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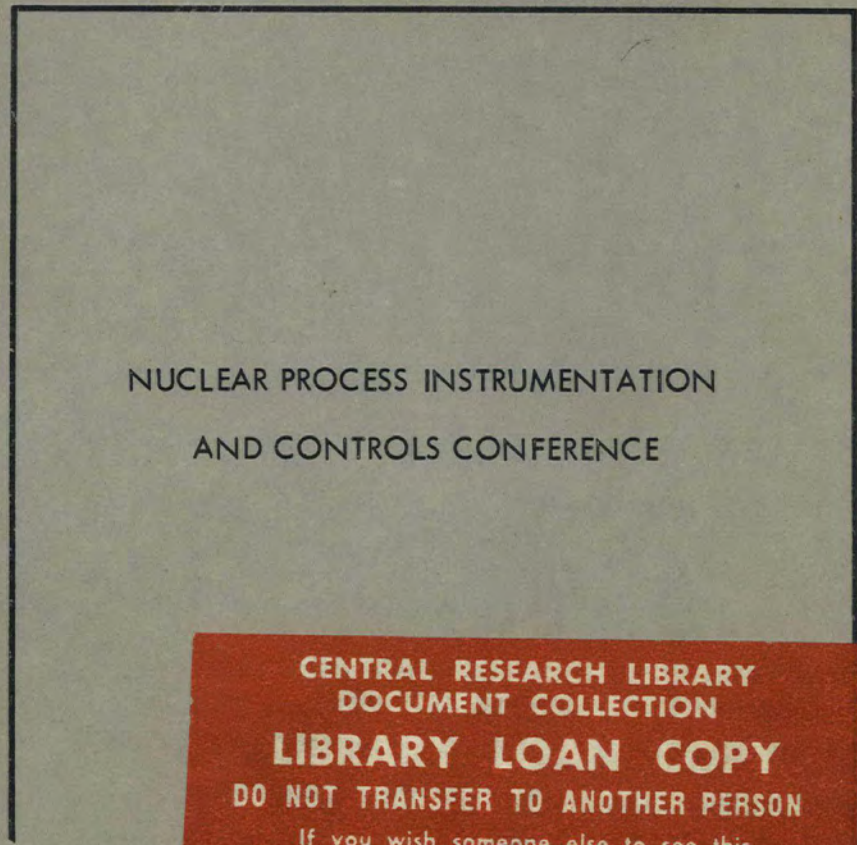
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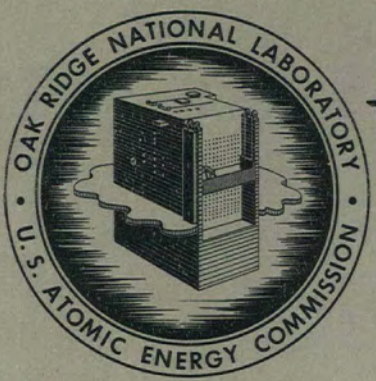
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NUCLEAR PROCESS INSTRUMENTATION
AND CONTROLS CONFERENCE

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NUCLEAR PROCESS INSTRUMENTATION AND CONTROLS CONFERENCE

Held in Gatlinburg, Tennessee

May 20-22, 1958

SPONSORED

by the

OAK RIDGE NATIONAL LABORATORY
INSTRUMENTATION AND CONTROLS DIVISION

C. J. Borkowski, Director

C. S. Harrill, Associate Director

and the

U. S. ATOMIC ENERGY COMMISSION

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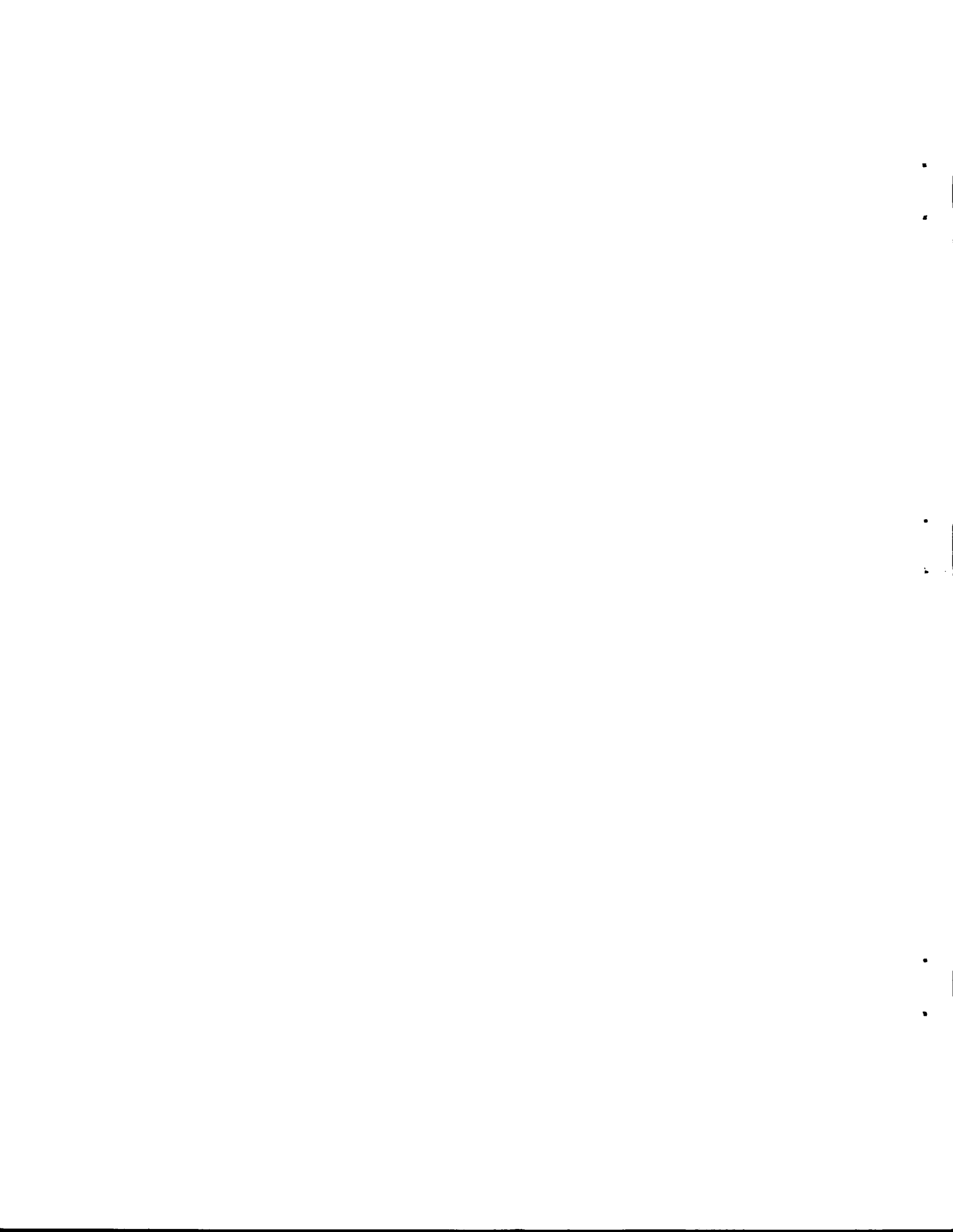
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FOREWORD

A Nuclear Process Instrumentation and Controls Conference was held in Gatlinburg, Tennessee, May 20-22, 1958.

This volume is a record of the papers and formal discussion sessions. They are grouped topically, rather than in the order of presentation, to facilitate reference. In most cases the paper was composed from the transcript of the author's oral presentation and his subsequent edited copy. Some are verbatim reproductions of papers received prior to the conference. Because of the difficulties of obtaining an accurate stenographic record, there are probably errors in the record of the discussion sessions. The editors (the conference committee) assume all responsibility for any such errors and hereby apologize to all who are misquoted or mismatched with recorded statements. The conference was conducted on an informal basis to promote the free exchange of knowledge, and it is felt that the effects of this informality outweigh any resulting minor errors in the record.

Because a stenographer was not available, a record of the discussion sessions on "Valves for Nuclear Reactor Service," conducted by J. C. Curran, and "Process Instrumentation in High-Pressure Environments," conducted by C. J. Madsen, could not be obtained.

The paper by E. A. Bake was presented in his absence by C. J. Madsen.

The conference committee members were R. G. Affel, chairman, H. J. Metz, C. A. Mossman, D. S. Toomb, and C. S. Walker.

PREFACE

The opening session of the Nuclear Process Instrumentation and Controls Conference was called to order by Dr. C. J. Borkowski, Director of the Instrumentation and Controls Division of the Oak Ridge National Laboratory.

C. J. BORKOWSKI: I wish to welcome you on behalf of the Oak Ridge National Laboratory, the Union Carbide Nuclear Company, and the Atomic Energy Commission. I hope that your stay here in Gatlinburg will be a pleasant one.

Gatlinburg is rapidly becoming a mecca for scientific conferences; last week there was a conference on nuclear fuel elements and prior to that there were conferences on accelerators, biology, and mathematics.

East Tennessee appears to be ideally suited for informal conferences of this kind.

We hope that this conference will take on a shirt-sleeve informality, in contrast to the large ISA and IRE meetings, and we hope that the participants will have time for informal discussions of their problems. Of course, the main purpose of this conference is to exchange information and experiences concerning hardware and systems design. I think that the acquaintances and contacts that you make at this conference will be rewarding. In bringing together a group of specialists, we feel that there will be vigorous discussions on controversial ways of doing things or on controversial instruments used in the various environments that we are going to talk about. These environments, of course, you are aware of: high temperature, high pressure, intense nuclear-radiation fields, and corrosion -- all pose a challenging problem to the engineer designing components or working out a systems philosophy that protects not only the equipment but the operating personnel and the community.

We hope to discuss the design of instruments and controls for in-pile loops; to discuss data-handling systems -- our business is becoming more complicated and the output of many sensing elements has to be recorded. Data-logging systems are therefore becoming more useful. Data-reduction methods must be used in order to quickly give operators an insight into what is happening in a complex process or environment.

The complete proceedings of this conference will be published, and I would suggest that if there is any discussion from the floor, it would be best for the person to identify himself: to give his name and his affiliation for the purpose of the record, and also to aid Mrs. Caplin, our stenotypist.

CONTENTS

FOREWORD.....	iii
PREFACE.....	iii
I. PROCESS INSTRUMENTATION IN HIGH-PRESSURE ENVIRONMENTS	
Primary Elements for Boiling Water Reactors.....	3
B. E. Woodward General Electric Company A.P.E.D.	
Instrument Transmitters for High-Pressure Aqueous Nuclear Reactors....	13
R. L. Moore Oak Ridge National Laboratory	
Operational Experiences with Primary Elements and Valves in Slurry Service.....	40
E. A. Goldsmith Westinghouse Atomic Power Division	
Primary Coolant Valves for Nuclear Plants.....	59
E. A. Bake Westinghouse Electric Corporation	
II. PROCESS INSTRUMENTATION IN HIGH-TEMPERATURE ENVIRONMENTS	
Instruments Used in High-Temperature Sodium at Argonne National Laboratory.....	67
F. A. Smith Argonne National Laboratory	
Liquid Metal Instrumentation Practice.....	86
P. Bliss Pratt and Whitney Aircraft Division of United Aircraft Corporation	
Specialized Instrumentation for Liquid-Bismuth Loops.....	102
D. V. Huszagh Brookhaven National Laboratory	
Temperature Measurement Discussion Session.....	117
W. P. DiPietro and C. J. Madsen, Chairmen Westinghouse Atomic Power Division	
Discussion of Process Instrumentation in High-Temperature Environments.....	132
R. G. Affel Oak Ridge National Laboratory	

III. SYSTEMS ENGINEERING OF SAFETY AND CONTROL SYSTEMS

System Design and Instrumentation of In-Pile Test Loops.....	155
M. A. Vogel Westinghouse Electric Corporation	
Safety and Control of In-Pile Experiments.....	170
E. P. Epler Oak Ridge National Laboratory	
Built-In Testing in Process Instrumentation.....	179
E. Siddall Atomic Energy of Canada, Limited	
The Role of the Instrumentation and Controls Analyst in Power Reactor Design.....	195
E. R. Mann Oak Ridge National Laboratory	
Discussion of Elementary Considerations of Safety and Control Systems.....	206
W. H. Jordan and E. P. Epler, Chairmen Oak Ridge National Laboratory	

IV. DATA HANDLING SYSTEMS

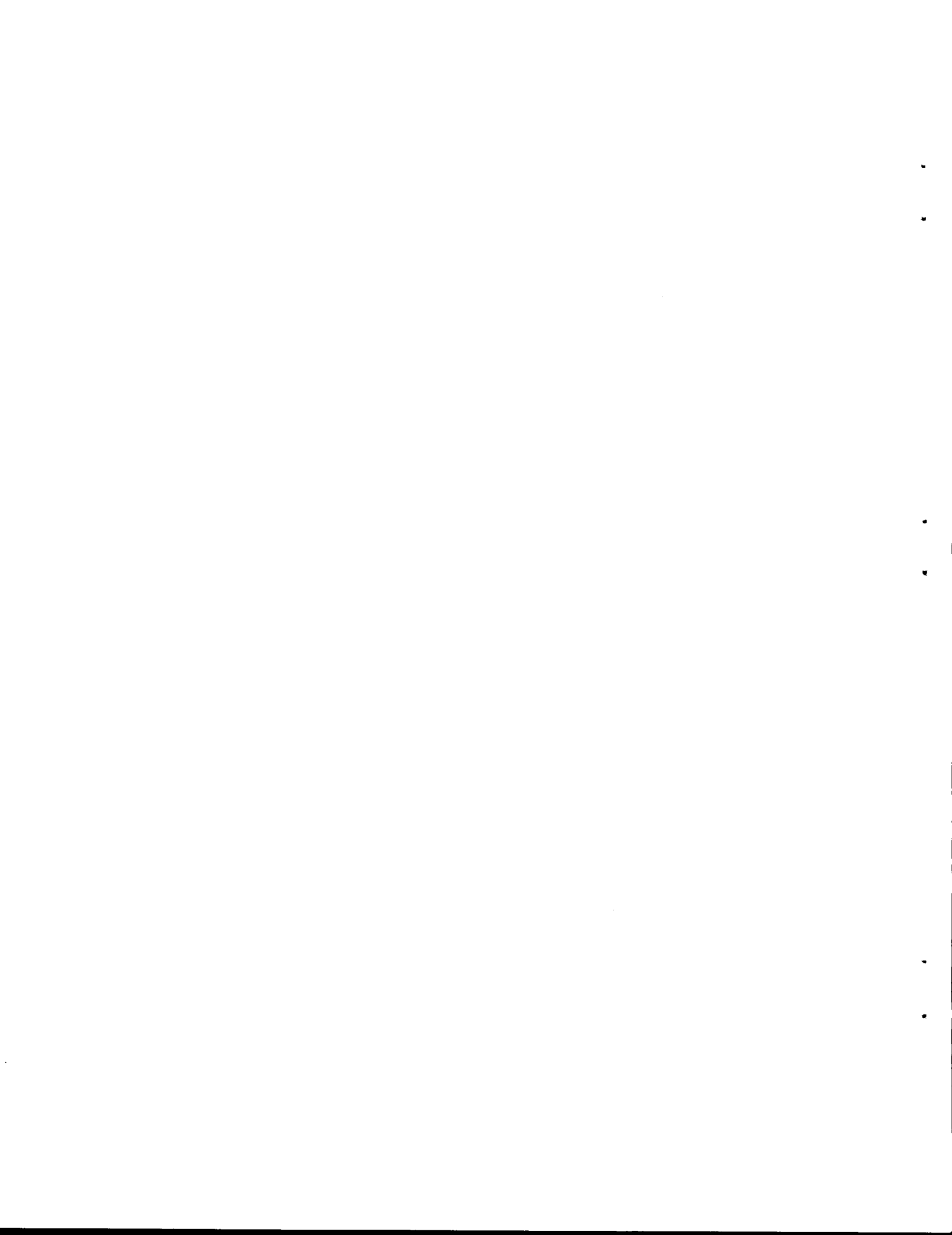
Experiences with Large-Scale Data Handling Systems.....	245
M. R. Mulkey Arnold Engineering Development Center	
Data Handling Systems for Nuclear Power Plants.....	259
R. A. Edwards Knolls Atomic Power Laboratory	

V. SOLID STATE COMPONENT INSTRUMENTS

Magnetic Amplifiers for Reliable Process Control.....	273
H. E. Darling The Foxboro Company	
Transistor Nuclear Instrumentation for Naval Applications.....	297
J. E. Moran Knolls Atomic Power Laboratory	
Magnetic Amplifiers for Nuclear Power Plant Safety Circuitry.....	315
K. H. Kline Atomics International Division	
LIST OF GUEST PARTICIPANTS.....	324

PART I

PROCESS INSTRUMENTATION IN HIGH-PRESSURE ENVIRONMENTS



PRIMARY ELEMENTS FOR BOILING-WATER REACTORS

B. E. Woodward
General Electric Company A.P.E.D.

B. E. WOODWARD: The instrumentation primary elements for a boiling-water reactor power station are similar in many respects to conventional power-station instrumentation. Our instrument design engineers have had a distinct advantage in that they have not been hard pressed by high extremes of pressure or temperature. There are enough unique applications, however, that it should be worth while for us to dwell on them for a few minutes.

Dresden will be a 180-Mw (net) electric plant. Presently operating are the two major forerunners, the EBWR at Argonne, and the VBWR at the Vallecitos Atomic Laboratory, Pleasanton, California.

Figure 1.1 is a cutaway of the steel-sphere enclosure with the reactor in the center. The reactor vessel is about 12 ft in internal diameter and approximately 40 ft in height. Above the reactor vessel, and integrally connected to it, is the steam drum, which is 65 ft in length and about 7 ft in diameter. There are four secondary steam generators in cells in the lower part of the sphere. The steam passes through pipes into the turbine building where it is used. The feed water comes back through the same opening.

Figure 1.2 is a cutaway view of the turbine building adjacent to the sphere. Here are the turbine and the electric generator, the condenser, the primary and secondary feed water pumps, and the feed water heaters; and from here the feed water is piped back to the primary and secondary loops. The control room is in the turbine building, and there is an auxiliary instrument area next to the control room.

Figure 1.3 is a very simplified flow diagram of the system. In the boiling-water reactor the moderator is liquid water and the initial heat transfer occurs in the core. The heat forms steam (voids) in the system, and the water and steam mixture is carried to the steam drum, high above the reactor vessel. Steam and water are separated in the drum, the steam carrying over to the primary admission valves in the turbine. The water is pumped through secondary steam generators, or heat exchangers, and then back to the reactor. The secondary steam formed at this point is

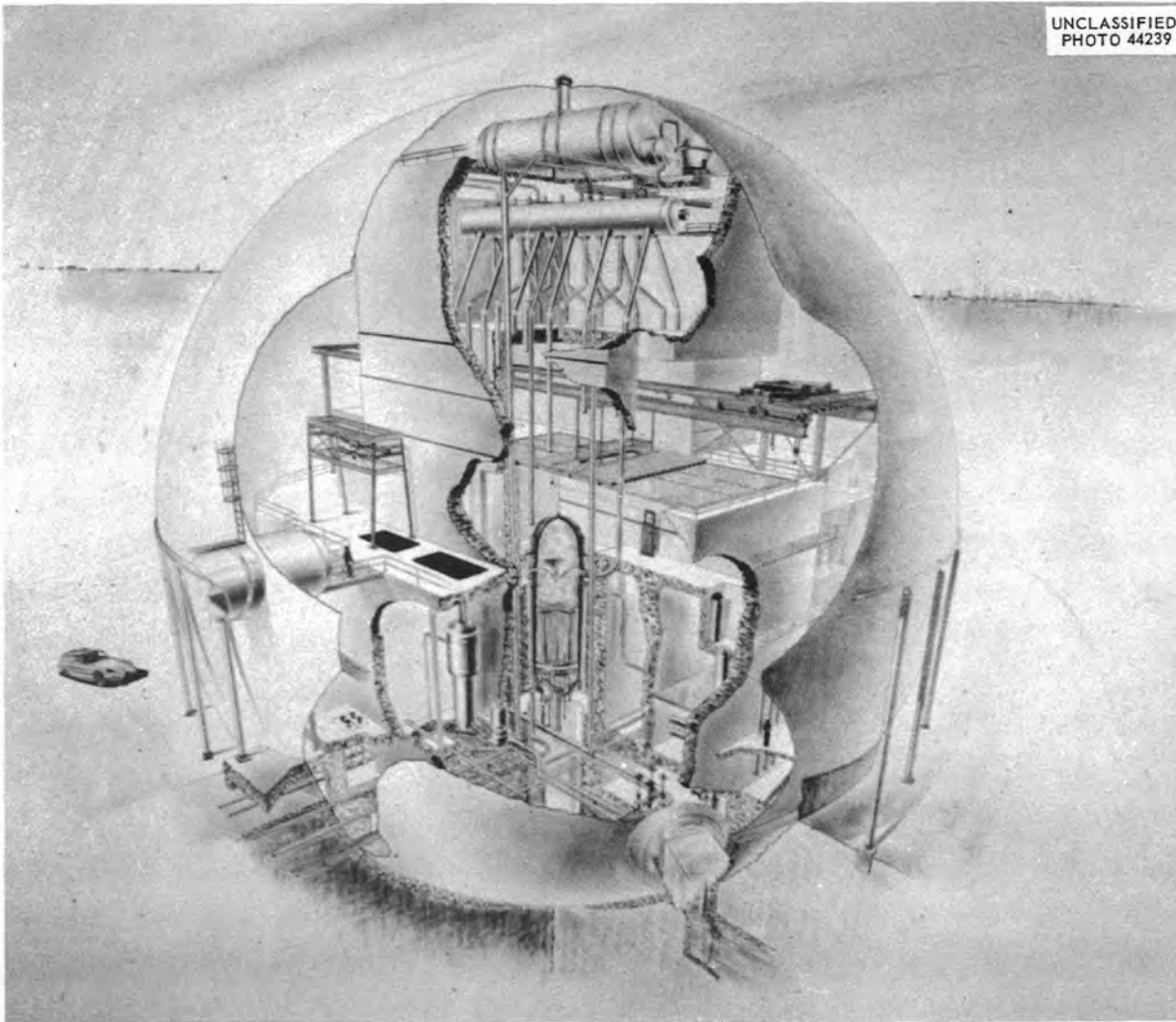


Fig. 1.1. Cutaway Drawing of Reactor Enclosure, Dresden Nuclear Power Station.

admitted by the turbine governor into the secondary admission valves in the turbine. The steam is condensed conventionally and returned to the system by pumps. The primary feed water is demineralized.

The second cycle illustrated here — this further extraction of heat from the condensed water — is the source of the term "dual-cycle reactor," and it is from this feature that we get a form of automatic control of the station. Pressure-regulating valves maintain the steam pressure in the primary loop at 1000 psi. As the hot water is pumped through the heat exchanger, additional heat is extracted from the water, forming secondary steam which is admitted by the governor at a lower pressure into the secondary stages of the turbine. This turbine secondary-stage governor compensates for variations in turbine generator load.

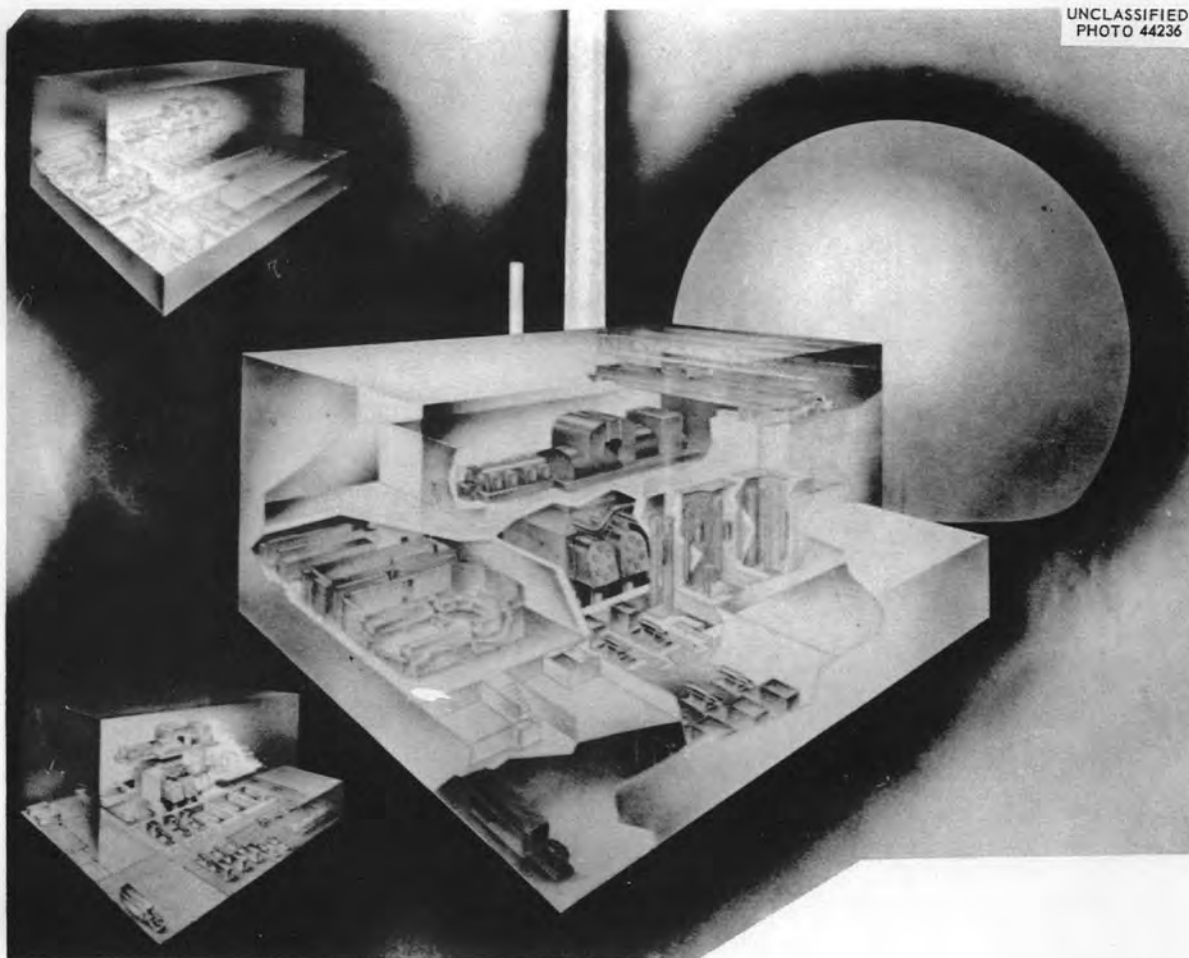


Fig. 1.2. Cutaway Drawing of Turbine Building, Dresden Nuclear Power Station.

At steady state approximately half the steam for the turbine flows from the primary system and about half from the secondary system, and load variations are handled by opening or closing of the turbine governor. If the pressure of the primary system should go higher than the turbine will accept at a given load, the steam can be directly bypassed to the condenser. If the station is operating at steady state and a load increase is required - assume it is an increase in station power - the turbine governor will open further the secondary admission valves allowing additional steam to flow through the secondary system. This will extract additional heat from the water in the secondary heat exchanger and reduce the temperature of water returning to the reactor. Since it is at a lower temperature it remains liquid for a longer passage through the reactor core, and is a more effective moderator for

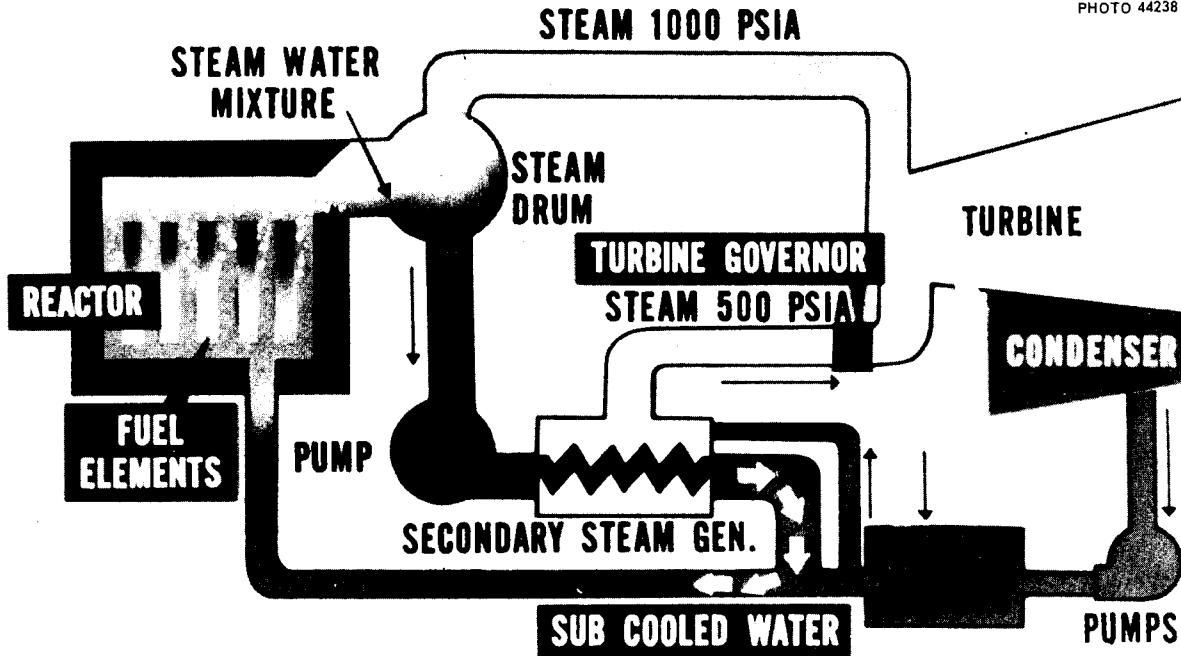


Fig. 1.3. Flow Diagram of General Electric Dual-Cycle Reactor.

the core; there is a consequent increase in nuclear reaction to compensate for the increase in power that was called for by the turbine generator.

A decrease in station load has the opposite effect: the closing of the secondary admission valves extracts less heat from the water; the water is hotter as it returns to the reactor core. Consequently the steam voids are formed earlier in the path through the core, and the nuclear reaction is thereby automatically reduced. We can see, then, that the station should be quite stable.

Since the volume of water flow is very great compared to the total flow of steam in lb/hr in the primary and secondary loops, this is the major flow, and control of the coolant temperature by the secondary-steam-generator effect is the primary means of automatic control in the station.

The control-room philosophy for the large-scale power plant is one of almost all-remote control. Information for all the major process operating decisions will be brought to the control room; and the control room will be accessible and available under all process conditions. It will be shielded from all the process equipment and accessible in the

event of difficulty anywhere else in the process. Portions of the turbine building and of the steel reactor enclosure will be accessible during normal operation.

While most of the operating functions will be initiated in the control room, operators can perform certain functions and operations at local panels or racks in the turbine building, or in cells or instrument rooms inside the sphere. No process line will be connected directly to control room instruments, and electric transmission will be used on all signals between the sphere (enclosure) and the control room. By coincidence, pneumatic transmission predominates from the turbine-building sensing points to the control room.

In general, the transmitters are locally mounted and in habitable areas. Exceptions to this are temperature-sensing elements and certain locally operated level switches.

The non-nuclear instrumentation associated with the reactor includes that for measurement of differential pressure across the core and for measurement of differential pressure between a point just above the core and a point above that where the steam lines leave the reactor vessel. Sensing devices are remotely mounted differential-pressure transmitters that send their electrical signals to the control room. There are two pressure transmitters; one transmits to a conventional round-chart recorder and the other to a digital pressure indicator on a vertical panel in front of the operating console.

Temperature of the reactor vessel at several locations is measured by thermocouples. The temperature of inner and outer vessel walls will be used as a guide in bringing the vessel and the system up to the rated temperature during startup and also in cooling the system in preparation for a shutdown.

The major instruments on the primary steam drum are principally concerned with level control and indication. The three-element level-control system employs a programmed set point based on steam flow, and there are three differential-pressure transmitters connected to the primary steam drum. Each is associated with a temperature-compensating column. The first transmitter is at one end of the drum and its signal is tied into the three-element control system; its signal is also taken to a recorder and to a small vertical-scale indicator on the operating console

in the control room. The second transmitter is at about the center of the steam drum, and its signal goes to a large vertical-scale indicator on a vertical panel behind the console where it is visible to the operator. The third transmitter is identical to the first. It is at a sensing point at the other end of the steam drum and it, too, sends a signal to a recorder - a small instrument in the control room. It can be interchanged with the first transmitter in that it may be used in the control system for level control.

There are also four float-operated level-alarm switches, two at each end, tied into the reactor-protection system. There is a gage glass, with remotely operated shutoff valves, located on one of the water columns at one end of the steam drum; the level in the gage glass will be visible by means of mirrors to an accessible point in the reactor enclosure. It is not visible from the control room.

Each of the four secondary steam generators will have two level transmitters, each connected to its own temperature-compensating column; the electrically transmitted signal from one goes to the three-element control system. This system on the secondary level control has to have a rather wide range of pressure compensation because the pressure in the secondary system may go from a low of 500 lb at low load to as high as 1000 lb when the plant is using very little secondary steam.

The primary elements on all the steam- and water-flow measurements are flow nozzles installed in the pipes. The differential-pressure transmitters are dry-type meters outside the radioactive area and are accessible at all times. The temperature of the water flowing through the secondary heat exchanger is measured at the inlet and outlet by means of resistance thermometers; actually we are measuring differential temperature. We also measure differential pressure of the water flowing through the heat exchanger.

The condensate-handling equipment has the normal complement of controls on the pumps, demineralizers, boiler feed pumps, and feed-water heaters. In most cases operation is from the control room or from local panels in the vicinity of, but isolated from, the process equipment. For example, the feed-water heater level control, which is frequently handled by float-operated or float-type level controllers, will be performed in this case by a combination of differential-pressure transmitters feeding

into a pneumatic controller. The differential-pressure-sensing lines are piped outside the heater cell to a station just outside the shielding walls. However, the high- and low-level alarm functions on the feed-water heaters are performed by float-operated level switches which are mounted right on the heaters.

There are numerous systems of measurement of temperature, pressure, level, conductivity, oxygen concentration, and other conditions. In general, their measurement is similar to existing practice. The philosophy of remote operation and control has prevailed, and in nearly all cases it will be possible to approach and work on this equipment while the station is in operation.

We believe that the design of the Dresden control and instrumentation is in harmony with the present trend. It will be a very completely instrumentated station. However, we are sure that future designs will make possible the elimination of some of the instrumentation that we are furnishing. The devices that we are specifying will lead to operational ease and, we believe, confidence in one of the most modern of our power stations.

L. P. INGLIS: Which instruments do you have in mind eliminating first?

B. E. WOODWARD: The obvious ones to eliminate are those associated with some of the process equipment that may be eliminated; but, in addition, some of the analytical instruments can be considered. We are sure that there will be enough confidence and enough additional knowledge available that some of them can be reduced at least in quantity.

M. A. SCHULTZ: Are any of your instruments in duplicate or triplicate?

B. E. WOODWARD: Yes, in certain cases. In nuclear measurements we have numerous cases of duplication. In the safety-circuit measurements, or the reactor-protection system, the instruments are all duplicated. For example, we have four level-alarm switches tied into the reactor-protection system on the primary steam drum. Similarly there are four pressure-alarm switches tied into the system on the reactor pressure.

As for the process-type instruments, the best example of duplication is on the three-element steam-drum-control system, where we have a second

transmitter that can be exchanged in the control system just by throwing a switch.

S. A. HLUCHAN: What measurements must be made in high-intensity radiation-flux fields; and how about the use of resistance thermometers in these fluxes?

B. E. WOODWARD: The flux level in the case of resistance thermometers, which I cited, is not significant enough to damage the thermometers. We have operated thermometers of this type in flux fields at about this intensity before, for a number of years.

M. A. SCHULTZ: Apparently you have made an attempt to take the transmitters out of the high-flux field.

B. E. WOODWARD: Yes, simply from the standpoint that a man needs to go in and bypass, zero occasionally, or otherwise work on this equipment. Temperature measurement is one example of a case where we have made no effort to make transmitters accessible. As for steam or water temperature, the station can operate without this information, and if an element is faulty it is very doubtful that we will have conditions whereby it will be impossible to operate due to the lack of that instrument.

J. J. STONE: In the control philosophy in going, say, from 10% of power load to full load, how does the ratio of the power obtained by the turbine from the secondary exchangers to the 1000 psi primary source vary?

B. E. WOODWARD: The ratio can vary over a rather wide range. Normally it would be approximately 1:1 for secondary to primary at any one load. The power level range can vary, without control-rod manipulation, from about 40% rated power up to full power simply by increasing the steam flow in the secondary and by the consequent increase in the primary system. At steady state, at any one power level, it will be approximately 1:1; but if the operating personnel are anticipating a load change, such as the power load demand increasing in the morning, they can vary this somewhat by control-rod manipulation.

D. I. COOPER: What fraction of the cost of the total station is instrumentation; and what reduction of this could you envision through dropping duplication?

B. E. WOODWARD: The control and instrumentation cost, exclusive of control-rod drives for the station, is in the vicinity of one million dollars. That is the purchase cost, not installed. The reduction I

would anticipate would be in the vicinity of 20%. This would depend on many things; it would first depend on exact duplication of that power station, which almost certainly would not occur. The next one of that size might well have process complications that would require additional instrumentation not presently involved.

D. I. COOPER: Would you tell me the total cost of the station?

B. E. WOODWARD: I don't have it in mind. It is quite high. However, the selling price was 45 million dollars.

N. HUSTON: How are you able to convince the reactor-operating personnel that they don't need to look at the reactor while they are controlling it? Do you have in mind television? There seems to be a policy, at least among all that I have contacted, to have the control room overlook the reactor room.

B. E. WOODWARD: The philosophy of having the control room overlook the reactor was considered. We do not intend to use television. Actually, there isn't much that can be accomplished by looking at it, just as there isn't a lot they can do by looking at the steam drum level, which is as traditional in industry as the primary gage.

W. E. VANNAH: I have a question which is perhaps a little more general. Apparently you are using quite a few techniques of instrumentation that are taken from other steam-generating plants which are non-nuclear. Looking at it the other way, can you think of examples of techniques of measurement or control developments in nuclear instrumentation that are feeding back into non-nuclear steam generators?

B. E. WOODWARD: Some of the temperature-measuring devices were developed for nuclear measurements. The requirements of the nuclear industry have aided in encouraging the instrument manufacturers to contribute to electric transmission and control. I don't have a good example other than this.

G. F. PARKER: Is the bypass system into the condenser expected to operate very frequently? Also, I am interested in the de-superheater that is utilized so that the steam really condenses; you didn't show that on the chart, but you mentioned it.

B. E. WOODWARD: The bypass system is not anticipated to operate frequently. That is, it just isn't economically sound to be bypassing large quantities of steam; we would anticipate that the bypass condition would

only occur during rather wide fluctuations of power level. There are de-superheaters in the bypass system. I don't happen to have any details of them here.

G. F. PARKER: A question regarding three-element water control: Do you have a program change in that for changes in rated load?

B. E. WOODWARD: The level is anticipated to be programed from steam flow. With increasing steam flow, the set point on the level controller will be raised to a higher level in the drum.

G. F. PARKER: Are you using a straight constant pump-discharge pressure or are you using a differential-pressure control which will reduce the pressure drop across your feed-water valves?

B. E. WOODWARD: We are using a constant pump-discharge pressure.

H. H. STEVENS: You mentioned that all the instrumentation inside the sphere is sending out electrical signals to the instrumentation in the control room. Conversely, if you have a control signal going inside, say, to a valve drive or a positioning device, is that in turn all-electrical?

B. E. WOODWARD: It is electrical from the control room to the sphere, and converted to pneumatic at the valve.

H. H. STEVENS: Are your air compressors for the valves located inside the sphere, or in the main building?

B. E. WOODWARD: They are located in the turbine building, outside the sphere.

H. H. STEVENS: So you are then pumping air from outside in; but you don't consider this a problem?

B. E. WOODWARD: No, it is only one penetration and it is a well protected line.

INSTRUMENT TRANSMITTERS FOR HIGH-PRESSURE AQUEOUS NUCLEAR REACTORS

R. L. Moore

Oak Ridge National Laboratory

The design of primary sensing elements for use in the measurement of process variables in high-pressure aqueous nuclear reactors presents many problems not usually found in the design of primary elements for more conventional high-pressure systems.

Design Considerations

In addition to the usual problems of obtaining structural strength, accuracy, reliability, corrosion resistance, and freedom from temperature and pressure effects, the reactor applications may impose one or more of the following requirements:

- (1) Absolute containment of the process fluid.
- (2) Resistance to radiation damage.
- (3) Amenability to remote adjustment and removal.
- (4) Immunity to damage from immersion in water.
- (5) Drainability.
- (6) Small liquid-holdup volume.

The requirement of containment is common to all reactor high-pressure instrumentation and is the primary consideration in the selection of acceptable types of sensing elements. Since absolute containment over long periods is required, pressure-containing elements must be seal-welded or sealed with closures which present a suitably low probability of leakage. Also allowances must be made for corrosion in specifying the minimum thickness of pressure-containing parts.

Resistance to radiation damage may be provided by the use of shielding or by the elimination of materials susceptible to radiation damage. In general, shielding of field-mounted instruments is difficult and expensive, since such shields are usually large and heavy. A more desirable method is to locate the instruments in a shielded compartment and connect it to the process by small instrument lines. This method is feasible where transmission lags may be tolerated, or where vapor-space connections may be made in such a manner as to assure a low-activity-condensate fill of the instrument lines. It should be noted that an external shield is useless when the instrument contains a high-activity process fluid.

If the instrument cannot be in a shielded location, or is subject to internal contamination with radioactive fluids, then provisions must be made for remote adjustment and removal. Regardless of location, it is desirable that the instrument be drainable. This is particularly true if the instrument contains highly radioactive process fluids. Drainability simplifies the problems of removal and replacement, since the possibility of spills of radioactive material is eliminated, and since the activity of the instrument may be substantially reduced by flushing and acid rinses prior to removal.

In the case of instruments which contain uranium or other fuel solution, it is also desirable that the internal volume be small in order to minimize holdup of costly materials in areas external to the process.

Types of Instrumentation Encountered

Although a wide variety of instrumentation is required in the complete reactor system, the process instrumentation of the primary reactor system is usually limited to that required for measurement of temperature, pressure, differential pressure, flow, liquid level, and weight.

Temperature Measurement

The thermocouple is the most commonly used element for the measurement of reactor temperatures at ORNL. Its simplicity, accuracy, flexibility, reliability, and low susceptibility to radiation effects make it ideal for reactor services. Commercial grade thermocouples with silicone-impregnated Fiberglas insulation have been found to be quite satisfactory for reactor service in areas external to the reactor core. For service in areas having extremely high neutron and gamma flux or for prolonged service in areas of lower activity, the mineral-insulated sheathed thermocouple shows great promise. This type uses an inorganic material such as magnesium oxide or aluminum oxide for insulation, and therefore is practically immune to radiation damage. The insulation is contained by an outer metallic sheath which is swaged to pack the insulation. Since such mineral insulation is hygroscopic, care must be exercised in the use of mineral-insulated couples in order to ensure that the sheath and the end closures are completely waterproof.

In areas where the flow rate approaches the critical corrosion velocity, it is recommended that thermocouples be spot-welded to the outside of

the pipe or vessel, since the installation of thermocouple wells may produce turbulence and result in acceleration of corrosion. Also the installation of thermocouple wells involves at least one reactor-grade weld, and therefore greatly increases the cost of the installation. In stagnant areas, or in areas of low flow, the use of thermocouple wells is permissible.

Where the thermocouple is to be attached to a pipe or vessel wall, the accuracy of measurement may be increased by laying the couple against the surface for several inches from the point of attachment and insulating.

In areas where the couples are subject to immersion in water, the use of Chromel-Alumel wire is recommended, since it is less susceptible to corrosive attack by water than are iron-constantan alloys and the signal is less affected by galvanic emf's.

Pressure Transmitters

Several types of pressure elements suitable for use in reactor service are commercially available. Figure 2.1 illustrates the 0-3000-psi Bourdon tube supplied by the Taylor Instrument Companies and used with a pneumatic pressure transmitter. Specifications provide for an overrange to 3750 psi, an operating temperature of 140°F, a hysteresis of ±15 psi, a sensitivity of 3 psi, and a weld-sealed type 347 stainless steel element. Figure 2.2 shows a pressure transmitter manufactured by the Swartwout Instrument Company. This transmitter utilizes electric transmission and incorporates a secondary pressure housing for containment of fluid in case of rupture of the Bourdon tube. The motion of the Bourdon is directly coupled to a magnetic core. The motion of the core is sensed through the pressure housing by a differential transformer.

Figure 2.3 shows a pressure transmitter, manufactured by the Callery Chemical Company, which may be mounted directly on a high-temperature, high-pressure system. A motion proportional to pressure is developed by range springs located in the upper body. This motion is sensed by a differential transformer. Temperature effects on the range are practically eliminated by the use of cooling fins. The pressure seal is provided by a bellows. A secondary bellows provides protection against the failure of the primary bellows. Bellows leakage may be detected by rise or drop of pressure in the chamber between the two bellows. The pressure-drop method is preferable since it results in leakage into the system.

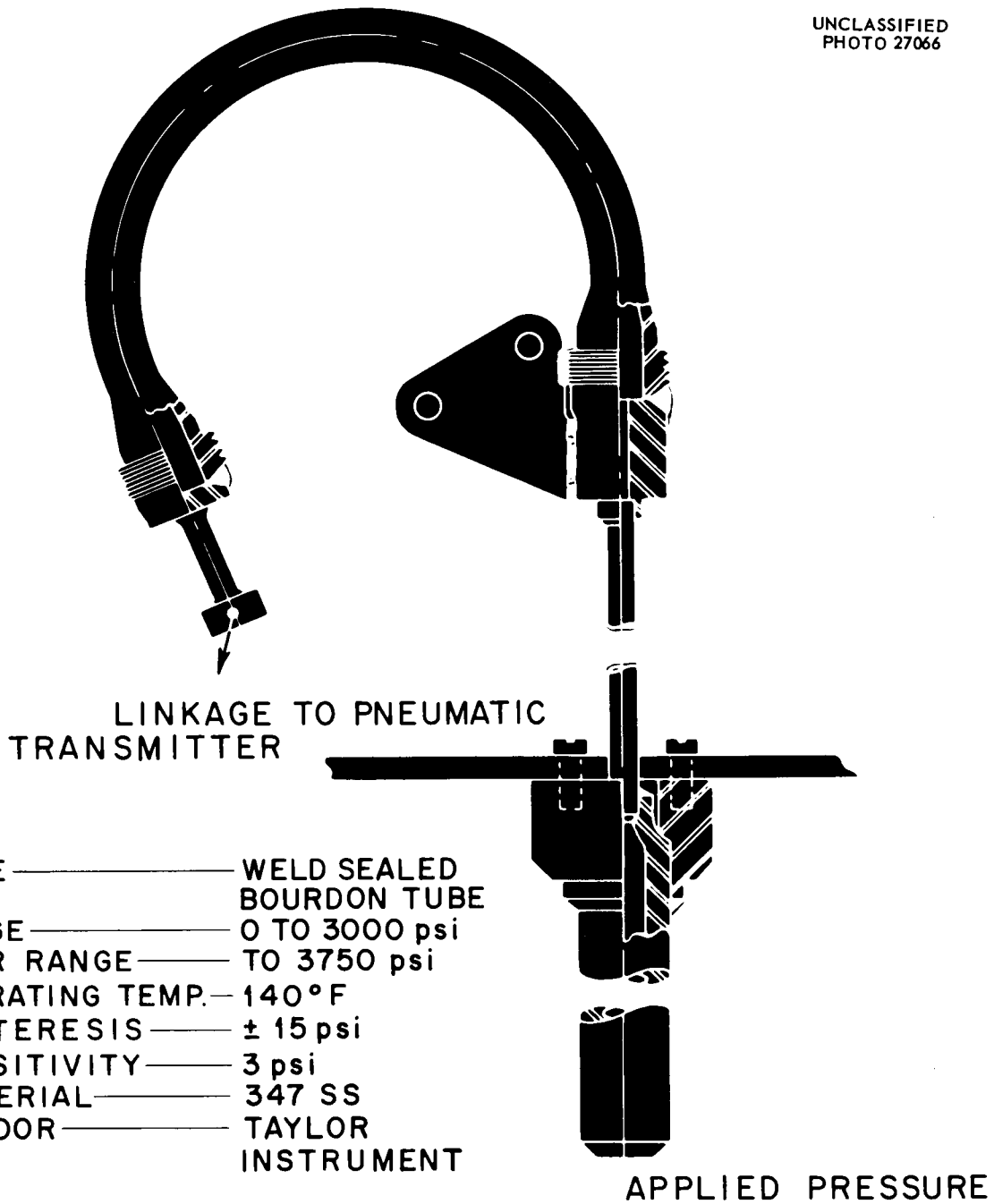


Fig. 2.1. Weld-Sealed Bourdon Pressure-Transmitter Element.

