will, however, maintain the highest quality of measurement services, so that those people who want to check their measurements will be able to do that. In addition we have developed a new reference measurements system called a 6-port system, which is 2 inputs and 4 outputs, and if you measure the power there at any frequency you are then able to determine the circuit parameters of interest in the microwave region. Those are the facts. What are we doing finally to prevent this great misunderstanding or at least to inform people in advance? We are having meetings. We have two of the primary government users of our services, the Defense Department and the ERDA, holding meetings right now in which we are saying things similar to what I'm telling you. We're describing what we're doing, telling what services we think we ought to be offering in the future; for example, pulse measurements in the nanosecond region for extra high voltage measurements. We don't offer that now because there's no way of providing the reference measurements right now, although we're working on it. Again in order to provide these new services we have to utilize our own money, and we therefore have to look again to renewed reduction of services. We have reduced the cost of our radio stations by automation and by turning off a total of 3 little-used radio frequency transmissions for our time and frequency information. Sometimes we have a hope of actually getting new funds, possibly in the area of electromagnetic interference, which everybody is concerned with. We may be allowed to ask Congress for new money. We're still trying to fight that through. At least that shows you that in one case we tried very hard to add to our resources. Most of what I've told you this morning is that we have to look internally for new resources in order to develop the new services that are needed in the future. That's all I wanted to say this morning and I'll certainly be happy to try to answer any questions or concerns that you have.

Pierre Fuselier, Lawrence Livermore Laboratory: I thought that humidity was also cut back.

Dr. Goldman: Yes, thank you. The humidity section was cut back. That really means that we're not going to provide methods of measuring moisture. However, we will still calibrate primary humidity standards. We will not be developing new humidity standards. We don't see the need for a better standard than the one we have now, which as I recall measures humidity of one part in 10^6.

Bob Lally, PCB Piezotronics, Inc.: You mentioned vibration was transferred to another person, transferred to the acoustic section.

Dr. Goldman: Yes. I happen to be Chief of the Acoustic Division. Don't ask me why. It's complicated enough. The section is a SAM section Ed McGrab is the Chief and Bev Pain is doing the work. Since Pain used to work in the old Engineering Mechanics Section the confidence will be able to be continued. As I say that's one area where we looked into it and decided that it was an erroneous decision to terminate that part of the service. We may look in the future to its transferring back to some other kind of institution. I'm not saying that we're going to do it.

Lally: First of all, I have been associated in this industry with users and with aircraft companies for a long time, and I want to say that people at the Bureau of Standards, like Paul Lederer and Seymour Edelman in the engineering mechanics section, have provided an extremely valuable service. We're very grateful for this service and somehow this didn't come across in your talk too well. I would say that it was probably associated with measurement assurance, but it goes much deeper than that. It seems to me they are kind of a coordinating body and a directing body for the whole field of measurement technology and the field of voluntary standards. It's a very vital function that NBS performs. It's kind of like the NASA of measurement technology. It's been an invaluable asset to the country, to industry and government.

Dr. Goldman: Yes. Sorry if I just cut you off. Let me say that I regard those kinds of services as part of what we should be doing.

In fact one of our problems is how do we disseminate our measurement capability? I mentioned a couple of things, that is, the calibration and measurement assurance program. In addition such things as private consultation, coordination of voluntary standards, interaction with international standards, and advising other government agencies are all part of the way we disseminate our services. If we just maintain them in-house without ever disseminating them, we might just as well disappear. I think I agree totally with what you say. The problem is deciding how much of one's resources one is going to provide for maintaining the various units of measurement, and how much of the resources one then puts into their dissemination. I trust that when I say that we've maintained the calibration service, I have not left you with the impression that we have terminated the dissemination services associated with that. You mentioned Paul Lederer, and of course Paul is still at the Bureau of Standards in the Institute for Applied Technology. You can still call upon him for those services. It is a fact that as we terminate some of our services, some of the coordination work that we did unfortunately must be reduced also. Our job is to make sure that we properly evaluate the impact before we terminate those services.

Lally: Probably the most vital function of the Bureau, in my viewpoint, and the one that seems to be suffering the most, is the pioneering work in the field of measurement technology.