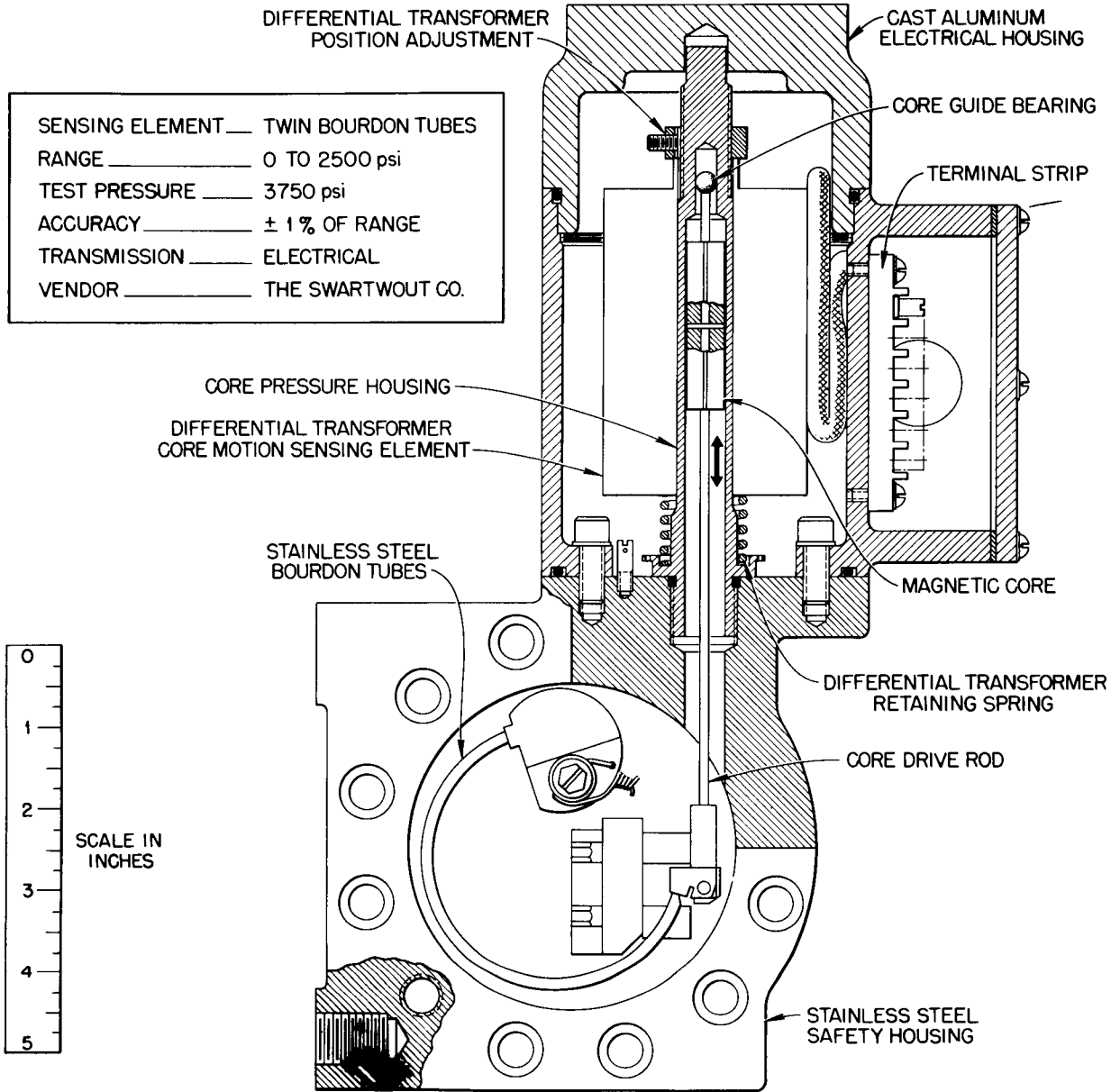


Fig. 2.2. Pressure Transmitter with Safety Housing.

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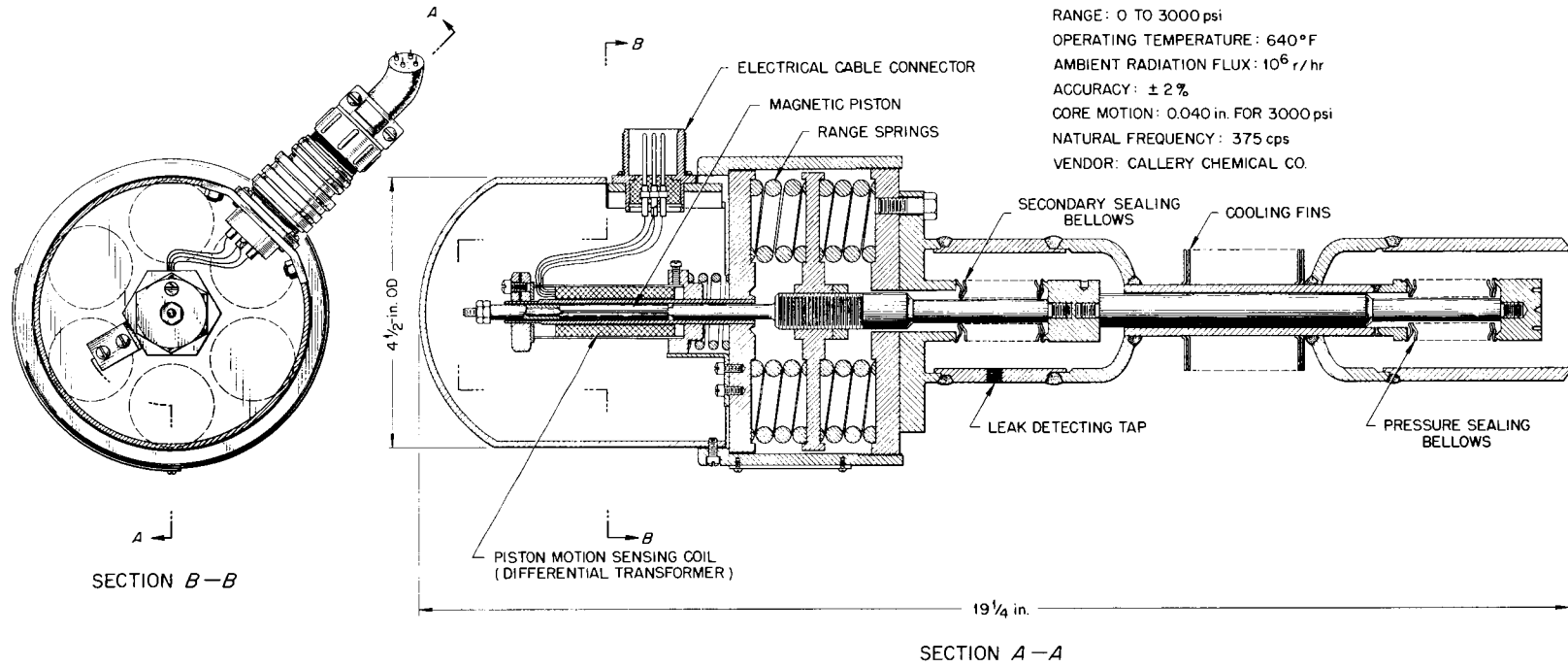


Fig. 2.3. Pressure Transmitter for Direct Mounting at High Temperature.

Figure 2.4 shows a weld-sealed, electric-transmission, absolute-pressure transmitter with an operating range of 0-300 in. of water, which is capable of withstanding overranging to 2000 psi. Overrange protection is provided by the convoluted-diaphragm backup flange, which limits the diaphragm motion. The absolute-pressure range is obtained by evacuation of the back side of the diaphragm. This instrument was designed and manufactured by The Foxboro Instrument Company.

The pressure instrument shown in Fig. 2.5 was supplied by the Taylor Instrument Companies. It has a range of 0-30 psia and may be completely weld-sealed. The instrument will withstand overrange pressures to 500 psi without shift and will contain pressures to 1000 psig without rupture of the body or sealing bellows. The signal output is pneumatic. The sealing bellows serves as a vacuum seal and as a secondary pressure seal. Since it is not normally exposed to high pressure or process fluids, the use of a thin bellows section is not objectionable.

The Norwood Controls pressure transmitter shown in Fig. 2.6 has many features desirable in reactor instrumentation. The twisted Bourdon element is weld-sealed and will withstand high overrange pressure. A secondary seal provides protection against bellows rupture. It is drainable and

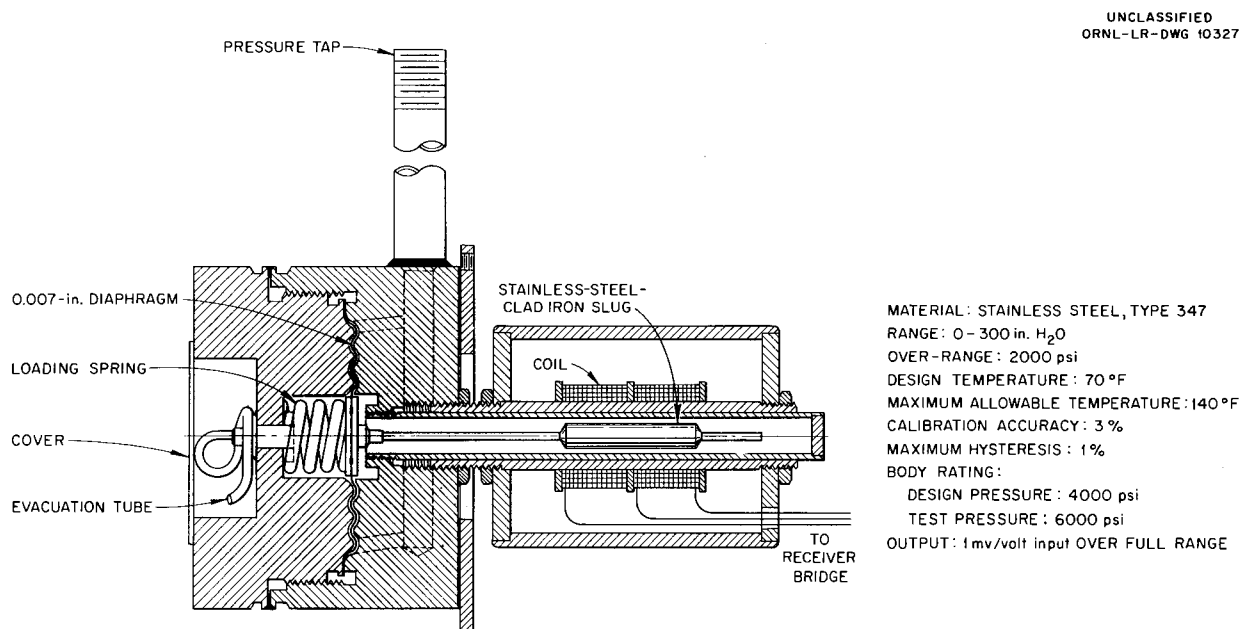
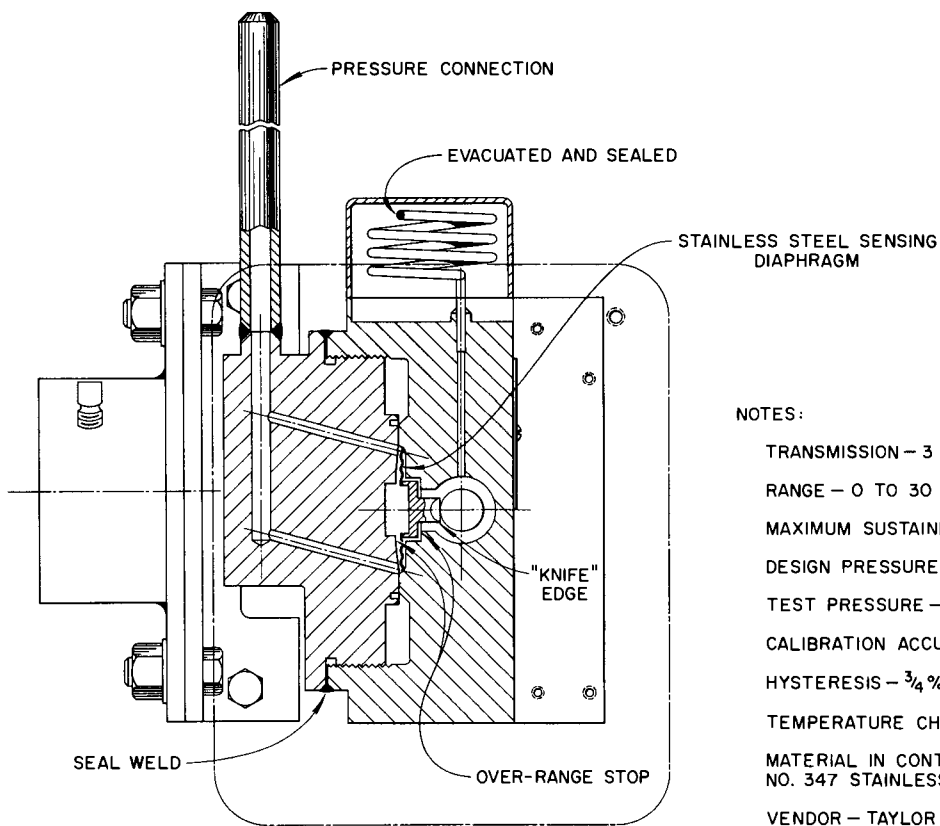
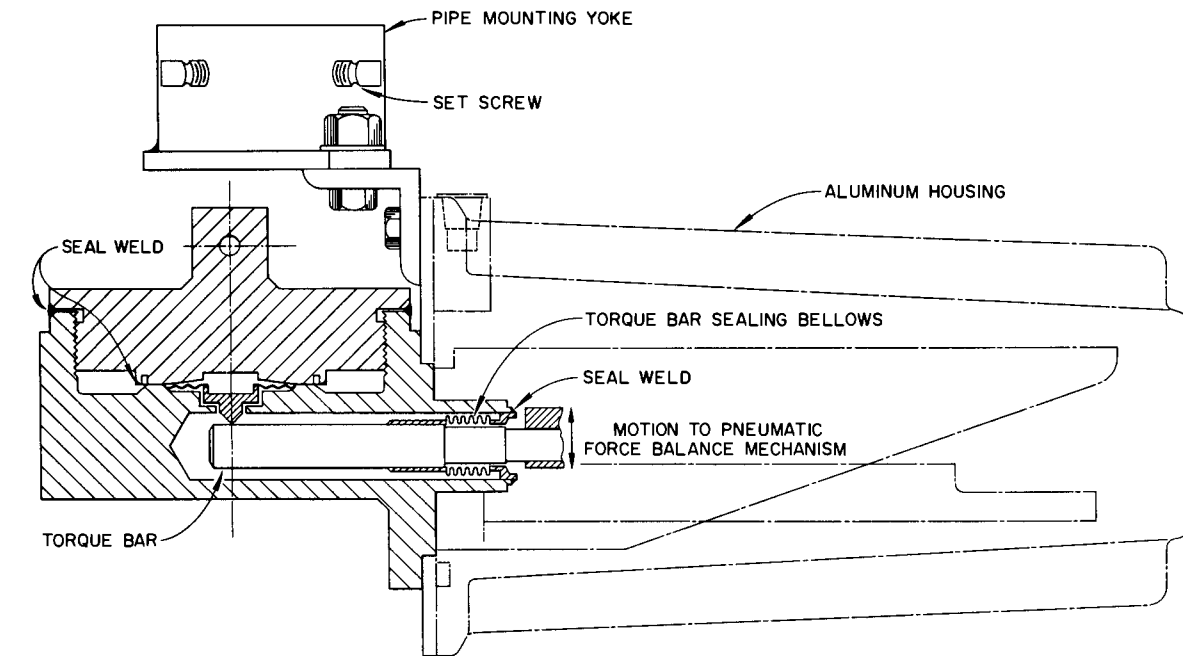


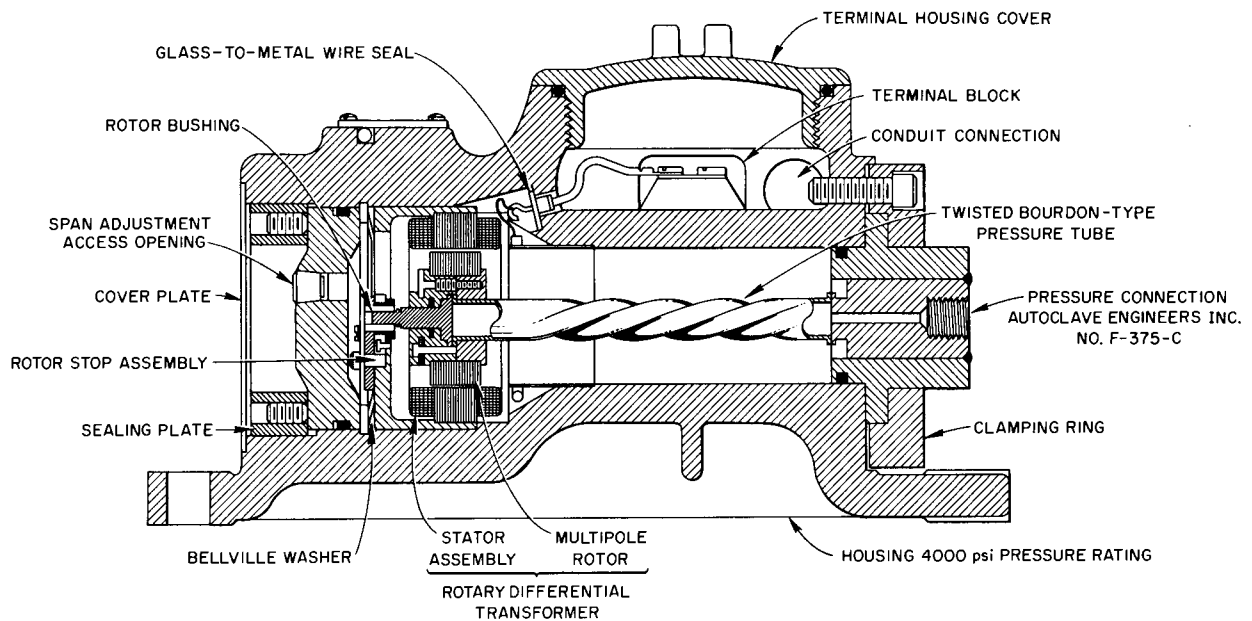
Fig. 2.4. Weld-Sealed, Electric, Absolute-Pressure Transmitter.



NOTES:

- TRANSMISSION - 3 TO 15 psi PNEUMATIC
- RANGE - 0 TO 30 psia
- MAXIMUM SUSTAINED OVER-RANGE - 500 psi
- DESIGN PRESSURE - 500 psi AT 70°F
- TEST PRESSURE - 1000 psi (BEFORE EVACUATION)
- CALIBRATION ACCURACY - 1%
- HYSTERESIS - $\frac{3}{4}$ %
- TEMPERATURE CHANGE ERROR - $\frac{1}{4}$ % PER 60°F
- MATERIAL IN CONTACT WITH PROCESS FLUID - NO. 317 STAINLESS STEEL
- VENDOR - TAYLOR INSTRUMENT COMPANIES

Fig. 2.5. Pneumatic Absolute-Pressure Transmitter.



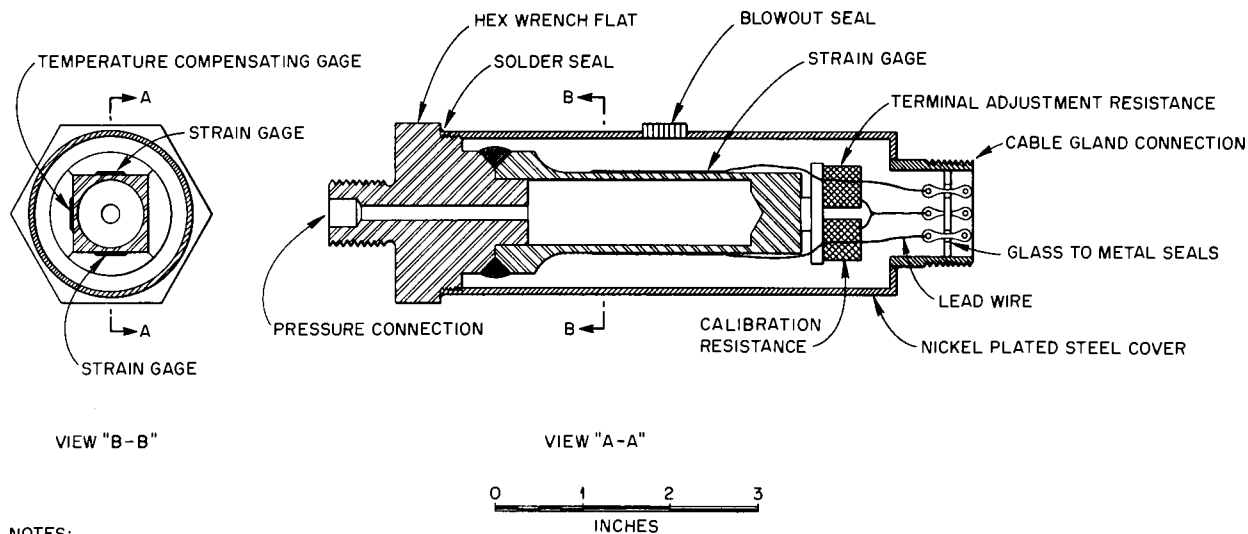
NOTES:
 PROCESS CONTAINING PARTS—347 SS
 RANGE—0—2500 psig
 OUTPUT SIGNAL—0 TO 8.5 volts, 60 cps OPEN CIRCUIT
 ERROR— $\pm 1\%$
 TEST PRESSURE—5000 psig
 PRESSURE RATING WITHOUT RUPTURE—12,500 psig
 MAX.—ENVIRONMENTAL TEMP.—200°F
 VENDOR—NORWOOD CONTROLS DIV

Fig. 2.6. Twisted-Bourdon-Type Pressure Transmitter.

has a low holdup volume; and the housing may be sealed to provide protection against immersion in water. Units using Ni-Span C Bourdon elements have performed satisfactorily in high-pressure water service¹ on the PWR. A transmitter of this type, but with a type 347 stainless steel Bourdon element, has been ordered for test and evaluation at ORNL.

Another type of high-pressure element which may be used is the Baldwin SR-4 strain-gage pressure cell shown in Fig. 2.7. It satisfies the requirements of containment, drainability, and holdup volume, but must be shielded from radiation to prevent damage to the strain-gage bond. Care must be exercised in installation to prevent errors due to moisture absorption in the cable and to prevent temperature differentials across the cell. Unbonded-strain-gage devices show some promise for use in high

¹S. Baron and T. L. R. Williamson, "Instrumentation of a PWR Atomic Power Plant," ISA Journal, vol 5, No. 1, 1958.



NOTES:
 TYPE—"SR-4" RESISTIVE STRAIN GAGE
 RANGE—0 TO 5000 psi
 MATERIAL IN CONTACT WITH PROCESS FLUID—NO. 410 S.S.
 ACCURACY— $\pm \frac{1}{4}$ % OF RANGE
 OUTPUT SIGNAL—1,000 mv/v
 BRIDGE RESISTANCE—120 Ω
 MAX. OPERATING TEMPERATURE—150°F
 OVERPRESSURE TEST—200% OF RANGE
 VENDOR: BALDWIN-LIMA-HAMILTON CORP.

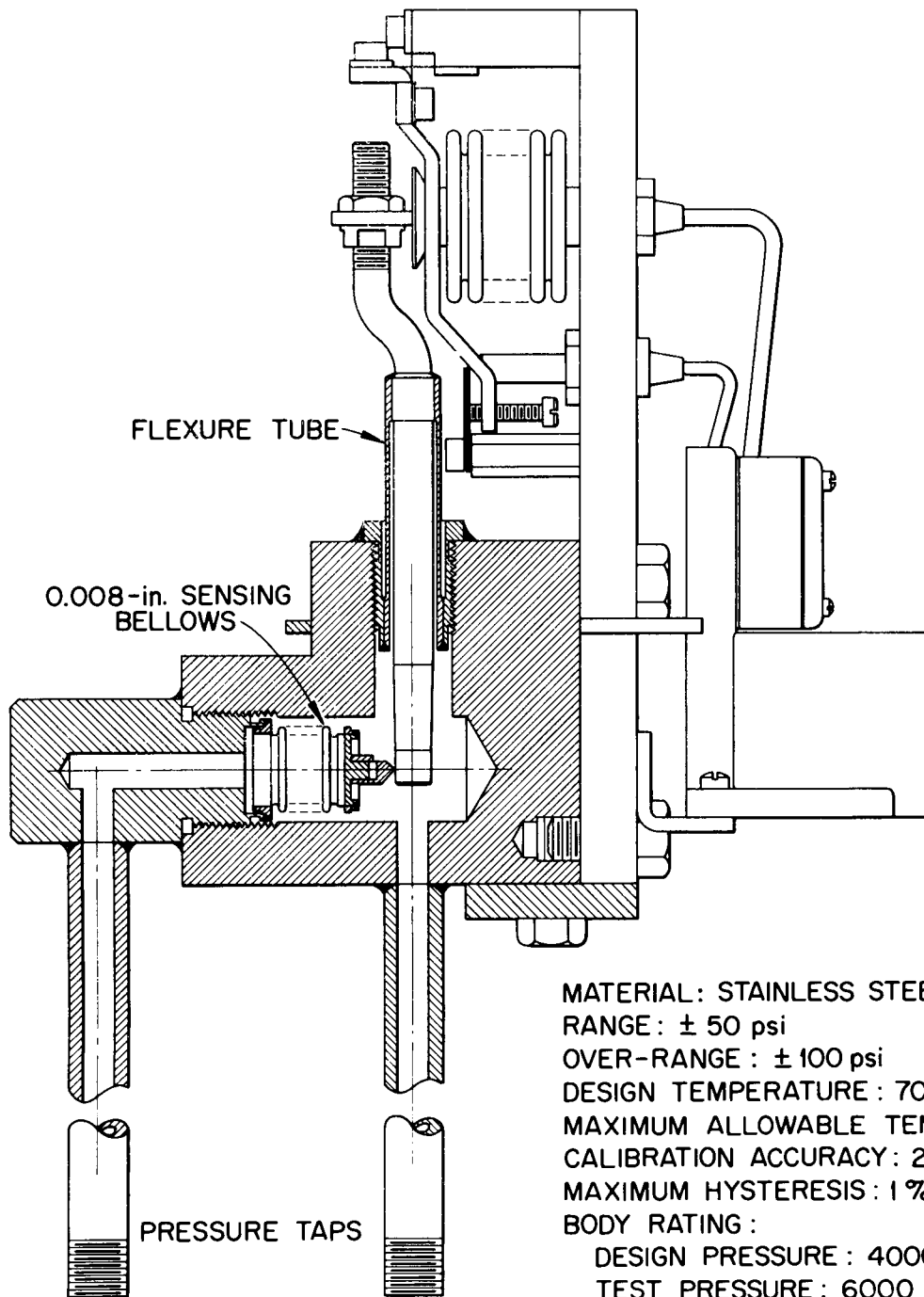
Fig. 2.7. Strain-Gage-Type Pressure Transmitter.

radiation fields. However, more information is needed on the behavior of strain gages in radiation fields.

Differential-Pressure Transmitters

The design of differential-pressure transmitters for reactor service is more difficult than that of gage- or absolute-pressure transmitters since the forces available for producing motion are much lower. Therefore, since thin flexure members present the possibility of rupture, the use of motion or force transmission through pressure-containing flexure members is limited to high-range applications. For low ranges, the use of the variable-inductance or differential-transformer techniques to detect the motion of an iron core inside a pressure-containing tube is preferable.

Figure 2.8 illustrates a pneumatic-transmission differential-pressure cell (3-15-psi output) which uses a bellows sensing element and an all-welded flexure-tube seal. The range of the cell is ± 50 psi with an over-range rating of ± 100 psi. The housing design pressure of the cell is



MATERIAL: STAINLESS STEEL, TYPE 347
RANGE: ± 50 psi
OVER-RANGE: ± 100 psi
DESIGN TEMPERATURE: 70°F
MAXIMUM ALLOWABLE TEMPERATURE: 140°F
CALIBRATION ACCURACY: 2%
MAXIMUM HYSTERESIS: 1%
BODY RATING:
DESIGN PRESSURE: 4000 psi
TEST PRESSURE: 6000 psi
OUTPUT: 3-15 psi AIR

Fig. 2.8. Pneumatic Cell for High Differential-Pressure Range.

4000 psi. The unit is completely weld-sealed and fabricated from type 347 stainless steel.

Figure 2.9 illustrates an electric-signal differential-pressure transmitter which uses a diaphragm or a spring-reinforced diaphragm to cover differential ranges of 50 in. of H₂O or 50 psi, respectively. The static pressure rating of the transmitter is 4000 psi.

Figure 2.10 illustrates a differential-pressure transmitter of a design similar to the above but utilizing a bellows sensing element which can be specified for differentials from 50 to 125 psi. This instrument, as well as the two preceding instruments, was supplied by the Foxboro Instrument Company.

Liquid-Level Transmitters

The continuous measurement of liquid level in vessels at pressure has been successfully done by both displacement and differential-pressure techniques. The displacement transmitters have been used with ranges as

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TYPE—INDUCTIVE BRIDGE FOR 1000 cps EXCITATION
RANGE—50 in. H₂O TO 50 psi BY CHANGE OF RANGE SPRING
OVER-RANGE—TO 2000psi WITHOUT DAMAGE
MAXIMUM TEMPERATURE—140°F
ACCURACY—3%
BODY RATING—4000psi DESIGN, 6000psi TEST
OUTPUT—1mv per volt INPUT FOR FULL RANGE
MATERIAL—TYPE 347 STAINLESS STEEL.

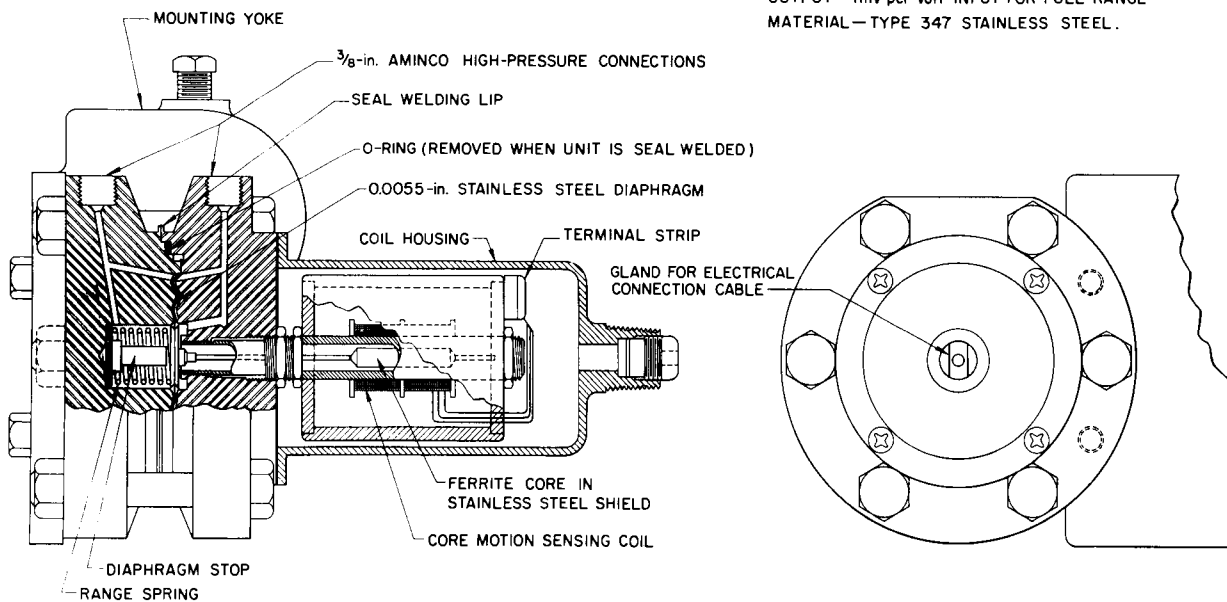


Fig. 2.9. Electric Cell for Low Differential-Pressure Range.

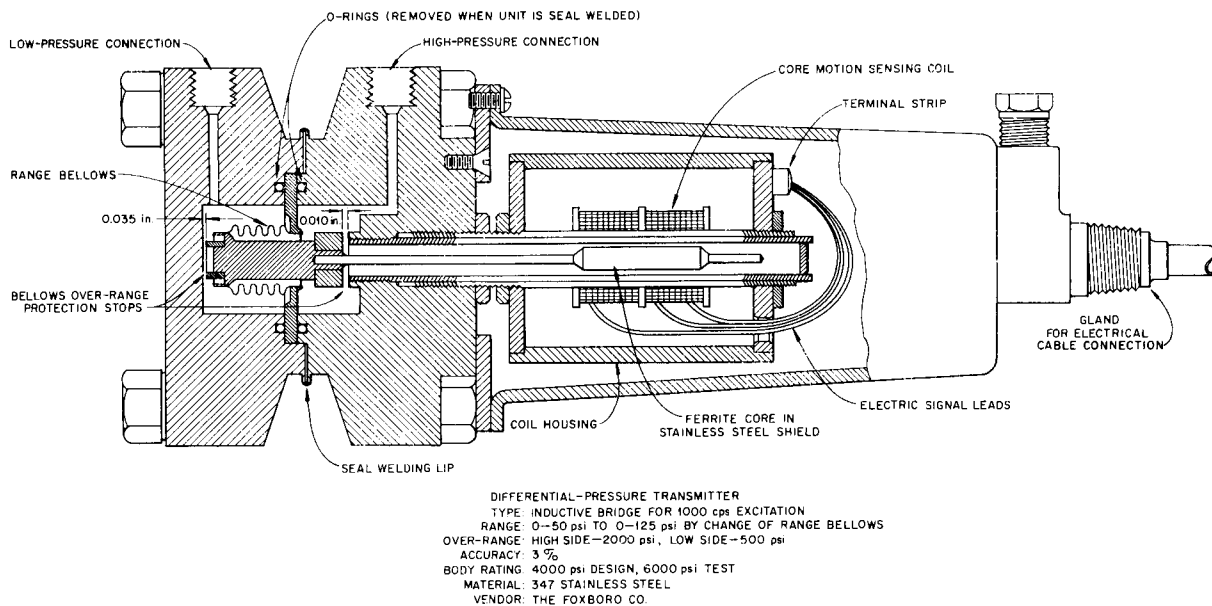


Fig. 2.10. Electric Cell for High Differential-Pressure Range.

low as 0-5 in. of water. The minimum range of the differential-pressure transmitters suitable for high-pressure service in reactor environments is about 25 in. of water. The use of differential-pressure techniques has the disadvantage that, in order to avoid extraneous pressure effects, the differential-pressure cell must be located below the lower level tap, and is, therefore, not drainable.

The ORNL-developed displacement transmitter shown in Fig. 2.11 consists of a 5-in.-long displacer suspended by two helical springs. An extension rod above the springs positions an iron core in the center of a differential transformer. Since the instrument is inherently a spring-mass system, it is subject to oscillation at the natural frequency of the system. In order to prevent these oscillations from reaching an amplitude sufficient to impair the operation of the receiving equipment, eddy-current damping is provided by the action of a permanent-magnet field on a one-turn copper ring. The copper damping ring and the iron core are enclosed in a type 347 stainless steel jacket. All materials in contact with the process fluid are type 347 stainless steel, except for the springs, which are gold plated. The pressure shell is of all-welded construction except for an ASA 2500-psi ring-joint flange. The flange construction was provided to make the instrument amenable to remote replacement. The transmitter was designed to operate at a pressure of 2000 psi and a temperature

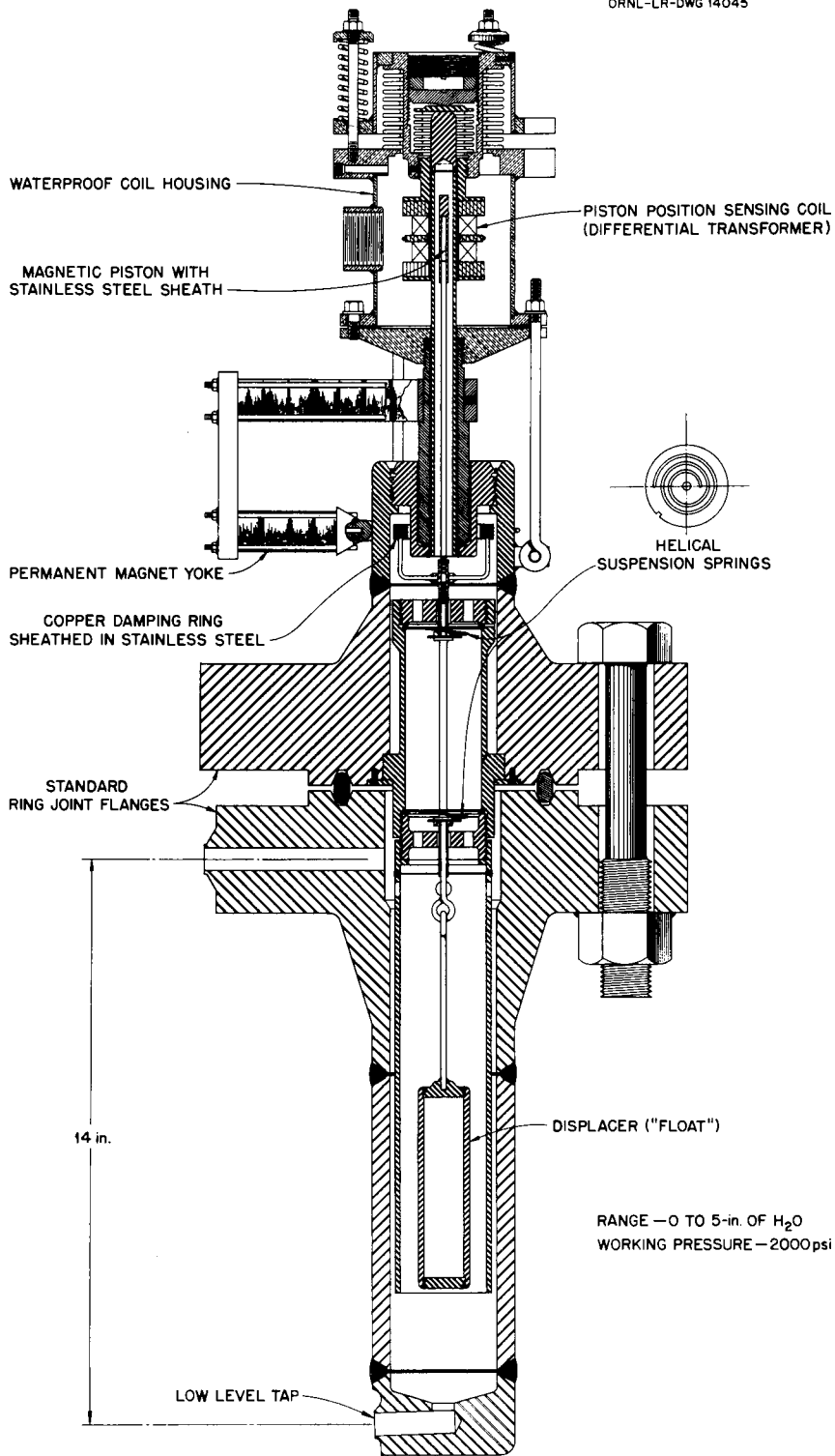


Fig. 2.11. Magnetically Damped Displacement-Type Level Transmitter.

of 300°C. It will withstand a cold hydrostatic test of 3750 psi. The range of the instrument may be varied by changing the float length. The sensing coil is a special ORNL-designed high-temperature differential transformer. Constant current excitation is provided to minimize temperature effects on the coil. The transmitted signal is 0-20 mv/v at 1000 cps.

It is to be noted that neither the displacement transmitters nor the differential-pressure transmitters are compensated for span shifts due to fluid-density changes. This can be done manually or automatically, in most cases, from a measurement of the fluid temperature. The displacement transmitters are also subject to zero shifts because of spring modulus changes with temperature. This effect is avoided in the differential-pressure transmitters by not insulating long lines connected to the transmitters.

A second displacement-type level transmitter, also developed at ORNL, is shown in Fig. 2.12. This unit has a 47-in.-long displacer and is damped by the interaction of two moving vanes attached to the float and a baffle attached to the housing. The rated operating pressure is 500 psi.

Figure 2.13 is an all-weld-sealed float switch of the type used for high- or low-liquid-level alarm or control. The mercury switch is tripped by the action of the moving magnetic piston on a permanent magnet. All parts exposed to the process fluid or its vapor are stainless steel. The device is supplied by Magnetrol, Inc., and is rated for 600-psi service.

Heated thermocouple probes of the type shown in Fig. 2.14 have been satisfactorily used for liquid-level alarm or control. The thermocouple junction is normally held a few degrees above the vapor temperature; as the level of the surrounding liquid rises, the increased heat transfer to the fluid from the probe lowers the thermocouple-signal output.

The capacitance probe illustrated in Fig. 2.15, and manufactured by Fielden Instrument Division, has been recently received by ORNL but has not yet been evaluated. The ceramic-to-metal seal is rated at 2000 psi and 636°F.

Inventory Systems

In the operation of aqueous nuclear reactors, it is often necessary or desirable to obtain accurate, continuous inventories of the liquid

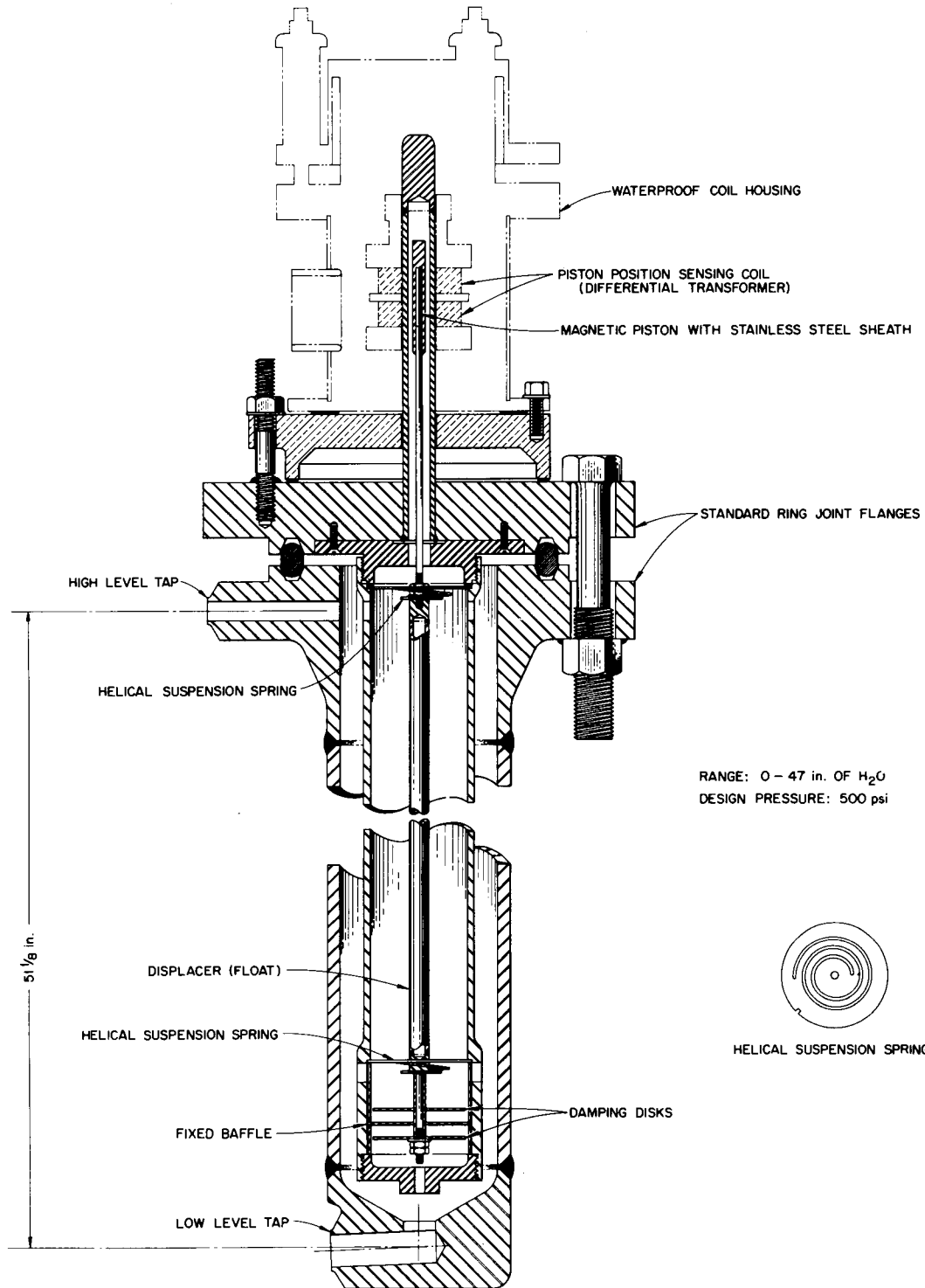


Fig. 2.12. Hydraulically Damped Displacement-Type Level Transmitter.