Dr. Goldman: We call that "development of new measurement technology." I think that's true. For example, I mentioned specifically in response to a question that we are no longer going to develop the measurement of humidity technology. In vibration it wouldn't surprise me if we do not develop a measurement technology there either. Although as I say, that's still an open question. Which technologies do we develop? Those where we think we will be needed 5 years from now. That's where we put our resources. I mentioned a couple of new technological areas that we have to develop; very short pulses, very high voltages. There are others, measurements associated with non-equilibrium conditions. How do you measure temperature when the system never really settles down? How do you measure temperature in extremely hostile environments, such as inside reactors or a coal gasification plant? These are new technological areas that we have to get into. I don't want you to think we will stay in all that we are in right now. We have to reduce some of these.

Lally: We're not concerned so much about the new areas as the ongoing areas. Just over the past couple of days here I see a lot of value in what's been done in those areas here. We don't see it so much as being new but rather as being a normal process of ongoing work in a field of basic mass, flow, motion, and it concerns me very much that what appears to me to be vital functions of the Bureau as a pioneering agency of instrumentation technology being obscured or being lost.

Dr. Goldman: Well again, you know everything is a question of priorities. You have to decide where the need is greatest or where you can satisfy that need in a unique way. You mentioned mass flow, I think that's an area where we can actually make a unique contribution. For example, we will have or are in the process of developing models for fluid flow around barriers and through constrictions. I trust sometime in the future, maybe 10 years in the future, we'll have a model that's totally checked out and we'll be able to replace our standard method, which is the calibration of meters. I would be perfectly happy to see some industrial laboratories say we'd like to calibrate meters for you. Why? Because these are expensive facilities to maintain. We'd like them to do that. We might very well look to some laboratory or maybe some independent research institute to replace us as the national standards of fluid flow, traceable back to us by this modeling and certain transfer standards. We will do that. In fact, we're going to do the modeling in Gaithersberg. I didn't mention all of our Boulder activities. We deal with cryogenic liquids, but we hope in the future to do gas flow and have it traceable back to the mass standards, the catch basins and weighing tanks, and use that as our transfer mechanism to get it out in the field. What I'm trying to tell you is we're looking toward the modern world. We do our best to find out what the needs are. By the same token we have to reduce services in certain areas where we find the services can be gotten other places. We'll develop those other places for the future.

Charles Thomas, Wright-Patterson AFB: I guess the thing that bothers me most of all is the shortsightedness of this whole operation. Here we are apparently taking the priority from auditing, the standardization of things, which seems to me an overriding consideration. You're relegating that to a secondary position, bringing to the fore what I would consider mundane things, such as measuring the efficiency of air conditioners. Now that certainly can be done by a commercial organization.

Dr. Goldman: Well look, you will have to believe me when I say that decision is made by bigger minds than mine. I cannot respond to that. All I can say is that Congress decided that the Bureau of Standards was to get involved there and decided not to give the Bureau of Standards extra money. I share your concern relative to that. We do the best that we can.

Ken Cox, Naval Weapons Center: In calibrating force, I think you've answered it, but I'd like to have a direct yes or no so I can go back and tell my people yes or no. We're connected with some top priority missile programs. In one case I have a 300,000 lb Morehouse proving ring used for a standard. These people get paid on how much thrust they develop. Very important. Will I be able to get this 300,000 lb proving ring calibrated?

Dr. Goldman: Absolutely. Exactly as you have before, never any question about that.

Cox: For how long?

Dr. Goldman: Indefinitely.

Fuselier: Seems that the real issue here is money, and the lack of it, and as somebody said, priorities. Congress considers its own priorities. I think they go somewhat by what we tell them. Instead of hollering at Goldman, we ought to be hollering at our Congressmen, and telling them this is important. We don't want you to cut it out. With all the money this government spends, cut back somewhere else. NBS has the capability and the personnel to do more important things than evaluating reclaimed oil and men's shirts. We don't think that's important. If NBS had the money I'm sure they would continue to do whatever is necessary to support the measurements fraternity and the measurements required in the economy of the country. That includes the pioneering that Bob Lally is rightly concerned about, and the coordination that we need in the area of flow measurements, and in its serving as a fiduciary for all of us; someone we can look to when we have arguments and need advice.

Lally: I can bring you a whole stack of letters from Congressmen that are replying to exactly what you suggest. I've done it, gotten all the way to the President. I got beautiful letters; a beautiful letter from the Director of NBS assuring me that it is a case of priorities. It is a case of money; just leave the decision up to the "Great White Father." I have one thing on the money question; certain money exists, it is there and it is utilization of that money. You talk about priorities. You talk about modern needs. I can tell you a little story about the space shuttle, where they have a very real problem. A very modern problem, high priority, and it is associated with the Pogo effect when the vehicle is launched. NBS did get involved in this, drawn into some of the measurement problems. They did some very vital and very important work. It was not adequate to convince the people making decisions. Now pressure measurements are another basic area where the Bureau provided a very vital function. In spite of some really meaningful work of the Bureau, the decision went contrary to recommendations. Decisions were made on how the measurements would be performed, what instruments would be used, and unfortunately they were bad decisions. I suspect enough money in that one instance has gone down the drain to finance all this pioneering and coordinating work the Bureau will be doing, say, for the next year. Somehow when I experience things like that I can't buy this business of priorities.

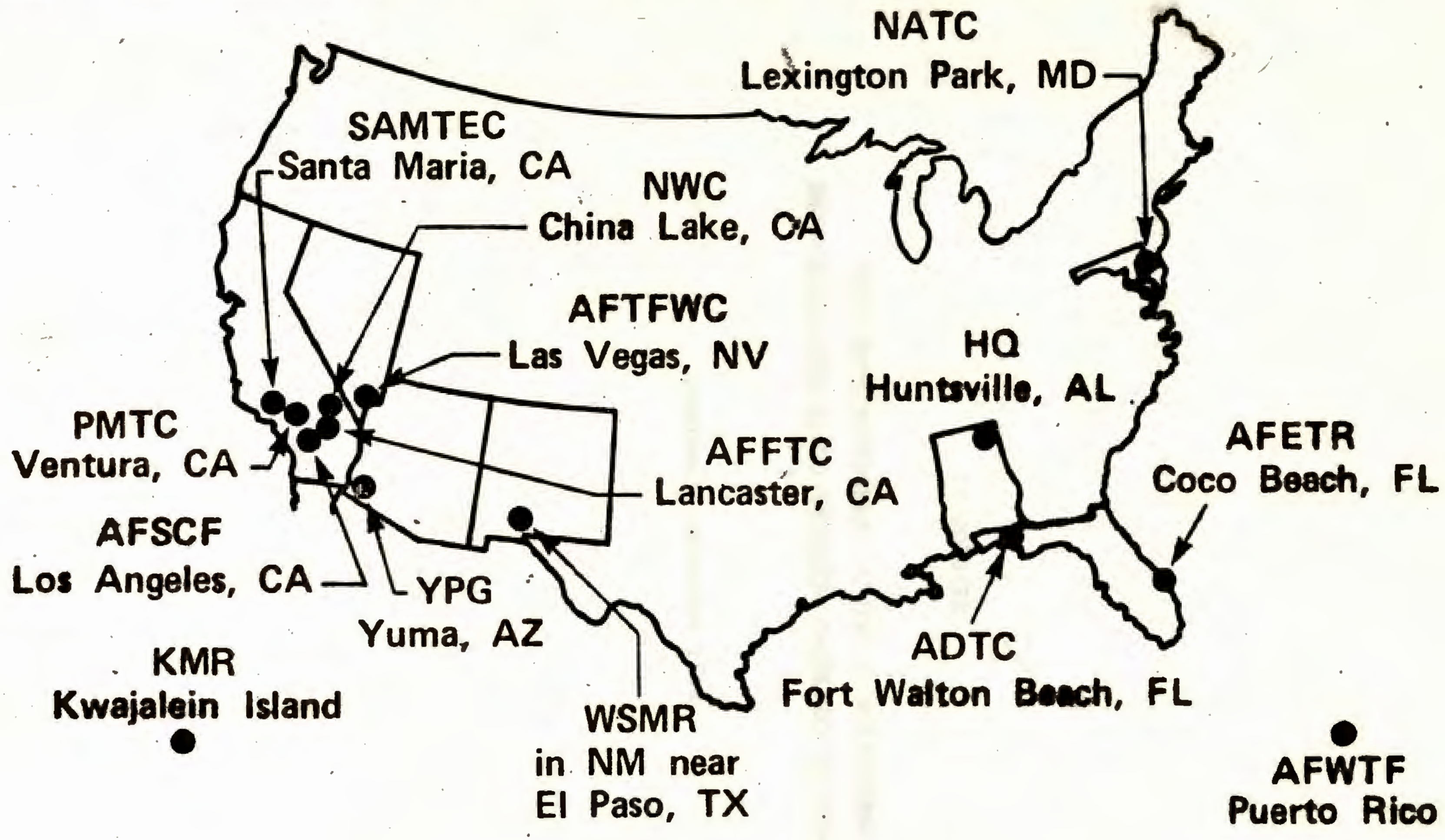
Dr. Goldman: Let me comment on your comment. I think pressure measurements are very important. Again, there is no intention now or in the foreseeable future to reduce our pressure or vacuum program. Just the opposite. We're likely to get back into the vacuum standards business as the needs develop there. In fact some of the resources reprogrammed that I mentioned have gone into that. What we can do about it, in addition to writing letters which get answered as form letters? One of the things that we are likely to do is raise our calibration fees. Please don't quote me but that's one way to determine the market, as you know. Another thing which you all can do is go back to your agencies and say gee, we think the Bureau of Standards is so important. We need them for the following reasons... . One-third to 40% of our budget comes from other agencies; some of it for developing basic measurement capabilities, for example, to the Defense Department Calibration Group. NASA has provided this money in the past and still does, also the Atomic Energy Commission. There is something else that can be done. We tried very hard to develop those contracts in an effort to supplement our base program. Now we have a problem as to which money we can take and make sure we don't compete with private industry.