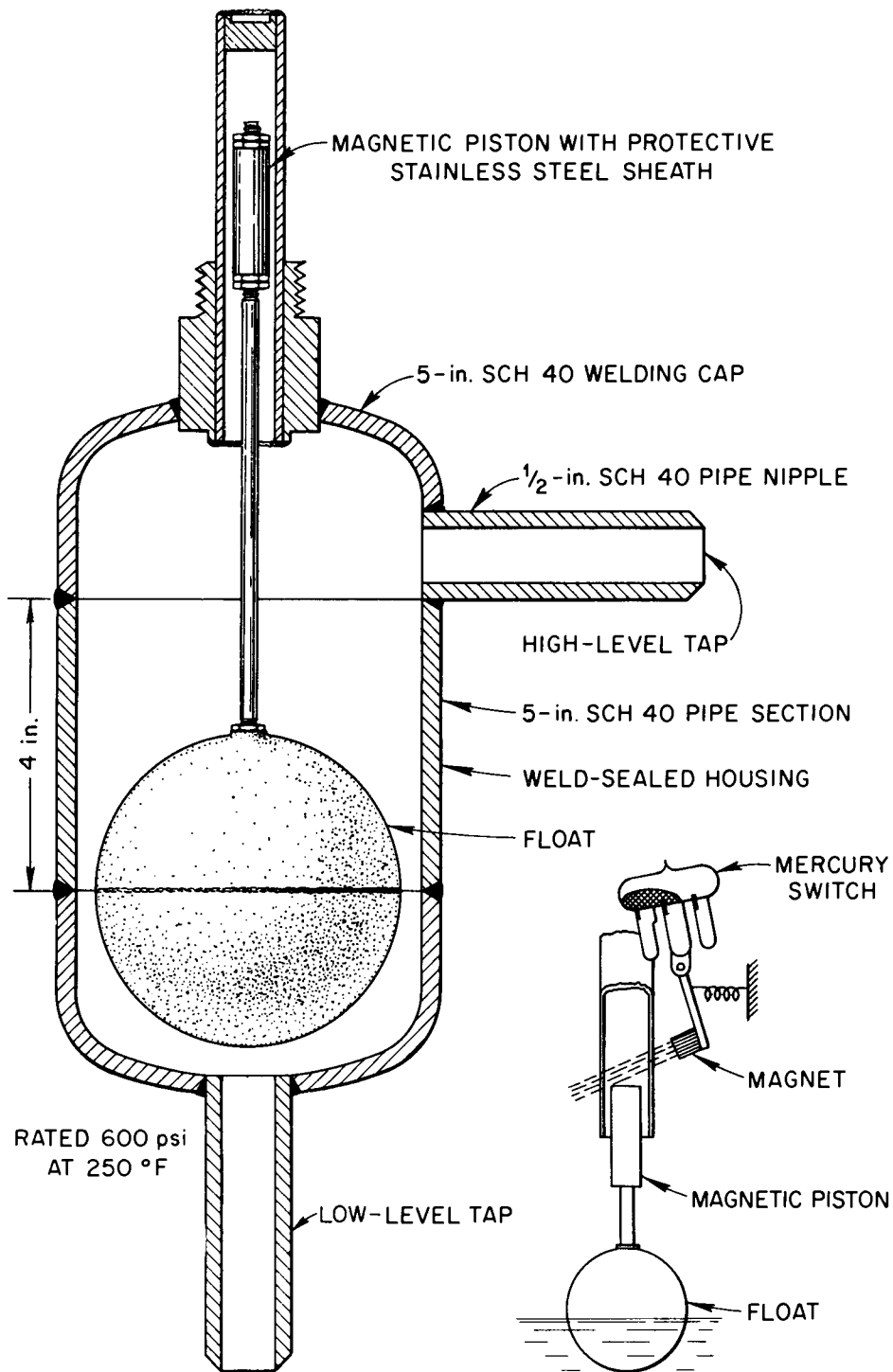


Fig. 2.13. Float Switch for Level Alarm.

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Fig. 2.14. Heated-Thermocouple Level Sensor.

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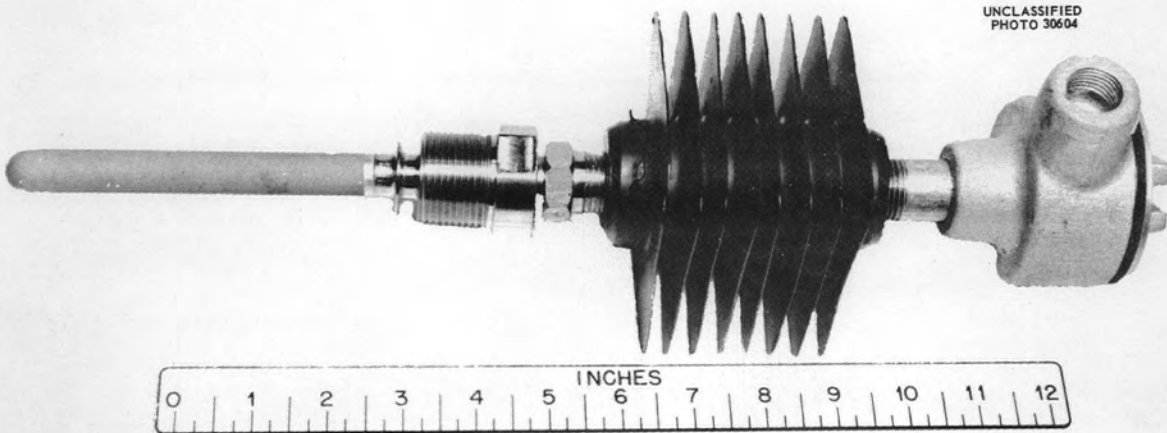


Fig. 2.15. Capacitance Probe for Level Sensing.

within a high-pressure vessel. Such inventories may be made by measuring the level and correcting for density and vessel shape or by weighing the vessel and subtracting the tare weight.

The choice of the system is strongly influenced by the shape and weight of the vessel, by the accuracy and range of measurement required, and by temperature, pressure, and other environmental conditions. In general, level systems lend themselves to high narrow vessels and the weight systems lend themselves to low wide vessels.

Figure 2.16 shows a system used for measurement of dump-tank and condensate inventories in the HRT. Because of criticality considerations, the tanks were of long, small-diameter, cylindrical configuration. The condensate tanks have two 1/2-in. sched-40 pipe connections. Piping connections to the dump tanks consist of four 1/2-in., two 3/4-in., three 1-in., and one 3-in. sched-40 pipes. The system shown is used in the fuel low-pressure system. A similar system is used in the blanket low-pressure system. The dump and condensate tank capacities and tare weights are:

Tank	Capacity (lb)	Tare Wt (lb)
Fuel Condensate	500	500
Fuel Dump	1600	4220
Blanket Condensate	500	500
Blanket Dump	2500	5800

Requested accuracy was ± 10 lb in the presence of system variations of 0-500 psi and 0-100°C, and ambient variations of 7-15 psia and 70-140°F.

After investigating several level-detection systems, it was decided that the only feasible method of obtaining the desired accuracy was by weighing the tanks. The major problem in weighing was the eliminating of the effects of thermal expansion and pressure on the pipe loading. This was accomplished by the use of long pipe runs in horizontal planes and by careful attention to support and anchor points. A suspension system incorporating beams and knife edge was designed so that a given weight, regardless of its location in the tank, would be transmitted to a pneumatic load cell.

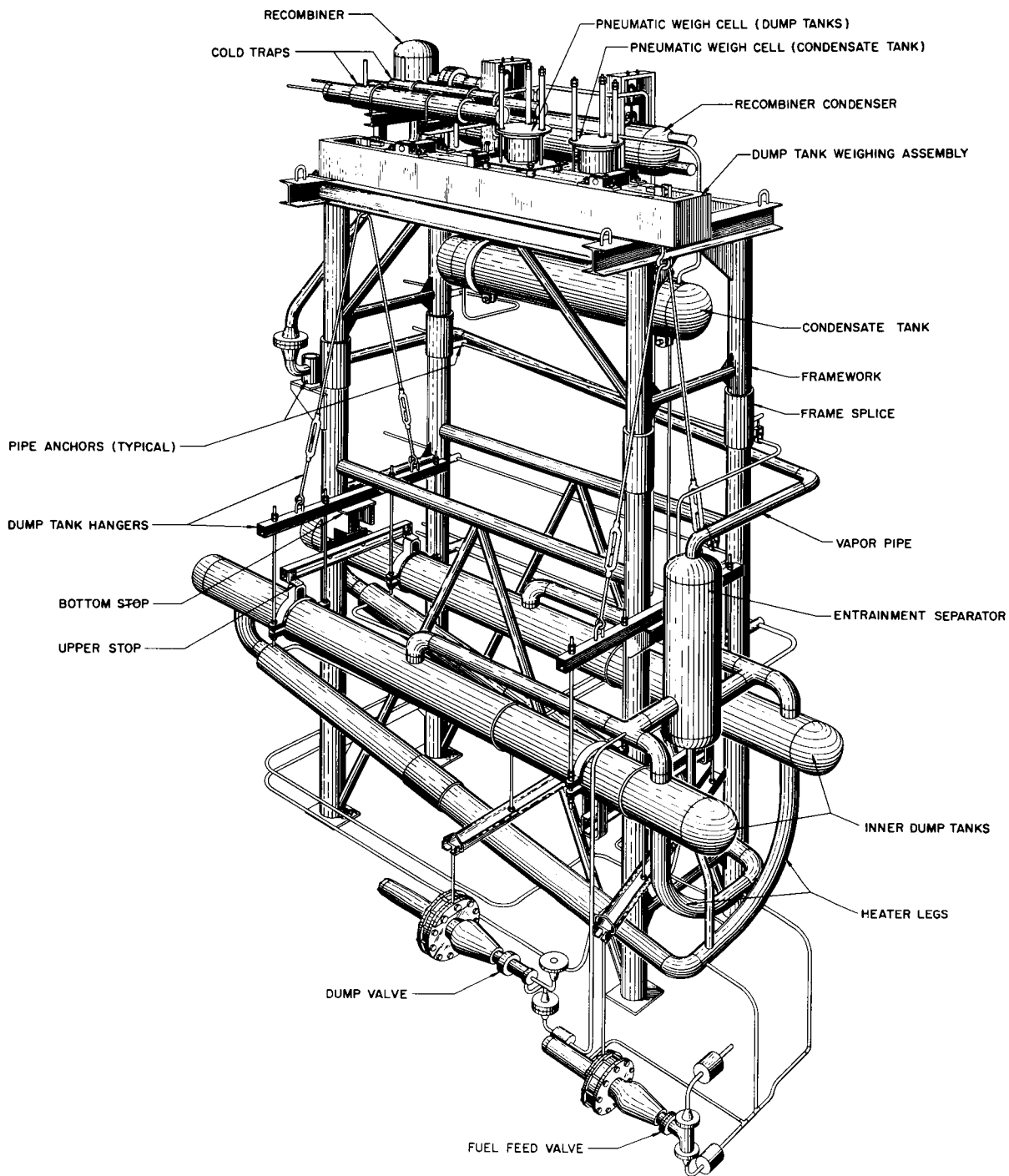


Fig. 2.16. Inventory Measurement System.

The pneumatic load cell, which was supplied by the A. H. Emery Corporation, is shown in Fig. 2.17. This type of load cell was selected for the following reasons:

1. It is of all-metal construction, and is therefore immune to radiation effects or damage.
2. Tare-load suppression may be accomplished remotely by adjustment of a pneumatic regulator.
3. It may be completely submerged in water without subsequent impairment of its operation.
4. The operation utilizes null balance principles, and is therefore free from zero or range shifts.
5. Operation of the cell may be checked remotely.
6. Compensation for ambient-pressure variations may be provided by the simple addition of a reference pressure connection on the tare regulator.

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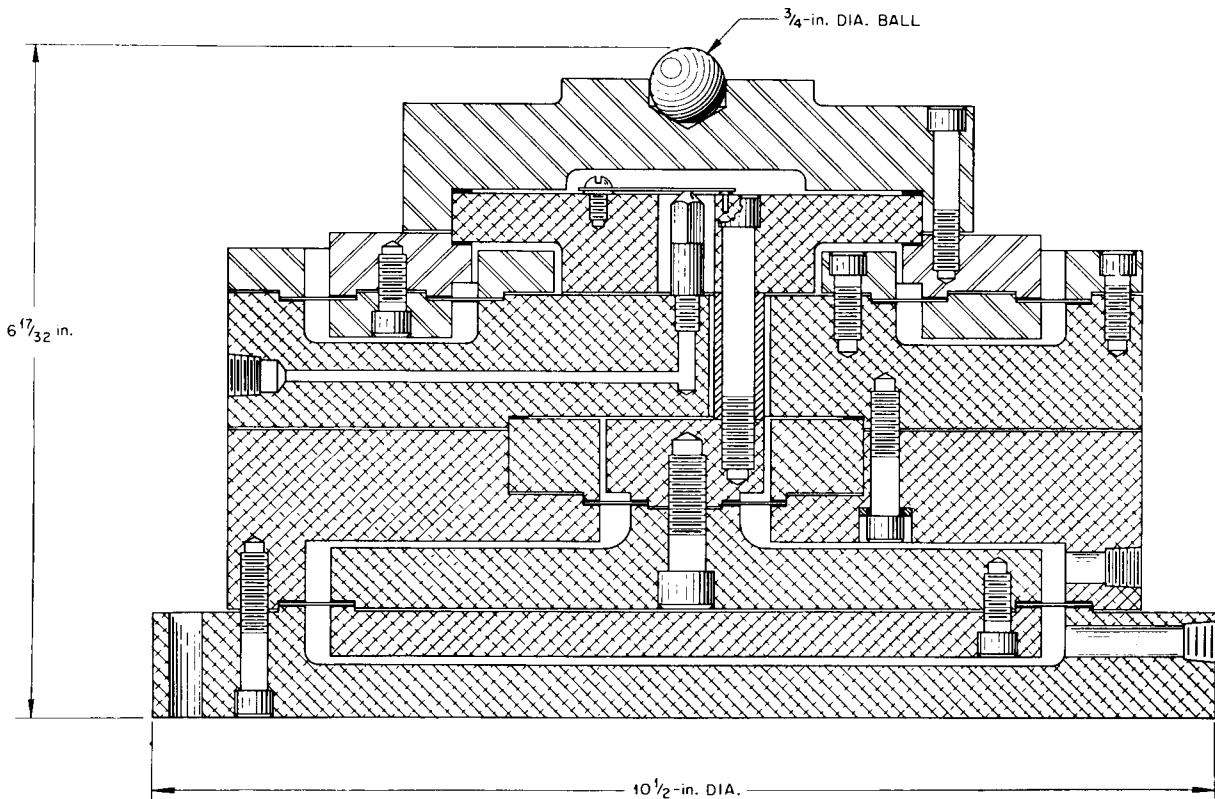


Fig. 2.17. Pneumatic Load Cell.

The load cell system is shown schematically in Fig. 2.18. Air pressure in the weighing chamber is automatically adjusted to counterbalance the applied force minus the force supplied by the tare chamber. This adjustment is accomplished by means of an internal baffle-nozzle and a remote pilot relay.

The load cells are accurate to within $\pm 1/2\%$. The system is accurate to within approximately $\pm 1\%$ under quiescent conditions and to within $\pm 2\%$ under operating conditions.

Flow Measurement

Flows may be measured in high-pressure reactor systems by measurement of differential pressure across orifices or capillary tubes or by use of variable-orifice or heat-balance devices.

Suitable differential-pressure transmitters have been described in a previous section.

A variable-area flow transmitter which may be seal welded is shown in Fig. 2.19.

In the heat-balance method of flow measurement the temperature difference across a heated or cooled section of pipe is measured. This method

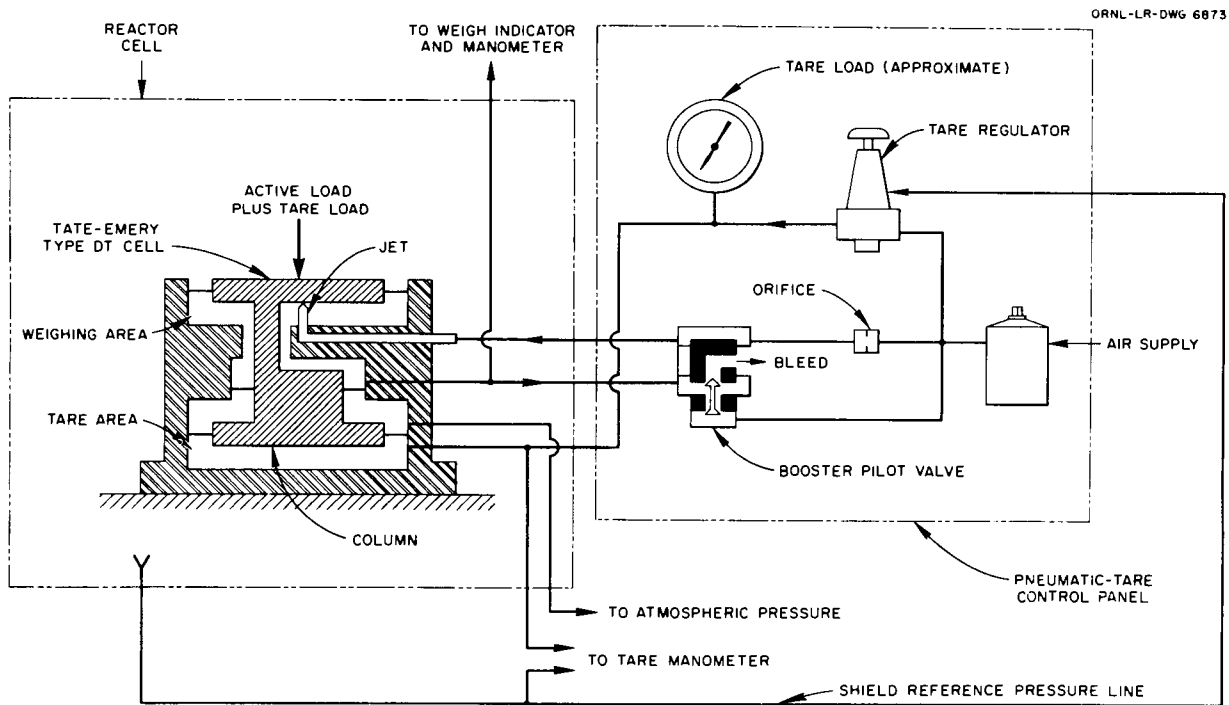


Fig. 2.18. Schematic Diagram of Load-Cell System.

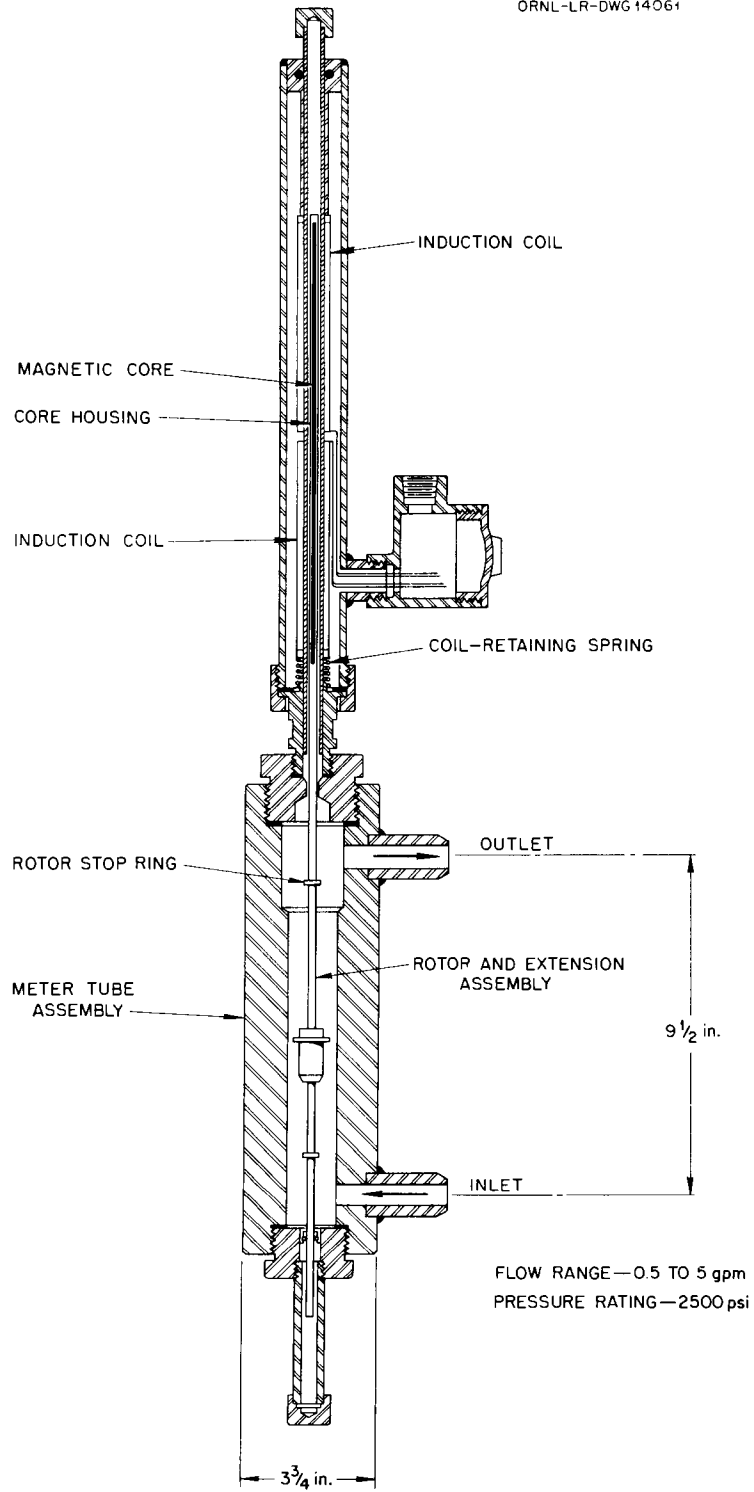


Fig. 2.19. Variable-Area Flow Transmitter.

is valuable for measurement of erratic, pulsing flows and where restrictions in the pipe cannot be tolerated.

Slurry Instrumentation

The devices discussed were designed for high-pressure aqueous-reactor service. High-pressure slurry reactors may be instrumented by using these devices and providing purge streams to prevent plugging and caking. However, in reactor systems the purge water must be obtained from within the reactor; and the equipment necessary to generate and circulate this purge water adds complexity and expense to the reactor system. A development program is under way at ORNL to develop instruments which do not require purging. A flush-diaphragm differential-pressure element is being designed; capacitance, heated-thermocouple and differential-expansion level devices are being evaluated, and the electromagnetic and heat-balance methods of slurry-flow detection are being investigated.

Pneumatic vs Electric Transmission

In a typical reactor system the primary and final control elements are in a radioactive area sealed from the control area by a vapor container and a concrete radiation shield.

Advantages of electric transmission under these conditions include the ease of readjusting system zeros and spans from the control room and the ability to sense motion from weld-sealed transmitters without the use of flexure seals. The speed of information transmission and the ease of switching signals may also be important. The major disadvantage of electrical transmitters is the possibility of radiation damage to the insulating materials.

Advantages of a pneumatic system include the utilization of all-metal radiation-resistant construction for the transmitters. The advantages of the high state of commercial development, low cost, reliability, miniaturization, and ease of paralleling of receiving elements are also considerable. A great disadvantage of the pneumatic system is the tubing transmission line, which affords a possible path out of the radiation enclosure for contaminated fluids or vapors in the event of an explosive rupture of the reactor pressure vessel.

Summary

A wide variety of primary elements suitable for use in high-pressure aqueous nuclear reactors is presently available. These instruments are accurate and reliable, and satisfy the primary requirement of containment. However, many of the instruments described do not have all the features desirable in reactor instrumentation.

The instruments described could be adapted to reactor slurry systems; however, it is desirable to develop new instruments to eliminate the need for purge streams.

Additional information can be found in refs 2 and 3.

A. PEARSON: Have you any information on radiation damage that might occur to the differential transformer?

R. L. MOORE: We have been concerned with damage to the insulation. However, we do have the level instrument, which I described, in operation on the HRT, and we have never noted any effect on the differential transformer, as the reactor power was raised or lowered, which could be attributed to radiation.

J. E. OWENS: Have you used the thermocouple level probe strictly as a one-point alarm device?

R. L. MOORE: I believe we have one assembly that has several probes in it.

H. D. WILLS: We have essentially a one-point system, except that we use many of them. We use discrete steps, and we are working further to make it more analog than digital.

J. E. OWENS: Have you done any work to build an analog device which is heated by heat balance techniques or by spotted-on thermocouples?

H. D. WILLS: Not yet. We plan to do so.

W. P. DiPIETRO: Do you have any difficulty with this device right at the surface of the water?

R. L. MOORE: To my knowledge: no. We are using it for one-point indication. Are you thinking of the difference in heat transfer between the vapor and the water?

²D. S. Toomb, Jr., "Instrumentation and Controls for the Homogeneous Reactor Test," Proceedings of the 1956 Conference of the Instrument Society of America, paper No. 56-13-3; Nucleonics, vol 15, No. 2, 1957.

³R. K. Adams and C. S. Lisser, "Instrumentation of the Chemical Plant for the Oak Ridge Homogeneous Reactor," ISA Journal, vol 4, No. 7, 1957.

W. P. DiPIETRO: Yes.

R. L. MOORE: According to the report we have had, it has been fairly accurate. This might be a problem in an application such as a heat exchanger having a boiling surface instead of a smooth surface.

W. P. DiPIETRO: We have been doing some work with that. I have not been directly working on it, but I understand that it is very good in water or in steam but not at the surface.

S. A. HLUCHAN: What is the external diameter of this thermal probe? This leads to the response time.

R. L. MOORE: About 4/16 or 5/16 in.

S. A. HLUCHAN: What is the response time for a level rise or fall?

H. D. WILLS: We have not investigated it as thoroughly as we would like. We have only been able to calibrate it against a displacement-type level transmitter in the same stream; so we could not tell exactly, but it looked like 10 to 15 sec.

C. GOTTILLA: I understand that at Shippingport they had some drift in the temperature measurement due to the thermocouples. Have you had any experience with that; and can you give us the order of magnitude?

R. L. MOORE: This is a problem which I believe is still under investigation and for which, to my knowledge, there is no definite information available. With the experience that we have had on the homogeneous reactor, we have never had a change in thermocouple calibration which we could definitely attribute to radiation. However, at ORNL work is going on to investigate this with which I am not familiar. I think that possibly some other members of the Applications Group might be able to add something to this.

R. G. AFFEL: We will cover this in the temperature session.

J. N. WILSON: We have had some strange thermocouple effects which may account for part of this. With iron-constantan magnesium-oxide-insulated stainless-steel-sheathed couples inside our reactor at Savannah River, the couples being insulated from the stainless steel sheath, we have found up to 40 v - I repeat, 40 v - between the wires and the sheath under high neutron and gamma flux. This is hard to understand but it has led us to a number of errors in measurement of temperature. Apparently it is tied in with secondary emission of electrons by the magnesium oxide. The voltage that you would expect from that phenomenon is about 40 v. It

decays after shutdown with the half-life of copper. So apparently it is due to betas emitted into the magnesium oxide from the copper. Of course, the current is very small.

R. L. MOORE: This is an ungrounded system?

J. N. WILSON: The sheath is grounded. The couple itself is insulated. I would be interested in hearing if anybody has seen a similar effect.

R. G. AFFEL: The effect of irradiation on insulation has been investigated by Pigg et al.⁴

C. J. BORKOWSKI: What is the flux?

J. N. WILSON: I cannot tell you what the flux is in our reactors, but you can duplicate the effect with an x-ray machine in the laboratory, although the currents produced are very much smaller. It is a real effect and does not depend on extremely high flux.

L. P. INGLIS: What is the reason your couples are not grounded?

J. N. WILSON: We have a common ground. We have many thermocouples. We do not want to ground to the sheath.

⁴J. C. Pigg et al., Trans. Am. Inst. Elec. Engrs. 74, 717 (1955).

OPERATIONAL EXPERIENCES WITH PRIMARY ELEMENTS AND VALVES IN SLURRY SERVICE

E. A. Goldsmith
Westinghouse Atomic Power Division

E. A. GOLDSMITH: The paper¹ that I distributed gives the background up to what we knew last September, when it was written. I didn't feel that there would be time to go over its contents; and I would like today to continue with what we have learned since September in the operation of these loops.

Operating Conditions. - The fluid is basically a thorium oxide slurry with roughly 300 to 350 g of ThO₂ per kg of H₂O. It contains perhaps 8 to 10 g of uranium oxide per kg of H₂O; and often has simulated fission products added, plus anything that the chemical people can dream up at the time. As you may know, this can vary quite a bit. We never really know from one run to another what is in the slurry or what kind of a slurry it is. But the general problem remains the same: abrasion by very hard particles of the order of 7 on the Mohs scale; temperature and pressure conditions of around 600°F and 2000 psi; the normal problems associated with the properties of slurry, such as plugging and settling.

Capacitance Probes. - In the paper that I passed out¹ we described our capacitance-probe problems at 630°F and 1800-2000 psi. Since that was written we have added to this experience. At that time three capacitance probes were in operation. None had been operated over 3000 hr.

In two of the loops, where we had made a 1500-hr run and the probes had functioned very well, the operator pumped in some cleaning solution by mistake. This was a water solution of trisodium phosphate and All detergent; the results of this are shown in Fig. 3.1. At the base of the stainless steel probe is all that is left of the alumina protective tube. This was rather a shocking experience, particularly since in later testing work we were not able to show any possibility of alumina dissolving like this. We tried to reproduce conditions in autoclaves, and we were able to get a fairly high corrosion rate but nothing of this order. Though we hope that in the plant nobody will pump in such a solution, it must be

¹E. A. Goldsmith and W. W. Wentzel, "Instrumentation Experience with 2000 psi, 600°F Slurry Test Loops for PAR Homogeneous Reactor Project," 1958 Nuclear Congress, Chicago, Ill.

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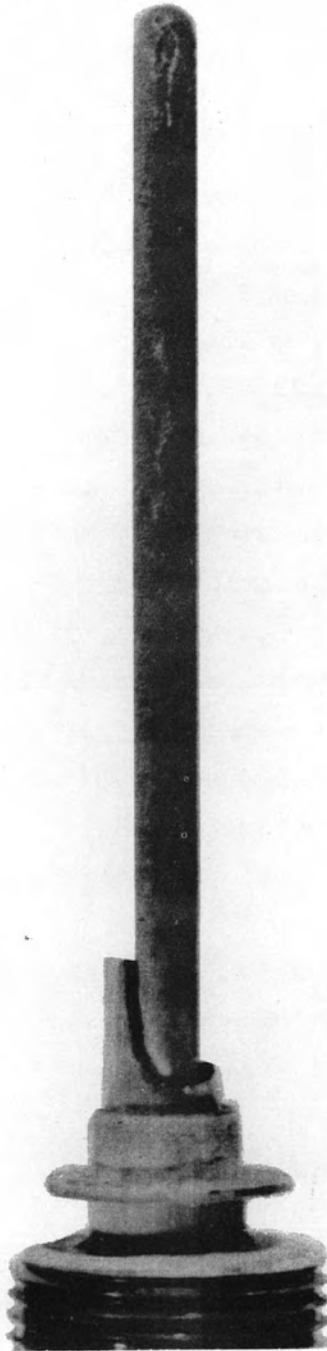


Fig. 3.1. Capacitance Probe. Alumina protective tube is almost completely eroded away.

kept in mind that things like this are possible. We would feel much better if we had reproduced the results so we would know just what caused it.

A lot of work has been done by the manufacturer and by the Bettis plant on alumina corrosion in water. We have done some, too; and alumina, which is a very pure aluminum oxide, shows good corrosion resistance in 600°F water up to a pH of 8 or 9. Normally we operate at a pH of around 6-1/2 to 7-1/2, and we don't expect any high rates of attack. However, further corrosion data on alumina is necessary before we proceed very far.

The third probe in operation also failed, in a slightly different way. Figure 3.2 shows a crystalline break right at, or close to, a seal between the alumina tube and the stainless steel plug that goes into the pressure wall. We suspect from the appearance of the fracture that perhaps a slight initial crack was extended by thermal stress from the type of brazing used in attaching the metal flange. The darker color on the outside is the characteristic yellowish color of our slurry, and is a sure sign of leakage when found at the surface of a cracked pipe. We suspected the alumina had been cracked for quite a while and that, much as in metal, it had then progressed until sudden fracture occurred. This probe had operated about 3000 hr successfully. There had not been any leakage all the way through, because it still retained its full electrical resistance.

The probes have been replaced now with three new ones. We are in the process of getting them into the loop again. The manufacturer has a better method of brazing, in which the temperature is held even, then reduced slowly; and the techniques of brazing are improved enough, we think, to eliminate this type of problem.

We are not sure, in the final analysis, that we want to use a probe like this, particularly in the high pressure regions. However, we do like the capacitance probe. We think that it has a good future because it is independent of density, and when you are working with a slurry this is quite important. So we are still quite hopeful about the development of a capacitance probe for the plant.

10 in. Venturi. - The next loop that we built is a large one with 10-in. pipe and a 4000-gpm normal-flow design. It is quite a large installation. The loop is about 40 ft long and the pressurizer is 24-in. pipe about 20 ft high. It was primarily built for component and system



Fig. 3.2. Fractured Surface of Alumina Tube.

testing, whereas the loops described in the original paper¹ are corrosion test loops. In this large loop we continued, on the basis of past experience, with the same general type of instrumentation. One thing we used was a cast venturi with a much higher throat velocity than we had used before. At 4000 gpm, velocity in the throat is 45 fps and can cause serious erosion-corrosion if the flow is turbulent. We recently disassembled the loop, and the throat has not suffered, as far as we could

measure, any wear. In fact, as we had found before, there was an oxide film of approximately 0.001 in., which had, if anything, reduced the diameter of the throat. So we continue to feel good about the success of the use of venturis in measuring these slurry flows.

Plugging of Pressure Taps. — Blocking of the vertical pressure-tap lines had been considered highly improbable, but we never had a chance to look into the smaller pipelines. When we disassembled the above loop, we were able to see into the vertical lines from the 10-in. pipe, and found partial blocking of the lines. In the smaller loops this may have been occurring also, but some of them have operated for over two years, and have never plugged the lines enough to prevent the transmission of signals.

Figure 3.3a shows the appearance of the buildup seen from the 10-in. pipe. With flow, as shown, a glob of slurry had stuck and perhaps blocked

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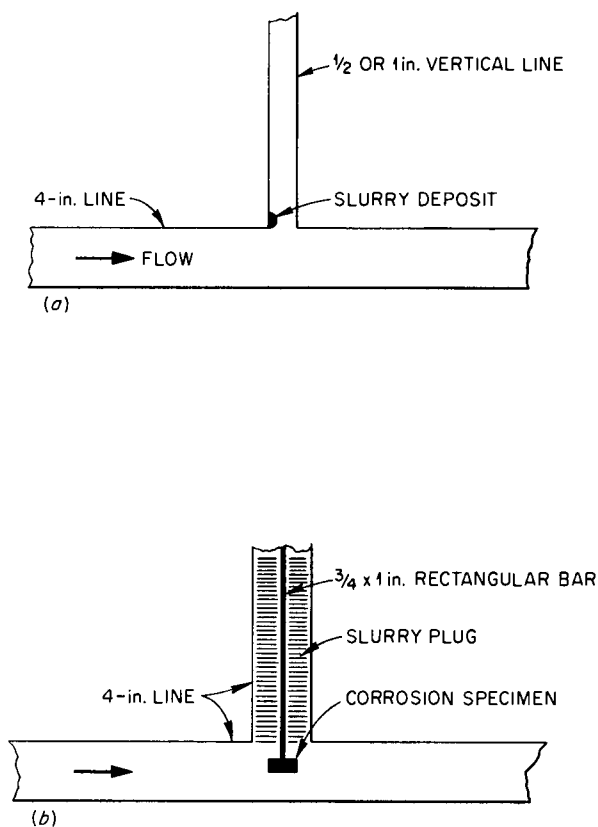


Fig. 3.3. Plugging of Vertical Pressure-Tap Lines.

30% of the cross section. Whether this in time would continue to grow I don't know. The longest we have operated any lines, as I say, is about two years.

This is the first indication that we have been able to find that there is at least a semi-buildup of slurry in a vertical pressure line. We use the vertical line a lot because of our belief that it will not plug as easily as a side or bottom connection. Flush water of the order of 2 gph flowed through some of these lines. This produces a very low velocity in a 1/2- or 1-in. line. It does keep the net flow down, which discourages the settling. I could not detect any difference between flushed and non-flushed lines.

One other plugging occurred in one of a pair of 4-in. vertical lines - this is quite a large diameter and we certainly did not expect the lines to fill up with slurry. Some corrosion specimens hung into the stream. When we opened this we found it solidly filled with slurry, as shown in Fig. 3.3b. We had another 4-in. connection with nothing in it, so luckily we were able to make a comparison. It had no slurry accumulation. I can conclude only that the specimens hanging down into the steam either threw slurry up into the line, or gave the slurry something on which to settle and build. It gave us something to think about. Apparently slurry will build up on certain types of obstructions.

Thermowells. - This loop extended our experience with thermowells to a higher velocity. Loop velocity in the pipe was 24 fps, which is about 4 fps more than we expect to use in the final plant. Again the thermowells (described in the paper¹) show no noticeable wear; so we feel now that the thermowells are as good as the pipe at 20 fps in the final plant. In fact, the thermowells actually had a very slight coating of slurry after shutdown. Apparently there is a stagnant region formed by the flow around the thermowells.

We are planning, for future Loop-D runs, a fairly extensive test station for instruments and valves, and we hope to get some good testing experience there - something we have not been able to do before except as a by-product of the normal loop operations.

Layout Philosophy for Instrumentation. - In a homogeneous plant probably one of the greatest problems is maintenance and layout for remote maintenance. Our first attempt, in the layout we are making now, is to

try to bring all transmitters out into a separate cell where access (at least semi-direct maintenance, and we would hope direct maintenance on the transmitters themselves) can be had. We do not feel at the moment that instruments are so good that they can be put inside the cell without concern for future maintenance. When this day comes we will be very happy to do so. The present philosophy also gives us a chance to operate purge lines from the outside.

There are other things that can be done, such as duplicating or triplicating instruments and just leaving them inside after they fail.

The layout is important. Lines have to be accessible. We are particularly vulnerable here because we have a slurry that will settle, and we have gases in the flowing stream which will tend to rise. I think that at the moment we will be willing to accept certain inaccuracies which could be eliminated by putting the transmitters close to the pipelines as we have done in the loops.

Valves. - Figure 3.4 shows a plug from a standard 3-in. Y-pattern valve operated in one of the corrosion-test loops which was used for controlling flow in place of the orifices described in the paper.¹ It was operating in 580°F slurry with a through-flow of about 100 gpm and about a 40-psi drop across the seat. You can see that the results were quite disastrous to the valve. Stellite-6 trim has been quite badly chewed. In test the valve passed 40 gpm tight-shut after this had occurred; though it was not meant to be a tight-shut valve, this gives an idea of the extent of the damage. Figure 3.5 shows the seat.

In Loop D the necessity of continuous flow control precluded orifices. Therefore, even though this 3-in.-valve wear had started, we put a valve in. We chose a butterfly valve since we thought perhaps it would not be quite as badly chewed up. The pressure drops, and therefore the velocities, are higher. The valve must pass about 4000 gpm with a 60-psi drop and smaller flows with greater drops. We just shut the loop down, after 1500 hr of slurry run.

This butterfly valve is seen from upstream in Fig. 3.6. The greater damage was at the lower portion, even though the larger flow probably went through the top of the gap. The disc is horizontal here, but in service it was tipped so that the part visible in the photograph rotated down and

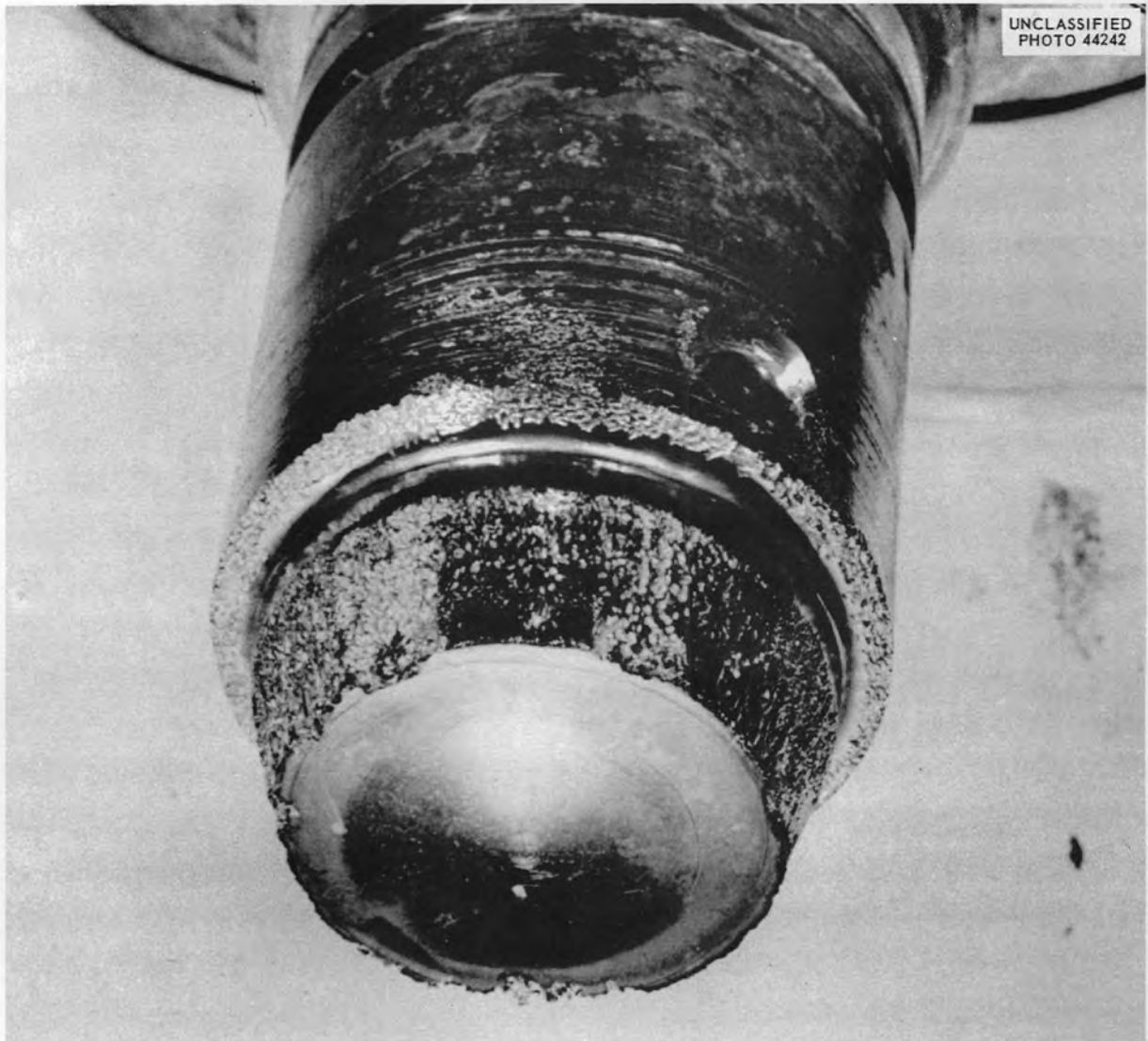


Fig. 3.4. Damaged Stellite Valve Plug.

the downstream edge rotated up. The damage here was no worse than we expected. These materials are plain 300-series stainless steel; no Stellite, no special protection. We had little faith in Stellite after the 3-in. globe valve experience. We are now in the process of having repairs made. The most dangerous part, which cannot be seen in Fig. 3.6, was that the shaft was very badly cut, almost one-third through, where it was exposed to slurry flow at the disc ends.

Based on some very inadequate information from a recent experience, we are going to try some of the 400-series stainless steels, with a heat

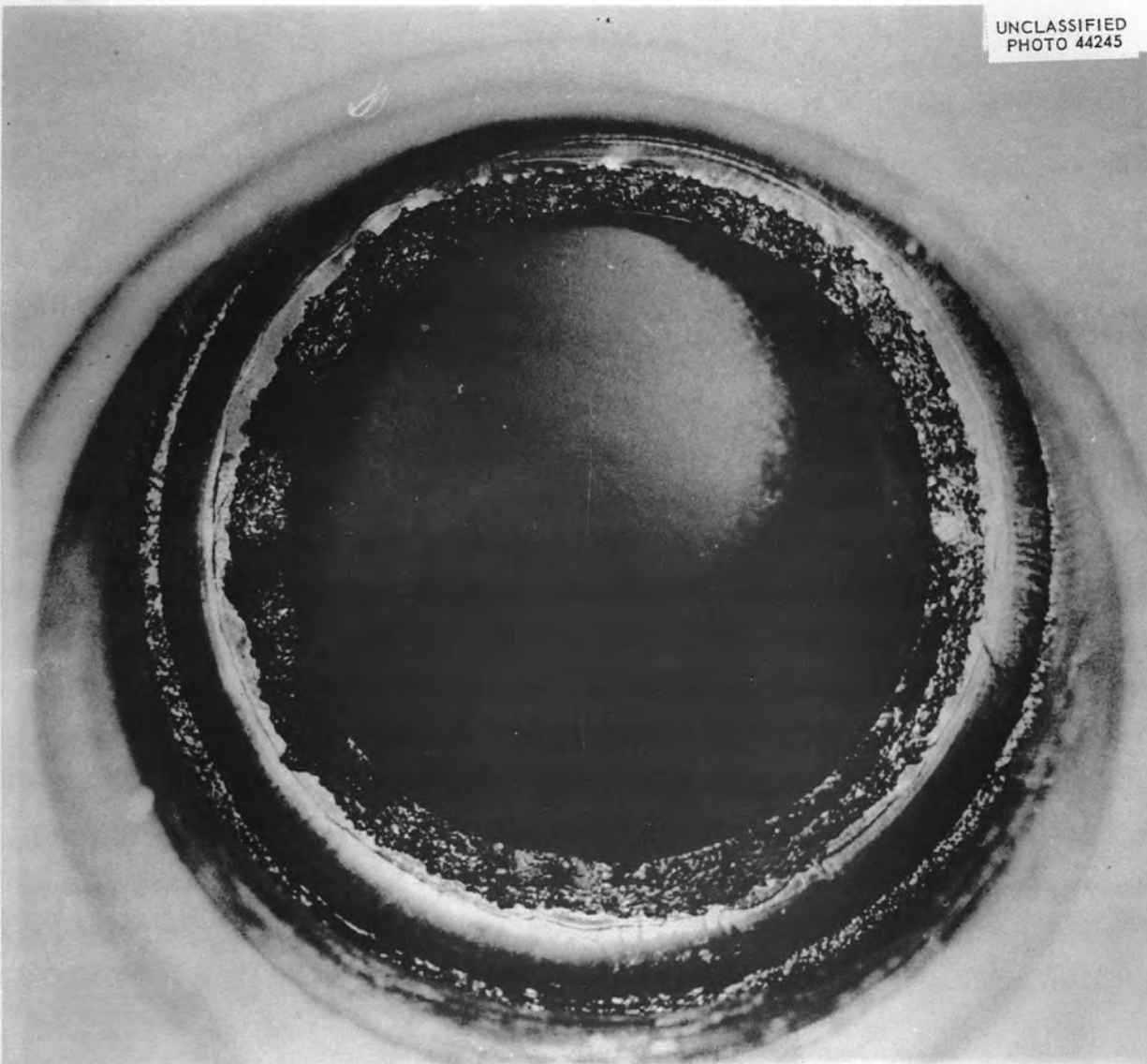


Fig. 3.5. Damaged Stellite Valve Seat.

treatment to get hardness. The flange downstream of the butterfly suffered quite badly; a liner will be installed to protect the flange. We experienced some practical corrosion here. Unfortunately, corrosion tests in loops very seldom test the metals desired for operational use, and never at the velocity desired from the component point of view. Operating components such as these valves furnish an expensive but direct way of learning what can and what cannot be used. In the higher velocities, from 40 to 80 fps, we definitely have problems.

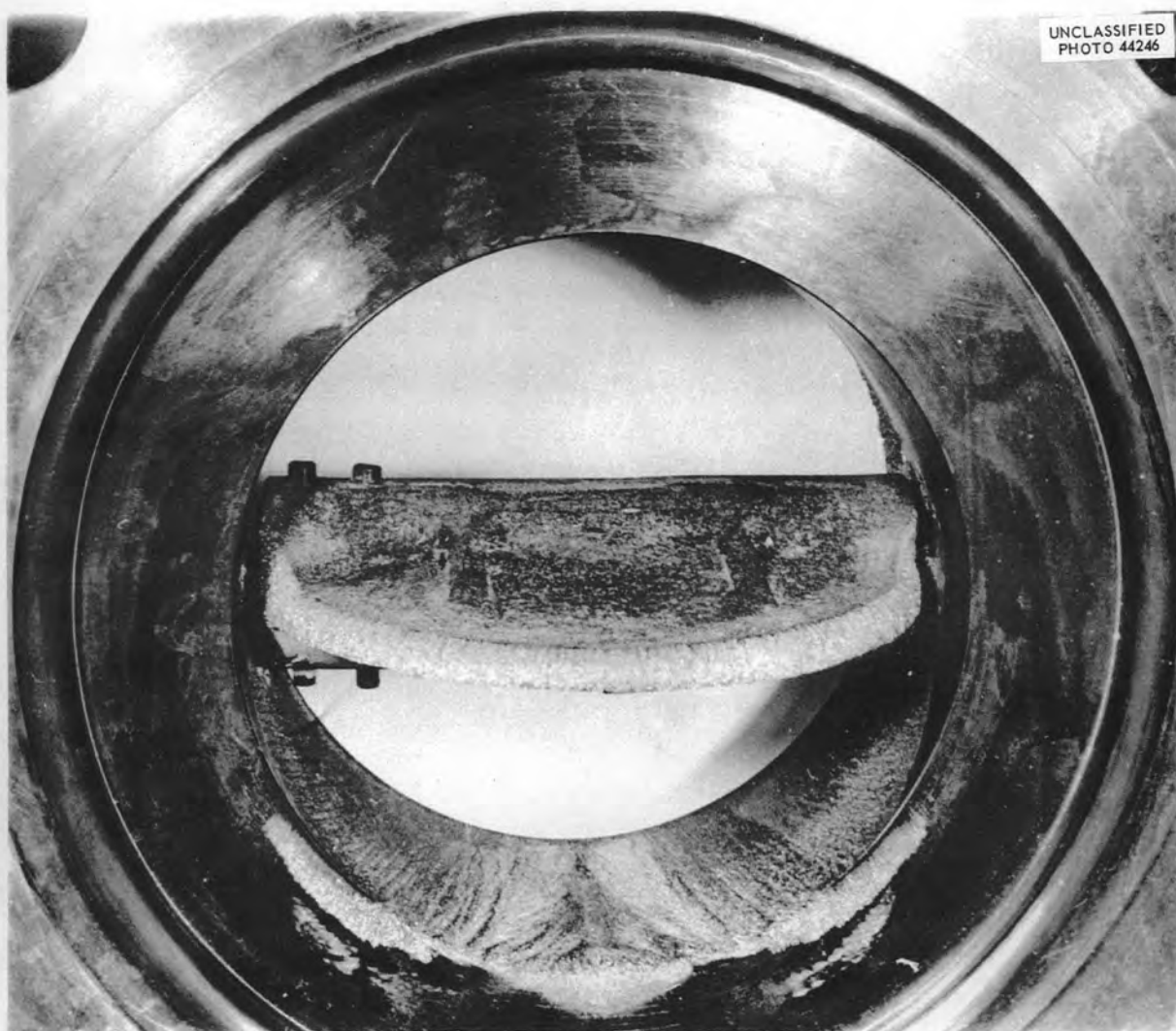


Fig. 3.6. Damaged Butterfly Valve.

We have quite a few materials that we are planning to try to test at these velocities. Work has been done at Oak Ridge on the higher velocities, and we have now convinced our people that they should run such tests in our corrosion test loops on a wider range of metals. There are only a few basic design choices: reduce the velocity, change the environment, or change the metal. It is difficult to change the environment, and to minimize pressure without increase in turbulence and velocity. So our main approach now is an attempt to increase the life metallurgically.

Fortunately, in the final plant, there are not many valves that will have to withstand such severe conditions.

Figure 3.7 shows a smaller valve plug which was passing about 10 gpm with perhaps a 60- or 70-psi drop. The material is 17-4 PH. The figure illustrates the "before" and "after" conditions of the plug. We thought this might be a little better because it showed promise in the test work at Oak Ridge. As can be seen, it isn't too much better. This view is after only about 10 days of continuous operation, about 250 or 300 hr. This valve is being repaired with a Zircaloy-2 plug and a Worthite seat.

Letdown Valves. - We have had some experience letting cold slurry down from 2000 psig to atmospheric pressure. We have done this in several ways. The safest way, but with no control, is to use a long piece of coiled tubing, keeping velocities to 40 or 50 fps. We have had very good luck doing this.

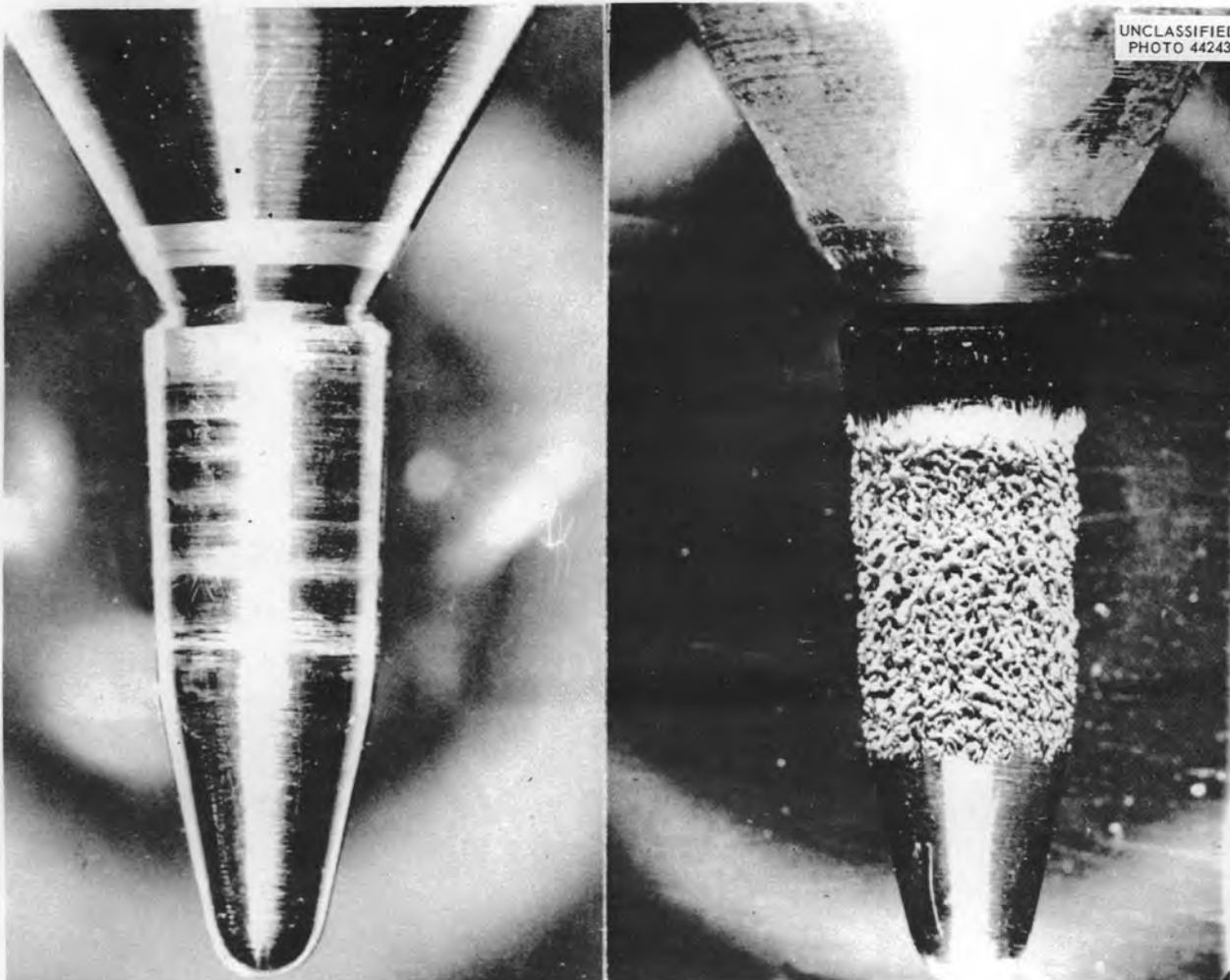


Fig. 3.7. Valve Plug, Before and After Use.

However, we also would like to use a valve for control, and we have tried valves, particularly in loop D where we were able to let down 10 gpm and pump it back to the loop. We had an interesting experience with two valves, identical except that one had a type 416 stainless steel disk and a type 316 seat, and the other a type 316 disk and a type 416 seat. They were used in cold slurry, taking an 1800-lb pressure drop. We were surprised to find that the type 416 stainless steel parts were considerably less corroded than the type 316 ones. It is on this basis that we are going to try some of the 400 stainless steel series. I want to emphasize that this was cold slurry and the results may very well not apply to hot-slurry service.

Check Valves. - We have used valves of one other type in cold-slurry service: check valves in our high-head makeup pump, which pumps 10 gpm into loop D, and smaller amounts in other test loops. Here we tried Carboloy for balls and seats; Fig. 3.8 shows one of the Carboloy valve seats. This valve was the worst we have ever tested. It lasted less than 50 hr, at the end of which no pressure could be built up; and we did not even finish water-testing - much less proceeding to slurry operation.

Figure 3.9 shows an enlargement of the failure. This is typical, in that initial failure is extended by wire-drawing at the ends. On the Carboloy, perhaps the brittleness of the seat caused the initial failure. Type 316 stainless steel seats have performed far better. Perhaps their ability to deform prevents the wire-drawing from starting, and the lack of brittleness may prevent the chips from first being knocked off. Although they have progressively failed over a couple of million cycles, even at the end of that time they were pumping at 2000 psi and half the initial capacity. In other words, the back leakage had reduced the capacity 50%.

We are working with various metals and other materials to develop proper balls and seats for the valves. Our counterparts at Oak Ridge have a very similar program. We hope that between us we can find proper materials for the severities of slurry service.

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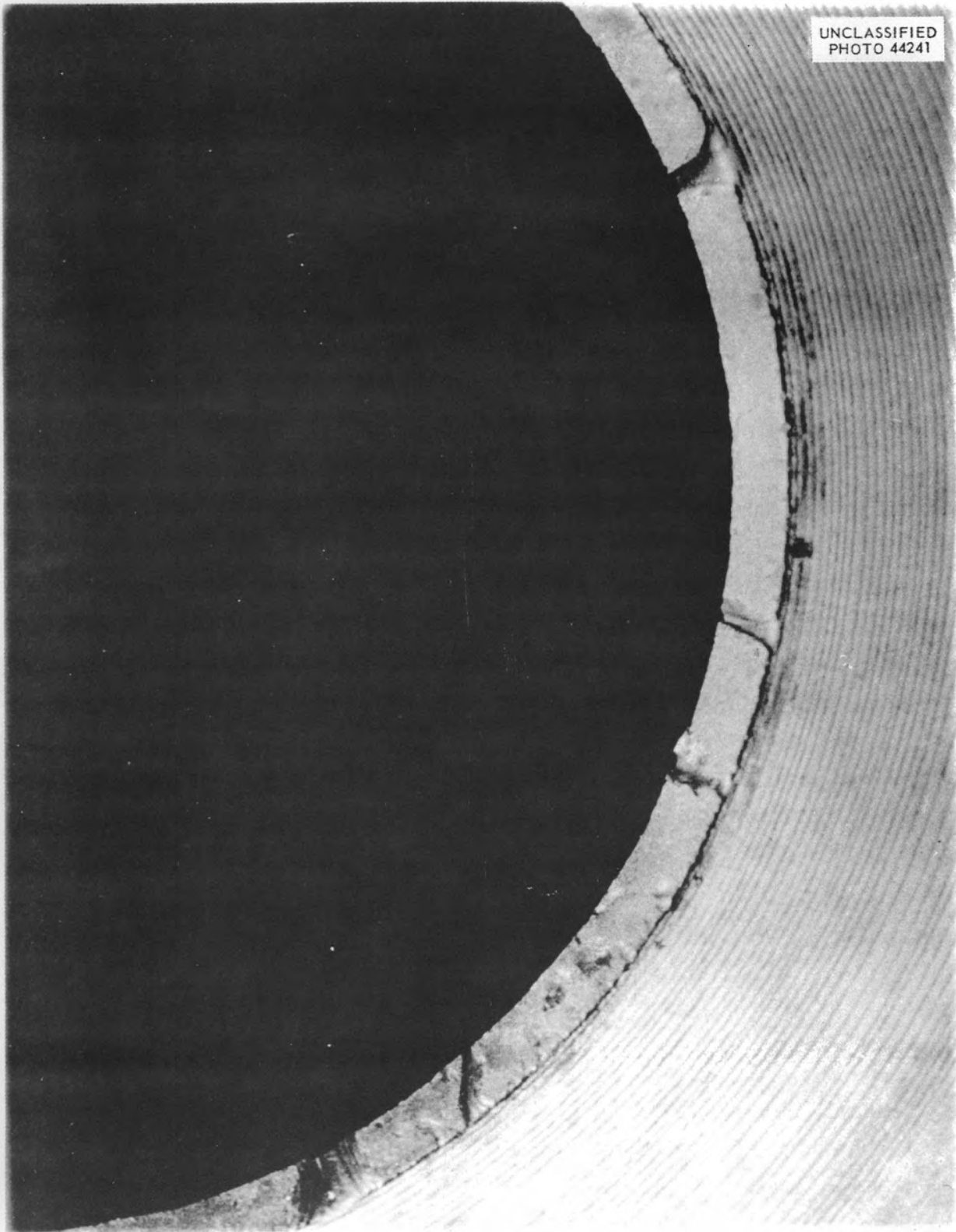


Fig. 3.8. Carbonyl Valve Seat After Water-Testing.

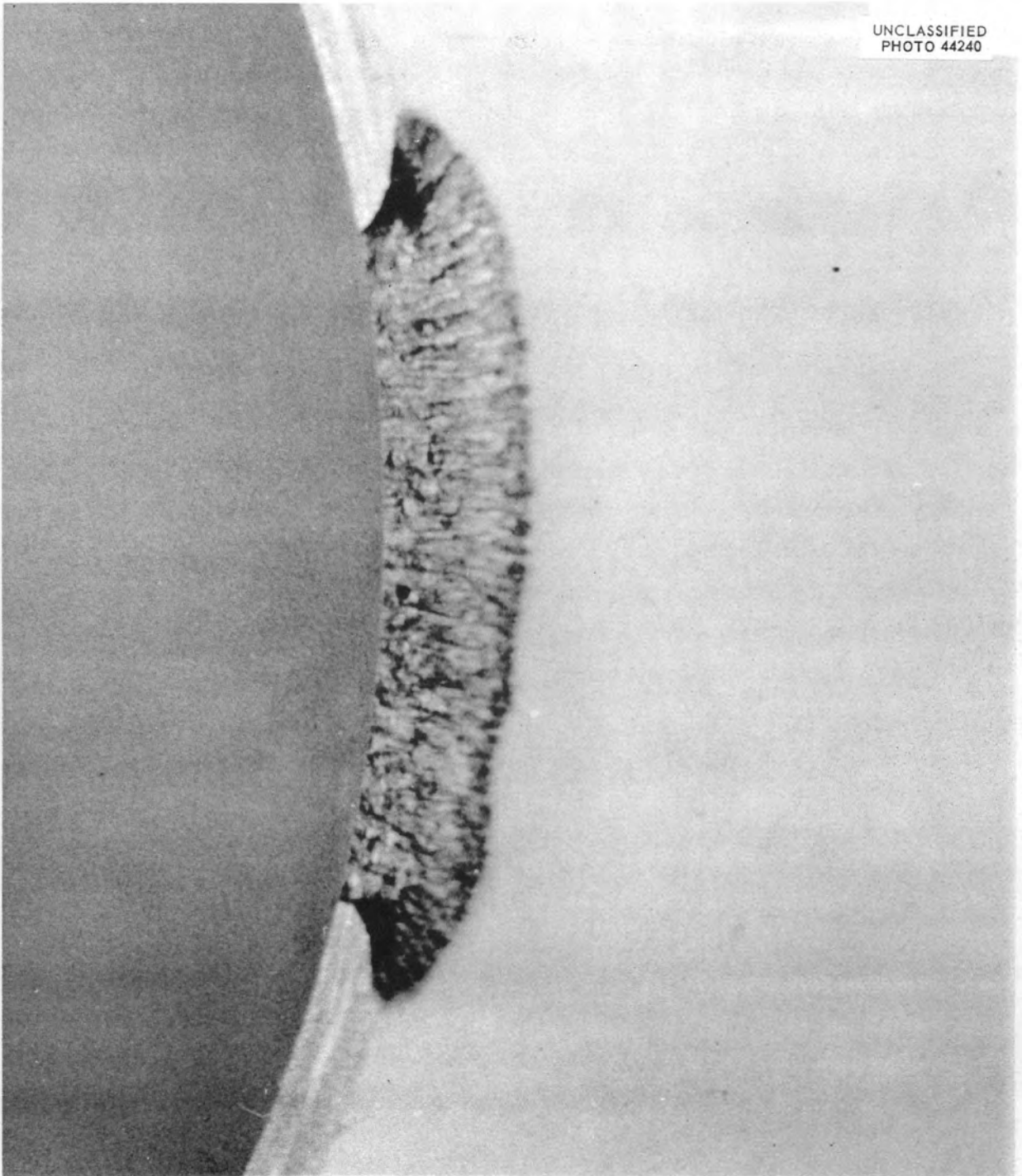


Fig. 3.9. Enlargement of Failure in Carboloy Valve Seat.

J. R. MAHONEY: What type of bearing protection do you have on your butterfly valve?

E. A. GOLDSMITH: The bearings are protected from the flow. The butterfly disk is pinned to a shaft that runs through on each side. There are bushings which carry the bearing load. These are 400-series stainless steel and, as far as we could see, about as good as new. There is no flow through them, and there was no significant slurry buildup in them. We had some purge connections, but the only time we used purge was in preparation for moving the butterfly.

D. W. HUSZAGH: We have abandoned valves entirely for exactly that reason; we have changed to controlling pump speeds. What does this slurry do to your pump impellers?

E. A. GOLDSMITH: Our experiences are so varied that we have not completely analyzed the causes. Undoubtedly velocity again is the worst offender. The Loop-D pump with its present 4000 gpm and 200-ft head is the worst from the standpoint of relative velocities and specific speed. It operates at relative velocities of the order of 100 fps and was in the worst condition of any that we have ever seen. We are installing hydraulic parts in this pump which will duplicate exactly the relative velocities of the final plant. These will be down to the order of 60 fps.

We feel that the pump designer can definitely improve, by shaping vanes, the hydraulic flow patterns. We have seen some smaller pumps run 4000-5000 hr with no trouble. On the other hand, under exactly the same conditions, some have looked pretty bad after less than 2000 hr. As I said, we change the slurry and operating conditions every time we make a run. In general, I think velocity is the main demon, and we hope that the plant, with its lower velocity, will show much better corrosion-erosion resistance.

J. E. OWENS: What are you using for seals on your pump and valve shafts?

E. A. GOLDSMITH: The pumps are canned-motor pumps, much like the standard ones used in pressurized-water plants, and manufactured by the Westinghouse Cheswick Division. We have added a few extras to keep slurry out of the top of the pump. We flush water into the top, thus keeping a net flow down around the shaft seal. In general, our bearing

experience has been very good. We feel quite confident of the canned-motor pump in this service, and I also feel that we will overcome hydraulic erosion.

The valves to date have not been weld sealed. In the test station that we are putting in on loop D we plan to use sealed valves. We have several types on order: bellows, on which Oak Ridge has done a lot of work; the United Shoe Machinery harmonic-drive; and the AiResearch cohelical operator.

We feel that, if we had to build a plant today, the hydraulically operated valve would be the one that we would have to use. In our layout study we use hydraulically operated valves because they are the most complicated from this standpoint. We cannot get away from valves. However, we do not have them in the main coolant loops in the plant. But this is a large complex plant and you have many valves in many places.

J. E. OWENS: Are the electric and magnetic properties of the slurry any different from those of distilled water?

E. A. GOLDSMITH: Not initially. However, after it picks up corrosion products and fission products something like a magnetic flow meter could be used.

J. E. OWENS: What about using a magnetic or an electromagnetic flow control? We have done some work in sodium systems with electromagnets used as eddy-current brakes for flow control.

E. A. GOLDSMITH: No, I don't think the magnitude of the conductivity is anywhere near that. We can barely measure the flow.

L. P. INGLIS: I was very much interested in the capacitance problem that you mentioned. Do I understand correctly that you are not using that for any continuous indication but only for alarm-point indications?

E. A. GOLDSMITH: That is true. We have considered using it for continuous measurement. I think there may be some people here from Bettis who would like to comment on this. I believe in their program they are using the vertical one on pressurized water. In these particular loops, we had differential-pressure instruments which very successfully measured the level, and the capacitance probes were secondary backups for alarms or calibration. But there is no reason, besides mechanical reasons, that they could not be used. Alumina is a little fragile for

shock service; but if they are usable on submarines, land plants should be able to use them.

C. J. MADSEN: Capacitance probes have been made for continuous level indication. They have passed a shock test. They have run into the same difficulty that you encountered in progressive corrosion. Our early tests seem to indicate that by pre-aging we could use the probe for a considerable period of time. In other words, the corrosion seemed to be fairly rapid at first and then diminished with time, so that after three weeks it was relatively low. However, in the 4000-hr test we have found a progressive leaching, of the silicon impurity from the aluminum oxide, which eventually would render the barrier highly porous. We have temporarily suspended this aluminum-oxide-probe work and are now investigating other ceramics.

L. P. INGLIS: What others, for example?

C. J. MADSEN: They will be basically aluminum oxide with some other type of bonding. I don't believe that I am at liberty to discuss the other materials that will be used, because the company carrying on this work now is doing so at its own expense.

C. GOTILLA: I have had a little experience with some highly abrasive hydrate slurries in caustics much of the nature of yours, and in some cases we have specified Paul valves, which are venturi-throated ball valves. We found that, in general, we tended not to get caking in the little inner passages of the valve because of the venturi action in it, and our corrosion rates dropped down considerably. The abrasiveness seemed to affect our seat very little, regardless of the material, compared with some of our other valves.

Have you had any experience with that valve that you would like to comment on?

E. A. GOLDSMITH: Yes, we considered this as one of the valves in our development program. The valve that you saw with the badly worn plug is interchangeable with a P-K or Paul valve at this point. Unfortunately - as is often the case, unexpected troubles, such as packing leakage, occur - something wrong in machining caused the stem to catch; this prevented us from getting more than a few hours' run, and we had to jerk it out and send it back to be reworked. We have just received it

again and we hope to put it in the same spot or into our other test station. Perhaps this valve has more carefully guided flow, and Mr. Paul has lots of reasons, most of which are pretty hard to explain, why it does not wear. We have hopes that it will be a helpful valve in this program, but our experience is, as I say, that everything else has gone wrong. We have never been able to keep it in the slurry long enough to find out.

W. R. MILLER: Are the differential-pressure cells on the venturis gas-locked so that the slurry does not reach the pressure centers?

E. A. GOLDSMITH: No, we mount them vertically close to, and directly above, the pipe. We here depend on gravity and the high density of the slurry to hold it down, leaving more or less pure water in the d/p cell. In the final plant we may get gas from generation in the slurry. We would be happy if it filled both of them completely. As I have said, we may have to accept some inaccuracies, such as uneven amounts of gas in the two legs. Close mounting minimizes this but is not practical in the final plant.

T. M. GAYLE: We have tried some rather small capacitance probes in small containers of slurry, and for quantitative work we found such a tremendous variation that we could not use them. A small addition of depleted uranium tended to change the dielectric properties to the extent that we found the probe unusable.

E. A. GOLDSMITH: You were making continuous measurements?

T. M. GAYLE: Yes; continuous quantitative measurements.

You mentioned that you are planning to perhaps remove the differential-pressure transmitters on the venturi to a remote location accessible for servicing. Do you plan to purge these lines with high-pressure water?

E. A. GOLDSMITH: Yes.

D. S. TOOMB: Concerning your capacitance-probe failure, did the secondary seal, which I think is soapstone, affect containment?

E. A. GOLDSMITH: Mr. Toomb is referring to a compression type seal which acts as a secondary pressure seal in case the alumina tube does break. It also insulates between the probe and the pipe. We have now had enough failures that we can say that in all cases, from our loop standpoint, the secondary seal was very satisfactory. In one case there was a slight leak, perhaps a drop a minute, coming out, and the others,

I think, held tight as far as we could see visually. Whether this would be satisfactory in the plant is doubtful. That secondary seal is lava or soapstone.

In one case an energetic young fellow put 500 v on a probe to check its resistance to ground. Apparently this secondary seal is not very good from this standpoint, because he broke down the electrical resistance; and the only place we could see it could possibly have broken down was in the secondary seal.

PRIMARY COOLANT VALVES FOR NUCLEAR PLANTS

E. A. Bake
Westinghouse Electric Corporation

E. A. BAKE: When the submarine Nautilus went to sea under nuclear power three years ago, a new era of naval propulsion began. The engineers and scientists who had participated in the development of the Nautilus recognized that their accomplishments actually represented only a beginning.

In addition to its reactor, the Nautilus carried scores of special components, many of which were developed specifically for the Nautilus or its prototype and had been designed, built and tested on a custom basis. These emerged with a history of hand fitting, bench assembly, and close engineering follow-up. The valves in the primary-coolant system had such a history. It was recognized that major improvements in valve design and producibility were necessary to permit production of nuclear plants in practical numbers. With the increase in production volume demands, developmental activities have had to maintain a rapid pace in order that production valves might satisfactorily meet new plant requirements.

Primary-coolant valves in pressurized-water reactor plants must perform reliably over extended periods of plant operation without inspection or service. Valves which normally or frequently pass radioactive water must be hermetically sealed by welding. The valves must operate satisfactorily in high-pressure, high-temperature systems. They must be corrosion-resistant, not only to preserve their own operational integrity but also to prevent contamination of the flowing fluid. These service conditions are not inordinately severe when considered individually, and limited combinations have been met in valves developed for established industries. However, in combination they constitute a very stringent set of design criteria that is practically unique to nuclear power plants.

A review of these design conditions reveals that the hermetic-sealing requirement is the most critical one affecting valve designs. Remote operation of valves through a hermetically sealed pressure barrier is

frequently encountered in chemical processing systems, but the temperatures and pressures encountered in primary-coolant service and the requirement for unquestionable reliability prevents the use of flexible hermetic-sealing devices. While significant advances have been made in the bellows art within recent years, uncertainties associated with the reliability of bellows have limited their use in nuclear power plants to applications which cannot be filled without them. Thus, remote operation of primary-coolant valves generally requires the use of electromagnetic or hydraulic schemes. Electromagnetic operation is used in canned-armature solenoid valves. The energizing of an external coil creates a magnetic field which moves an armature within the pressure wall. Since the force available with this scheme is limited, its use is restricted to small or balanced pilot valves. Where hydraulic operation is employed, the hydraulic-control fluid must be water identical to that used as primary coolant, so that intermixing of the two will not be detrimental to the reactor operation.

Valves used in primary-coolant service have ranged in size from 1/2 to 16 in. They range in complexity from small lift check valves to the large gate valves used as main-stop valves in the primary-coolant loops. In most current plants, austenitic stainless steel is used as the major material for valves that contact water which may recirculate through the primary-coolant loops. In early designs, valve bodies were generally machined from bar stock and billet forgings. Improvements in stainless-steel-casting techniques and in increased production volume now permit wide use of cast valve bodies in the larger sizes. While the normal operating conditions in most plants are in the range of 2000 psi and 500°F, the design conditions for most valves are based on successful operation at 2500 psi and 670°F. Until recently, these valves were considered as a special class, so the design of valve bodies and other pressure-containing parts was based on the stress values stipulated for the corresponding materials in Section VIII ("Unfired Pressure Vessels") of the ASME Boiler and Pressure Vessel Code. More recently, high volume requirements have brought about a degree of standardization of certain valve types, so that most of the smaller, less specialized valves are now ordered in accordance with applicable class designations in ASA B16.5 ("Steel Pipe Flanges and Flanged Fittings"). While the ASA 1500-psi

class is satisfactory for primary-coolant pressure and temperature conditions with some materials, the general use of type 304 stainless steel requires most small valves to be of the ASA 2500-psi class. This standardization has progressed rapidly, and current valve procurement is based on maintaining interchangeability as much as possible, so that similar valves may be used in various reactor plants.

The selection of materials for internal working parts requires a considerable amount of attention. The requirement for hermetic sealing of valves dictates the use of internal guides, linkages, springs, and seals. Since primary-coolant water has no practical lubricating value for high-load applications, and since no other lubricant may be introduced into the system, the design of internal working parts must be based on satisfactory operation without lubrication. With very few exceptions, internal surfaces that undergo relative contacting motion must be of materials such as the cobalt-base Haynes Stellite alloys. These are the only generally accepted materials which will provide adequate resistance to corrosion, wear, and galling in high-load applications. A limited use of precipitation-hardened stainless steels was once permitted, but use of these alloys in valves has been suspended. The Stellite alloys are used in the form of weld deposits on working surfaces and as cast bushings and liners. Haynes alloy No. 25 is used extensively for highly stressed machined parts. Inconel X is generally used for internal valve springs, although cold-reduced and aged Haynes alloy No. 25 is now being applied for some springs where high stress levels and good resistance to relaxation are required. Internal seals, such as those used on the pistons and stems in hydraulically operated valves, do not have to be exceptionally tight, but the temperature and the water environment preclude use of organic seals. Temperature variations and the possibilities of crevice corrosion also preclude close-clearance sliding fits. Piston rings of Haynes alloy No. 25 are generally employed where control of internal leakage is required.

Since valve tightness is a critical requirement in nuclear plants, seat design, material, and finishing require close attention. Valves for isolation purposes are generally required to meet an exceptional tightness standard. This standard limits the maximum acceptable valve-seat leakage to 2 cc of water per hr per in. of nominal pipe size. Stellite seat materials have been found acceptable for all but the most severe

seating applications, but attention must be given to seat design, finishing, and rigidity to maintain tightness. Distortion of seats due to mechanical loading or welding effects can be a problem if not adequately considered during the valve-design phase.

There are no proportional-control valves in reactor-plant primary-coolant systems. Available electromagnetic and hydraulic valve-operating methods are not suitable for practical proportional control of valve position. Valves are generally employed for isolation purposes or for on-off control. Hydraulically operated, parallel-seated gate valves are included in each main coolant-piping loop to permit isolation of steam generators and circulating pumps from the reactor vessel. Smaller hydraulically operated gate and globe valves are installed in auxiliary piping to permit isolation of auxiliary components and systems. These valves must be operable from outside the reactor compartment so that operating personnel may make changes in plant operation or isolate leaks in the event of damage to piping or components. The operation of these hydraulic valves requires auxiliary equipment to apply a controlled differential pressure to the valve piston. Various methods have been employed to establish a differential pressure. An elevated pressure may be set up in a hydraulic service system by the use of small canned-motor pumps, or by a separate system of pressurized flasks. With these schemes, the primary-coolant system itself serves as the low-pressure side of the control system. In other plants, valves may be operated by using primary-coolant pressure as the high pressure and venting the low-pressure side of the valve piston to atmosphere. In any case, control of the differential pressure acting across a hydraulic valve piston is generally supplied by a hermetically sealed, solenoid-operated pilot valve.

Capped, manually operated valves are used in many lines within the reactor compartment. These valves are used where valve operation is necessary only when access to the reactor compartment is permitted. These valves are packed, but weld-sealed caps cover the stem and contain packing leakage. To operate these valves, it is necessary to cut the seal weld and remove the cap.

Pressure-relief valves must be provided on each reactor plant to prevent overpressure damage. The performance of these valves must be

of the highest order. Failure of a valve to open could result in over-pressure damage and failure to reseal is equally undesirable, since it could result in a loss of coolant. Extensive development work has been done on pressure-relief valves, and an intensive development program is still in progress. Plant requirements dictate that relief valves must provide excellent set-pressure accuracy and low blowdown. The valves must shut tightly on resealing in order to prevent excessive erosion of seats. Valves must be provided for the relief of subcooled water from the reactor. The latter application has been more difficult, since the relief-valve industry had no significant prior experience in designing valve control elements to work with flashing water. The problem is further complicated in some plants, since the operation of the valves must be unaffected by variations in back pressure. While self-actuated valves similar to normal steam safety valves are being applied in some plants, other plants have required piloted relief valves to provide satisfactory operation. The pilot employs a pressure-sensing bellows to operate a small valve element. The opening of the pilot applies pressure to the operating cylinder of a hydraulically operated main relief valve. Since the pilot-valve bellows must withstand full primary-system pressure, the development of satisfactory bellows required an extensive evaluation program and lengthy, statistically designed tests. The most important problem remaining is that of maintaining acceptable seat tightness. The high-velocity steam and flashing water flow encountered in relief valves makes this the most critical valve-seating application in primary-coolant service.

In summary, it must be recognized that primary-coolant valves call for close attention to detail in design, material selection, manufacturing, and testing. So long as production volumes were low, primary-coolant valves could be handled as specialty items. The production volume encountered today does not permit such a luxury. Valve manufacturers are learning that close attention to detail must become routine throughout their plants. Current standards can be met provided that manufacturers set their own standards accordingly. Manufacturing and quality-control standards which are suitable for commercial packed steam valves are not adequate for primary-coolant valves.

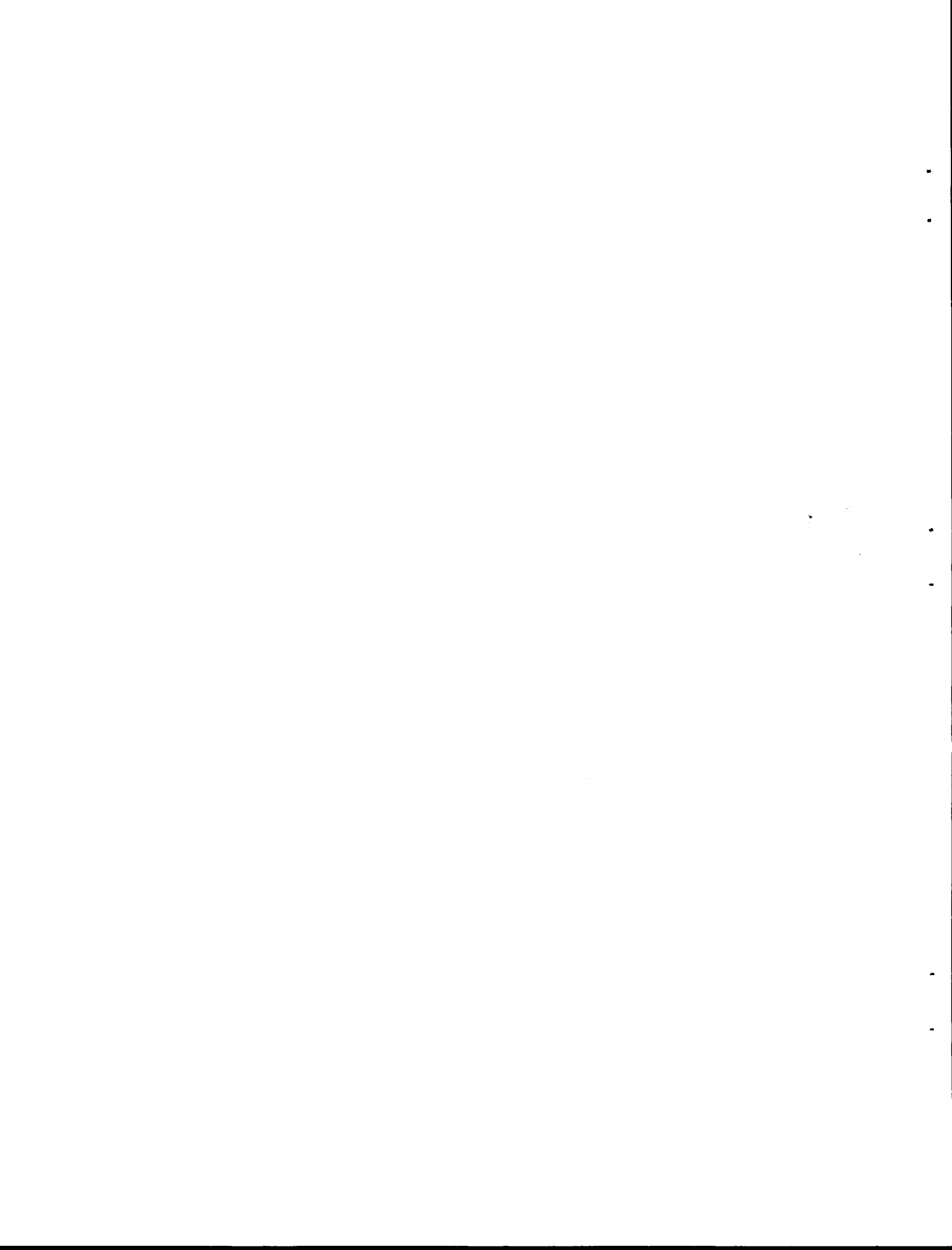
Developmental activities cannot be relaxed with the advent of volume production of valves. New plant designs often bring about new valve requirements which cannot be adequately met in normal production. Major valve improvements often require prototype manufacturing and testing before application in a plant can be justified. As in any industry, continuous developmental support is necessary for advancement.

P. BLISS: Did you say there is no proportional control valve?

C. J. MADSEN: Not on our systems, to the best of my knowledge. We have recently initiated a program for the study of relief-valve-seat configuration and materials. As on many developments for improving an existing product, our primary problem was bellows; we now have reliable bellows. In locating rattles in an automobile, after the big rattle is out of the way others are heard. Just so, leakage of seats that have operated has become our major problem.

PART II

PROCESS INSTRUMENTATION IN HIGH-TEMPERATURE ENVIRONMENTS



INSTRUMENTS USED IN HIGH-TEMPERATURE SODIUM
AT ARGONNE NATIONAL LABORATORY

F. A. Smith
Argonne National Laboratory

There has been mention of temperatures in the tens of millions, but the temperatures that I have to talk about are in the hundreds; and therein lies the big difference between civilian power reactors and other reactors.

The EBR-1 is one reactor in the sodium- or liquid-metal field that has survived three separate nuclear cores. It has survived a nuclear core meltdown, and the various instrumentation components remained intact. They were originally put into the reactor in 1951. Again I emphasize temperature. It is significant that the EBR-1 is currently operating. Temperature limitations make our problems in designing instruments and components, in general, rather simple.

I called people at Idaho to confirm the sensitivity and calibration of our sodium process instruments. The meltdown of the EBR-1 core and the installation of a new core last winter afforded opportunity to check the nonnuclear instrumentation of the reactor. Inspection of flow meters, thermocouples, level indicators, and pressure transmitters revealed that they are as reliable and accurate as they were the day they were installed.

Figure 5.1 shows some of our modest work, which dates back to about 1948. There are some instruments - ordinary three-dollar pressure gages. An inexpensive millivoltmeter was hooked to an electromagnetic flow meter which is not shown. The large block of iron and copper is the pump. The purpose of this loop was to test the pump. When we started to work on the EBR-1, there were no commercial instruments for a liquid-metal system, even at 660°F. Most of the instruments illustrated - electromagnetic flow meters, pressure indicators, and level indicators - had to be developed. Level, for example, in those days was measured by putting a hand on the outer surface of the expansion tank. NaK, as it comes out of the container, is cold, and as cold NaK enters the expansion tank, it becomes colder, and you stop filling.

Figure 5.2 is a sketch of the exterior of EBR-2. The prime objective of the EBR-2 development program is to ensure safe reactor containment.

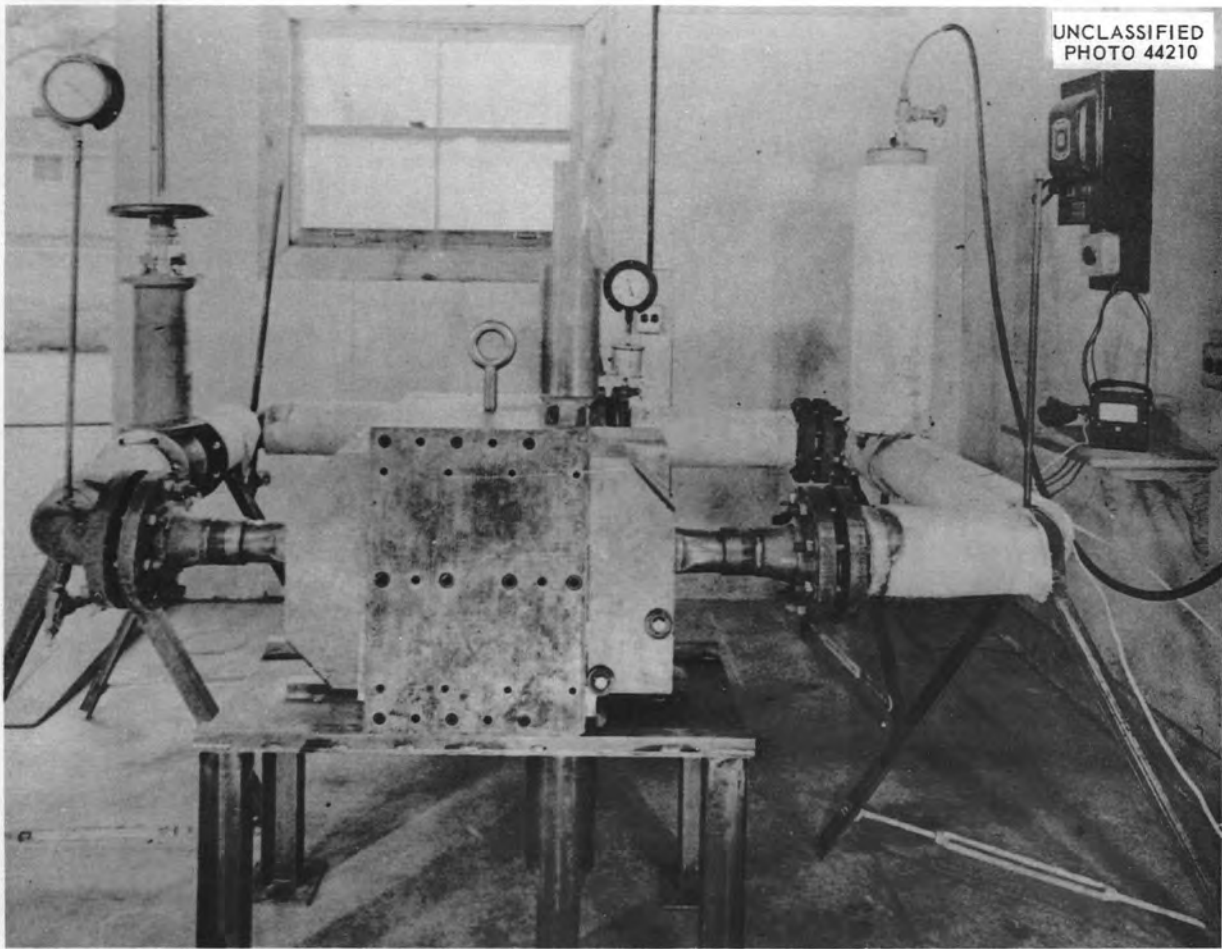


Fig. 5.1. Early (1948) Loop for Pump Testing.

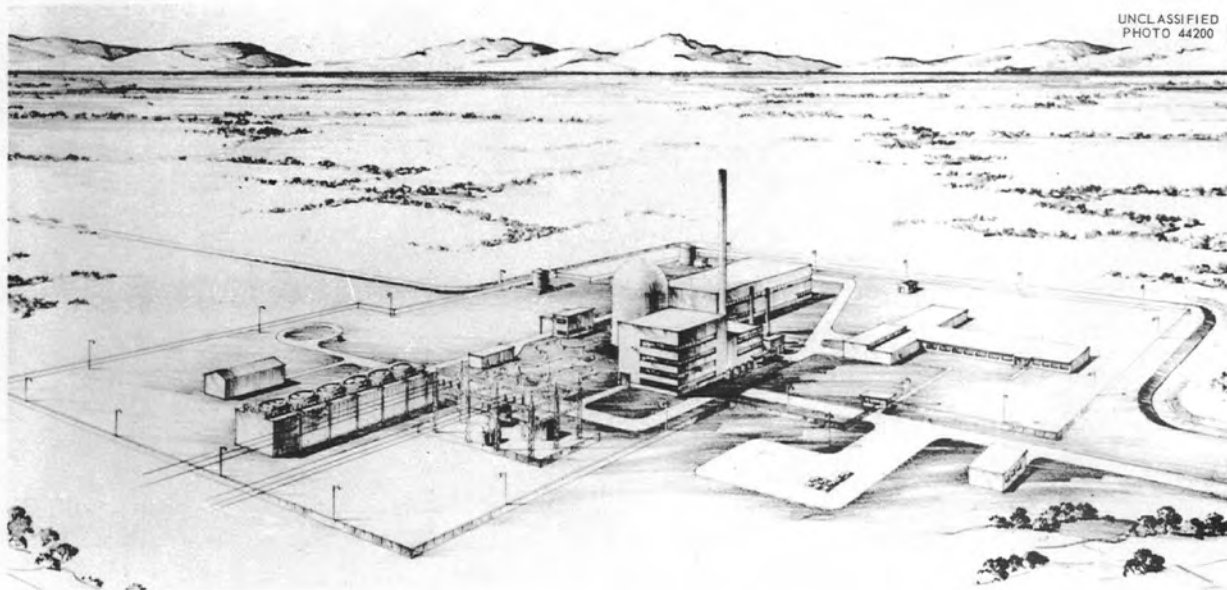


Fig. 5.2. Sketch of EBR-2.

In order to test the submerged-in-sodium radioactive primary coolant containment, it was necessary to build what we call an EBR nonnuclear model system (Fig. 5.3). This is the result of work that was started about three years ago, primarily to indicate the feasibility of fuel transfer or the handling of fuel submerged in sodium. Incidentally, the necessary instrumentation had to be accomplished in some manner.

The equipment protruding from the top is the fuel-handling apparatus. The rather modest control panel is an interlocked sequence panel which guarantees the reliability of the fuel-handling process. W. Zinn, who insists on reliability and integrity, said, "Design this so that it cannot drop fuel."

Figure 5.4 shows approximately what is inside the model tank. This sketch was prepared primarily to illustrate the fuel-handling mechanism. Subsequent figures will show the problems of submerging an EM flow meter in sodium at about 750°F. Since this is the radioactivity containment vessel, access to the flow meter is impossible. Access to pressure transmitters is practically impossible. So we must stress the reliability of this type of instrumentation. Figure 5.5 is a view of the tank under construction. This is unique in EBR-2. It is not unique in all of the electromagnetic flow meter applications that many of you are familiar with - this is a permanent magnet assembly. The magnet section was built and installed in a pump head-capacity test loop, with a box welded around it to duplicate the 700°F-sodium application. The output leads of the flow meter are placed in concentric tubing. The thin-walled, stainless steel sleeve insulates mechanically and electrically. The two output leads from the flow meter and the ambient-sodium thermocouple are brought up through the reactor top. In order to determine the effect on the absolute calibration of this type of instrument, we had to build a pump-test loop external to the model tank and calibrate the flow meter, using orifice calibration techniques. Then the flow meter was put into the tank. According to an orifice calibration, the instrument was determined to be reliable.