SESSION VI

DEFINITION OF RCC/TG TRANSDUCER COMMITTEE EFFORTS TO IMPLEMENT WORKSHOP GOALS AND CONCLUSIONS

Bill Anderson, Chairman

NATIONAL & MILITARY SERVICE TEST RANGES



The Range Commanders Council: a medium for exchanging ideas and discussing mutual problems in the operation of the test ranges.

SESSION VI

DISCUSSION SUMMARY

Session Chairman: Bill Anderson

Panel: Transducer Committee Members: Thomas, Cox, Fuselier, Hilten, Lederer

DISCUSSION:

Bill Anderson, Chairman, Transducer Committee, RCC/TG: The Range Commanders Council is a group of 13 national ranges. The Telemetry Group is a subgroup of that Range Commanders Council and the Transducer Committee is one of the five committees of the Telemetry Group. I'm not going to introduce the members here because from the discussions you'll recognize that they have been introduced as part of the Workshop. The only member of the Transducer Committee who is not here today is Joe Haden, Holloman AFB. Joe sends his apologies, he had a test at Holloman that he just couldn't get out of that was scheduled for this week. What I'd like to do is discuss the efforts and work of the Transducer Committee that we're doing right now. Then I'd like to open the discussion for suggestions from the participants as to what areas the Transducer Group at NBS can address themselves; what problems do we see as transducer users and manufacturers that need development or elevation work. Paul Lederer will chair this discussion. I also want to mention two of the products of the work of the Telemetry Group and of course the inputs to the Transducer Committee to that work; RCC Document 106; which is the "Telemetry Standards"; and RCC Document 118 which is the "Standard Test Procedures for Telemetry Systems and Subsystems." I think these are very useful documents, but to be useful, these documents have to be applied by the people who need them and are familiar with them. I want to make sure that everyone understands that these documents are available from the Secretariat. If you're not a government agency you may have to pay for them. I think they are well worth the money. The address is: Secretariat, Range Commanders Council, White Sands Missile Range, New Mexico 88002.

Now for the work of the Transducer Committee... there are many standards related to transducers and Paul Lederer in his talk for Dale Rockwell today gave the numbers that relate to these standards. It's been our decision that we are not developing transducer standards ourselves, as a committee. We have in RCC 106 a listing of existing standards that we have reviewed and consider as appropriate and are good standards. We upgrade that list each time 106 is upgraded; every 2 years. Anyway, I would like Charlie Thomas, who headed that effort, to come up and describe what we've done and what the status is right now.

Charles Thomas, Wright-Patterson AFB: We've gone through the list that he has indicated, updated this, primarily dealing with the terminology; things that are common or that we would like to be common among the user community. The list is now in pink sheets (distribution for review) and we are in the process of going through the final revision. We expect to have it out in the next issue of 106. I don't think there's much more I can say but we will try in that compilation of standards related to transducers to get an indication of the heiritage of the document; for example, if it's an ISA initiated operation and the ANSI designation is more recent you can cross reference the standard.