I have grouped flow meter assemblies together. The pipe illustrated previously is 4-in. pipe. Figure 5.6 is a photograph of a piece of 12-in. sodium plumbing, and a 12-in. flow meter. I would like to talk briefly about flow meter philosophy. The KAPL project has done much work on flow meters and all other sodium instrumentation. We have what I will call a

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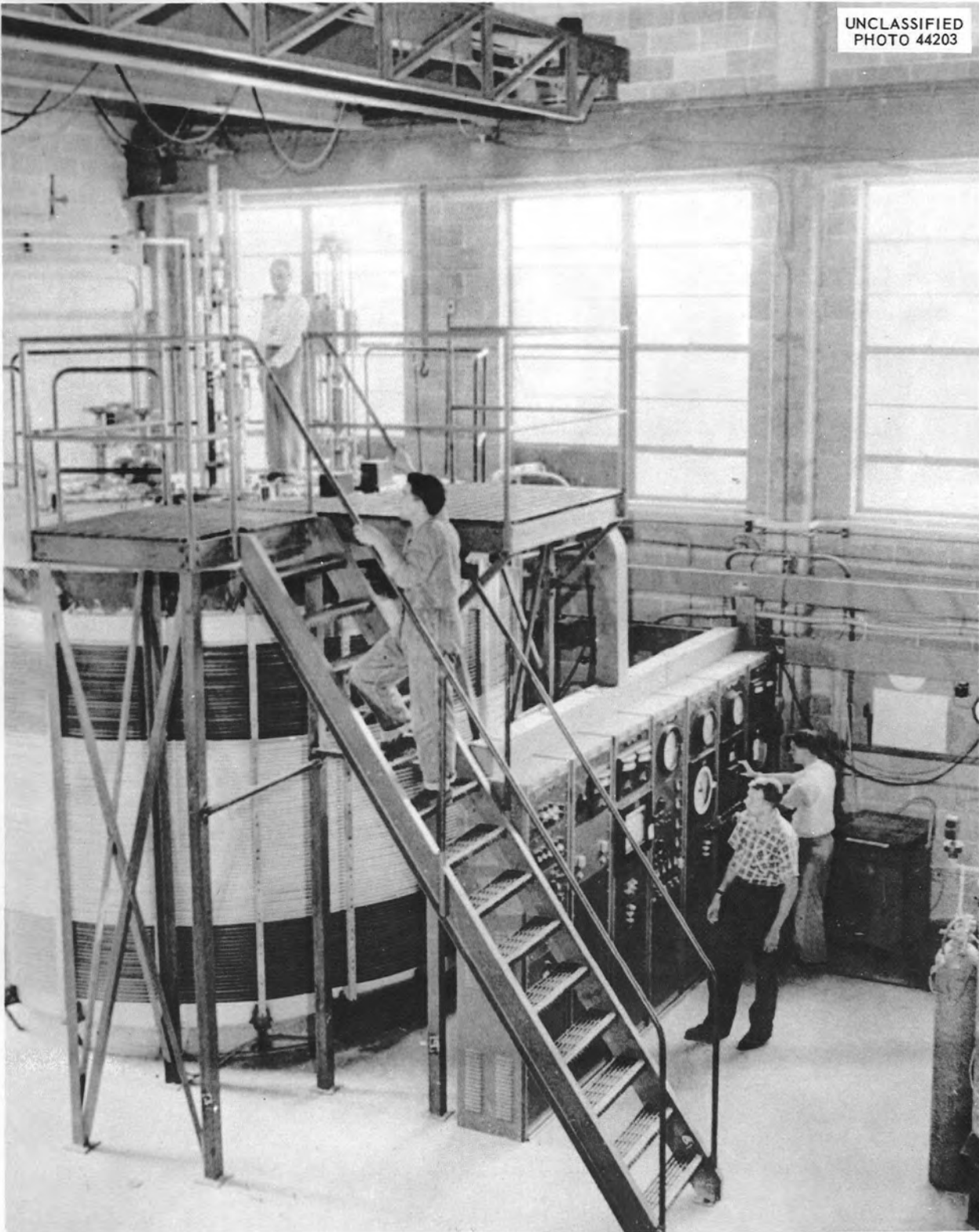


Fig. 5.3. The EBR Nonnuclear Model System.

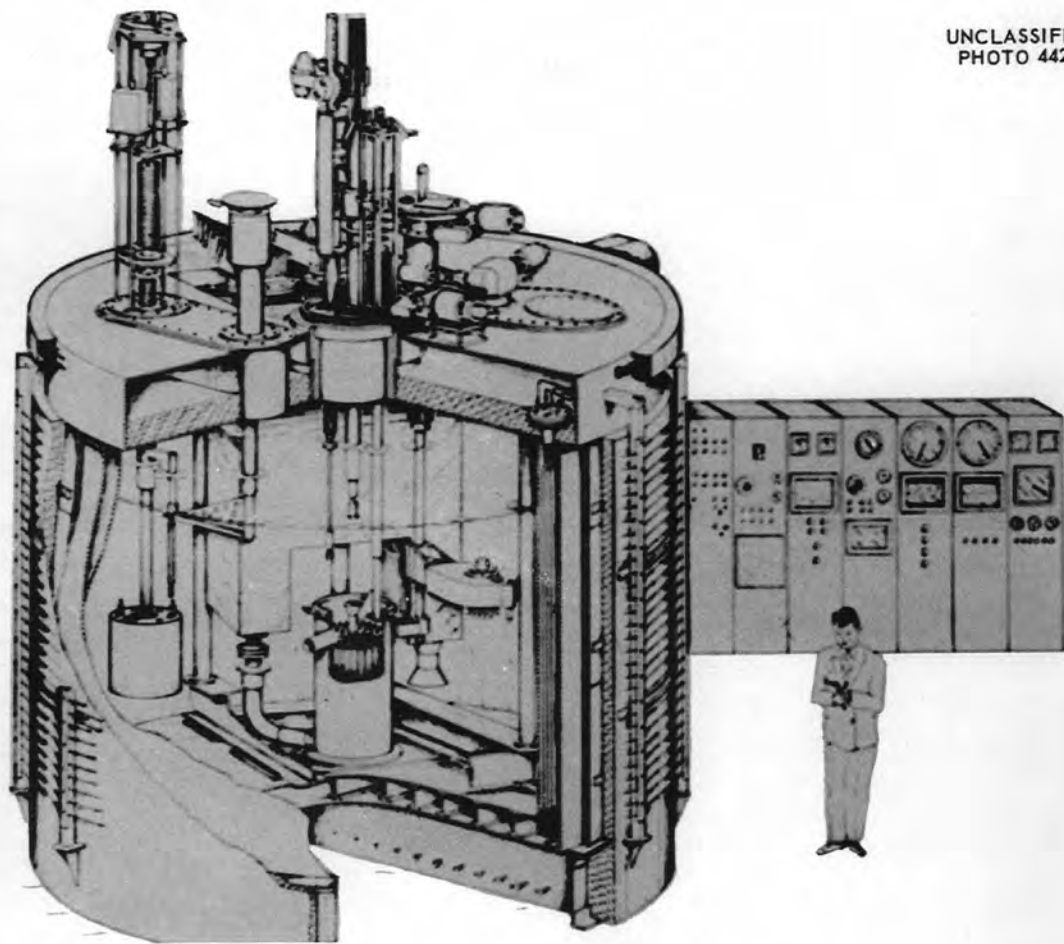


Fig. 5.4. Sketch of Model System Tank.

nonlinear electromagnetic flow meter. From the academic and the instrument man's objective viewpoint, it is desirable to have linear flow meters.

Since we are in what I call the civilian power reactor program, we cannot afford to make them linear. We would like to do so. For anybody who would like to work on a cheap, simple, linear electromagnetic flow meter there is opportunity; it is a problem. We accept the nonlinearity of large permanent magnets and live with it. In the EBR-2, we plan to duplicate flow-measuring techniques. We will have an orifice with a low head drop and, perhaps, a flow tube. We will have pressure taps on known lengths of pipe. And we will have electromagnetic flow meters - nonlinear, but inexpensive and reliable.

Figure 5.7 shows the EBR-1 continuous level probe. It is a single-layer solenoid wrapped around an iron core. This is the usual type of device that we worked on in 1948 for level indication. It is temperature

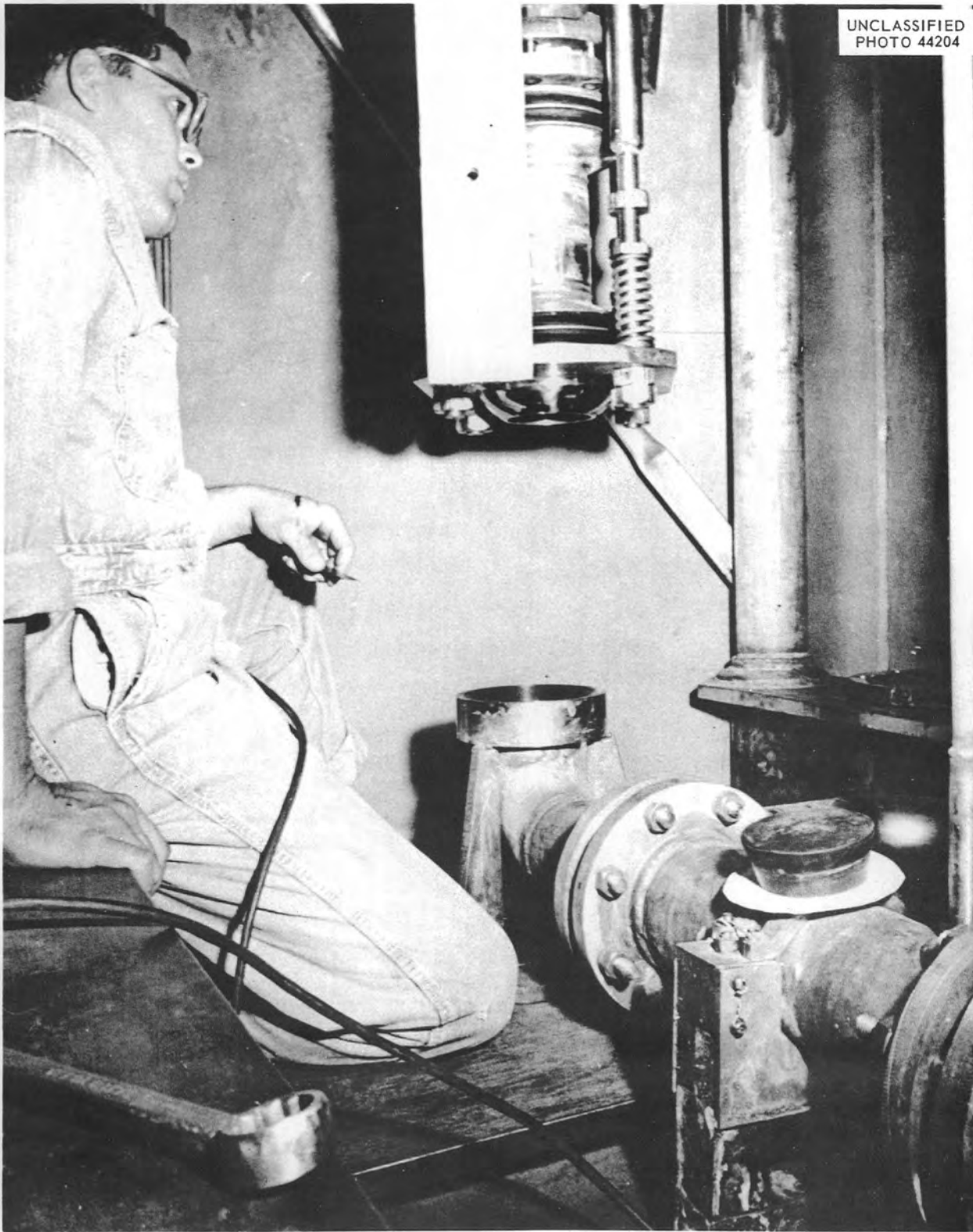


Fig. 5.5. Installation of Flowmeter.

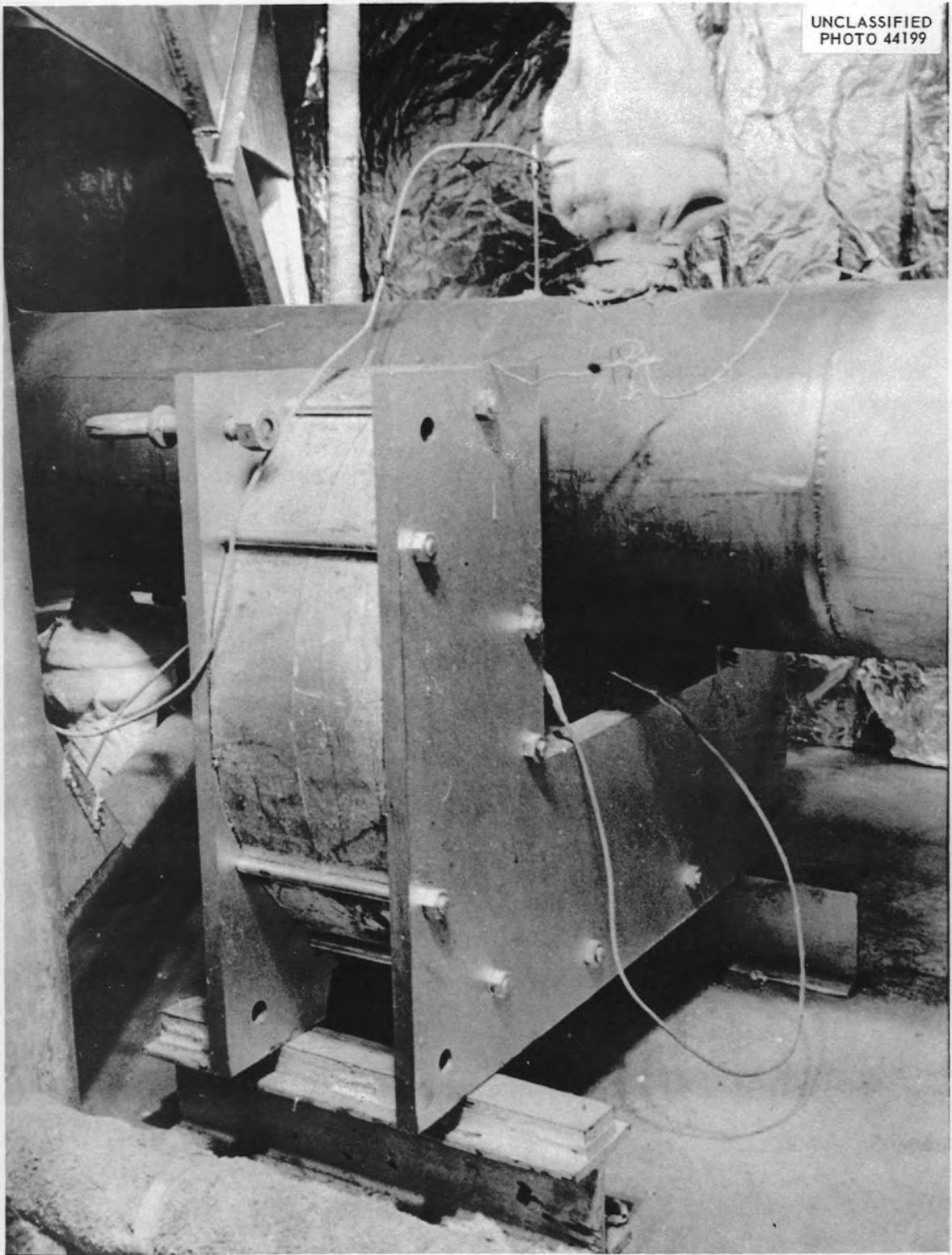


Fig. 5.6. View of 12-in. Flowmeter.



Fig. 5.7. EBR-1 Continuous Level Probe.

sensitive; there are end effects, and the response is not linear. For economy and simplicity, don't work on an end of the coil; put a coil like this in a thimble in the tank and the end effects disappear. Again, here is a fruitful field, I think. From the work I have seen, Oak Ridge people have done a very fine job on their J-tube level indicators. We at Argonne have been encumbered and hindered by having to build a reactor, and couldn't spend much time on research and development.

Figure 5.8 shows a slightly different type of liquid-sodium-level indicator. Instead of being a single solenoid, it is a small probe manually operated with a rod that has inches inscribed on it. A weld mark indicates the level of the static-sealed system. Because the coil is temperature sensitive, we have to heat the test container (on insulated hot plates or by other means), and balance the high-frequency bridge circuit to compensate for the temperature-dependence of the particular installation.

I am sure that we will hear more about level-indicating instruments from other speakers; so I will say only that these instruments have been used in conjunction with the continuous level-indicator solenoid shown in Fig. 5.7. In fact, it has been used to calibrate the continuous solenoid in our experimental work and even in our reactor work. In any tank where we want to know the level accurately, we install two thimbles. The small indicator probe (manual or automatic) is placed in one, and the long solenoid alongside in the other. We vary the level in the tank and calibrate the solenoid against the probe. Again the EBR-1 has demonstrated that this works. It is simple, reliable, and inexpensive.

I mentioned that we measure level automatically. Figure 5.9 is a photograph of such a device, showing the coil and the drive. Our instruments people and our electronics people over a period of years have tried

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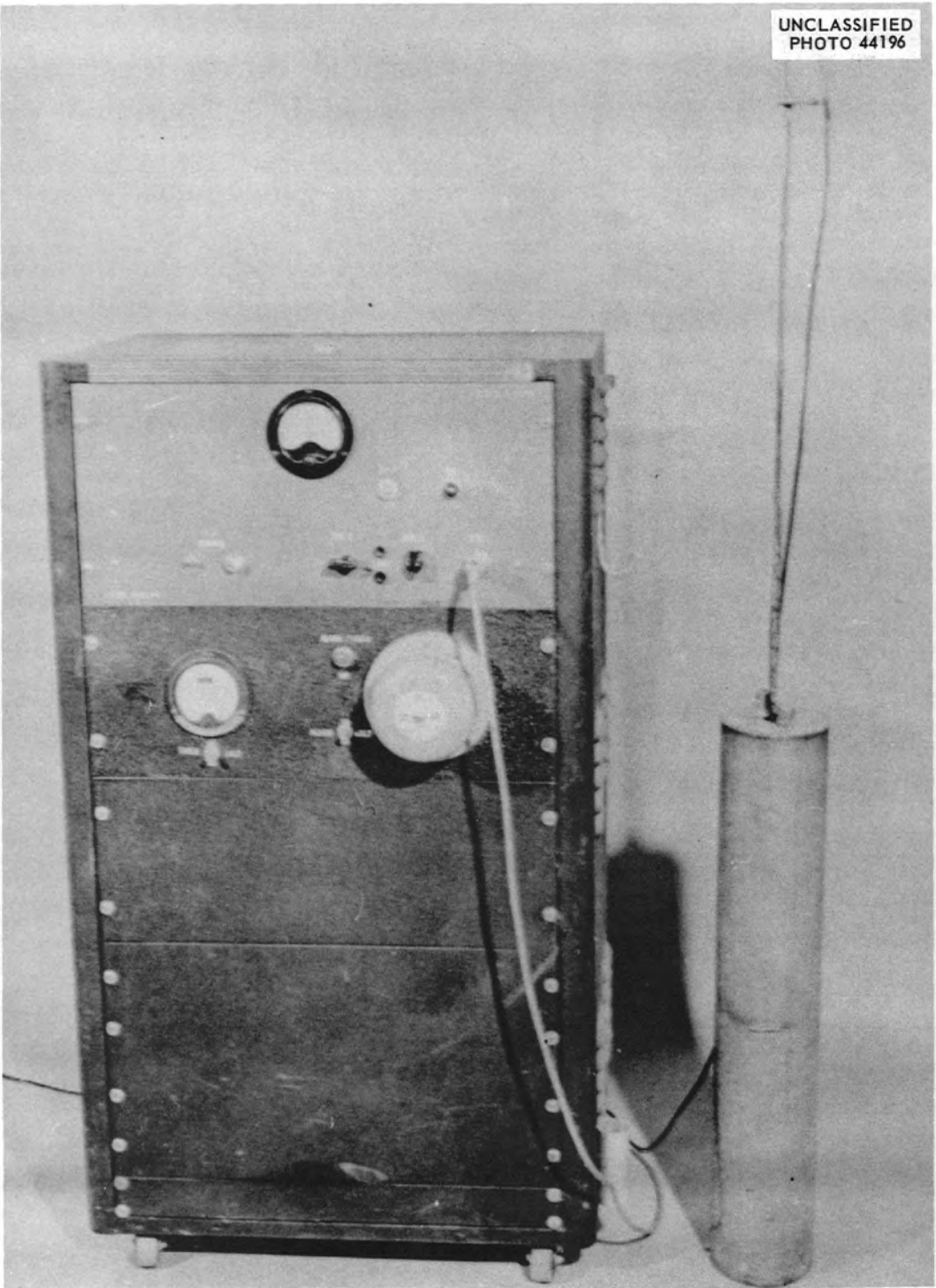


Fig. 5.8. Liquid-Sodium-Level Probe.

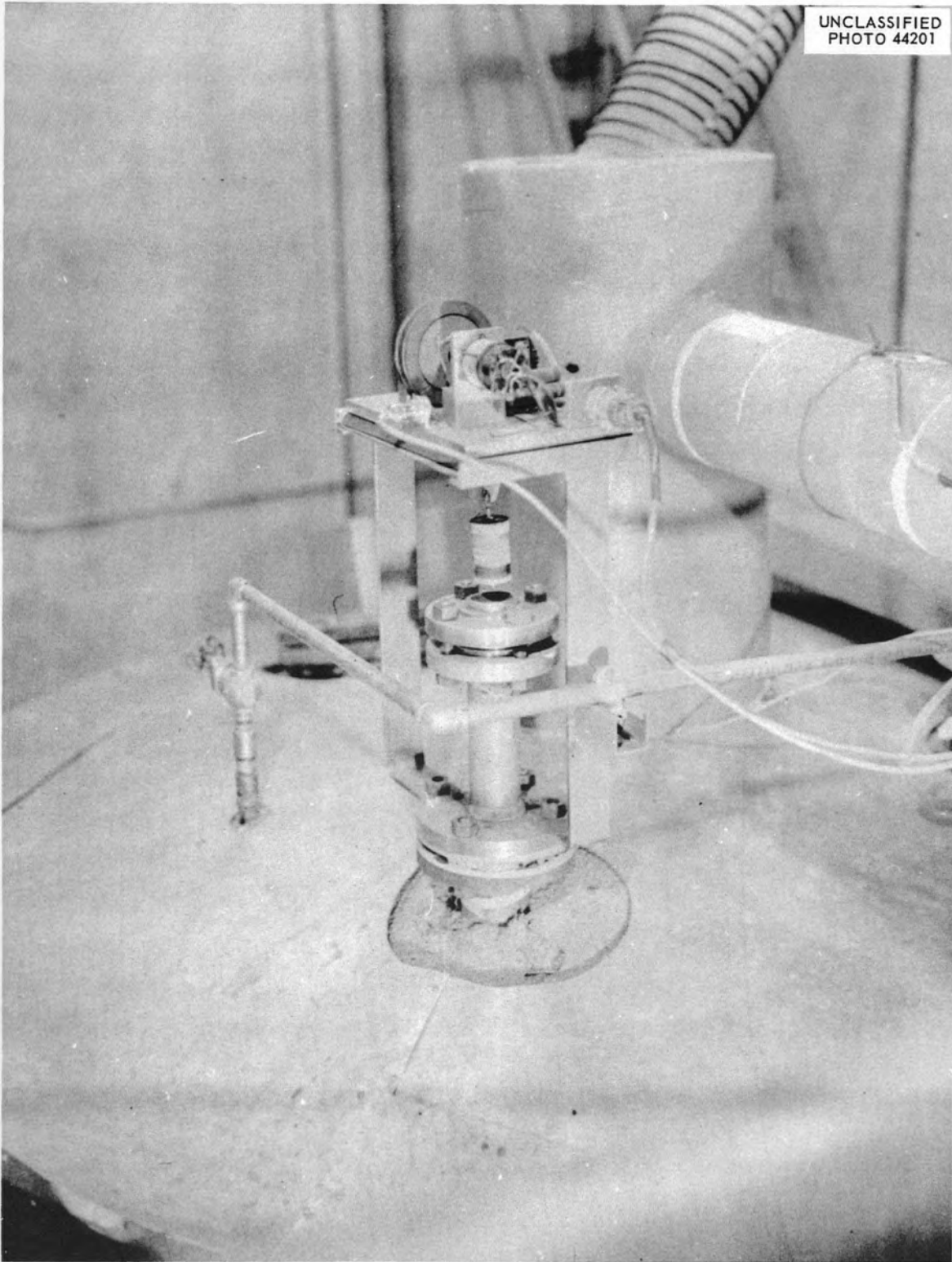


Fig. 5.9. Automatic Sodium-Level Measuring Device.

to improve upon the electronic circuit. I would call it a remote control application of this particular type of inductance probe. The instrument can be wired to seek the sodium level. In other words, if the level is changing, the indicated level will change automatically. No operator is required. This device has been very useful in EBR-1 in the excessively radioactive decay tanks for our primary coolant. It has enabled us to calibrate the entire storage tank system. It is temperature-dependent, hence there are instrument problems.

The new EBR-2 full-size-components building is shown in Fig. 5.10. Three pumps, one 10,000-gpm and two 5000-gpm, were under test.

Figure 5.11 is a photograph of a 5000-gpm, linear, electromagnetic pump. I chose this slide because it indicates the general use at Argonne of Moore Products, Inc., pressure transmitters. We have used them for a number of years. They present some problems, but in our case the problems have been few and minor. On experimental equipment, where one has access

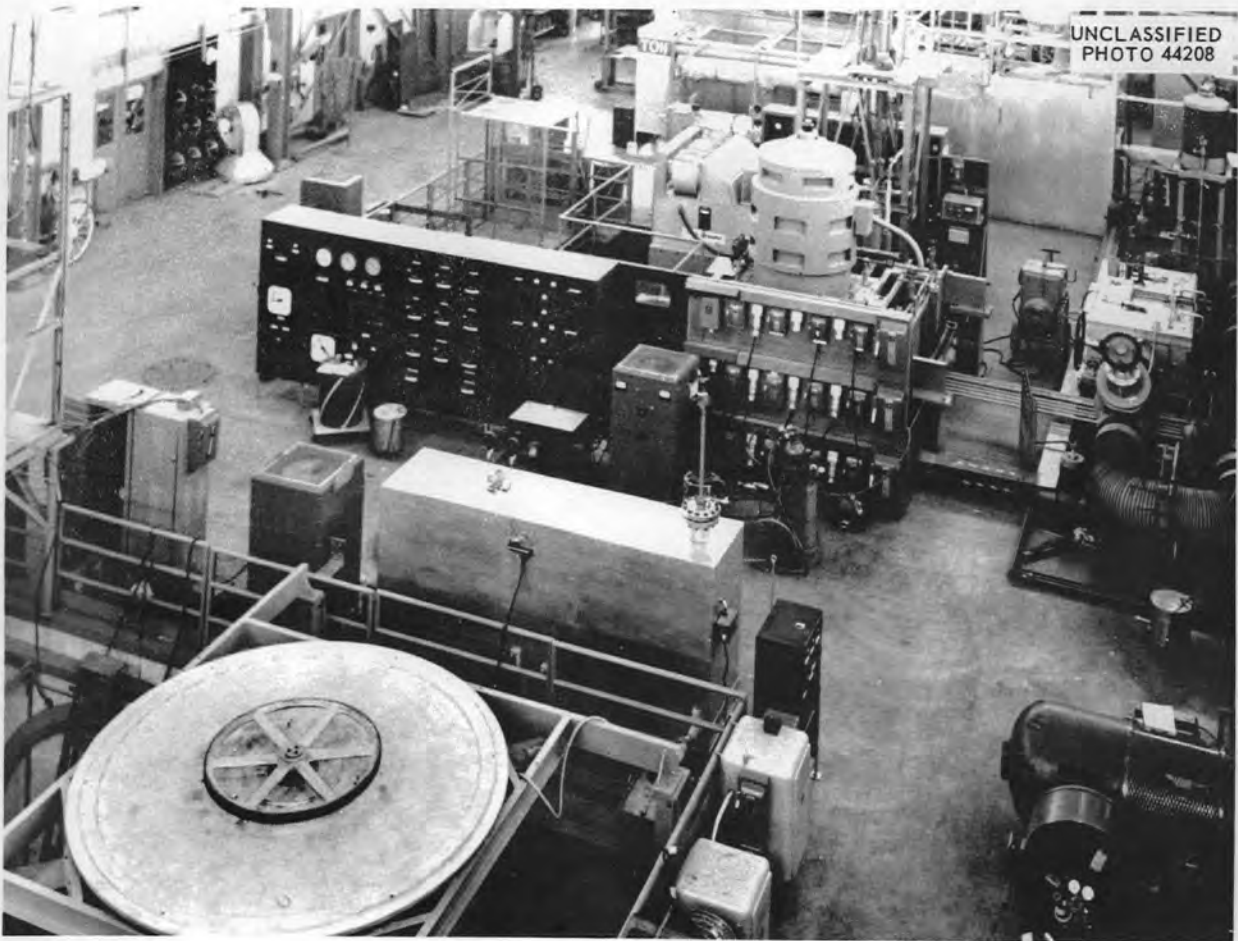


Fig. 5.10. EBR-2 Pumps on Test Stands.

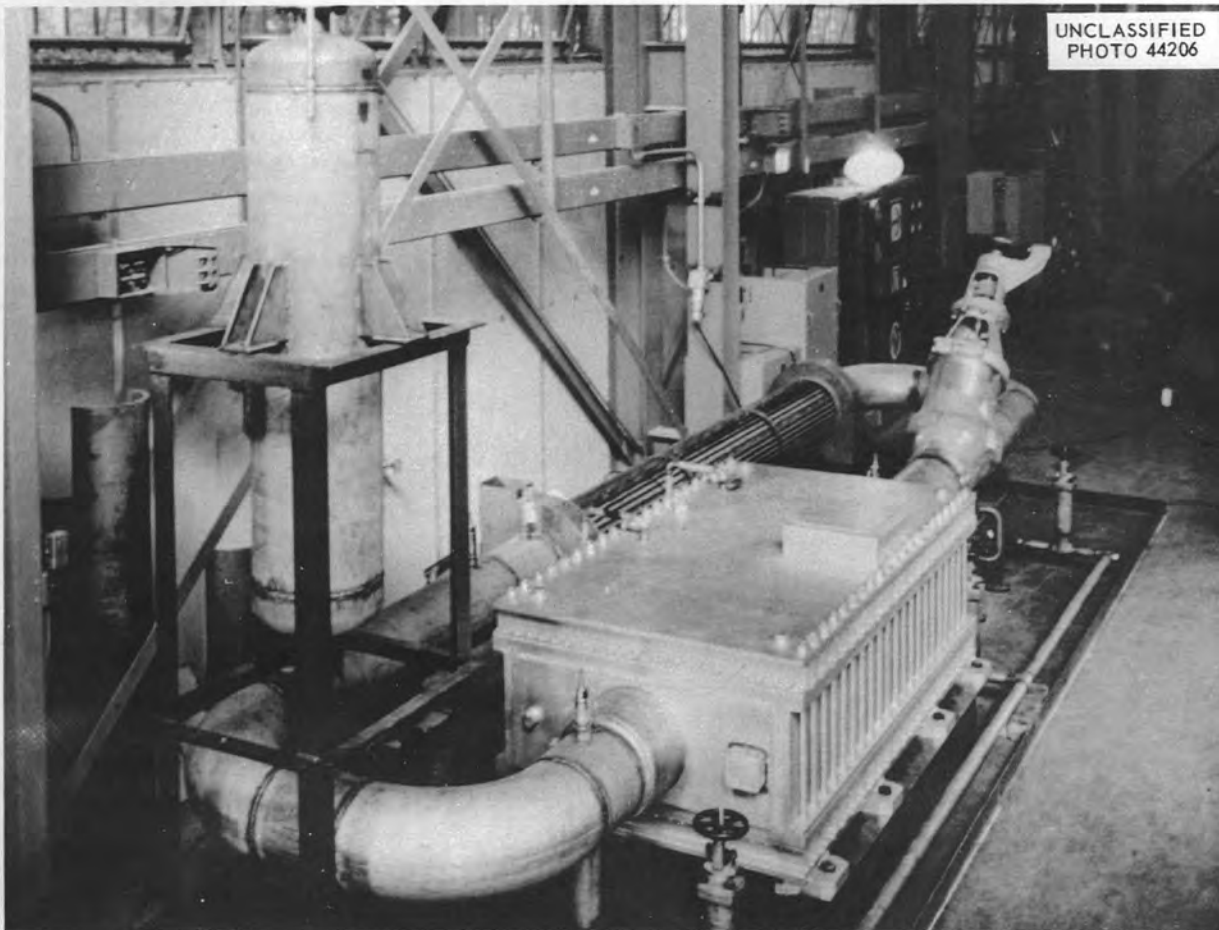


Fig. 5.11. A 5000-gpm Electromagnetic Pump.

to the instruments, the minor malfunctions of operation of this particular type of transmitter have not caused any concern.

I have not talked about our thermocouples. I don't know whether it is appropriate for this meeting. However, there is a thermocouple thimble, behind the pressure transmitter. We tend to favor No. 14 thermocouple wires in wells for experimental dynamic systems.

I might talk a little about our experience with thermocouples in EBR-1. For neutron physics measurements it is necessary and desirable to measure fuel temperatures in a very tight, fast, critical assembly. In this case we have used stainless-steel-jacketed, magnesium oxide-packed thermocouple wires about 0.0040 in. in diameter. It is calibration and the integrity of the initial installation that count. There has been no difficulty in operating the reactor with this type of thermocouple, properly calibrated and installed.

Figure 5.12 shows an over-all view of the pump test loop. It illustrates what we think is one satisfactory way for heating the system. This is 60-cycle eddy-current heating. If we do a good job of insulating, the heating is no problem. This type of insulation and heating provides high thermal efficiency of plant operation, and also puts the heater wire on the outside of the pipe. We like it. Other resistance and hot-air heating methods have been tried; they all work, but this is the one we plan to use for our nonradioactive secondary system. Shown in Fig. 5.12 is an a-c linear pump which has operated continuously for over 7000 hr at temperatures between 700 and 900°F. Instrument problems in these laboratory-type installations literally have not existed. We have had very good data. The head-capacity data on both pumps - the General Electric a-c linear pump and the Allis-Chalmers mechanical pump - agrees precisely with the manufacturers' calculated data.

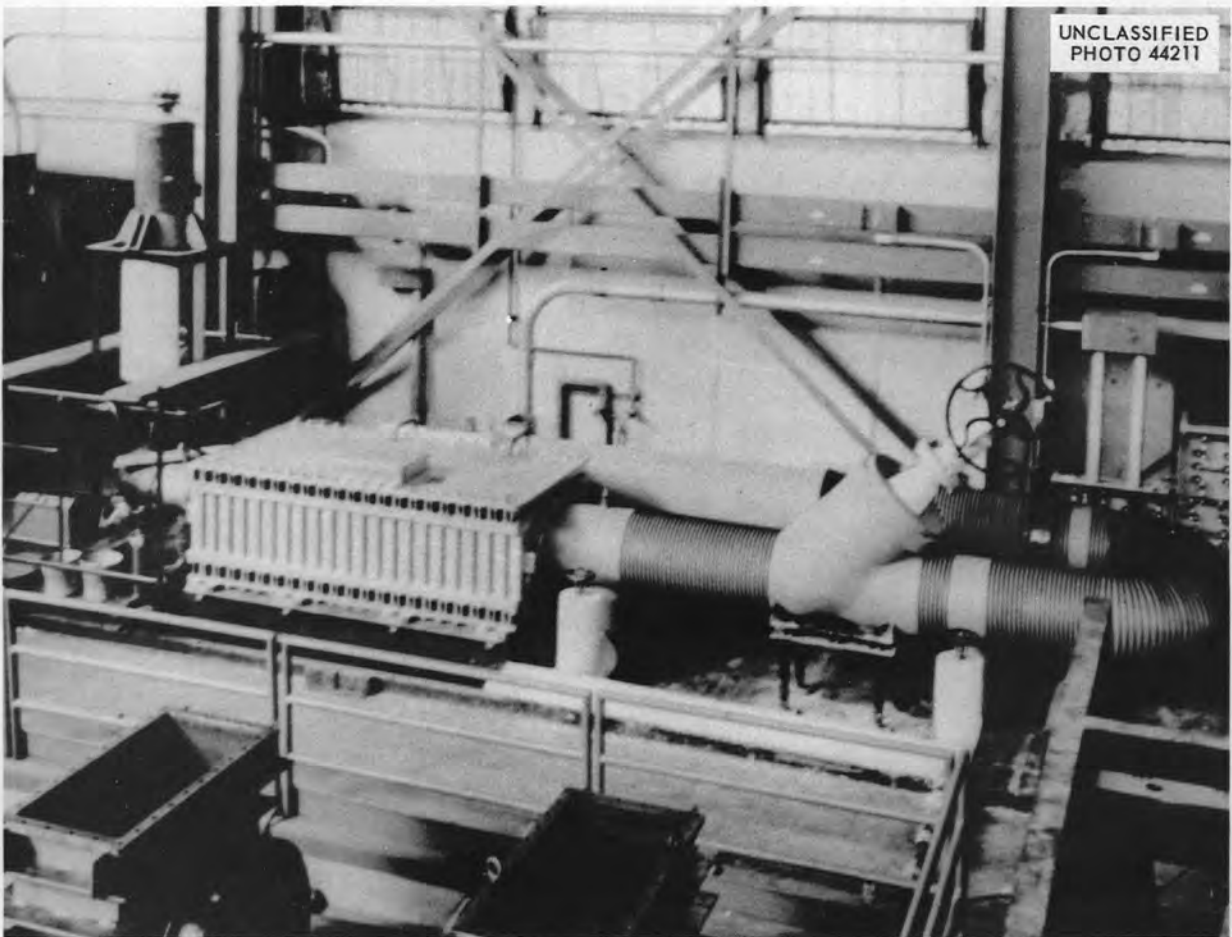


Fig. 5.12. Test Loop for A-C Pump.

The mechanical pump is shown in Fig. 5.13. The 12-in. electromagnetic flow meter shown in Fig. 5.6 is used in this section of the loop. The pump is a matter of engineering interest. The 5000-gpm, centrifugal sump pump has a controlled liquid level. The expansion tank provides a free surface of sodium.

Figure 5.14 shows a d-c electromagnetic pump, operating at 800°C. This loop was primarily built to test the pump. It was not built to test materials or instruments, because we had no reactor that required it. We started on high-temperature work primarily to demonstrate that a d-c electromagnetic pump is reliable. We used high-temperature operation as a criterion of reliability. At one time, we were seriously considering a 10,000-gpm, d-c electromagnetic pump that would operate at 700°F. Our argument was that if we could build one that will operate at 1500°F, it would be a better pump at 700°F.

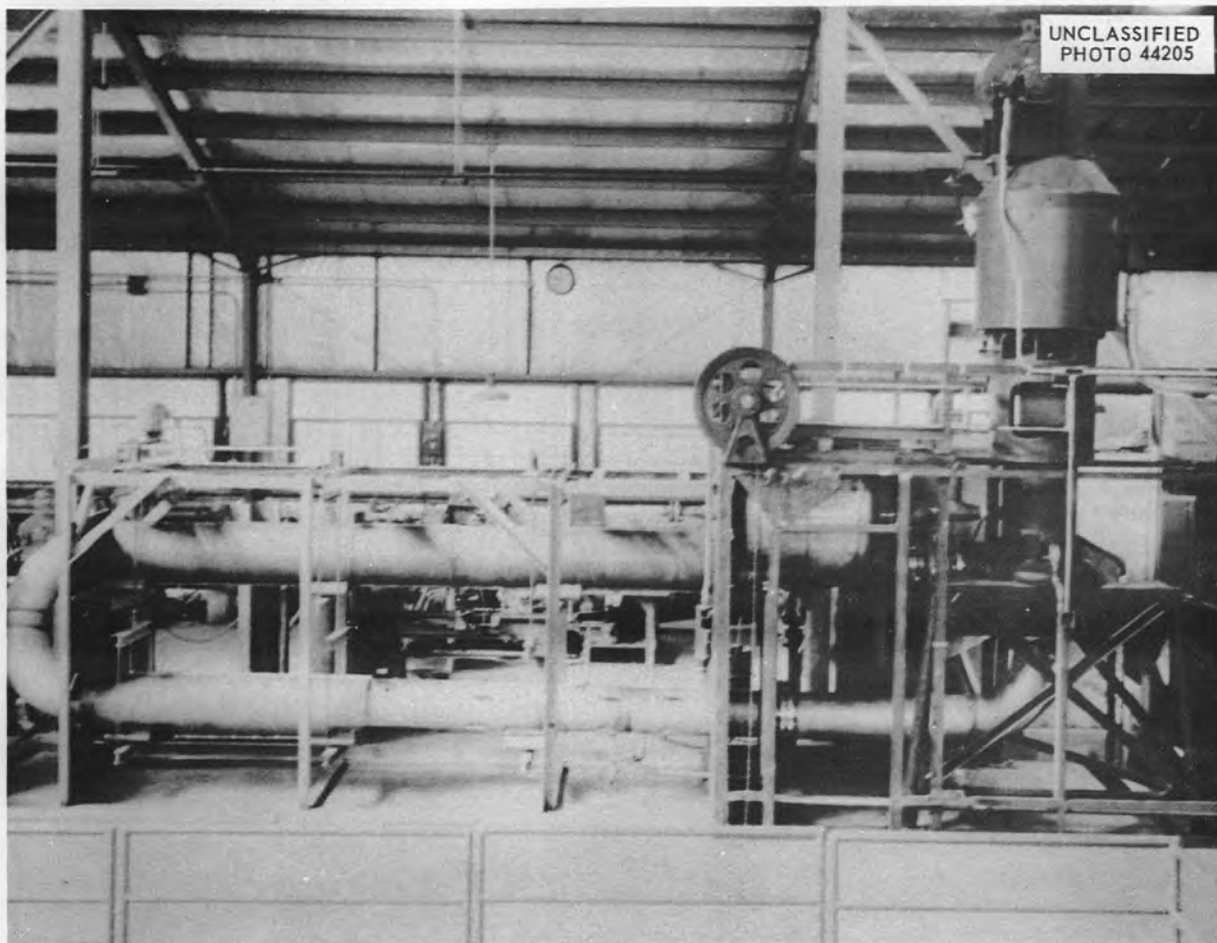


Fig. 5.13. Mechanical Sodium Pump.

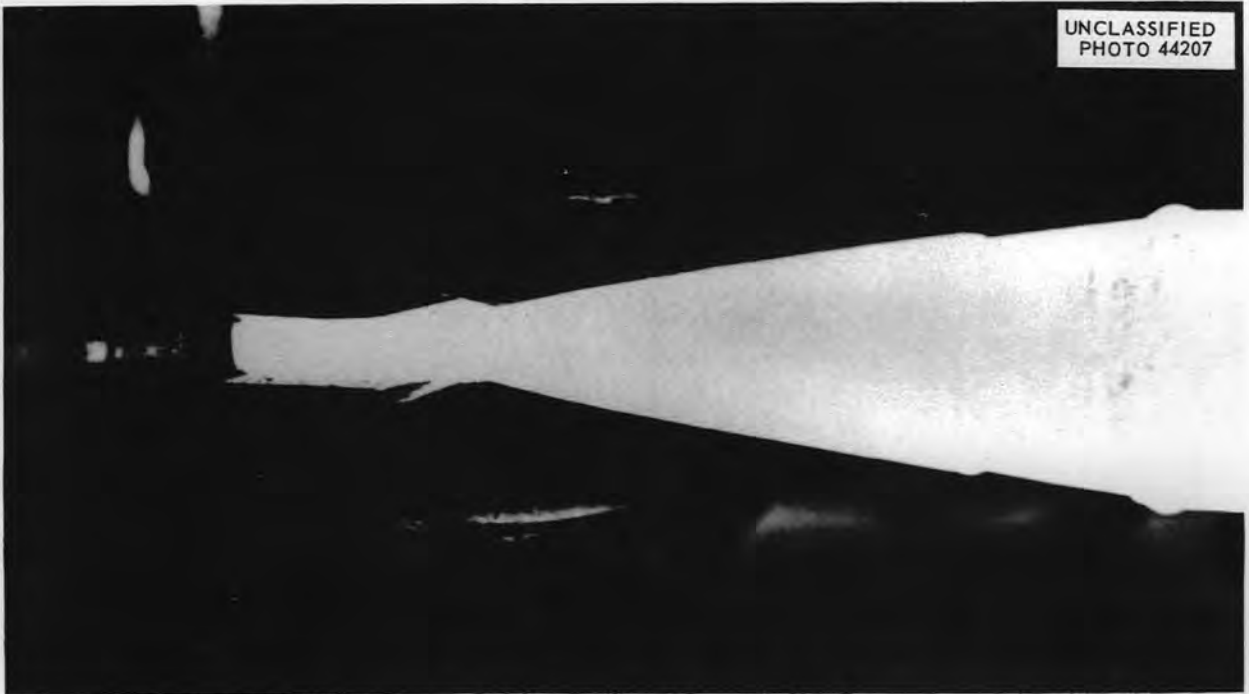


Fig. 5.14. Direct-Current Electromagnetic Pump in Operation at 800°C.

Figure 5.15 is a photograph of the loop nearing its final stage of construction. Here again we have the same old three-dollar pressure gages. The pump is not commercially available. We are using facilities like this to study the effect of hot-trapping and cold-trapping on reactor materials of construction. The one significant thing, I think, that has plagued most of our work at Argonne is the basic fact that there is no really good, simple, accurate, reliable instrument for sodium quality control. I would like to emphasize this fact to give any of you people in the instrument business a little incentive to work on this. A lot of us in the National Laboratories have done so; it is not an easy problem.

Impure material is significant. I hope to discuss this analytical procedure problem with any of you who are interested, later, because work at Los Alamos and Atomics International indicates that a few parts per million of sodium oxide does affect the corrosion of materials at high temperatures. We have been fortunate with our EBR-1 and EBR-2, because they are low-temperature reactors - 900°F, maximum. I point out the need to work on instrumentation at higher temperatures and to do a better job than I think most of us have done, because the situation in the civilian power reactor program dictates that we should come up to 1050°F steam, ultimately. If we don't, we have not really taken advantage of the unique

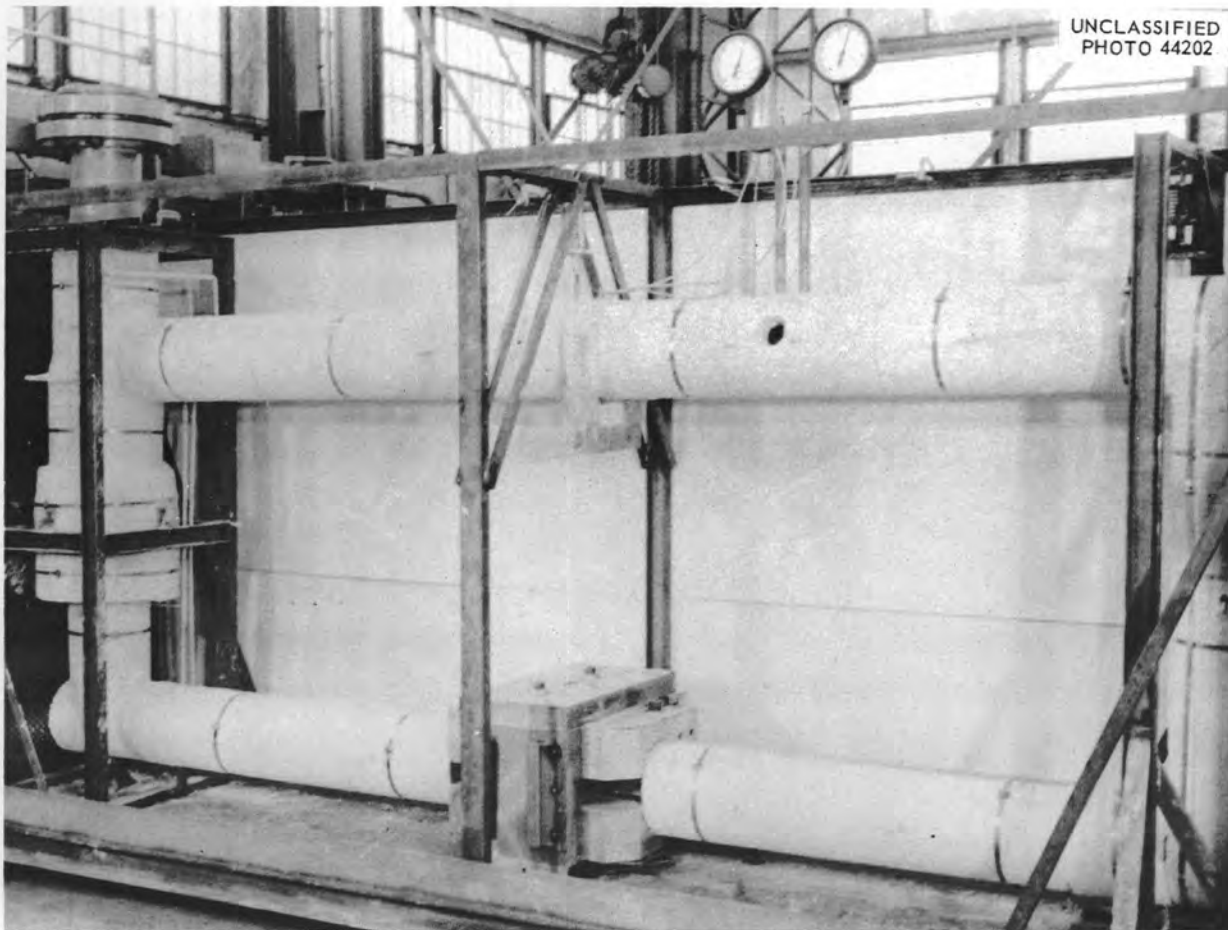


Fig. 5.15. High-Temperature Loop During Construction, Showing Permanent-Magnet Flowmeter.

properties of liquid metals as reactor coolants. The sodium components group sponsored by the Atomic Energy Commission talk of maximum sodium temperatures of 1200°F. We have not done very much work in this range at Argonne. I think more work is necessary. I hope this meeting presents the opportunity to discuss this more thoroughly.