Anderson: Thank you, Charles. Our main efforts are in the area of RCC 118, Standard Test Methods. We've looked at where do we need standards that don't presently exist for evaluation of related signal conditioning units necessary for transducers. The first input that we had in this document really was a result of the 7th Transducer Workshop. One area that was considered very important was a total system calibration, not only just to the transducer, just the signal conditioning, just your recording system, but a calibration technique that could be used to verify or calibrate the entire measurement system. We spent about 2 years documenting what we saw as procedures, expectations and techniques to use for total system checks and calibrations. This at present is a part of RCC 118. We're going to continue to look at that document, enlarge it, change it, or modify it, as needed. Another area where we formed an ad hoc committee and have worked is in the area of signal conditioning. We looked and saw there really were no firm standards for defining terms used in specifications for signal conditioning, specifically dc differential amplifiers and charge amplifiers. So we started an ad hoc committee chaired by Joe Haden. The members in the charge amplifier area are Fred Shelby from Sandia, who's here at the Workshop, Allen Diercks from Endevco, who is also here, and Jack Brown from the Naval Weapons Center. The dc differential amplifier part of the ad hoc committee was assigned to Earl Cunningham from Ectron, Elvis Skidgel from IED and myself. We have worked on this for 2 years and it is now in pink sheet form. Let me define what pink sheet is; when we have a modification for RCC 118, we have to document this modification, then we have the Secretariat print this in a pink sheet form. This form is then distributed to all of the TG Group members and to the manufacturers involved. We ask for their comments. After we receive these comments, we rehash the standard, or test procedure, and rewrite when everybody is satisfied. We than submit it as a change in RCC 118. At present we have the dc differential amplifier on pink sheet form. We have had comments back and we've rewritten the document. We're still working on this and we're trying to finalize this by June so that it can go into the '77 issue of RCC 118. Another area of RCC 118 that we have is a test procedure and this is mainly from the work of the NBS Transducer Group. It is a thermal transient test method for pressure transducers. I'd like John Hilten to come and describe a little bit of their work and describe the status of that addition to RCC 118.

John Hilten, National Bureau of Standards: The issue of RCC 118 in '77, which will be coming out before the end of the year, will include the test procedure which has to do with generating thermal transients. Many measurements are made, not only in a dynamic environment but involve situations which have a rather harsh environmental condition, causing concern. Several years ago when we were first assigned the task it was to try to generate or try to develop a procedure for generating thermal transients. Now this was assigned to us by the Transducer Committee, which is sponsoring this meeting here. This is the task we have been working on. Several things were required of this test procedure. It would be a very simple thing and a very inexpensive procedure. We wanted to vary the thermal transient from 1 millisecond to longer time durations. It would be of variable magnitude and you could quantify the size of the thermal transient. As a result of these suggestions we have developed this procedure which will be coming out in RCC 118. When I talked to you yesterday I indicated that there was a tech note that had been published by the Bureau concerning this particular task. It can be obtained by writing to Paul Lederer for a copy. This also will be coming out in RCC 118-77. Basically, we use a series of flash bulbs to generate thermal transients. We have taken this particular procedure and we are proceeding a little bit further along. Oftentimes people use coatings of various and sundry types to protect the transducers. We've taken a series of 18 different coatings and, using dummy transducers, we've used the test procedure to generate a thermal transient against the transducer and looked at the output. Then we have taken four types of transducers and coated them with selective coatings from the previous task. Then we have taken these and put them in a shock tube, using one transducer as a control, and the second transducer was coated. We run a series of tests to try to see what happens to transducers that have these protective coatings, how this affects rise time, how it affects frequency response, how it affects sensitivity and what kind of acceleration sensitivity is generated by this sort of procedure. So this also will come out as a tech note in perhaps 2-3 months. So really what we have been working on the last year or two has been the development of procedures for generating thermal transients. Then taking and using what we've learned, we tried to find ways to protect transducers against thermal transients without affecting the dynamic characteristics of the devices themselves.