Figure 5.16 shows a graphic panel for the EBR-2, with associated instruments.

L. P. INGLIS: You mentioned a nonlinearity associated with the 12-in. flow meter. Will you show us the type of curve you got from that meter?

R. G. AFFEL: If you recall an accuracy figure, will you state that also?

F. A. SMITH: Although the curve is nonlinear, it is smooth; the output at 5000 gpm is about 100 mv. As to our flow meter calculations, we had the opportunity to measure as nearly as possible the absolute accuracy of the permanent-magnet electromagnetic flow meter on the EBR-1. We have

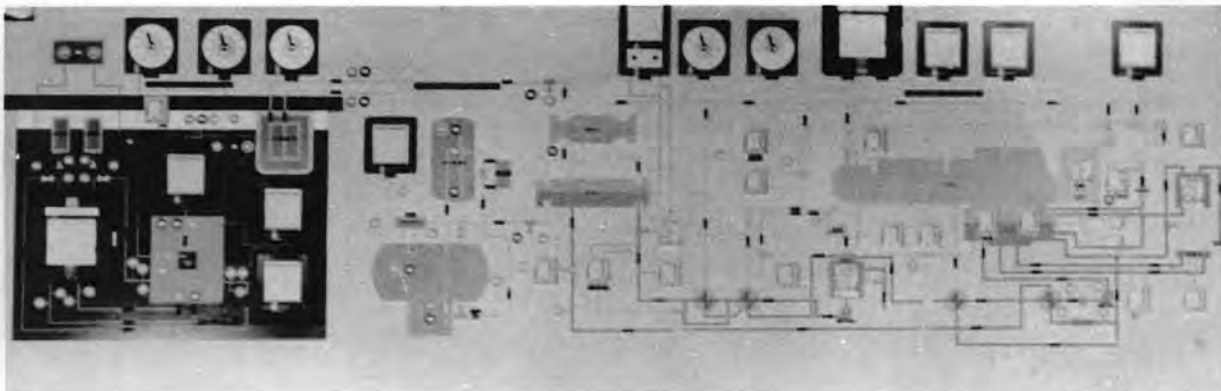


Fig. 5.16. Graphic Panel for the EBR-2.

large capacity storage tanks, a 5000-gal tank in the EBR-1 system. We precalibrated our original 4-in.-pipe electromagnetic flow meters in test loops at ANL in Chicago, using an orifice and ordinary flow instrumentation techniques or calculations. The subsequent installation of these flow meters in the EBR-1, where we could measure a change in level rather precisely in a large tank, indicated an accuracy within $\pm 2\%$.

The level of control, I think, for our EBR-1 and -2 reactors has not really been a critical item. I say this because sodium systems don't fail. They don't leak; therefore you don't have to worry about where the level is. So you should do a better job on trying to make the thing tight and then do a minimum job on instrumentation. Why? Because you save money.

L. P. INGLIS: What is the field intensity in the gap?

F. A. SMITH: On the 12-in. system, we have purchased to several different types of specifications on these magnets. For the one I illustrated, it was a rather handsome specification of 600 gauss. It was still reasonably cheap, by the way. The flow meter in the a-c linear loop is smaller, and easier to install. The intensity is only 200 gauss, and the output on a 10-in. pipe is almost 100 mv. On the large, 600-gauss flow meter, we get approximately 10 mv per 200 gauss.

L. P. INGLIS: Would you tie that in with one flow figure?

F. A. SMITH: Five thousand gallons per minute.

L. P. INGLIS: You mentioned a loop in which you employed flow control. We have found that the signals from these electromagnetic flow

meters are generally rather noisy for one reason or another. Do you use any type of filtering to clean up the signal for the control circuit?

F. A. SMITH: No, but we have the same problem with noise. I don't know whether it is noise or turbulence from that 5000-gpm flow. It is hard to pinpoint. We just turn the sensitivity of the amplifier back, and we don't worry about it.

Flux measurement is a tough one. Some of the other people at Argonne may know more about this than I do, but the EBR-1 or the EBR-2 core tank with the reactor is like this: see Fig. 5.17. Around it is, of course, a gamma and neutron shield and a thermal insulation shell. The instrument tubes sweep in roughly as shown, through the sodium. They terminate external to the core. We are not measuring any flux in the core, and currently these are gas- or auxiliary-cooled instrument thimbles.

Let me show this concentric piping design (Fig. 5.18). We submerge flowmeters and pressure transmitters by enclosing or cladding the process instruments lines, whether they are air or electric, with stainless steel tubing. I exaggerated the dimensions. Then, jacket this with an external thin-walled pipe, and the magnetic flowmeter leads are tiny electrical taps into the pipe. The emf-generated output signals come through a pipe, and on up through a bellows seal, used to compensate for thermal expansion of the pipe.

N. E. HUSTON: Do you have a coil in there for measuring magnetic-field variation?

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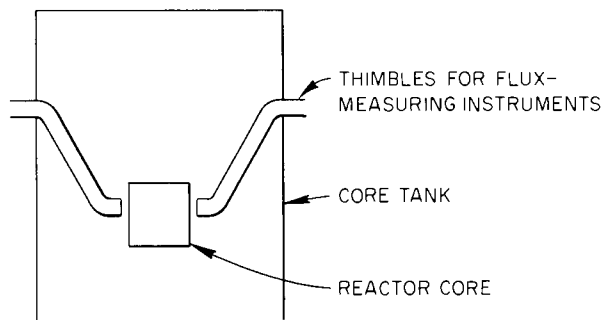


Fig. 5.17. Flux-Monitoring Thimbles.

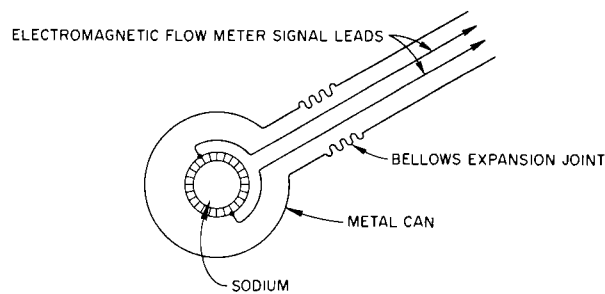


Fig. 5.18. Protection of Electromagnetic Flow-meter Leads.

F. A. SMITH: No, we temperature-age our magnet assemblies, and we have had really good luck with those. Even at 800°F, there is a tremendous temperature effect on the permeability of the magnet. We live with this. We put our permanent magnets in ovens for weeks, at a temperature above the anticipated operating temperatures. Then we calibrate them. In our reactor, this calibration would be a problem.

In general, if you are asking whether we rely on calculated flux measurements for calibrations, we don't. Because of the nonlinear effect, we want to know precisely what the flow is and not rely on a calculated formula, and we think it is more economical to calibrate the flow meter.

P. BLISS: In use, do you still get a repeatable variation between room temperature and the operating temperature, or is it constant?

F. A. SMITH: Variation between room temperature and operating temperature is another effect. There two temperature effects — you are correct. Again, for EBR-1 and EBR-2, we have a family of curves. As I mentioned, we have several ways to cross-check flow.

P. BLISS: How much do you estimate the flux will go down?

F. A. SMITH: In which reactor? In EBR-1, or at some of these high temperatures?

P. BLISS: At 1000°F and in taking it up to 800°F from room temperature?

F. A. SMITH: About 5%. Again, for most of our experimental work, we have insisted on reliability for these pump loops; the accuracy of the instruments has not really been a problem. For heat-transfer study, of which we are doing some, the situation would be different. We have done very accurate flow meter calibrations.

LIQUID METAL INSTRUMENTATION PRACTICE

P. Bliss

Pratt & Whitney Aircraft
Division of United Aircraft Corporation

Introduction

This report covers the techniques of instrumentation for liquid metal systems as practiced at Pratt & Whitney Aircraft, CANEL. The techniques and hardware described are not necessarily original.

Temperature Measurements

It is fortunate that the oxide-filled, metal-clad thermocouple became commercially available just before our liquid-metal test program started. This type of thermocouple has performed very well in all our applications where it has been necessary to immerse a thermocouple in liquid metal. The clad thermocouple which has found the greatest use in our operations is the 1/16-in. thermocouple made in our own laboratory. The thermocouple stock, as purchased, consists of a pair of Chromel-Alumel wires (approximately 30 gage) packed in powdered magnesium oxide insulation inside a 1/16-in.-OD stainless steel tube whose wall thickness is about 0.011 in. Steps in the fabrication are illustrated in Fig. 6.1. The stainless steel sheath is cut back about 1/8 in. from the ends of the wires, and the magnesium oxide removed to a depth of about 1/8 in. with a small grit-blaster. The wires and tube are then puddled with a Heliarc welder, heating the wires first. In our experience, this has resulted in a closure of satisfactory integrity. The technique is described because it is understood that some laboratories have had troubles achieving wall integrity without resorting to more elaborate techniques. While extensive closure integrity tests have not been performed, the absence of user complaint is considered of some significance. The other end of the thermocouple is sealed with deKhotinsky cement, since it is usually near ambient temperature. A quick-disconnect plug is attached as shown in Fig. 6.2.

In addition to the above, a few 1/8- and 1/4-in. thermocouples have been used. The same techniques were used for fabrication. Inconel has also been used as a sheath material. Based on numerous calibrations, the initial accuracy of these clad thermocouples can be assumed to be within ISA specifications for Chromel-Alumel. Higher accuracy can be achieved

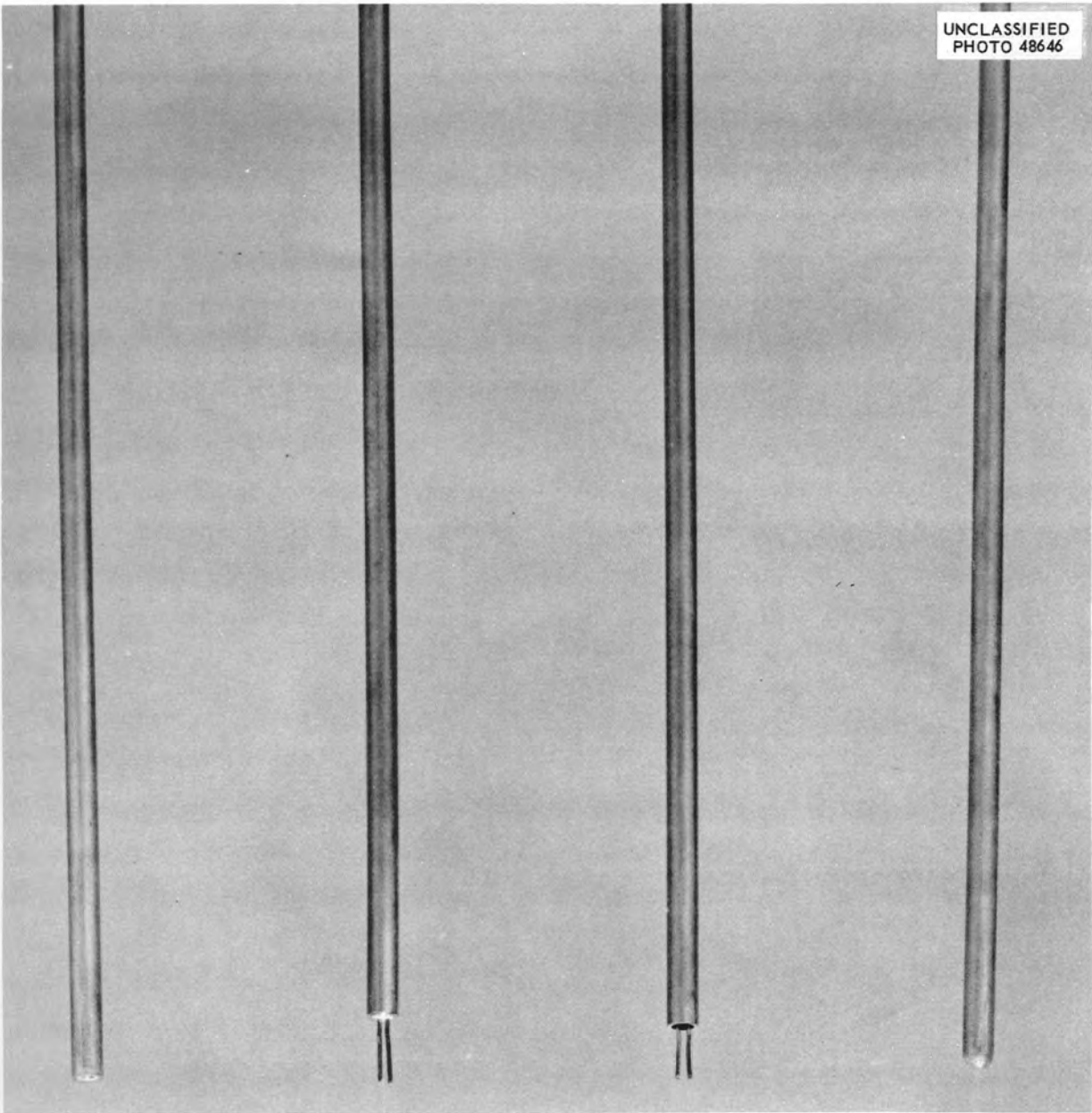


Fig. 6.1. Steps in Fabrication of a Clad Thermocouple.

by individual calibration. The calibration stability appears to be superior to that of nonclad thermocouples.

It may be of interest that this magnesium oxide construction has also been used with copper wires for passing through container walls where integrity is required. In this case, flash-sintering techniques have been used to seal the open ends of the cable against moisture. Magnesium oxide is quite hygroscopic, and must be sealed when high leakage

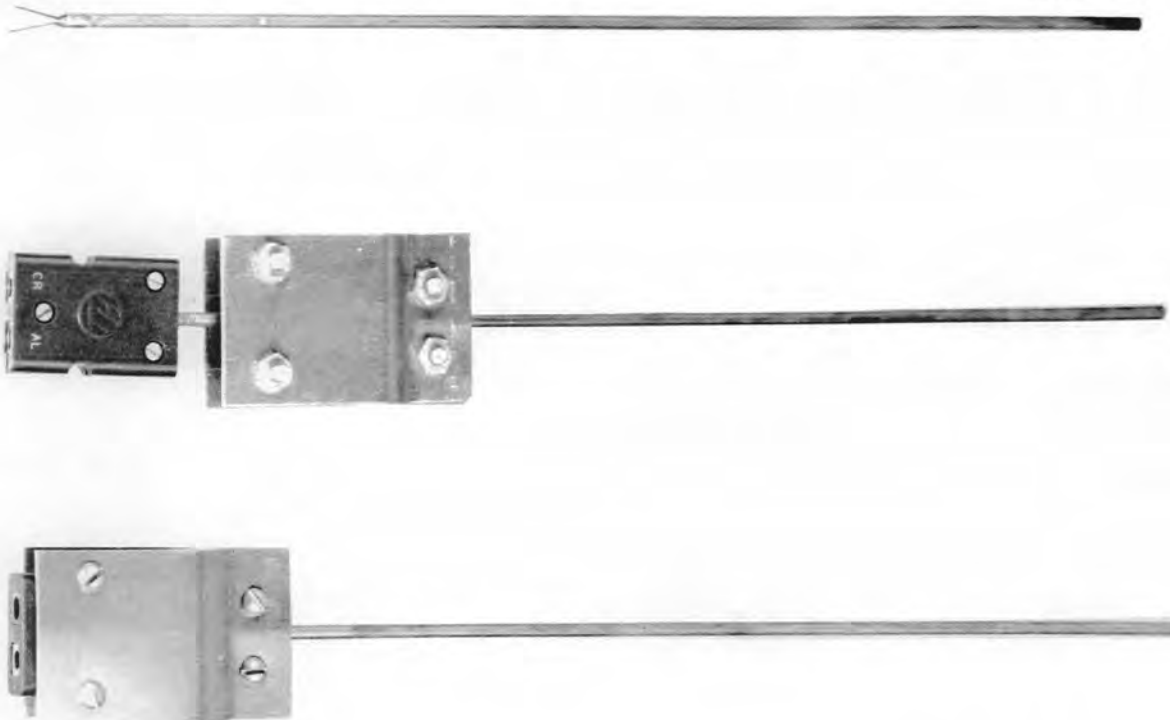


Fig. 6.2. Attachment of Quick-Disconnect Plug.

resistance must be maintained. Aluminum and zirconium oxides are reportedly better in this respect.

Because of the excellent heat transfer qualities of liquid metals, wall temperatures agree quite closely with liquid temperatures, particularly if the vessel or plumbing is thermally lagged over the thermocouple. Where wall temperatures are used, it is our usual practice to weld the individual unclad thermocouple wires to the surface, rather than to form a bead which is subsequently welded to the surface. If the surface is not lagged, a considerable gradient can exist between the wall and the effective junction of the thermocouple wires — we have noted errors as large as 50°F. Another consideration in the choice between the beaded junction and separately attached wires comes from the consequences of junction failure. On a lagged surface, if one of the separately attached wires becomes detached or broken, the condition is readily recognized by the loss of signal. With the beaded junction, the bead can become detached from the surface and produce erroneous readings not easily recognized because the junction is still near the surface temperature.

Usually, a clearly recognizable failure is preferable to an erroneous indication.

Thermal-conduction errors are minimized by dressing the thermocouple leads near the junction along isothermal lines. The speed of response of wall-type thermocouples is usually determined by the wall rather than by the thermocouple. However, in some cases where wall sections are thin, and fast response is required, ribbon-type thermocouples have been used successfully. The ribbon used is 0.0025 by 0.015 in. Clad material is frequently used as the extension lead. This limits the ribbon length to about an inch, for mechanical strength.

Our experience with removable thermocouples in wells has not, in general, been good. In the first place, the equality of junction temperature and liquid temperature is open to question, because of questionable heat transfer between the wall and the wire. In the second place, Chromel-Alumel thermocouples frequently suffer rapid deterioration in relatively deep wells, which do not have adequate air circulation. It has been suggested that this effect may be due to selective oxidation of the constituents of the Alumel wire.

Pressure Measurements

The problems of the measurement of pressure in liquid metal systems are basically those of corrosion and temperature compensation. Pressure sensors are usually dead-end devices and can frequently be operated at temperatures somewhat lower than system temperature. This can be achieved by mounting the sensor a foot or more away from the pressure tap, because of the relatively poor heat transfer to the dead-end sensor volume. When this is done, the lower sensor temperature and the absence of flow reduce the corrosion problem to such a degree that materials can be used which would be unsatisfactory in hotter sections of the system where flow is present.

Where accuracies within 5% or so are adequate, or where the sensor is exposed only to liquid-metal vapors in inert gas, Bourdon-tube pressure gages have been used. Except for using stainless steel for the Bourdon tube and all fittings, these gages are completely standard. Because of the remote possibility of rupture, they are never mounted in the operator's test panel. Welded connections are used throughout.

The effect of temperature on sensor calibration strongly suggests the use of a force-balance system, in which the spring characteristics of materials are not a factor in calibration. The Moore Products Company model 15 liquid-metal transmitter shown in Fig. 6.3 operates on this principle. Also shown is the same type of transmitter with the standpipe cut away to show the sensing bellows and nozzle push rod. When used with its associated booster pilot, the nozzle acts to keep the inert gas pressure within the bellows at all times equal to the liquid-metal pressure outside. While it is true that the bellows movement is small, the real temperature independence stems from the balance of pressures, independent of bellows characteristics. Sensors lacking this force-balance feature tend to be temperature sensitive, in spite of their small movement.

The flow diagram of the Moore transmitter as used to measure pressure is shown in Fig. 6.4. This figure shows the pressure transmitter, the booster relay, and a pressure gage, for which a recorder or pressure controller can, of course, be substituted. The pressure on the gage is the same as the liquid-metal pressure, no conversion to 3-15 psi range being provided in this system. The supply pressure must always exceed the pressure being measured; otherwise the gage will merely read supply pressure, and the transmitter bellows is apt to be deformed. Inert gas is used in the system to prevent severe corrosion of the nozzle block at high temperatures.

The temperature compensation of this transmitter was verified in the test rig shown in Fig. 6.5. The transmitter is seen at the right, with a Calrod heater wound around the bellows section, covered with thermal lagging material. Gas pressure was applied to the inlet of the model 15 transmitter and to one side of a differential pressure transmitter whose output was connected to a 0-5 psi gage. The other input of the differential transmitter was connected to the output of the model 15 transmitter. Thus the gage reads the difference between the liquid-metal transmitter input and output. Readings were taken at 10, 25, 50, 100, 150, 200, and 250 psi, at room temperature and at 1400°F.

The first tests indicated a linearity within $\pm 0.12\%$ of full scale at 1400°F and $\pm 0.04\%$ at room temperature. However, the zero shifted +2.5 psi in raising the temperature through this range, and recovered only 0.4 psi

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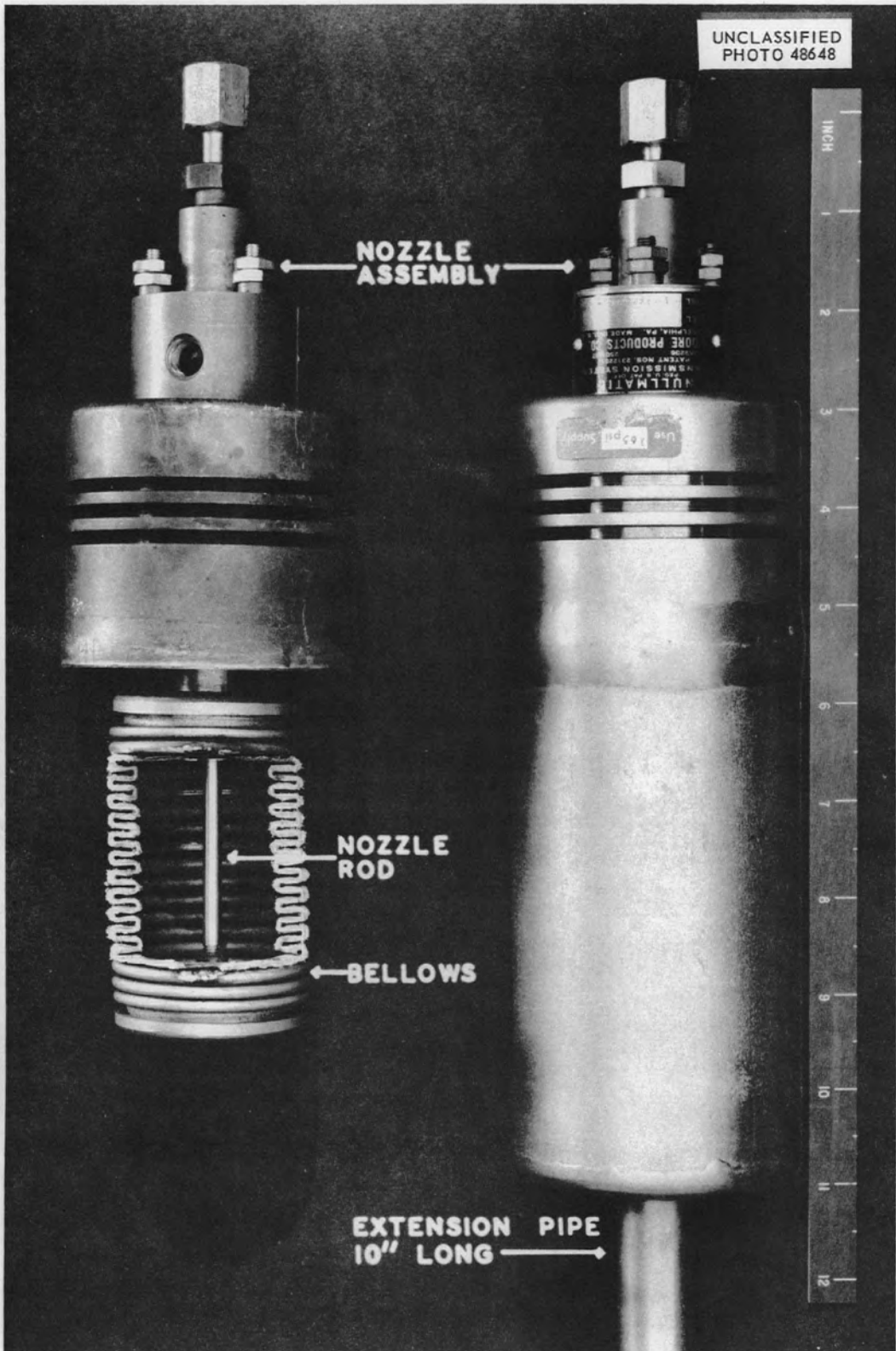


Fig. 6.3. Moore Products Company Model 15 Liquid-Metal Pressure Transmitter.

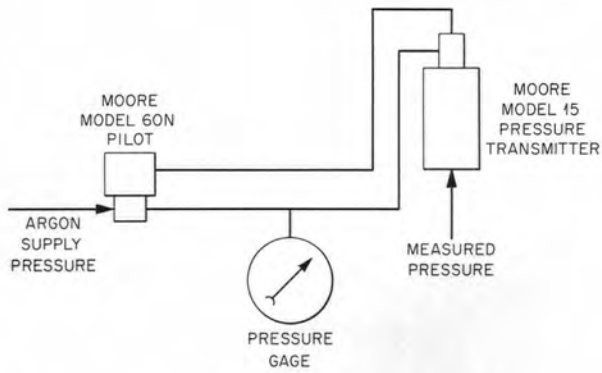
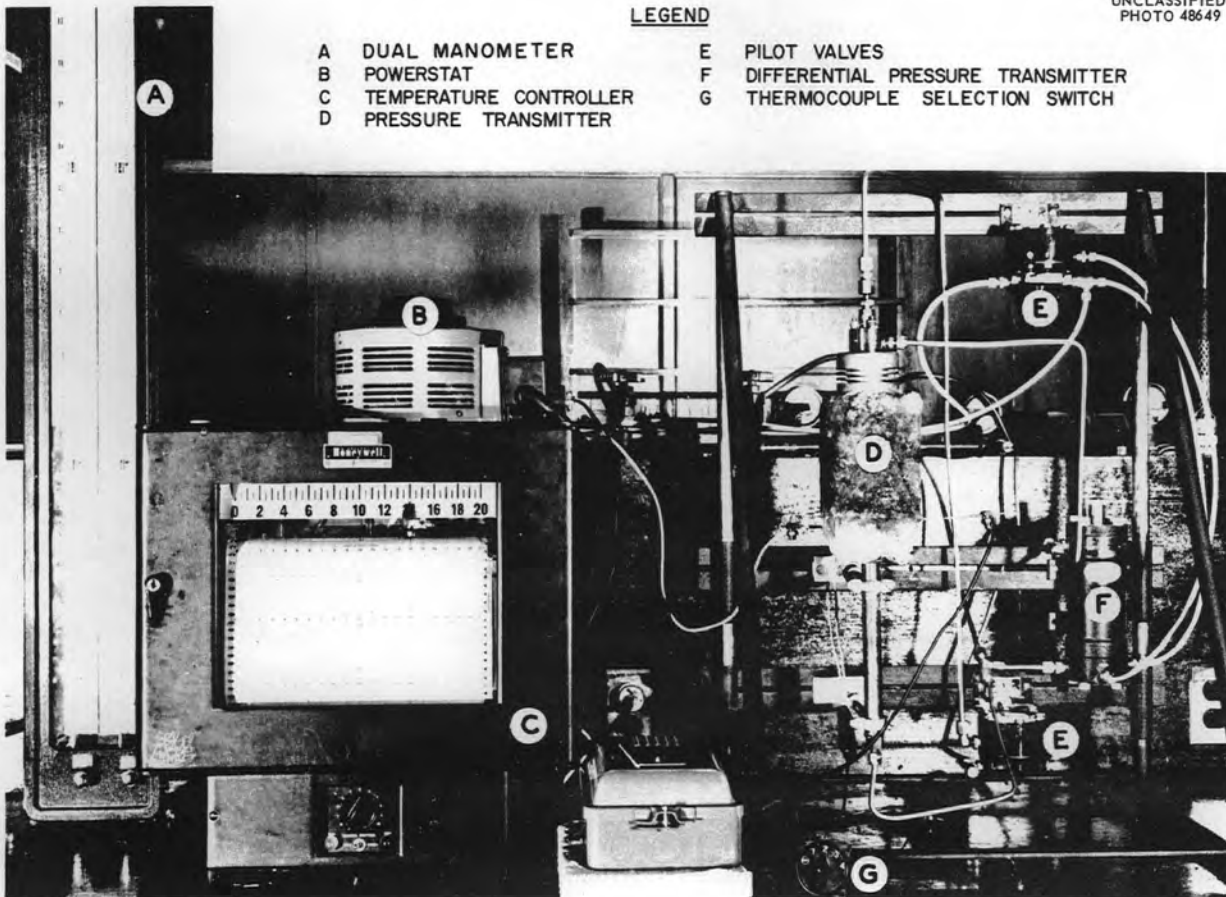


Fig. 6.4. Flow Diagram of Pressure Transmitter.



- LEGEND**
- | | |
|--------------------------|-------------------------------------|
| A DUAL MANOMETER | E PILOT VALVES |
| B POWERSTAT | F DIFFERENTIAL PRESSURE TRANSMITTER |
| C TEMPERATURE CONTROLLER | G THERMOCOUPLE SELECTION SWITCH |
| D PRESSURE TRANSMITTER | |

Fig. 6.5. Test Rig for High-Temperature Pressure Transmitter.

in returning to room temperature. Successive readjustment and recycling merely resulted in using up all the adjustment provided.

Two causes for this creep shift were found, and corrected. The first problem was that, while the bellows was made of Inconel, as ordered, the nozzle rod was made of type 316 stainless steel, as standard production. Replacement of the nozzle rod with an Inconel rod reduced the shift to +0.25 psi on the first cycle and nearly zero on successive cycles; however, a small cumulative positive shift remained. This was found to be due to insufficient area of the annular seal between the transmitter body and top works. The radial thickness was doubled and the shift disappeared. Before-and-after compensation curves are shown in Fig. 6.6. Incidentally, in present production these corrections have been made at the factory.

In the installation of Moore transmitters in systems filled with a liquid metal which is frozen at room temperature, attention must be paid to drainage. When a transmitter filled with frozen metal is thawed out, serious distortion of the bellows can result. The transmitter can be installed above the pressure tap, trapping gas in the transmitter, if head corrections are known. It can be installed below the pressure tap if provision is made for filling and draining. Some transmitters have such a drain provided. The transmitter works equally well in either position.

The transmitted gas pressures are accurate to within $<0.1\%$, which is sufficient to permit differential pressure measurement in most cases by using a differential instrument in the gas system. A liquid-metal differential transmitter is not available from Moore at this time.

Another transmitter, used at ORNL but not yet at CANEL, is made by the Taylor Instrument Companies. Although it is not a force-balance instrument, reports from ORNL indicate it is very satisfactory. This transmitter is available in differential form. Both pneumatic and unbonded strain gage outputs are available. An older pneumatic version is shown in Fig. 6.7. This transmitter consists simply of a NaK system, isolated from the process liquid by a diaphragm, and acting on a Bourdon element whose motion is sensed by the pneumatic or strain gage system.

Liquid Metal Leak Detection

The liquid metals in general use in the reactor field present several hazards if they escape from their containers. They are usually hot, they are always extremely corrosive, and they are all pyrophoric to varying

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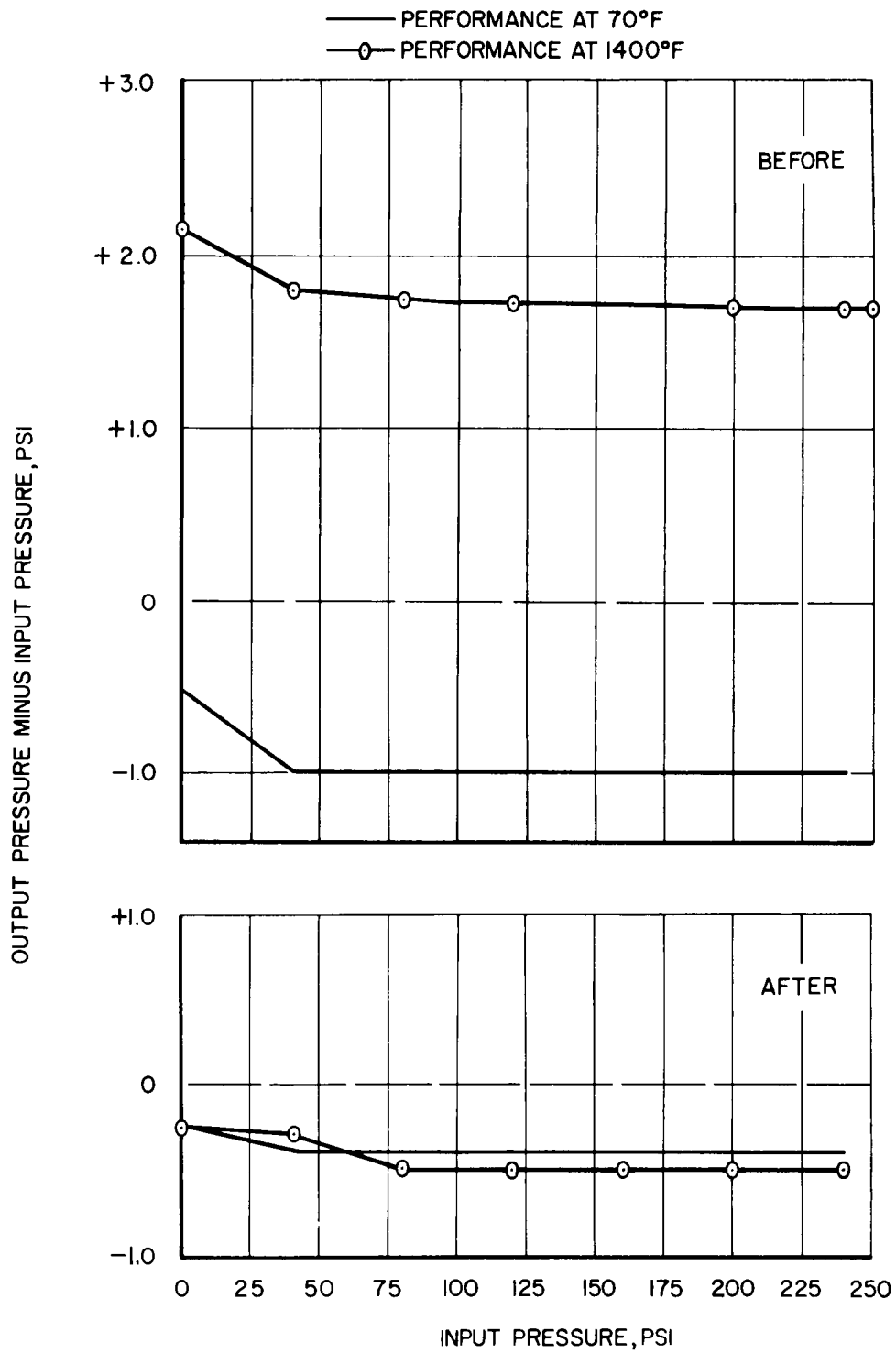


Fig. 6.6. Performance Curves, Before and After Improvement of a Moore High-Temperature Pressure Transmitter with a 0.020-in.-Thick Welded Bellows.

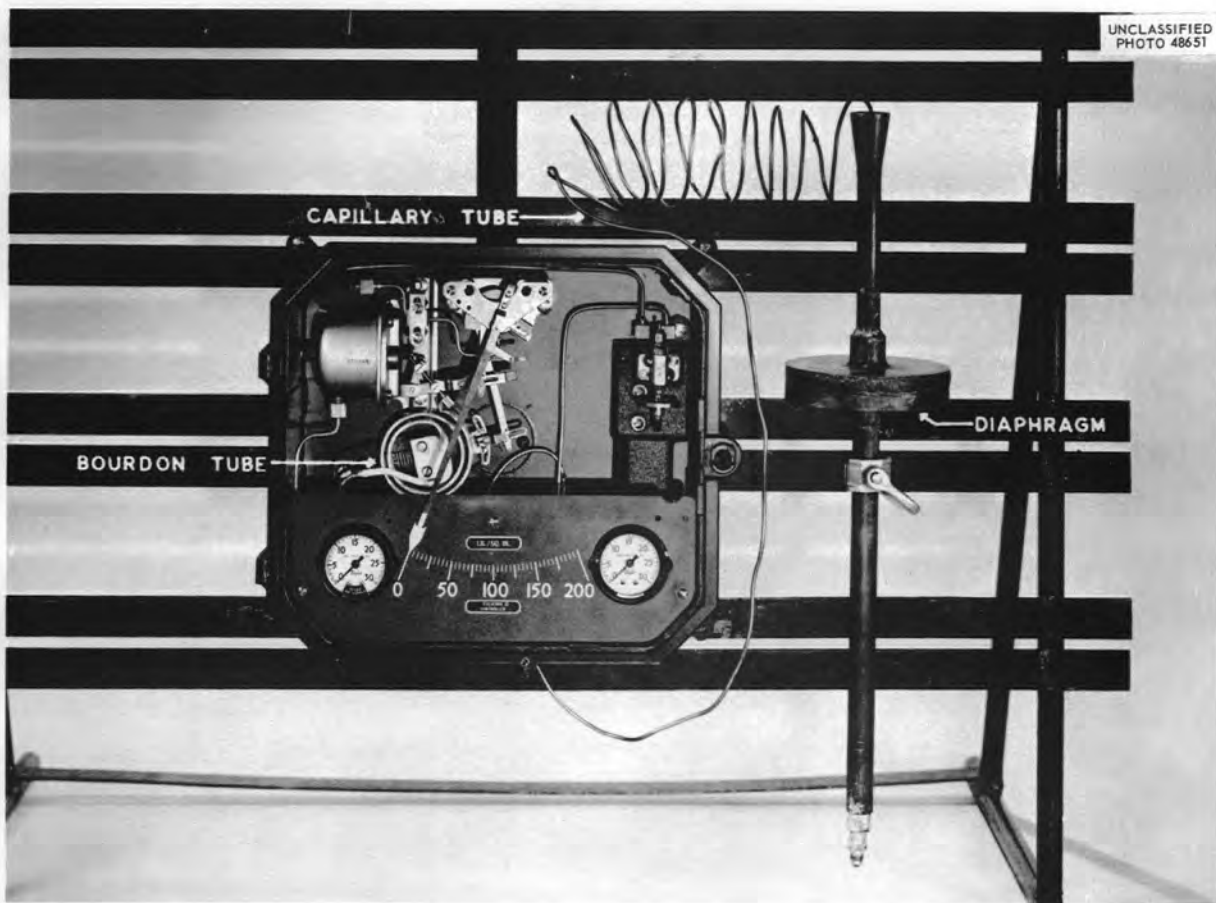


Fig. 6.7. Pneumatic-Output Pressure Transmitter.

degrees. The behavior of a leak depends on the liquid metal involved, the container material, the temperature, the atmospheric environment, and the nature and size of the leak. The effect can range from a self-sealing deposit of liquid metal oxides and products of reaction with the container to a spreading or bursting of the container with a serious fire. Early detection of leaks is therefore important in some cases.

Leakage of liquid metal from piping is detected rather easily. Even slight leakage in uncovered sections can usually be detected visually, either by sputtering, smoke, or surface deposits as noted above. Covered sections can be wound, prior to covering, with conductors such as that shown in Fig. 6.8. This is a tape, made by the Russell Manufacturing Company, consisting of stranded No. 18 Chromel wires held in place by weaving in a Fibrefrax tape. Liquid-metal leakage is detected by electrical conductivity between the wires. Part of the tape disintegrates at

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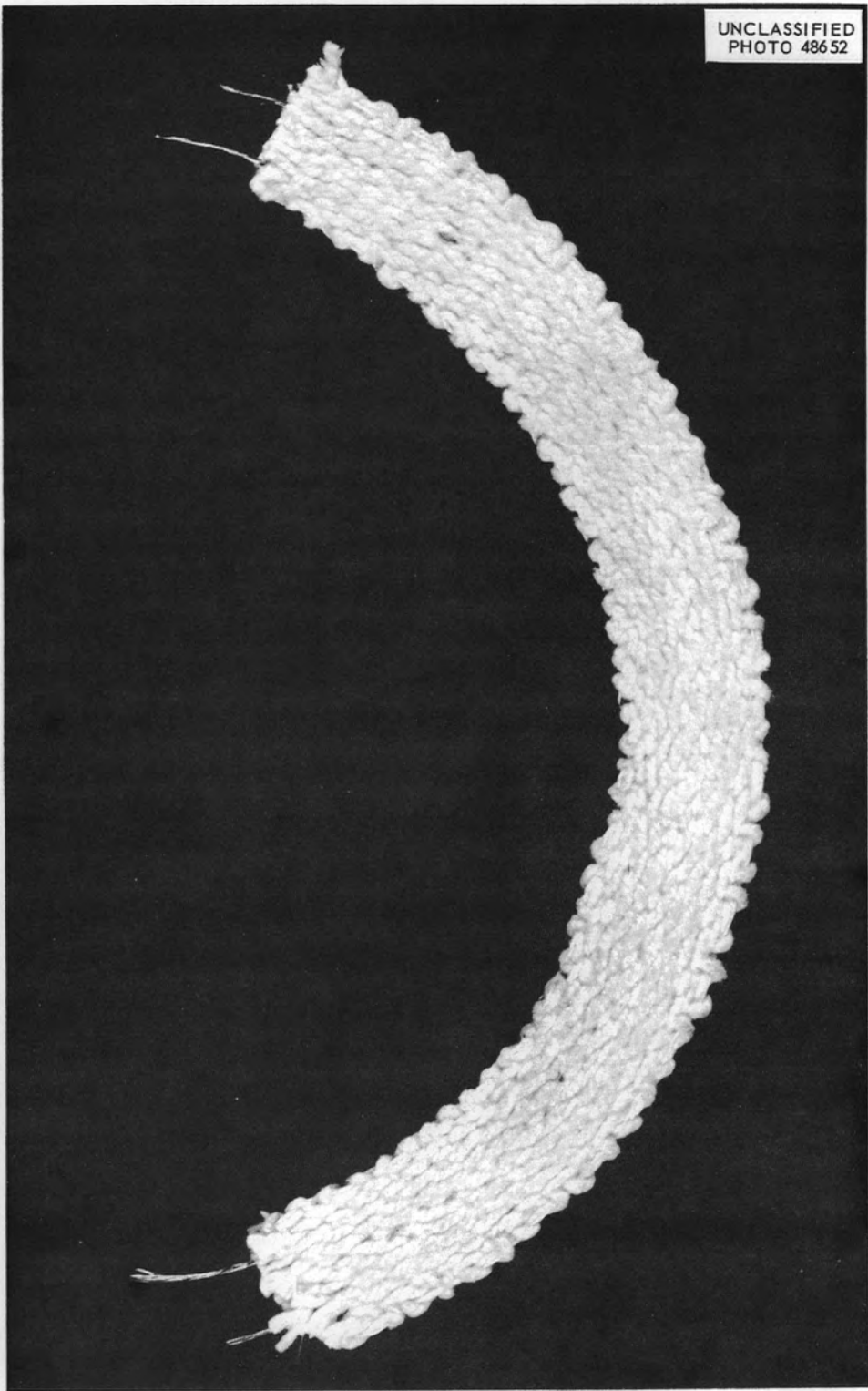


Fig. 6.8. Leak-Detecting Tape.

1600°F, but the wires are held in place by the thermal lagging. Electrical circuits can be arranged for any necessary degree of localization of the leak.

The problem of detecting leaks from a radiator into the airstream of a jet engine is much more difficult, and we do not yet have a satisfactory solution. Here the liquid-metal container inherently has a very large surface-to-volume ratio and thin walls. A large part of the surface is completely inaccessible. The radiator usually is not visible to the operator. Heat and vibration of the engine is intolerable for most optical equipment, and, while sampling methods are necessarily being considered, the sampling principle is questionable. Because the radiator will probably burst once its integrity is destroyed, it is desirable to identify and isolate a leaking radiator while the leak is still small. At present, however, there is some evidence that the leaking metal will oxidize and may stay on the surface of the radiator, and not appear in the airstream until the leak is fairly large. Various sampling detectors considered have been based on smoke detection and emission spectrometry. Both presume that the only source of particles would be from the liquid metal. The smoke detector has been used with some success on test radiator sections.

Another principle which has not proved particularly successful is based on the fact that the gas temperature cannot rise after passing through the radiator except by reaction of liquid metal. This effect is too localized to be useful.

Liquid Level Measurements

The traditional method of measuring liquid-metal level has been with contact probes, made by modifying automotive spark plugs as shown in Fig. 6.9. The plug which we have found most satisfactory is the BG number 212. A probe wire, compatible with the liquid metal, and of appropriate length, is welded to the center electrode. A 24-v a-c supply is used for operator safety.

The spark plug probe has the advantage of low cost. Against this are the disadvantages of fouling and the ability to give only a single-point indication as to the liquid-metal level. The most serious disadvantage is that, due to the exposure of the probe and its insulator, the



Fig. 6.9. Spark Plug Modified for Measurement of Liquid-Metal Level.

unit is easily short-circuited, either by the probe's touching the wall of the vessel or by condensation of liquid metal on the insulator.

Because of the above disadvantages, various schemes were tried as a basis for non-fouling probes which could give a continuous indication of level. The first used an inductance probe consisting of a coil of insulated wire inside a metal tube. It was discovered, somewhat accidentally, that such a probe was very sensitive in mercury at room temperature, provided that the turns of the coil were rotated nearly parallel to the axis, so that the coil actually had the appearance of an inductance symbol on a schematic. Fabrication difficulties of a high-temperature model, in combination with a need for a probe suitable for non-conducting salts, put a stop to this project. There is some question whether this probe would be affected by clinging of liquid metal to the outside of the tube after a decrease in level.

The second principle tested used a heat-loss probe, consisting of a heater in a tube, maintained at a temperature higher than that of the liquid. Power required to maintain this temperature was a function of the depth of immersion. Working models were built and tested at room temperature. Figure 6.10 shows a model of this probe.