Anderson: The Transducer Committee has been working on how to foster communication among transducer users and manufacturers. I think the Workshop is a prime example of one method that we use to do that. Now, as I said in the introduction, Workshops have been sponsored by the Transducer Committee since 1960. This is the 9th Workshop. In the 7th Workshop, in a discussion of what areas the Transducer Committee should be working, it was suggested that we put out a directory of transducer users. We did within a year and we came out with a second edition after the 8th Workshop. This document is a fairly small one. Essentially what it does is to list personnel in each subscribing activity who are primary users of transducers, their address, phone number, how to contact them, details some of their expertise and some of their uses of transducers. It wasn't our purpose to list every user of a transducer, this would be a very large document, but to list one or two contacts at each activity. This document can then be used by transducer people to receive or give information to each other. We mentioned this at the beginning of the Workshop, that we do have papers to fill out if you are interested in having your activity represented in this directory. A directory like this is only useful if it's current. To make it current you have to upgrade it every year or so. We're going to have someone on the committee responsible for upgrading and he will probably contact attendees to find out if there are changes in particular applications or names, or additions to be made to the directory. One of the reasons the Transducer Committee was formed was to coordinate the efforts of NBS and the development of calibration techniques, evaluation techniques, uses of transducers, and assign priorities to that work and relate it to the Telemetry Group. The one problem we have is providing funds to get this work done at NBS. It's been a continual problem. Right now it is probably at its worst condition. The group had not been funded for '77, and the prospects of being funded for '78 are very, very small. The typical funding required in the group is between $150,000 - $200,000 a year to do an adequate job. We have written letters and we've tried to develop interest from the users in this group. This year we have not been successful. You as users can influence your management to get better support for this group. At each Workshop we ask the participants what they think this group at NBS should be working on, what areas do we see that need development and research to further techniques of transducer and transducer applications. I'd like your ideas on the efforts you see that should be directed by this group and we should establish a priority of work expected of this group.

Paul Lederer, National Bureau of Standards: I would like to start out by summarizing very briefly what the NBS transducer project is all about. Some of you may know it, the rest of you may not. Basically, in 1951 the Navy Bureau of Aeronautics asked NBS to look into performance characteristics of telemetry transducers and to develop evaluation and calibration methods to characterize such transducers. The work was accepted and continued through 1956 at which time the Range Commanders decided that the Transducer Committee should be formed as part of the Telemetry Group in order to monitor the work at NBS and also act as a liaison to feed back information to NBS as to needs. NBS would then send information back to those people who need it, both in the ranges and contractors. The Navy continued to fund us throughout most of the period. Subsequently other defense organizations came in, the Army and the Air Force at various times, including NASA. In the 25 year existence of this project we have received approximately $2.2 million dollars of which roughly 10% came from NBS. Now in our Institute the ratio is not like it is in the Institute of Basic Standards, but typically at the present time 16% of my budget comes from NBS. For the rest I have to shake the bushes. Now over the history of the Interagency project, roughly only 10% came from direct NBS contributions, approximately 70%

from the Interagency project, (that is the different defense agencies, coordinating their interests and spending), and the remaining 20% came from closely related tasks from other government agencies which fitted into the overall programs. During those 25 years we expended roughly 104 professional man years of effort. The project has resulted in approximately 120 publications including 18 NBS technical notes, 55 NBS Interagency reports, one patent, 18 standards, documents, papers, what have you. In addition, 18 organizations, to my knowledge (now possibly 19 including ADTC), are using devices of which we built prototypes for calibrations or evaluations, or are using our techniques. John mentioned what we had done in the thermal transient area and what we're finishing off now. At the 8th Transducer Workshop held in 1975 at Wright-Patterson, the participants had indicated a listing of priorities of tasks that they felt we should look into. There were four of them. One was the investigation of the availability and suitability of portable on-site calibration systems for transducers already installed ; in other words a simple, easy field calibration or let's say system verification of set ups. The next one was investigation of mechanical filters for accelerometer mountings so as to be able to acquire rigid body measurements in impact and explosive environments. Mechanical filters would filter stress waves coming into the accelerometer housing which would otherwise obscure the desired data. Number 3 was an investigation of methods for obtaining cross axis sensitivity of accelerometers over relatively wide frequency ranges. I think the common procedure now for manufacturers is to supply this information at essentially one frequency, a relatively low frequency. There's reason to believe that at the higher frequencies the characteristics of cross axis sensitivity change considerably. Number 4 was to develop and recommend techniques for the calibration and evaluation of angular accelerometers. As Bill pointed out to you, the funding crunch hit us, and we have not been able to tackle any of these jobs so far. What we'd like to get from you is some ideas and additional tasks that we should be looking into, in the hope that eventually we get funding. I'd like to point out, going back to Dr. Goldman's indication of this reprogramming business, that we also in a sense are reprogramming by shifting to areas in which the Bureau has received priorities. Specifically, we are now beginning a task in the area of energy conservation. We're directing it primarily to industrial processes. We have received funding for an initial task to develop evaluation methods for automotive transducers called MAP (Manifold Air Pressure) transducers. We have been actively working for almost a year in transducer applications to machine tool automation in the kinds of parameters that one likes to measure on a machine tool in order to assure oneself that the tool bit is still sharp and not likely to snap off in the middle of a run. There is a very great interest in this country in numerically controlled and computer controlled machine tools. One of the areas of need is transducers that will sense variations in tool characteristics in order to initiate corrective processes. So you can see that we have been forced to shift into areas where there is need as exemplified by the availability of funds. That's what it comes down to. If you have any needs, and if in some way funds can be made available, then we will be happy to shift our priorities in your direction. But it's up to you people and other people on the outside to let us know. So I think what Bill has in mind is for you to talk about areas of need related to transducer evaluation and calibration.

Bob Lally, PCB Piezotronics, Inc.: I heard an area of interest during the course of discussions here regarding the behavior of acoustic path coupling into a pressure transducer. I heard another one about behavior of the structure of pressure transducers. I'm familiar with the work that has been done by you at NBS sometime ago in applying instruments like spectrum analyzers. This is a very powerful technique pioneered by the Bureau of Standards which should be applied to sensor transducer structures. Basic fundamental problems exist, such as how structures behave acoustically, mechanically, electrically, etc. I would suggest work along that line.

Lederer: I had for years wanted to look into the analysis of the dynamic responses of pressure transducers using some of the newer techniques, Fast Fourier Transform Analysis, etc. Again, we're caught with the same crunch. It doesn't really matter what we would like to do or even with what we think is an essential area to start investigating; if we cannot identify somebody who is willing to fund it, we cannot do it. It's as simple as that. I think the paper that Phil Aronson presented today is a very valuable contribution in just precisely that area. I don't know how Phil was lucky enough to get money to do that investigation. By the way, parenthetically I'd like to mention something else. The funding problem I think really can be traced down to one particular development. In the early 60's when science and engineering in space flight was very popular, agencies did not hesitate to spend a little money on a broad-type program, as long as it really didn't catch too much attention. I'm talking about precisely the kind of thing that we feel should be done, a generic approach to measurements methods; the type of thing that Pete Stein is always talking about. When we consider the method for establishing the effect of thermal transients in pressure transducers, for example, this is really not tied to any particular application or any particular measurement. It's sort of a general approach. Now, today with the funding crunch in all agencies it is extremely hard to get somebody to fund something for, let's say,the general education or the advancement of the state of the art. If it does not fit within the mission of the particular agencies, they are not interested. If it does not help the F14 or the F15 send back the right information they couldn't care less, even if the amount is quite small; judging by the difficulty we had during the last year of obtaining roughly $6,000 apiece from each of 13 ranges. Now as you know very well from your own budgets how far $6,000 will carry a professional. It's probably 2 weeks, including the overhead. We're not able to get even this amount of funding to support something which would be of general use to every one of the ranges.

Lally: I hope that everyone recognizes the practical realities in the way these programs get funded. When I look at that, again maybe that's really a fault in our system. That's the type of work that ought to be funded by Congress. As I mentioned,. the basic funding of NBS ought to be directed toward that and they shouldn't have to scrape and scrounge for funds for this basic type of work.

Lederer: You spoke true words Bob, and as you saw from the discussion today even Dr. Goldman, who is Deputy Director of an Institute, has to follow the thinking of higher located minds than his. While I'm the chief of something or other at NBS, I'm much lower on the totem pole than Dr. Goldman. So I raally have very little influence that I can exert to change this direction of thinking.

Larry Sires, Naval Weapons Center: As a person who is on a project that has to spend money to develop something, I can tell you that we're in the same crunch on our end of it as you are on your end. We're given funds to do a job and then we're cut 25 or 50% of that and told to do the same job with that much less money. We just don't have the money to put into these kinds of things. We don't have the money unless it directly pertains to our project and getting our job done. We don't have the money to do it and I think you will find that throughout the government.

Lederer: I have found that out. It's nothing new, it's just getting worse each year.

Lally: You have to get more hard nosed because we can't afford to do anything else.