At this point interest centered in the "J-probe" developed by ORNL and General Electric and shown in Fig. 6.11. This probe functions by measuring the change in resistance of the short leg as it is shorted by liquid metal. This is done by measuring the voltage drop in the short leg at constant current. The probe consists of a tube with two current leads and two potential leads of Inconel wire, insulated from the wall of the tube by ceramic beads or powdered magnesium oxide. One current lead and one potential lead are connected to the top of the short leg;

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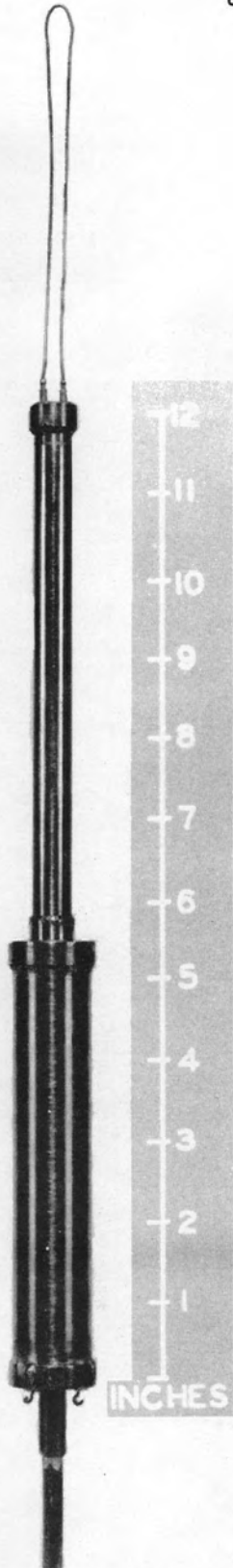


Fig. 6.10. Heat-Loss-Type Level Probe.

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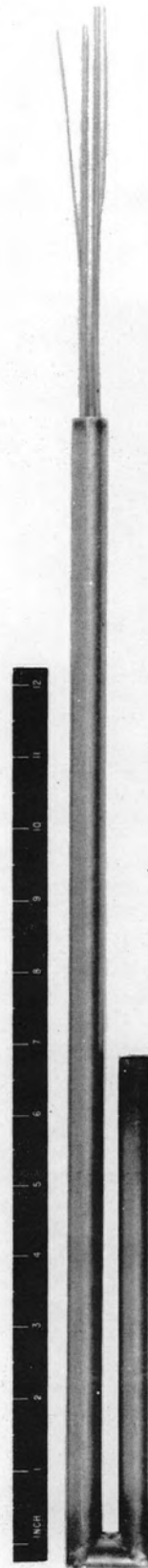


Fig. 6.11. A J Level Probe.

the other pair to the bottom of the long leg. The figure shows the first probe we have successfully fabricated. No test data are yet available. However, reports from ORNL on this type are good.

Flow

Liquid-metal flow is usually measured with the electromagnetic flow meter, which has been adequately described in the literature. Certain precautions are worth mentioning, however. Horn-type Alnico V permanent magnets of the type shown in Fig. 6.12 have been used in sizes ranging

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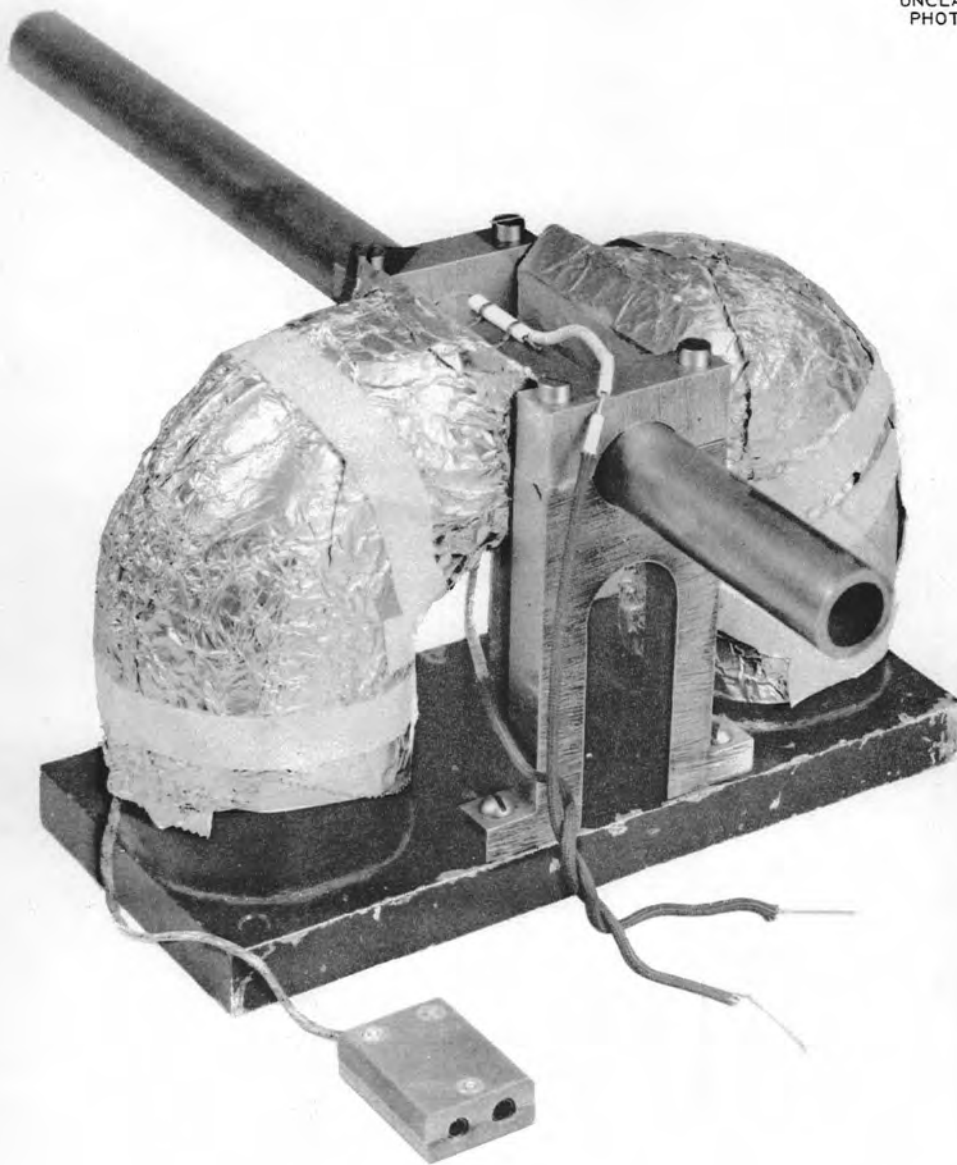


Fig. 6.12. Electromagnetic Flowmeter.

from 1/2-in. pipe, and smaller, to 6-in. pipe. The pole face is covered with asbestos sheet and wrapped with aluminum foil to reduce heating of the magnet. A thermocouple is attached to the center of one pole face to monitor magnet temperature. The flux density in the air gap at room temperature ranges from 2500 gauss in the small sizes to 500 gauss in the largest and is measured with a search coil and flux meter. For accuracy and reliability, pins can be welded into radial holes in the pipe, giving accurate location of the voltage taps. The voltage is usually between 1 and 50 mv and is measured with a self-balancing potentiometer. Measurements of the inside and outside diameters of the pipe are taken as near as possible to the point of voltage measurement. Over-all accuracy in the half-inch size has been found, by comparison of measured and calculated values, to be within $\pm 1\%$.

Flow in larger systems has been measured with venturi and Gentile flow tubes and Moore pressure transmitters. Operation is normal and needs no comment.

Flow mapping in liquid-metal systems has been considered. Kiel and Pitot tubes have been designed for water systems, and we believe that limited use in liquid metal might be practical. The frequency response of available transducers would probably limit us to measurement of steady-state pressures.

L. P. INGLIS: Will you elaborate on the use of the Pitot tube for flow mapping in liquid metal?

P. BLISS: We have not done it, but believe it would be possible to a limited extent. The presence of liquid metal in the probe and transducer would be a severe problem, as you suggest.

J. E. OWENS: Is your hot-wire level probe a single-level indicator?

P. BLISS: No, it is a continuous indicator with a length of about 3 in. Response time is of the order of 5 sec.

S. A. HLUCHAN: What is the outside diameter of the probe?

P. BLISS: About 40 mils.

R. C. FAUGHT: Are you interested in liquid-metal leakage into your thermal protection at temperatures lower than 800°F? I ask because of failure of sodium to wet the wires at lower temperatures.

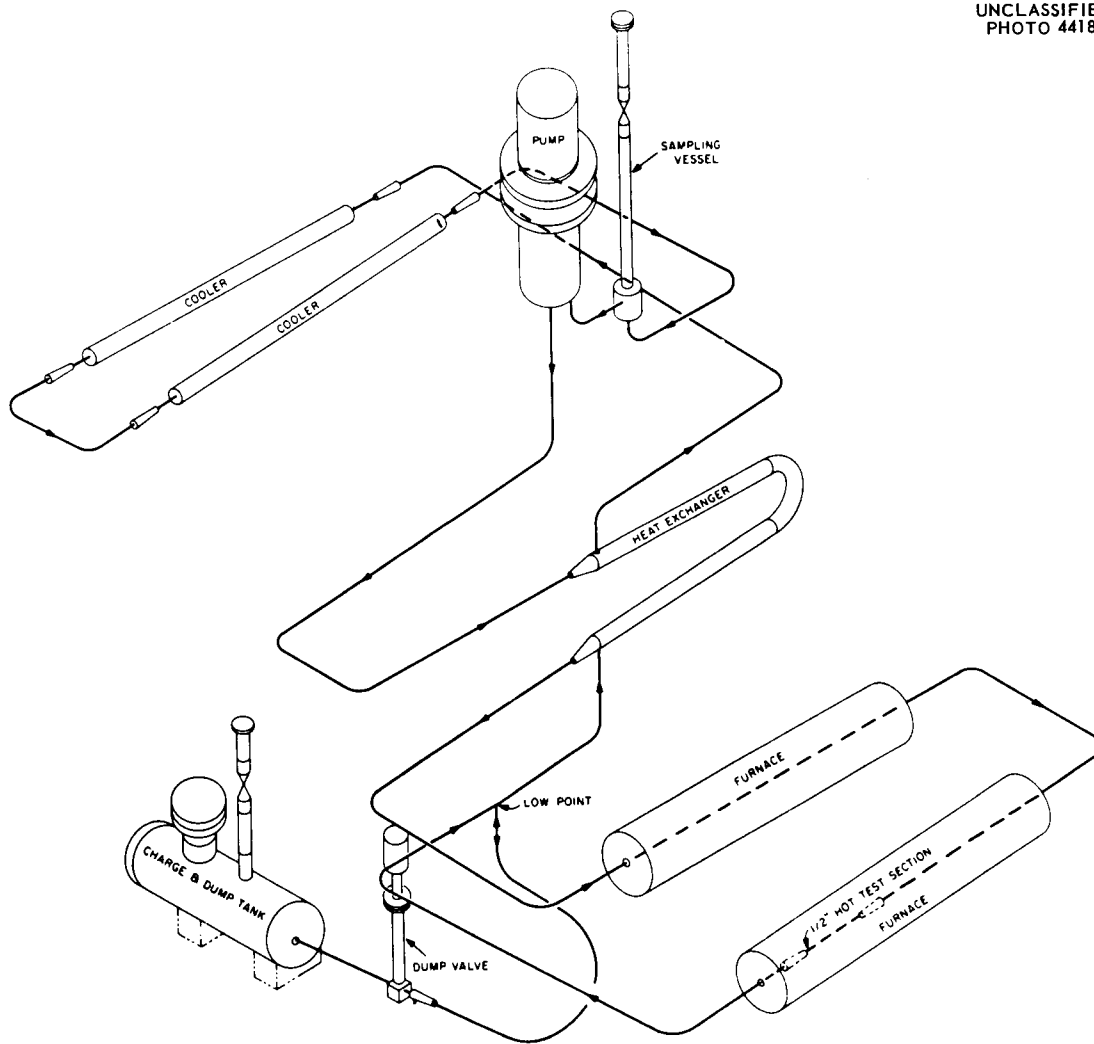
P. BLISS: Our present test data on this tape is insufficient to answer this question.

SPECIALIZED INSTRUMENTATION FOR LIQUID-BISMUTH LOOPS

D. W. Huszagh
Brookhaven National Laboratory

The nature and purpose of our work is the first thing I would like to discuss. It centers around the construction and operation of piping loops in which metallurgical test specimens are placed, and through which molten bismuth with various test additives is pumped. The prime purpose of our loops is to get metallurgical data of various kinds.

Included in the loops (Fig. 7.1) are circulating pumps, sump tanks, surge tanks, heating sections, cooling sections, liquid-to-liquid heat



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Fig. 7.1. Flow Diagram of Test Loop.

exchangers, liquid sampling stations, furnaces, a dump tank, and usually, unfortunately, a dump valve. I say unfortunately because that is one of our biggest problem items.

Bismuth is unfortunate in that it corrodes almost all known good container materials with the exception of the straight steels, which don't have adequate strength at high temperatures. The container material we have found most successful is boiler-tube steel, usually 2-1/4% chromium, 1% molybdenum steel. However, since this material is not perfect, the philosophy of the operation of our loops is to get as many operating hours as possible on each loop under controlled conditions.

These loops are not designed for testing components. The design parameters for the loops are pointed toward our liquid metal fuel reactor system except size, of course. We try to run the same temperatures and fluid velocities as those contemplated for the LMFR.

Figure 7.2 shows the back end of one of our early "high velocity loops." Actually, the over-all velocity through the loop was very low except in the test section where it was up to 10 fps, which was "high" at the time. We used an a-c electromagnetic pump. We used first a G-E, then a Callery Chemical Company pump. The efficiency of the pump using bismuth with a magnetic container material was about 1%. Power losses were so fantastic that we had a tremendous cooling problem in the pump cell. Since then we have gone to mechanical circulating pumps.

Figure 7.3 is a photograph of the control panel for the loop shown in the diagram. The loop was about 75% completed. The section at the left will contain the gas and vacuum system controls. In regard to the gas system, we must be absolutely sure that we have no oxygen in the loop, because it would remove uranium from solution in the fuel, and it would corrode the steel very rapidly. Hence, we resort to very elaborate measures to get rid of any oxygen. This panel is of interest only to show the general appearance of the types of controls we use. We primarily measure temperature. This is the factor that contributes most toward getting successful metallurgical data.

We have temperature-measuring and liquid-level instruments always, flow instruments usually, and pressure instruments sometimes. In many cases either pressure or flow, but not both, will be measured. In some cases neither will be measured except by indirect means. In one loop

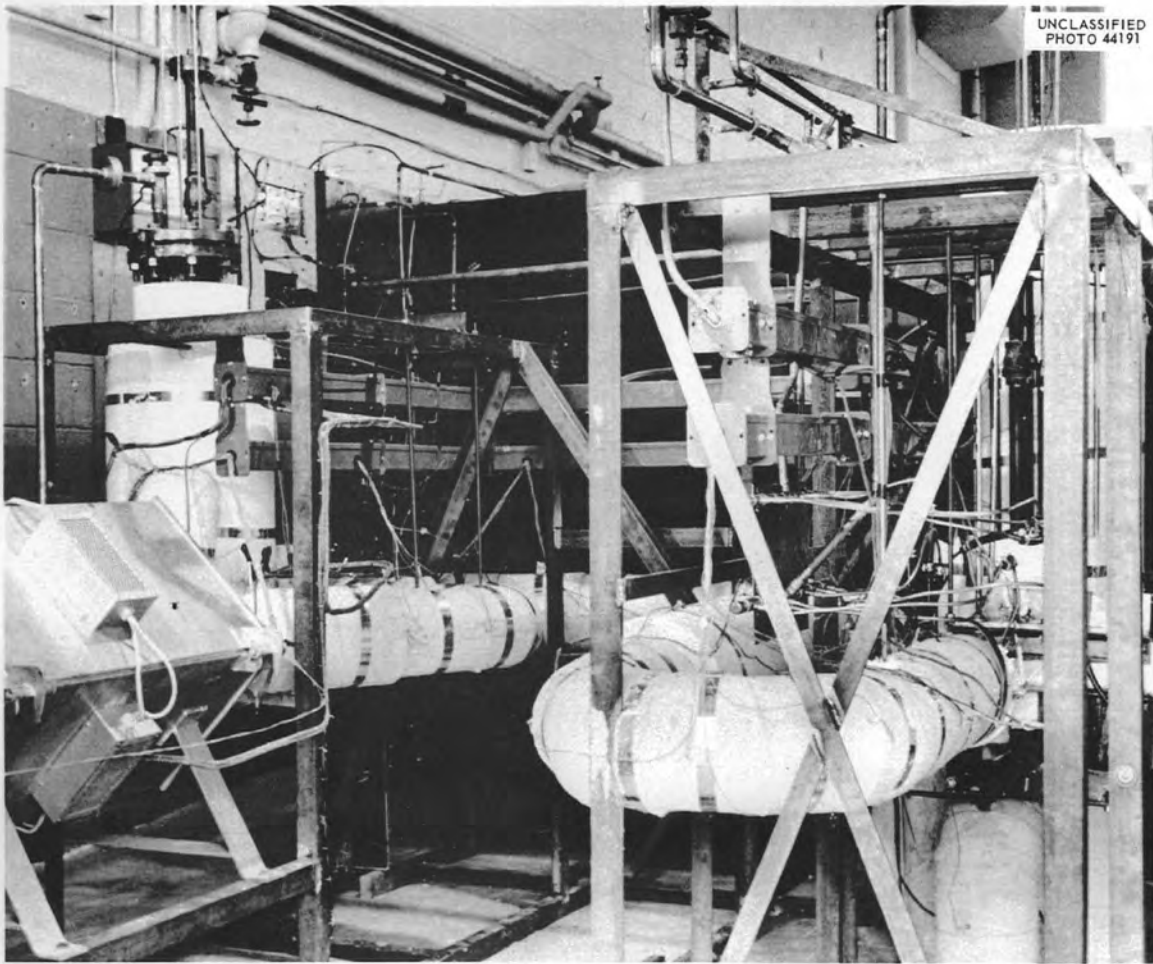


Fig. 7.2. View of Early Test Loop.

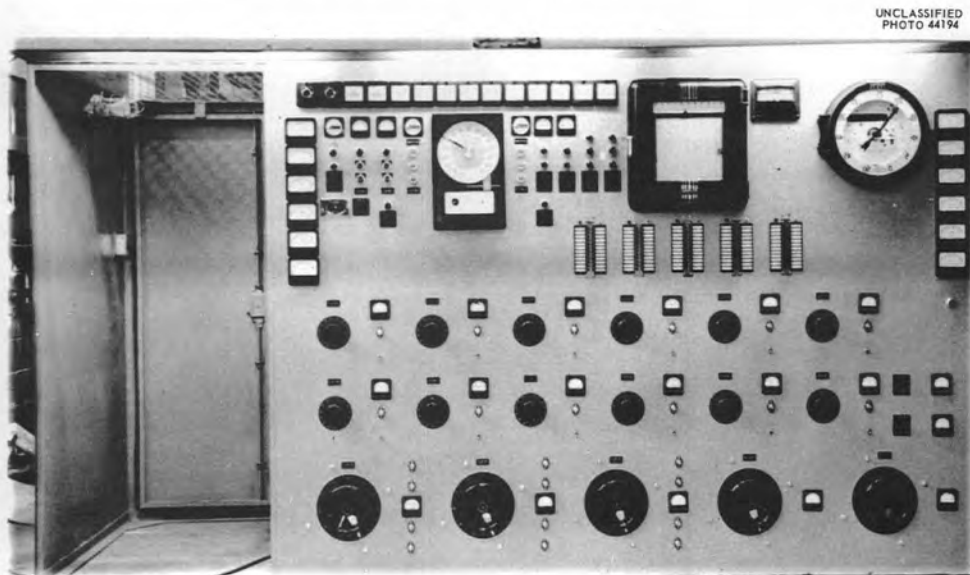


Fig. 7.3. Test-Loop Control Panel.

we have a very specialized instrument for measuring the deposition of solids on pipe walls.

A word or two about the instrument particulars: The temperature range is generally from 750 to 1000°F. We use standard millivoltmeters and potentiometers. We use thermocouples welded to the pipe walls by a puddling technique and thermocouples inserted in wells.

Where there is a gas phase we measure pressure with a standard Bourdon tube gage, but where we must measure pressure in the bismuth, we have two special problems, high temperature and the terrific corrosion properties of bismuth. We are forced to use diaphragm or bellows type primary elements made of 2-1/4% chromium, 1% molybdenum steel, which is like trying to build a spring of cast iron. It is not ideal in any sense, so naturally the instrument manufacturers want the thinnest possible diaphragm or bellows; on the other hand, the corrosion rate even with this material is substantial enough that we get failures due to plain eating through.

Figure 7.4 shows the Manning, Maxwell & Moore pressure tap, which has proved very successful for measuring pressure directly in the fluid stream. The pressure from the bismuth is transmitted through a diaphragm or envelope to a standard Bourdon gage via a NaK-filled capillary tube. Interestingly enough, we have had no temperature compensation problems. These instruments are temperature compensated. We have very carefully tested them. We have given them 9000 shocks at their full pressure rating. We got 9900 cycles out of one instrument, starting at 50 psi and going to 400 psi almost instantly, with no failures. Then we stuck the unit in a loop and operated for 3000 hr and still have had no failures. So we think we have an excellent instrument here, and see no reason why it would not be adaptable to other fluids.

We have also tried the Moore Products Company and the Callery Chemical Company bellows-sealed null-balance pneumatic types. We have used them with inert gas as the balancing medium because of corrosion problems again. They consume so much inert gas that we have practically abandoned this type.

Figure 7.5 shows a straight test loop used exclusively to test pumps and instruments of various kinds. This shows the Callery Chemical Company's null-balance type transmitter and the Taylor Instrument Companies and Manning, Maxwell & Moore units. You cannot see the pressure taps,

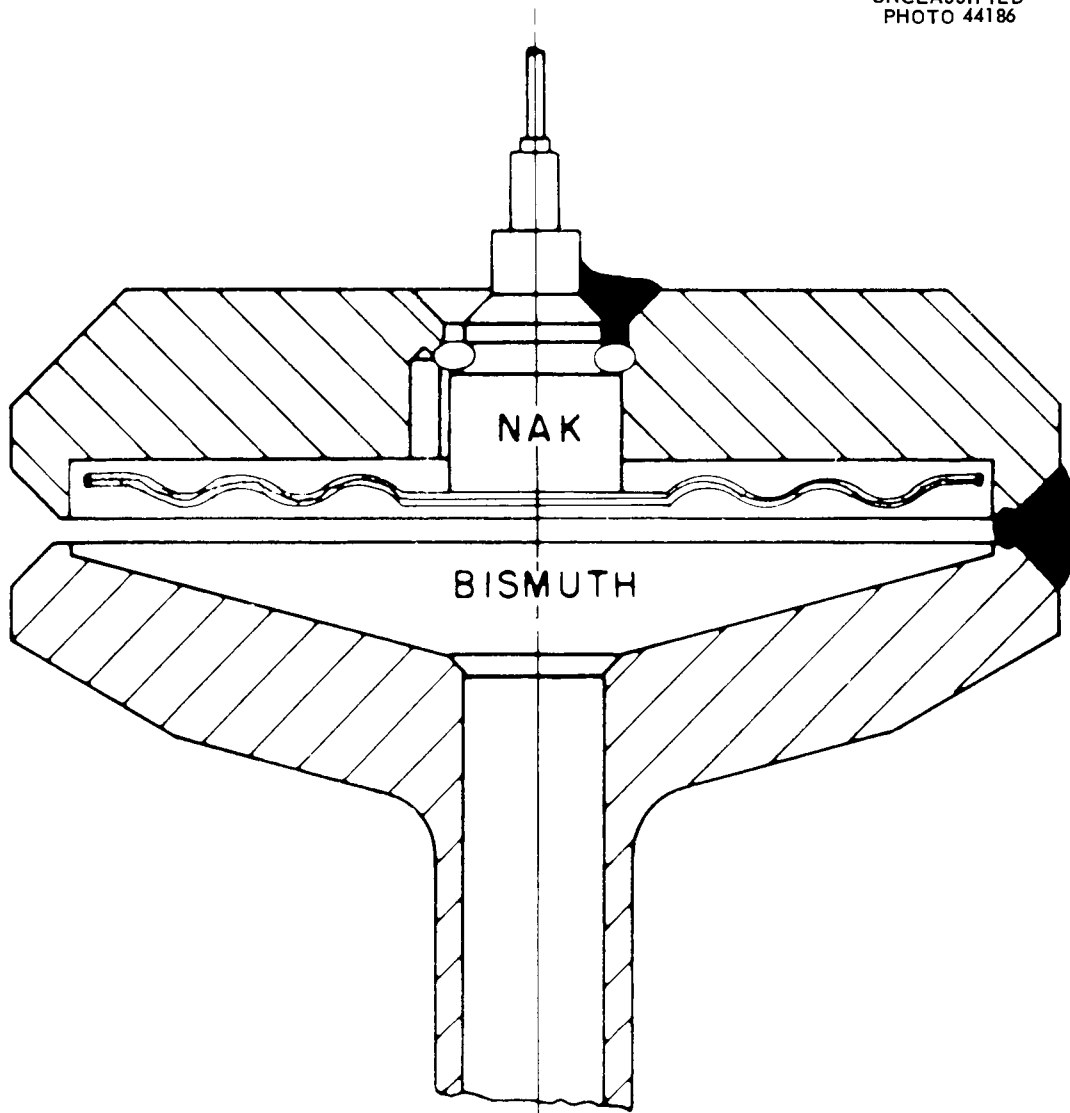


Fig. 7.4. Liquid-Metal Pressure Sensor.

but the capillary tubing comes out to a Bourdon gage. This Callery unit was somewhat erratic, whereas the Manning, Maxwell & Moore instruments (Fig. 7.4) followed the precision gage, which was reading in the gas phase.

The only commercial flow meter that has proved entirely successful, again because of problems relating to materials of construction at high temperature, is the G-E permanent-magnet meter, shown in Fig. 7.6. I am sure that many of you are familiar with that instrument. It has worked out extremely well. An interesting feature is a 10-mil thick, 2-1/4% chromium,

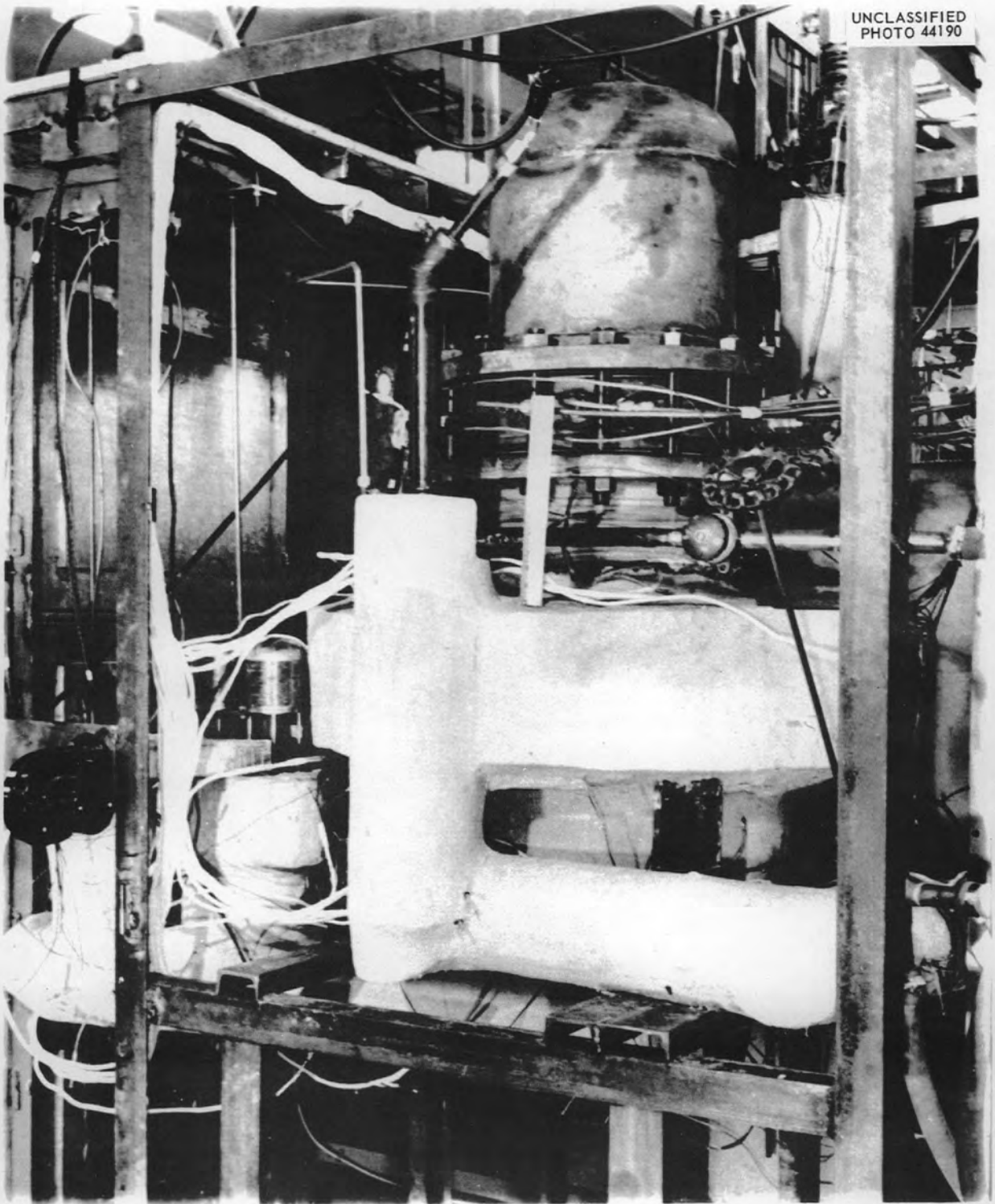


Fig. 7.5. Loop for Testing Pumps and Instruments.

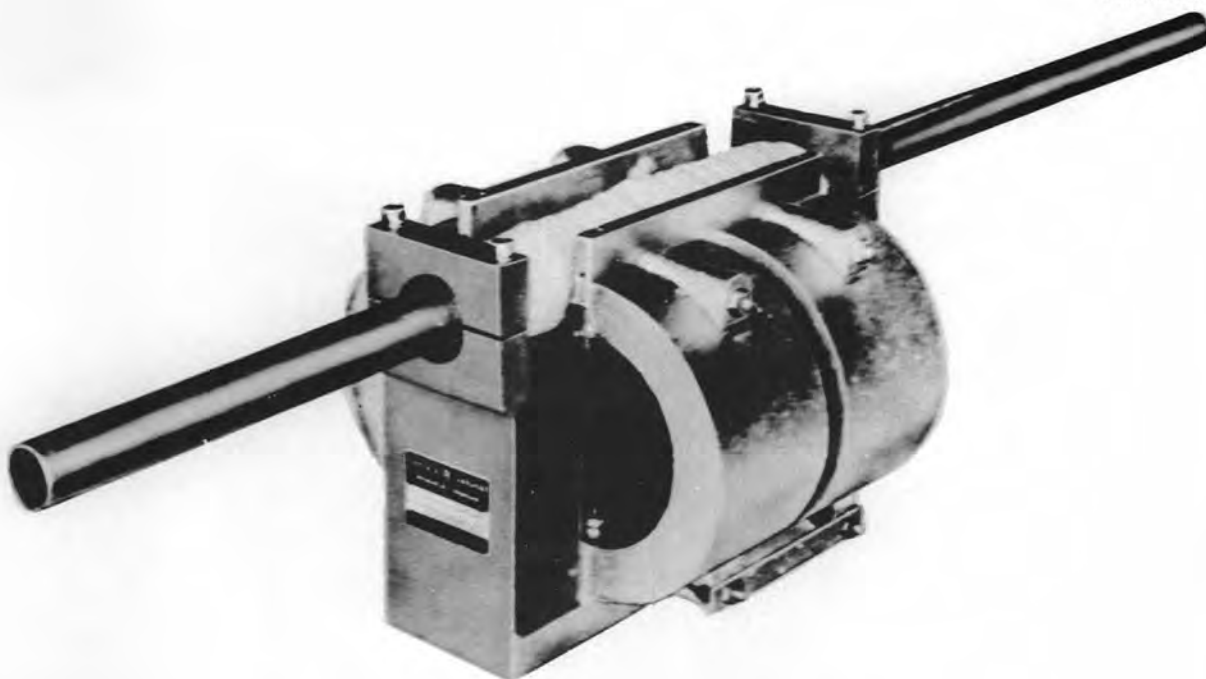


Fig. 7.6. General Electric Permanent-Magnet Flowmeter.

1% molybdenum liner fitted inside the pipe you see running through the flow meter. This gives the necessary corrosion resistance. The bismuth would take the nickel out of the stainless steel if we used straight stainless steel tubes; in 200 hr it would leak. With this liner we get the protection we need; yet we don't have the very severe magnetic shunting effect which we get if we use the straight 2-1/4% chromium, 1% molybdenum pipe. Our fluid has a very high resistivity and takes a strong magnet. I was amazed when F. A. Smith mentioned ("Instruments Used in High-Temperature Sodium at Argonne National Laboratory," this conference) that they get by with 200 gauss. Our little instrument uses 1300 gauss and we get 0.4 mv per gpm with that strong magnet. The instrument is entirely linear so far as we can tell and the response is excellent.

I am very fond of that meter, but our metallurgical section won't use it. They are convinced that it works, but they believe that it magnetically traps out suspended iron. This bismuth does dissolve iron at a very slow rate, and in a large loop there will be enough dissolution that the magnet will trap out the iron and form a plug. There is some evidence to indicate that this does happen. Because of this effect, and

because in certain cases our metallurgy section must measure flow, we have built a heat-balance flow meter, shown in Fig. 7.7. The flattened tube in the center is joined at either end to a pipe, not shown, through the split adapters. We had special (though simple) immersion heaters built. We sent 2-1/4% chromium, 1% molybdenum pipe to the Glenn Electric Company, in New Jersey, who will build a heater to anybody's specifications in a couple of days. They made a Calrod-type immersion heater with bent ends. These two heaters will be inserted in the flat tube and will apply a metered amount of heat, producing a temperature difference across the section, measured in thermocouple wells at either end. We used a thermopile on this particular meter instead of a single thermocouple and measured the output on a 3-mv center-scaled recorder. We alternately buck the two end thermocouples against a standard upstream from the primary element, and it gives a very accurate reading. It matches the G-E identically, and we find maximum error of about 0.25%.

This flow meter uses a considerable amount of power. At flow rates of around 10 gpm - our loops are 3/4 in. pipe size - this amounts to 10 fps, and we need about 2 kw to get a good reading. The sheath temperature rise on the immersion heater at 2 kw input is only 8°F. So we

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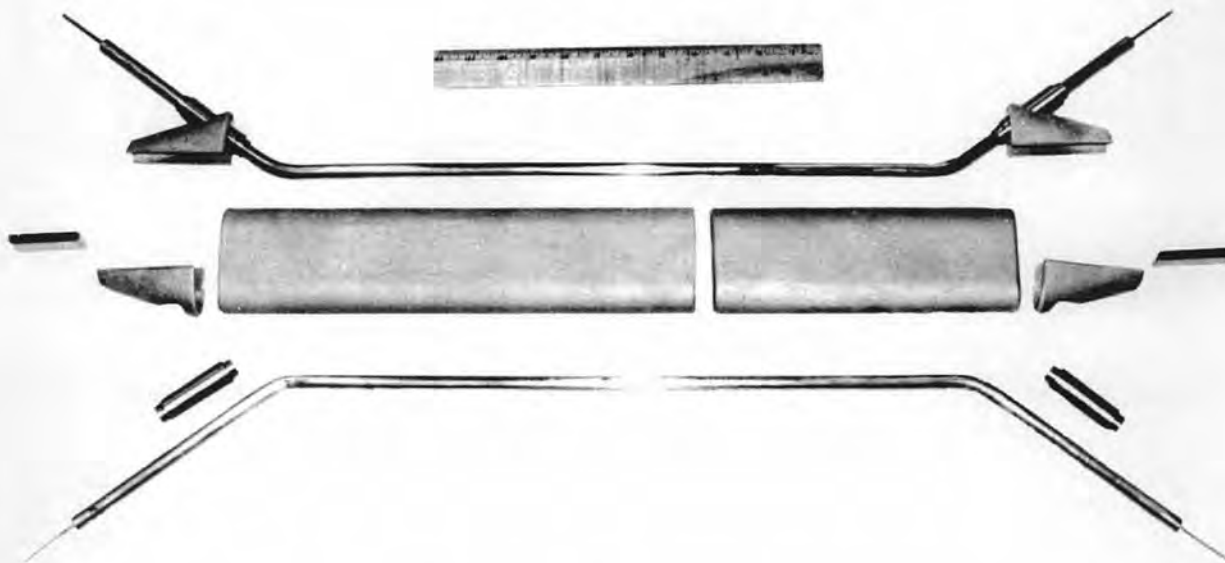


Fig. 7.7. Heat-Balance-Type Flowmeter Before Assembly.

don't get any serious corrosion of the heater element by using this technique.

Figure 7.8 shows the unit assembled. Our metallurgy section likes this instrument because it has no moving parts. As I said, in this particular model we use thermopiles and a sensitive recorder to get good readings.

The Callery Chemical Company pressure transmitter (one of the units shown in Fig. 7.5) is the differential-pressure type with two parallel elements measuring across an orifice. This type was the first that we used extensively to measure flow directly. We were delighted to be able to get away from this because of its high gas consumption. However, we did get some good results, and the units manufactured by Moore Products were tried even earlier than Callery's. This Callery instrument became erratic, though I think it may have been in the readout instrument rather than the pressure tap itself. The Moore model seemed to be more reliable, and it was consistent but with lower sensitivity. Our heat-balance meter works well but its consumption of power is substantial, and if you had a big pipe size you could not afford it.

We have purchased, from the Taylor Instrument Companies, six differential-pressure transmitters using a NaK-filled system. The sensitivity in measuring small pressures is slight enough that it was necessary to use a strain gage output. I am sorry that we have not had a chance to test these because I think they have wide applications for certain fluids.

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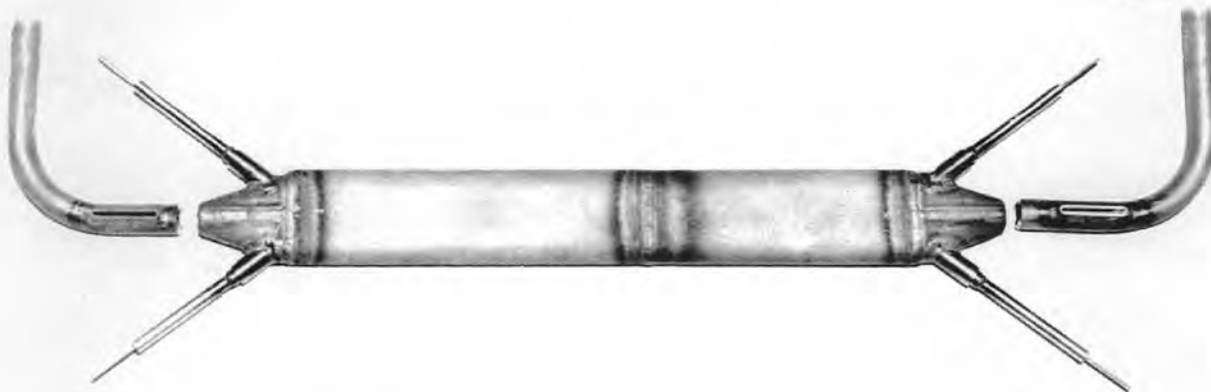


Fig. 7.8. Assembled Heat-Balance Flowmeter.

For liquid-level instruments, we use probe-type measurements exclusively. On Fig. 7.5 I can show you the technique we use. We mount a side handle to a pump or surge tank or anything that has a free surface end. We use two types of probes. One is a movable probe through a gas seal, very similar to the one described by F. A. Smith ("Instruments Used in High-Temperature Sodium at Argonne National Laboratory," this conference). This is fine where you have access to the probes. Where we do not have access, because of radiation fields or other reasons, we use a very compact assembly of six to eight probes. This assembly is staggered in height so that we can get the discrete level indication over a range. This works out very well for us. All we are worried about is pump surging or leaks through the dump valve into the dump tank, and the operator can catch this without any difficulty.

We merely hook the probes in series with a neon bulb, an NE 51 usually, at 100 v, and ground the tanks. The dropping resistor in series with the neon bulbs is so high that you cannot even feel a tingle from it, and you don't need anything else. The bulb is extremely fast in reading and it has a long life.

At top center in Fig. 7.9 is a bank of four level indicators for our in-pile loop. We have 6 levels on each indicator, each from one of the multiple-probe assemblies. Where we want a control level we use the Machinery Electrification MEK 3001 controller. We are very partial to this after all sorts of tests. The controller is very fast, and fairly inexpensive. It has a dpdt load relay and a very wide range of sensitivity. We were very skeptical at Brookhaven and we wanted to test one of these; so we gave it 3000 cycles and the test rig broke down. We believe in it.

In conclusion, our approach to process instrumentation is to use commercial types where possible. We don't try to develop instruments at Brookhaven if we can possibly avoid it. We use the simplest types that will do the job and we use instruments of good quality always. Figure 7.10 illustrates a typical in-pile loop control panel.

J. E. OWENS: What are you using for insulation on your level probes?

D. W. HUSZAGH: We use two types. We have what we call the Wilson seal. I don't know whether this is commercial or not, but it is a Teflon seal. We bring the top of the probe high enough so that it is at room

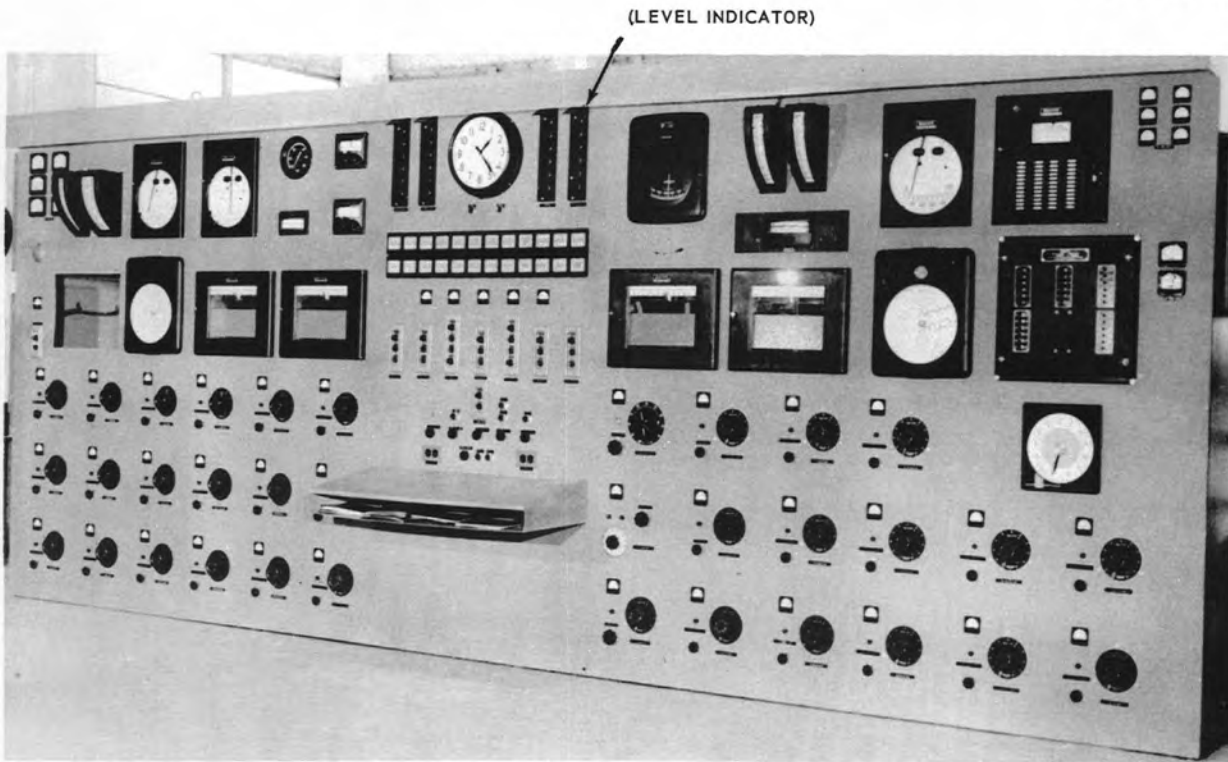


Fig. 7.9. Control Panel for In-Pile Loop, Showing Level Indicators.

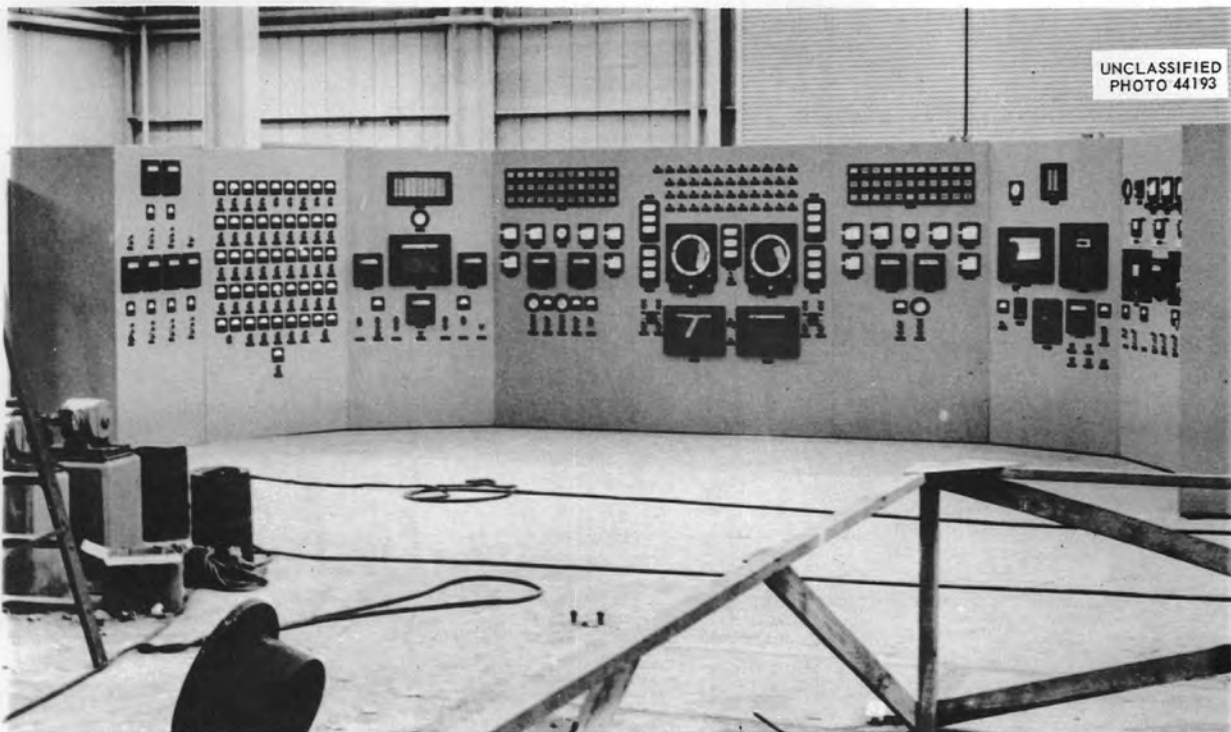


Fig. 7.10. Typical Control Panel for In-Pile-Loop.

temperature; this gives us our sliding seal. In the multiple-probe assemblies we duplicate the Amphenol fitting and then bring the wires out.

P. BLISS: Do you have any trouble with fouling of these insulators with liquid metal vapor?

D. W. HUSZAGH: That is the purpose of the side handle. We get very little surging in there. It damps out all the splashing and everything else.

P. BLISS: You don't have any condensation problems?

D. W. HUSZAGH: The small amount of free surface available in the side handle is such that we haven't had a single case of that.