Mike Obal, Wright-Patterson AFB: Maybe the higherups, the people we're talking about, want to slow the rate of growth in these devices and if you look at it, maybe that's not so bad. Sometimes your costs really outgrow the state of the art. What I'm trying to say is you can only go so fast and I think now the emphasis is placed on building on what we have as opposed to things that may be used 5-10 years from now.

Lederer: I think that what you say is quite correct, except that we are going sideways now, as I see it in this country, in the sense that we are beginning to make a lot of use of existing things, in areas which have never before made use of them. Machine tool automation is a case in point. Also there is a tremendous lack of knowledge of the capability of these devices in medical applications. The broad-based generic approach that we take is eminently suitable for precisely that. We're not trying to develop an improved measurement technique, we're trying to teach people how to use existing devices and make the measurements that they need to make. Yet there is very little support for that kind of attitude.

I'm not sure if we're really going to generate any new ideas for tasks. I've read you the ones that the Transducer Committee gave us almost 2 years ago. I assume, since I didn't hear any objections, you still consider this as a reasonable series of tasks?

Thomas: From our standpoint they're still reasonable. They are ones which we have to face daily, as indicated by Whaley's paper and by the work that we're doing. We're constantly up against these things. It may be shortsighted but if you want to bring it right down to the nitty gritty, these problems we're up against not just occasionally, but quite frequently.

Lederer: All right then, we'll assume that for the time being these four tasks that were identified are reasonable ones for us to consider doing, assuming that we get the necessary resources.

Paul asked for a show of hands on each of the tasks for relative priority. The results were as follows:

1. *On-site calibration setups.*
2. *Mechanical filters for accelerometers.*
3. *Cross axis sensitivity determination for accelerometers over broad frequency ranges.*
4. *Develop techniques for the evaluation and calibration of angular accelerometers.*

I think that's probably enough to give us some kind of guidance on these tasks. If you have any other problems or if anything occurs to you after we leave here, call me at NBS. I'm the Chief of Section 425.03, the address is Washington, DC 20234. I'm on the FTS line; call me anytime. If I'm not available John Hilten will be answering the phone. If you want recommendations on transducers, anything we have experience with, I will be happy to tell you over the phone exactly what is good and what stinks, based on my experience.

Anderson: Thank you Paul. In closing the Workshop, I'd like to thank first the participants of the Workshop for your attention during the paper presentations and also your active participation in all the discussion areas we had. Second I'd like to thank the presenters of the papers for the time and energy it took to prepare the papers and for the very interesting aspect of the papers. Third I would like to thank Kenny Cox, I think he's done a very effective job as Chairman of the 9th Transducer Workshop. Last I'd like to thank Sid Shelley, our local host for doing an excellent job in preparing the facilities.

Kenny Cox, Naval Weapons Center, Chairman, 9th Transducer Workshop: This concludes the 9th Transducer Workshop. Thanks again to everyone.

END OF WORKSHOP

how to use existing devices and make the measurements that they need to make. Yet there is very little support for that kind of attitude.

I'm not sure if we're really going to generate any new ideas for tasks. I've read you the ones that the Transducer Committee gave us almost 2 years ago. I assume, since I didn't hear any objections, you still consider this as a reasonable series of tasks?

Thomas: From our standpoint they're still reasonable. They are ones which we have to face daily, as indicated by Whaley's paper and by the work that we're doing. We're constantly up against these things. It may be shortsighted but if you want to bring it right down to the nitty gritty, these problems we're up against not just occasionally, but quite frequently.

Lederer: All right then, we'll assume that for the time being these four tasks that were identified are reasonable ones for us to consider doing, assuming that we get the necessary resources.

Paul asked for a show of hands on each of the tasks for relative priority. The results were as follows:

1. *On-site calibration setups.*
2. *Mechanical filters for accelerometers.*
3. *Cross axis sensitivity determination for accelerometers over broad frequency ranges.*
4. *Develop techniques for the evaluation and calibration of angular accelerometers.*

I think that's probably enough to give us some kind of guidance on these tasks. If you have any other problems or if anything occurs to you after we leave here, call me at NBS. I'm the Chief of Section 425.03, the address is Washington, DC 20234. I'm on the FTS line; call me anytime. If I'm not available John Hilten will be answering the phone. If you want recommendations on transducers, anything we have experience with, I will be happy to tell you over the phone exactly what is good and what stinks, based on my experience.

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