F. A. SMITH: You mentioned the importance of your oxygen content in your bismuth. What analytical procedures and instruments are you working on to do this measuring?

D. W. HUSZAGH: We have no instruments for this purpose. We take a fluid sample and send it over to our analytical laboratory where they do it by spectrographic analysis and other techniques. We rely solely on several samples.

C. S. LISSER: What is the lag in your heat-balanced flow meter?

D. W. HUSZAGH: It is very fast. We get a good reading in approximately 30 sec.

C. S. LISSER: Can you describe the instrument with which you measure flat wall solid deposition?

D. W. HUSZAGH: This is a clever thing, but it looks so cumbersome at this stage that I would not show it. It consists of a special pipe section and a dummy section, separately heated, which feed a differential transformer. We measure change in inductance since our deposition is almost always iron. It does not work too well because it picks up a million stray signals from all the pipe line heaters. We use induction heating wherever we can to heat our pipe lines and this hampers the instrument.

R. C. FAUGHT: Have you considered using externally energized magnetic flow meters, so that you can turn off the excitation once in a while, and perhaps let any accumulation of iron be swept out?

D. W. HUSZAGH: We did the same thing a different way. People don't like to put windings close to hot pipe if they can avoid it. So we try to use the permanent magnet type, but we used an air cylinder to move the

magnet in and out which is the same idea. We have not experienced any trouble with this G-E flow meter. Its magnet is much weaker than those we have always had to use before, when we had to contend with the magnetic pipe itself. We needed flux to saturate the pipe before we even got a reading in the fluid, and the fluid has a terribly high resistivity. So it is not good. With this lined pipe and the low flux we have not experienced trouble yet; so I think maybe we are all right.

J. E. OWENS: Have your metallurgists worried about collection pockets around the heaters?

D. W. HUSZAGH: We get a different effect. Around the heaters we find that we have advanced corrosion rates. One of the reasons our control panels are so elaborate compared with the ones shown by Argonne for the same type of loop is that we are trying to get as many of ours as possible under exact controlled conditions. We don't run a loop for a short time; we run it until it absolutely falls apart, so they try to control the daylights out of it.

S. BARON: On this heat-balanced flow meter, is there any difficulty in getting good mixtures?

D. W. HUSZAGH: It is possible. Maybe we are just lucky. But, as I said, we calibrated this instrument again and again. First we operated it with oil baths and calibrated it against a stop-watch and a weigh tank, then against a rotameter, and it was exactly accurate. Then we tried bismuth with a weigh tank and it was exactly accurate again; so apparently we are doing all right.

S. BARON: What is the purpose of the flattened tubing?

D. W. HUSZAGH: That was to allow us to put two immersion heaters in a section that had roughly the same cross sectional area, if you considered the fact that it was partially filled by the heater, as the pipe line. We wanted definitely to get enough heat in there, and also to operate in case one heater burned out.

R. L. MOORE: You mentioned that the Callery transmitter was erratic. Can you give me further details on it?

D. W. HUSZAGH: Yes, we appeared to get sticking somewhere in the instrument. The typical appearance of the gage when it was working properly would be the hunting effect that you find on any pressure gage. All of a sudden it would stop.

A. M. LEPPERT: At what temperature and pressure are you operating the loops?

D. W. HUSZAGH: The temperatures go as high as 1400°F in special cases, and we have to stay above 750°F or we are in very serious trouble. We use magnesium and zirconium additives to inhibit corrosion and they go out of solution at lower temperatures.

We adapted from the Linotype industry a standard mechanical pump for pumping type metal; not very efficient but very reliable. We get about 60 psi, and in our piping system the maximum velocity is about 10 fps.

A. M. LEPPERT: Do you use the heat-balance flow meter at your higher temperature of 1400?

D. W. HUSZAGH: We have not yet. We usually put it on right after the cooler, the lowest temperature in the loop.

A. M. LEPPERT: What is the highest temperature you use?

D. W. HUSZAGH: Actually we get a temperature rise of not more than a degree across it.

A. M. LEPPERT: I mean the fluid temperature of the system.

D. W. HUSZAGH: It usually runs about 800°F.

H. D. WILLS: You mentioned this heat-balanced flow meter downstream of the cooler. Is it not possible to instrument this cooler to take a heat balance across it?

D. W. HUSZAGH: It is. We went into this because we have an unfortunate situation down there. We had two departments figuring the cooler and the heat transfer, and they came up with somewhat different answers because of different heat-loss assumptions. This has happened so many times that we don't trust any calculated heat balance unless we know what our losses are. In this design all our losses are to the fluid except a very small loss out the end of the heater. We know that we are getting our heat where we want it in the fluid and it is relatively simple to build. We feel that we can trust it.

G. H. BURGER: Is your magnetic flow meter the standard G-E except for the lining?

D. W. HUSZAGH: That is right.

G. H. BURGER: What kind of accuracy do you get?

D. W. HUSZAGH: The error is less than 1%.

E. A. GOLDSMITH: Do you have any philosophy for maintenance of these instruments for final plant use?

D. W. HUSZAGH: It varies from loop to loop. In the case of our standard metallurgical loops, they shut down and take the loop out and put a new one in by hand. For the in-pile loop we have a different philosophy. They use only instruments they feel will stand up, and they attempt to operate without the instrument if it fails. There will be no maintenance until the run is completed. As I said, the most important purpose of our experiments is to get the time and we will do anything to keep operating. So if there is an instrument that fails they will bypass it.

"clinical" thermometer: a very small diameter tube, 1/8 in., and maybe 1/2 in. long, with the sensitive element encapsulated inside. We can make those now in nickel bulb units. Actually the design is not frozen.

W. P. DiPIETRO: In the in-pile test are there any figures on gamma flux?

H. DARLING: I believe that we have some in the research file. The report I saw was very encouraging.

W. P. DiPIETRO: At the Shippingport plant we use thermocouples and we do have quite a bit of trouble with them. They are all over the lot to start with. I asked our reactor people in particular about this postage-stamp-size resistance thermometer and they told me that space was probably going to limit it. This thermometer-type device, as I see it, is no different from the thermocouple. It sounds like it has possibilities.

H. DARLING: That is right. We do manufacture the clinical bulb at the present time for biological experiments. They are 120-ohm chrome-nickel bulbs. The platinum, of course, will be a little more difficult to get into that size for the higher temperature level.

W. P. DiPIETRO: Resistance thermometers are used in the process industry. Would anybody like to comment on that?

R. C. FAUGHT: We started with the idea that we needed to measure temperature distributions at the top of the matrix in the reactor core in a sodium plant. This meant that we had high temperature, low core flux, and direct immersion in sodium to contend with. Also, we were limited in size because of the reactor configuration and we didn't want to disturb it. So we went to considerable trouble actually to develop sheath-type magnesium-oxide-insulated thermocouples long enough to reach down to the core.

This was some five years ago. We found that it is tough to make couples of this size. You run into difficulties with sheath cracks. A leak-tight seal that you can immerse directly into sodium was one problem. Also all the couples that we made were given three-point calibrations in rather limited immersion in a copper block, and we found that in service the immersion length was of the order of 5 ft, so that initial calibrations did not mean much in service.

We have no real data on what the couple calibration does at flux in the core, but we have some checks on the effect of integrated neutron flux. I don't think I can say what the integrated flux is, but shifts of less than 3°F in the calibration, say at 1000°, were observed. In manufacturing these things we found that we could get a considerable calibration shift when we started to use a couple of this sort. So we finally aged these at a temperature higher than that at which we expected to use them for 100 or 200 hr. The magnitude of this drift might be of the order of 25°F at 1000°, so we found it necessary to stabilize them before we used them in order to avoid calibration troubles. After continual exposure these couples are fairly close to the standard curve at 1000°F.

W. P. DiPIETRO: After how long?

R. C. FAUGHT: This is in the Sea Wolf. They are close to the standard curve: I would say maybe a median within 8° at 1000°F.

W. P. DiPIETRO: We had about 8° also. It required a separate calibration after we had toasted them for a while.

R. C. FAUGHT: Did you stabilize yours beforehand?

W. P. DiPIETRO: They were stabilized beforehand, but to what extent I don't know. I would say from the time we had our hands on them until they settled down they changed calibration about 8°.

C. A. MOSSMAN: Are both of you gentlemen talking about swaged couples now?

R. C. FAUGHT: The earlier ones that we tried to make were swaged. The later ones that we used were made by drawing down to a small reduction, of the order of 10 or 20% of the sheath.

C. A. MOSSMAN: It is sealed in a sheath?

R. C. FAUGHT: Yes. I don't want to include the thermocouple junction in the closure weld on the sheath end, if you are going to immerse the assembly directly in sodium. One trouble is porous closure welds or porosity from gas evolution during the welding process, which leaves small holes in the weld that sodium can come back through.

You also get rapid rates of temperature change that can cause differential expansion between the sheath and the wire, and the sheath gets hot first and opens the circuit.

P. BLISS: What happens if you don't have the couple joined to the sheath?

R. C. FAUGHT: A 16-in. couple made without the junction in contact has a time constant of the order of 0.9 sec.

C. A. MOSSMAN: Was it your people who reported 40 v from couple to sheath in a radiation field?

R. C. FAUGHT: Not that I know of.

W. P. DiPIETRO: Savannah River.

R. C. FAUGHT: I talked to them a little about this, and will try to relate what was said. They attributed it to activation of the copper. Both thermocouple wires were not grounded to a sheath, and, the copper being a beta emitter, the unit leaked off, in effect, electrons and got a voltage of the order of 40 v between the two wires and the sheath. They found that by putting such a couple in an x-ray field they would get a voltage of opposite polarity, the sheath material being positive in respect to the wires in this case.

R. G. AFFEL: The physics people at ORNL ran into this some time ago with insulators in general.¹ However, I cannot really see why it should, once you know it is there, get you into too much trouble since it is a high-impedance source. These potentials are certainly there, but as far as loading is concerned, even with the insulated junction, they should not be troublesome. Have you had any trouble with them?

W. P. DiPIETRO: I can see a source of trouble because if the couple is bonded to the sheath at the bottom and another field is set up between the wires and the sheath, there are essentially two thermocouples in parallel.

R. G. AFFEL: One source of trouble is impedance. These measurements were made with a meter incorporating an impedance of 10^{12} ohms.

H. DARLING: This problem of high voltage between thermocouples and ground occurs in another application, too. It is particularly troublesome in the glass industry, where thermocouples are immersed in tanks full of molten glass. We find, to our surprise, that the internal potential of the molten glass may be as high as 500 v. With the ionization and the circulating currents around the molten glass all kinds of strange things happen. It is not uncommon to have 100 v between the body of the furnace and the center of the molten glass. Under those conditions the wires of

¹J. C. Pigg et al., Trans. Am. Inst. Elec. Engrs. 74, 717 (1955).

the thermocouples must be insulated. The sheath is grounded to the case of the furnace and the insulated lead is brought out. But it does no harm not to insulate. The only thing you have to watch for is the insulation of the input to the recorder or control. If you take this into account, the potential, even though it may be varying with time, does no harm. This is a far worse condition than the one you are speaking of, and we deal with it every day.

J. E. OWENS: We are using thermocouples similar to the one that R. C. Faught described. We have been hearing rumors of these voltage troubles and other things and we are getting worried because we have not seen them. We are somewhat reassured.

I am interested, Mr. Faught, in your comment about going directly to sodium. We have three in the sodium pool in our reactor. I guess we had better look at them the next time we plot the signals, but we have had no difficulty. We have some 50 thermocouples in the core, and we recently inserted some calibrated thermocouples to try to get some idea of where we were. At about 700°F every couple in our system agrees within about 5°. We have made no effort to calibrate them individually. As we go up in temperature we begin to get spreads, but we know that we are getting gradients in the reactor, so we don't worry about it.

The organic moderated reactor at Idaho is using similar devices, and they had some difficulty. They are using extremely small thermocouples. They first attached these to a standard null-balance recorder and could not get balance. It turned out that they had about 200 ohms impedance in the thermocouple circuit and the instrument was not built for it, but once they put in an input circuit built for high-impedance thermocouples they had no more trouble.

R. G. AFFEL: Of course, while investigating the closing of 1/4-in.-OD sheath thermocouples, we found some welds that were poor on the end. Inadvertently they were placed in a test tank of NaK and run at 1500°F; they immediately leaked. But throughout a period of at least 2000 hr the NaK inside the thermocouple never reached the same level as that in the tank, even though there was a pressure head in the tank. So I think it is a backed-up system. You are not immediately in trouble. You have no hole to the outside. The leak of the sodium seems to come through very

slowly. Actually, it washes out the insulating stuff. There is no reaction.

R. C. FAUGHT: There is a high degree of compaction of the insulation. The ones we used initially were not very dense, and from our experience, in a similar situation, the sodium would come up to wherever the tank level was. It flows rather well.

R. G. AFFEL: That was the reason we were interested in hard-packed insulation.

P. BLISS: We never noticed any trouble due to leakage.

J. F. POTTS: I think all of us drift into a state of false security from the interpretation of the meaning of calibrations. The vendor says that the thermocouple is calibrated. Then we perform an emf-against-temperature correlation and if we agree with his result, we are doubly assured that the thermocouple is good. But this is not necessarily what the thermocouple is going to tell you in the system; so I think that we need to re-evaluate our interpretation of calibration. However, until such time as we understand what we really need, it is just as well to hold the supplier's feet to the fire, because there are other things that we know from experience happen to thermocouples, which are evidenced by the departure from this more or less arbitrary calibration scheme.

Mr. Faught, you mentioned 100- or 200-hr aging for stabilization; over the last couple of years we have been taking hard looks at this, and we get arguments from vendors. We are going to use thermocouples at 1600°F. We say, "Let's age these thermocouples for a couple of hours at 1800°," or "1650," or something like that. The vendor says, "I cannot guarantee the couples if you do this, but if you will let me age them for 24 hr at 1350°F I will guarantee them."

He will guarantee his initial calibration. But if you are going to use the thermocouple at 1600° - and we have metallurgical evidence that there is nothing magic going on here in the way of heat treatment - why is it that you cannot age these thermocouples at 1600 degrees, yet you can use them successfully at 1600°? I was told that this 100- to 200-hr aging came about because you found that it did improve the performance of your thermocouples.

R. C. FAUGHT: We went into this because we tried to put some of these couples as manufactured at temperatures of 1000°F or so, and we

found that at first there would be a quite rapid change in calibration by as much as 25° at 1000° , most of which took place at about 100 hr, and this heat treatment was carried out up to 1000 hr or more. We found that in the first 100 hr it might change by 25° , the calibration shifting upward, and then over the next 900 hr or so it might drop off by several degrees but very slowly. So what we thought we were doing was getting rid of any rapid changes which might occur when we first put these things in. Therefore you would have more faith in the calibration before you installed the couples.

We also actually put these couples in a sodium test at 1000° and kept them there for 100 hr or so, having calibrated them before this test, and then calibrated them again after the test. Of course, the calibration immersions were short, but we found that calibration before and after such a test might differ by less than 5° at 1000° .

So for these various reasons we decided that we should try to stabilize the couples before we used them. That was the purpose of the 100 and 200-hr stabilization.

J. F. POTTS: I do not mean to imply that I would not have done the same thing that you did under the circumstances. I was told that later one of the big causes for this shift to 200 hr was that you were driving off water vapor from the magnesium oxide, and they found ways of doing this much more efficiently and in a lot less time. This has been borne out by the experience of several vendors who, more recently, are going to great length to preclude water vapor from magnesium oxide thermocouples. This is one of the big factors in improving these thermocouples. The point I am trying to make is that people who are new in the business say, "Well, here is a requirement," and they continue to specify it until they understand more themselves about the business.

W. P. DiPIETRO: I think, irrespective of what you do with thermocouples before they arrive, from what I have seen you had better calibrate them again, and then you are sure of what you have.

C. BRASHEAR: Does mechanical stress seem to have any bearing on this change in calibration?

W. P. DiPIETRO: If mechanical stress distorts the sheath, then of course, you are through; but other than that, I don't know of any change in calibration.

P. BLISS: Does anyone have any information on the relative performance of aluminum oxide or zirconium oxide instead of the magnesium oxide?

R. C. FAUGHT: Aluminum oxide is more abrasive, and if you make these couples by swaging you get more pitting. Zirconium oxide came in after we got out of the business.

P. BLISS: One of the advantages is supposed to be that they are not as hard to fabricate as the magnesium oxide couples.

R. C. FAUGHT: Also, I hear that, particularly in the pressurized water plants, MgO can react with the water, whereas aluminum oxide and zirconium oxide do not.

D. L. McELROY: I want to make a point with regard to mechanical stresses. It may be unfortunate that in some of the heat treatments we are dealing with two alloys that have a recrystallization rate at 1000°F; yet we are heat treating in this region, and if you put Chromel through a so-called dead anneal, at a temperature below 1000°, you will have a potential of a considerable magnitude. It is hard for me to see that in the swaging operation you are not cold working the wire quite a bit and causing this phenomenon.

W. P. DiPIETRO: I am quite with you.

D. L. McELROY: A cold-worked Chromel thermocouple, say at 80 or 90%, is going to read differently from a Chromel couple that is not cold worked. The recovery temperature is a lot lower. Supposedly, in a recovery which goes on before recrystallization of the cold-worked material, you recover all of your electrical properties. We have seen definite evidence that these things can contribute to pretty sizable errors in the thermocouples.

P. BLISS: But that anneals out.

D. L. McELROY: Yes, but in a temperature gradient it will not anneal out uniformly, and if you have a change in temperature flux you can be in trouble.

C. A. MOSSMAN: I infer that if you have a gradient at the same place you have your cold work you can have difficulties.

J. E. OWENS: What have been some of your experiences with thermocouple calibrations? We have calibrated some of ours at the more crucial spots, and we find that, sure enough, they are pretty well what the

manufacturer said, plus or minus maybe 5°. But cold-working, radiation, and other effects are probably going to change them 5 or 10° after you use them a while. Do you throw any thermocouples away because they are not according to the specifications?

W. P. DiPIETRO: Yes, indeed. We heated our plant water with our auxiliary boiler and pump at zero reactor power. We had a 3-point check over the operating range to see how our couples compared with our platinum calibrating bulb. We found that there was quite a scatter at first and the thermocouples were no good; the readings were useless. So we reworked the thermocouples and we found that there were quite a few installation problems that we could lick. We had thermocouples that were at least 8° off the vendor's calibration. We found that they were probably $\pm 1^\circ$ in scatter. So we assumed that we had an 8° steady-state error with accuracy within $\pm 1^\circ$. Anything that was outside of 10° we simply threw away and said, "We don't know why, but there is no sense in using that as data."

B. E. WOODWARD: You described the cold-junction temperature problem. Would you mind going over it again?

W. P. DiPIETRO: We brought the couples out through the head of the reactor into a pressure container, in which we had connections. This was a piece of ceramic with some stainless steel pins. There was a pressure seal, a NaK plug, and an Amphenol plug. We had trouble in our junctions. So we stripped the connector down. We had the Chromel to Chromel and Alumel to Alumel, and then we used the pin simply as a strength point and took out the errors. We followed by tracing the circuit back to the recorder, and at this point connecting Chromel to Chromel to see if we had any emf generated in a faulty joint in the wire going back. We found very few of those.

Then further, we find that the ambient temperature around this connector gives us difficulty for some reason. If we blow onto this box, we will find that our indicated temperature will vary when the blower speed is varied. That is one thing I cannot understand.

C. A. MOSSMAN: When you said that you calibrated the couples did you use Chromel-Alumel or did you use platinum-rhodium?

J. E. OWENS: Our couples which we used for standard were calibrated against platinum-rhodium. We used Chromel-Alumel as secondary standards.

C. A. MOSSMAN: And this is around 1000°?

J. E. OWENS: We have run them up to 700°F. Our operating temperatures are around 1000°F.

J. F. POTTS: Do you use your Chromel-Alumel repeatedly as a standard?

J. E. OWENS: Yes.

J. F. POTTS: And is your depth of immersion always the same?

J. E. OWENS: Our thermocouples are installed in a thimble. The thimble is immersed in sodium, so that only the tips of the thermocouples touch the wall of the thimble. What the ambient of the thimble is I have no idea.

[At this time C. J. Madsen (WAPD) assumed the chair]

J. E. OWENS: Someone was talking yesterday about the difficulties of thermocouples in wells. I wonder if anybody has anything to add to this?

J. F. POTTS: We know that with an exposed thermocouple of a non-swaged-type construction there can be difficulties. If there is a long length-to-diameter ratio in the well, and if the temperatures are conducive, various alloy constituents, particularly the nickel-chromium types such as Chromel-Alumel, are preferentially oxidized. These effects can be extremely serious in short times. You can get a shift of 10% in thermocouple reading in 24 hr at 1600 and 1700°F. Below 1200°F you can hardly see this effect. Work by Spooner and Thomas brought up this point, and at that time they had a limited amount of data supporting the theory of preferential oxidation. D. L. McElroy and those of us at ORNL verified the work of Spooner and Thomas, and in addition have come upon one or two anomalous situations in connection with this phenomenon.

Taking the case of a number of thermocouples drifting at 1800°F for periods up to a few thousand hours; with a short length-to-diameter ratio, particularly, we had 20-gage Chromel-Alumel in a 1/4-in.-OD Inconel well. The drift with time was a few degrees centigrade. Let's say 5 for instance. This is deviation from handbook value. The samples were from 1 to 3 ft in length. The data spread is something like 50°C in a few hundred hours. In this same well we had 1/8-in. wire thermocouple assemblies, with bead magnesium oxide insulators a little under 1/4 in. in diameter feeding into the 1/4-in. well. In these two cases we had Chromel wells and two Alumel wires with a junction, so that two bits of data helped to contribute to this result. In another case we replaced two of these wires with titanium

wires after the suggestion of Spooner and Thomas, and on an average for this amount of time we had improved the situation.

We think that we get a little drift with swaged thermocouples, and we have considerably restricted the influx of oxygen, which is one of the things affecting the Chromel-Alumel or the nickel-base alloys. We say that we will get further improvement in the performance by putting titanium in with the swaged thermocouples. We have some material fabricated in this manner. We don't have the results on this yet, but the sort of question you have to answer before you sit back and relax is that you have a reducing material, titanium, there. If this titanium is getting that little bit of oxygen, at 1800°F, for instance, you might have a situation of reducing magnesium oxide to metallic magnesium within the normal life of the thermocouple. The partial pressures for this are probably quite low and they would probably not be long enough to see this effect become deleterious, but it is a question that we have to answer before we dive off the deep end.

R. C. FAUGHT: Was the insulation of these couples as high after the test as before, or were you able to measure this?

J. F. POTTS: Unfortunately, my impression from a few isolated measurements is that the insulation resistance is lower after the tests than before. We have only most recently been taking really close looks at this. Also the difficulty we have encountered by having the thermocouple junctions in contact with the sheath material has limited the opportunity of making this measurement. This needs to be done.

R. C. FAUGHT: I was wondering if the effect could be attributed to a loss of insulation between the wires.

J. F. POTTS: We have seen this problem in connection with burnout protection where, if the thermocouple junction fails, there is a high-impedance source of emf which drives the recorder off scale, so that it is fail-safe. We have run into trouble a number of times because the impedance of the insulation drops low enough that the thermocouple can fail, yet because of the leakage resistance you don't get the burnout action from the recorder. That is the only case I know of where we have been into real trouble. The impedance of the thermocouple is of the order of a few ohms. With a few thousand ohms of impedance appearing you are safely shut down before you start to really worry. Incidentally, this

is one of the advantages of thermocouples over resistance thermometers. You have a low impedance in the thermocouple vs a relatively high impedance in the resistance thermometer.

P. BLISS: One particular condition brought that to our attention. We had a 1/4-in. well, about 2 ft long, and we were supplying Chromel-Alumel thermocouples to insert in that well. The operator could see the shift you described on his recorder. Within a couple of hours he would note a 100° shift. The well was the leakproof type. Nothing was getting into the well. In the thermocouples, after this exposure, both wires were extremely brittle. Both wires were magnetic. In this particular case we took the engineering approach; we had to fix the thing. We put in 1/8-in. thermocouples and we have had no further trouble.

D. L. McELROY: I might point out the result of this oxidation process. When that wire oxidizes it can give quite a short very quickly. Also, the oxides that grow are those of nickel and chromium. At 1000°C they generate 400 mv. These things can well account for the short.

J. F. POTTS: We find, as is obvious to many of you, that thermal emf is a more sensitive indicator of a number of phenomena than any other analytical technique we know of.

It finally occurred to us that one of the red flags that we could wave in determining when we should stop believing a thermocouple would be to make some more or less arbitrary rule regarding the resistance of the thermocouple. You start with a wire that is a metal, and you get an oxide layer built up on it which has a considerably higher resistivity than metallic material and in addition contributes a thermal emf of its own in parallel with the emf of the thermocouple. After 5000 hr we have examined some of these thermocouples and we have found it isn't Alumel any more.

Now the point is that if we have good data, correlating calibrations shift and resistance change, we have a resistance change by a factor of maybe 10 or 20; yet the calibration shift might have been 1 or 2%. We can have someone build in some sort of check, so that occasionally we will measure the resistance of the thermocouple. When it is changed by a factor of 2, for instance, we will pull the thermocouple.

P. BLISS: In this connection, is there any value to these thermocouple rewelding techniques?

J. F. POTTS: You mean those advertized gadgets that run a heavy current down the line and put you back in business? This is a personal opinion. I haven't gone along with those too well because you are never quite sure where you made your junction the second time.

R. C. FAUGHT: We have used the sheath type of thermocouples in long wells to get into difficult-to-reach spots and yet retain the features of the replaceable couple. This has usually been on 1/16- or 3/32-in.-OD sheath. Of course, it does not give a good response time.

C. J. MADSEN: Have you had any mechanical stress, perhaps from a fairly sharp bend which sets up stress in your thermocouple leads? Some of our physicists have indicated that we might have had some of that, although I don't know of any possible evidence that we have actually observed it in the reactor application.

D. L. McELROY: Anywhere you cold work the wire and you are below the recovery temperature you are going to cause a change in the emf if this cold-worked region is used in a temperature gradient. If you have a homogeneous region you probably are not in trouble.

J. E. OWENS: I should like to get some opinions as to how to stick thermocouples on to pipes.

On our sodium pipe we started out originally by making a thermocouple bead and strapping it to the pipe with a band or with stainless steel wire. We did not like that too well because we probably could not get good contact. We have tried in our developmental loops - not so much in the reactor itself - to drill small holes in the pipe, insert the thermocouple bead, and then peen it in with a hammer.

Also we have tried thermocouple beads with heliarc welds over them. The metallurgists don't approve of this. We have tried making up our bead and then capacitance-discharge arc-welding it to the pipe. We get into trouble here quite often because of the different resistivity of the two wires. We weld one to the pipe and the other one burns off.

We have recently begun to discharge weld the two wires to the pipe close to each other but not quite touching. This seems to give fairly good results.

In our wells, in an effort to get good response time and to make sure we had good contact, we have used this sort of technique where the thermocouple bead was pushed to the bottom of the well with a copper push rod

and then capacitance-discharge welded. These we usually have to do four or five times before we get one that won't pull right off on us.

C. A. MOSSMAN: We are using quite a bit of your third technique, but in most of these applications we are not worried about great accuracy.

P. BLISS: Particularly in this case where you have a bead, there is a certain amount of ambiguity as to just where your junction is. We have found with two No. 20 wires brought together in a platinum bead perhaps 0.003 in. thick, that there is as much as 50° difference in temperature between the top of that bead and the wall of the pipe.

G. W. GREENE: We have tried variations of the last method in which we wrapped the thermocouple around the pipe and then capacitance-discharge welded in several spots, and we have gotten fairly good results that way. Of course, it needs to be in an isothermal region around the pipe.

C. A. MOSSMAN: Has anybody put the sheath type thermocouples on the pipes?

R. C. FAUGHT: I will describe a scheme. We were neither interested in accuracy or fast response time and what we did was weld a little block for a well on the pipe and cross-section this block. We had a little U-shaped configuration and a matching piece that came down. These were glass insulated 3/32-in.-OD sheath couples and were completely insulated.

P. BLISS: We have one way that we have not quite licked yet, but we are interested in both accuracy and response time and we can't get either. In our in-pile work we are always cramped for space. We have a lot of thermocouples on a piece of 1/2- or 3/4-in.-OD tubing. This I merely mention because it emphasizes the fact that there are a lot of possibilities for heat transfer errors. At the end is a pipe with a wall thickness of maybe 1/16 in. Here is a 1/16-in. thermocouple. We were quite surprised to find out that by attaching a sizable bead here we gained more by the area of the heat transfer than we lost by the additional mass. We were able to reduce our time constant by a factor of almost 10.

J. F. POTTS: A lot of us are interested in in-pile radiation damage to thermocouples. We have personal reasons for being or not being particularly worried about it, based as far as I can find out on fairly inconclusive evidence, but the situation needs to be wrapped up, probably by a joint effort of the AEC and its prime contractors. F. C. Legler has

been thinking a lot about this, I know, and I would appreciate hearing any late comment he might have on the subject.

F. C. LEGLER: We are now making a survey of combined high-neutron-flux and high-temperature environments. Some of you have already been contacted. There are two problems: the high temperature environment alone, and the combined problem. We don't have any conclusive information. The MTR people have run some loops at fairly high temperature, at high dose rates and dosages, too, and they have not noticed any deterioration of the thermocouples.

Someone pointed out work hardening of the Chromel and Alumel. This can be produced by fast neutrons. I am not sure what the fast flux was in the MTR. So until we get all this information we won't be able to draw any conclusions.

R. L. MOORE: We have been talking about permanent effects, and about putting couples into the piles, then checking them to see whether the calibration has changed. Admittedly, that is one thing to look at.

I keep hearing about the immediate effects, the in-pile operation, whether there is gamma heating of the couples. I have never seen this. We have a number of couples that I think are in an ideal location for seeing this effect if it occurs, in the pressurizer on the HRT. It is under relatively high flux, maybe 10^{10} neutrons and maybe 500,000 or 600,000 r (gamma); yet within what we consider the accuracy of the thermocouple its readings check the steam-pressure measurements.

J. E. OWENS: If you are pulling Fiberglas thermocouples in and out of the reactor as part of the experiment you can get into trouble with activation and powdering.

DISCUSSION OF PROCESS INSTRUMENTATION IN
HIGH-TEMPERATURE ENVIRONMENTS

R. G. Affel, Chairman

R. G. AFFEL: We will begin with questions to be directed to the speakers, since I am sure some of you held off earlier, and then, if agreeable, talk on the general categories of pressure, level, temperature, and flow as applied to the interests of the group.

R. A. EDWARDS: Mr. Smith, in your induction heating system, did you wrap steel around the pipe?

F. A. SMITH: Yes, it is a 3/16-in. shell formed in a rolling operation in the Argonne shop. It does not cost much. We find it has to be tack-welded. Any current heating is local heating.

R. A. EDWARDS: Is it directly in contact with the pipe or spaced away?

F. A. SMITH: It is in direct contact.

R. A. EDWARDS: What is the maximum temperature you bring your system to for preheating purposes?

F. A. SMITH: For initial sodium filling from tanks into the system, or even in starting up a system that has been filled and is frozen, we are more or less arbitrary. We don't like to stick to temperatures near the melting point of sodium. We prefer roughly 275°F.

R. A. EDWARDS: How hot can you go?

F. A. SMITH: Well, the large 12-in. pump that you saw, with asbestos-insulated wire spaced about 1/2-in. apart per turn, has gotten to around 360°F. Heating is no problem; it is fairly easy.

D. W. HUSZAGH: At Brookhaven we have been very interested in induction heating. We are building a 4-in. test loop. All the piping will be induction heated. We ran some very extensive experiments on induction heating of standard pipes. In one case we wanted to put 10 kw in a 7-ft-long pipe. We used 60-cycle power and we used type TW insulated wire. It was wrapped around the pipe and we found that the temperature climbed so fast that the temperature recorder could not keep up with it. The temperature leveled off at 1400°F and it would not heat up any more, no matter what we did; so it is self-leveling. It reached the place where the iron was no longer permeable and the system controlled itself. That was ideal for our purposes.

The power factor goes down a little when you use more and more steel. You heat more readily in that case. We found that 8 or 9 turns of wire per running foot is about what is needed. In a 4-in. pipe you get about 100 w/ft of heat input and you need something like 30 amp.

R. A. EDWARDS: How finely can you control your temperature with that kind of system?

D. W. HUSZAGH: Perfectly. You can use a very accurate tap switch or anything you want. You could use a reactor but you would have a poor power factor. Although we are doing it, it is not a good idea.

R. A. EDWARDS: Have you considered scanning various circuits?

F. A. SMITH: This is the point: You have to differentiate, in my opinion, between what I call plant heating facilities and experimental heating. Are you interested in talking about reactor plants, or of experimental loops?

R. A. EDWARDS: Well, I am afraid I will have to look at the plant system first.

F. A. SMITH: For the plant system, in my opinion, you don't need any control. It is very simple to design the space factor for induction heating to give what you want. We have one breaker for a 440-v induction-heating circuit. That is for the whole loop.

D. W. HUSZAGH: If you come out with unequal lengths of wire, tack on a piece of dummy pipe and wrap it.

R. C. FAUGHT: When you have to bring up the temperature very evenly all around the plant, you cannot do it with one circuit. You have to use several. Let's say it is a 60-ft-high reactor, 25 ft in diameter, with 350°F at most. It should have a constant temperature, say, $\pm 30^\circ$.

F. A. SMITH: This becomes a design problem for your particular reactor tank. For example, I showed you a photograph of the EBR-2 model tank which is 150 ft high and 10 ft in diameter. For convenience and experimental ease, we put in two coils, and a back-breaking number of thermocouples.

R. C. FAUGHT: From an economic standpoint is this more economical than Calrod heating?

F. A. SMITH: Much more so; if you have austenitic stainless, then you have to admit the expense of cladding, but this is a relatively small expense compared to the cost of Calrods and the labor cost for installing

a Calrod system. The KAPL people who worked on the heat loop will confirm this.

R. C. FAUGHT: Mr. Lane, have you ever calculated how much of the power is going into pipe heating?

M. R. LANE: I have not been working on that piping. It is a lot of kilowatts but, of course, it is only for a short time.

R. C. FAUGHT: You are using induction?

M. R. LANE: Yes, all the pipes that we heat are heated by induction -- all the primary and secondary loops and the service loops. Everything except the reactor vessel.

R. C. FAUGHT: How is that heated; Calrod?

M. R. LANE: That will be resistance heating.

H. H. HENDON: We have a problem in heating test loops. It appears from the economic consideration that the system heating is best done where the loop itself performs as a shorted turn of a transformer secondary. How does this compare economically in your opinion with the induction heating you talk about?

F. A. SMITH: I think we ought to take turns in answering questions. I don't want to answer them all. But we have done an awful lot of resistance heating at Argonne. I didn't make the sodium technology talk but the 10,000 gpm d-c pump in our test facility is heated with resistance heaters of a variety of types, and for laboratory work where you can afford almost anything to justify convenience, fine. We are loaded with Calrods.

H. H. HENDON: I am referring to the containment itself being the resistance element.

F. A. SMITH: That has been done also.

D. W. HUSZAGH: We find that any weld or discontinuity where there is a restriction can seriously overheat; it is very tricky.

R. G. AFFEL: At ORNL this scheme is used for high thermal output. We have used currents of the order of 50,000 amp.

L. P. INGLIS: We have made many transformers at Atomics International. We have about two dozen, and they are probably one of the cheapest things you can make.

H. H. HENDON: Do you wind the secondary of the transformers?

F. A. SMITH: We do the interwinding with the vessel and wind the secondary on the transformer.

J. E. OWENS: We have found that these heating transformers work really good in simple systems where you have a nice square pump loop to play with, but if you try to design them for reactors the control problem drives you crazy.

D. W. HUSZAGH: You get network problems, too.

J. E. OWENS: I think over-all, for simple loops, they are cheaper than strapping Calrods to the system, putting thermocouples here and there, and buying switches.

L. P. INGLIS: I would like to mention that A.I. has a split opinion on that very question. Some of us believe that the control of these complex loops is not complicated and that the problem can be solved economically. I claim that transformer heating for either the reactor plant or the experiments is cheaper and better than resistance-element heating; with induction heating, I would not quarrel one way or the other.

H. H. HENDON: How about your power factor in the heating transformer?

L. P. INGLIS: It is very good at high current.

P. BLISS: What sort of lamination do you use?

L. P. INGLIS: We usually overlap them at the corners.

P. BLISS: Just rectangular?

L. P. INGLIS: Yes.

N. HUSTON: Do you find that it is necessary to supplement your induction heating with Calrod heating around the valves and the big elbows?

F. A. SMITH: We have done experimental work on 12-in. pipe, and we find that you don't need to heat the elbows. You should heat the valves. The G-E a-c linear pump is heated with Calrods. Other electrical heating circuits are necessary.

S. A. HLUCHAN: I would like to ask all of you what you consider should be the thickness of the membrane in contact with the process fluid, say sodium or NaK, for sensing elements. Would you say that a 0.005-in. membrane is sufficiently thick in a stagnant region contacting the sodium or some mixture containing sodium?

P. BLISS: At what pressure?

S. A. HLUCHAN: At 100 to 300 psi and 800 or 900°F.

F. A. SMITH: At those temperatures and pressures, with power plant reactor sodium techniques, I would stick with about 0.020 in.

R. G. AFFEL: We would prefer 0.025 in.

S. A. HLUCHAN: Would you consider four plies of 0.005 in. equivalent from the corrosion viewpoint? The theory is held by some that a multi-ply membrane would be satisfactory, in that corrosion starting at a pinhole would not penetrate the second ply necessarily and take off at another point between plies.

R. G. AFFEL: On your multi-ply units, we have not found any unwarranted effects with the diaphragms. They have been completely satisfactory. The bellows in the early days of fabrication, as you probably recall, were subjected to thinning in the extremes of the convolutions. In many cases 5 ply, each nominally 0.005 in., is only 0.010 in. thick because of the thinning, but on the convoluted diaphragm this has not been a problem.

P. BLISS: You say that it isn't a problem when it contacts the solids?

R. G. AFFEL: Not for the pressure transmitters. We have not, as a matter of fact, had the success with the single-ply diaphragm of 0.025 in. that we have had with the multi-ply units.

P. BLISS: We disagree.

S. A. HLUCHAN: The more thickness you add to your specification, the more cumbersome the instrument becomes. If you are using this membrane as a seal for some external system for transmitting motion, mechanically or hydraulically, it becomes bigger and much more expensive to fabricate.

W. R. MILLER: We are dealing with a number of manufacturers along this line, and in spite of what the manufacturers think, we do it strictly because of the corrosion-resistant properties that we think we need. We would like very much to go down to 0.0035-in. diaphragms because the transmission would be that much more accurate. Corrosion dictates greater thickness, and I think all of us are stuck in that respect.

N. HUSTON: Can we get the sodium coolant man in on this, too? I cannot envision excessive corrosion with sodium.

F. A. SMITH: Can you qualify that to 1000°F, or another temperature?

N. HUSTON: Let's say 1000°F.

F. A. SMITH: Unfortunately there are a variety of sodium-cooled reactors and the corrosion rate is tied up with the oxygen content. You have to know the leakage rate. Once you can determine the expected average bulk sodium oxide content — how much you can pay for cleaning it up

on a gross bulk plant scale -- then you might more precisely determine what you can live with. Sodium with 100 ppm of oxide will corrode stainless steel. I think that most of us in the sodium plant business are below 5 ppm. I would be inclined to stay on the conservative side for thicknesses of thin membranes.

S. A. HLUCHAN: You would only purify your sodium to a certain quality. You feel that that would be the economical stopping point, and then you would expect everything in the system to withstand the corrosion that is associated with that degree of oxide impurity?

F. A. SMITH: In my opinion, for application in central station power plants there is no continuous indicating meter that you can hang on the panel board that says, "This is the sodium oxide content." When that meter exists, then I am with you: we can make the diaphragms 0.002 in. thick, but until that instrument exists you don't know where you stand.

R. F. HYLAND: Isn't it also true that the data correlating corrosion and oxide content is poor?

F. A. SMITH: There is a lot of information available. A.I. is doing work on this; Argonne is doing work on this. It is a genuine problem to understand the fundamentals of corrosion, the effects of oxygen, and the effects of other impurities. We don't really know.

J. E. OWENS: We have had some trouble in test loops which were drained and cleaned out with alcohol, and what have you, and refilled. We have lost some bellows in cases like this, and we feel that probably if the system is kept full of sodium you are not too badly off, but if a test loop is drained and cleaned out occasionally, you will get oxide pockets, and probably holes, in every bellows.

F. A. SMITH: We agree with you. Because of the poor bellows-sealed 12-in. Y-valves that you saw in the figures showing the a-c linear pump and the mechanical pump, we have had to install fixed orifices to set the proper operating head for the pump for normal operation. We did this by freezing the sodium in the loops solid, using a commercial power-driven hacksaw to cut out a segment of the pipe, and then quickly taping over the ends. We used the balloon technique, and we did get a film of oxide over the area. We lived with it. We drilled back and welded the orifice

in, using an inert gas during the welding process to minimize any further oxidation or hydroxide formation.

As to the corrosion rate with sodium hydroxide, there isn't enough work being done to determine what these effects are.

R. G. AFFEL: Does anyone have any comments on level measurements?

T. M. GAYLE: There is a heated-thermocouple level probe, designed for the homogeneous reactor chemical plant, and the usual problem exists: a great time lag. I don't know how many thermocouples it uses. I think perhaps S. A. Hluchan, with the Taylor Instrument Companies, could tell us something about the level probe that is in the process of being developed.

S. A. HLUCHAN: We made a feasibility study with Westinghouse on the thermal type of probe. We got into the problem of response time as a compromise with mechanical strength due to vibration — this is an application where there is considerable shock and vibration.

Two ideas were involved. One was a sort of bridge with two loops, one heated and the other non-heated, connected to two arms of a bridge, using temperature-sensitive wires inside the sheath. I think the ID of the sheath was about 1/16 in., and the wire inside was tungsten. It was a special Calrod-like wire made by the Thermo-Electric Company. We could get considerable signals by adjusting the heater current. We could get about 20° rise on the portion of the sheath in the gas phase and, of course, the portion below in the liquid phase would be practically at the liquid temperature. With a construction like that you would get temperature compensation along the entire length of the two loops.

R. G. AFFEL: This would be actually only a plant-type instrument for slowly varying levels, would it not? Not an experimental apparatus, shall we say, for rapidly varying levels?

S. A. HLUCHAN: You could achieve response times in the neighborhood of 10 to 20 sec, I think. We calculated about 12 sec for a falling level and about half that for a rising level. But the loops had to be immersed in a stagnation well, because the over-all heat transfer at a given velocity differed between the two phases. After a few feet per second velocity the heat transfer was more or less constant for liquids, whereas the heat-transfer rate surged with a rise in air flow. Where there is a very great

over-all heat transfer ratio between liquid sodium and an inert-gas phase, I think further development work would be warranted.

H. J. METZ: As a matter of historical interest, the first thermal-type level probe I saw, 10 or 12 years ago, was being used in Israel on the Dead Sea in a potash plant. They put a heater in the center of some electric insulation. Then they wound a temperature-sensitive wire around that and some more insulation. Then another temperature-sensitive wire, so that as the probe became immersed, the rate of heat transfer changed from the inside to the outside.

T. M. GAYLE: Is the J-gage generally understood or described? I would like to see a description.

R. G. AFFEL: Mr. Burger, would you come up to the blackboard and elucidate?

G. H. BURGER: The J-tube has the same effect as a straight probe mounted from the bottom of the pot (Fig. 9.1). The one we use is made of 3/8-in. sched-40 pipe in the shape of a J. We tied one power supply lead in the bottom of the horizontal section, and the other in the top of the shorter vertical section. The two voltage leads were connected in the same manner. We used a power source of about 3 v, at 20 amp, 60-cycle. We took our output across the vertical leg which ran from 0 to 20 mv.

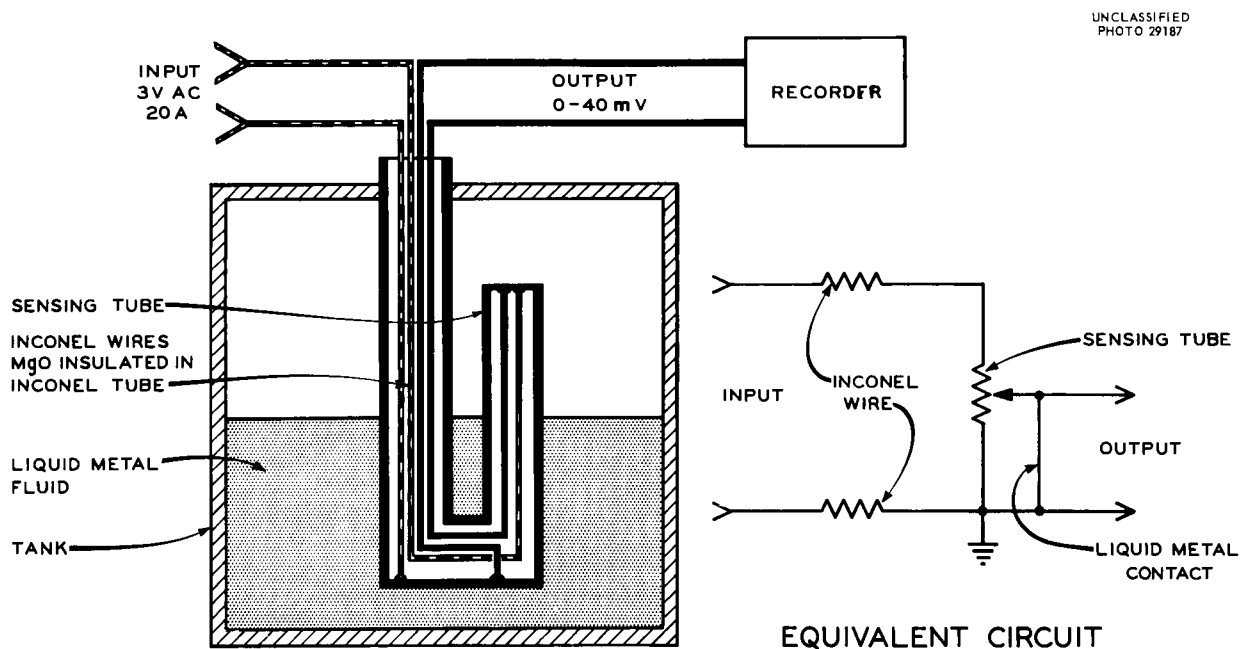


Fig. 9.1. Diagram of J Level Probe.

With no level in the tank we read the full output of 20 mv. As the level rises above the probe, the liquid shorts out a section of the probe with the fluid. Therefore in order for the probe to operate the liquid must have a higher conductivity than the pipe from which the probe is made.

We fed this output signal directly to a Brown strip-chart recorder modified for 60-cycle operation. The system accuracy is quite good: within less than 3%. The temperature effect, say from 0 to about 1400°F, is about 3%. This was tested in NaK.

L. P. INGLIS: Three per cent of the scale?

G. H. BURGER: Yes. The single straight probe inserted from the bottom produces the same results and eliminates the J section. Temperature compensation is accomplished by the resistance of the two input leads. The output voltage equals the input voltage times the resistance of the sensing tube (the shorter vertical leg) divided by the sum of the resistances of the sensing tube and the two input leads.

You make the input power lead wires of sufficiently higher resistance so that any change in resistance of the fluid or pipe itself is comparatively quite small. Also, the long wires restrict the temperature effect primarily to the bottom portion of the probe; therefore it is only a small portion of the total resistance. So what we call temperature compensation is actually a result of such arrangement of the wires that any change in resistance is fairly small compared to that caused by level rise on the probe.

C. S. WALKER: I don't understand how it measures level. Is the resistance of the single input wire in the sensing tube the only variable?

G. H. BURGER: Essentially it is.

C. S. WALKER: With temperature changes, all three of them vary?

G. H. BURGER: They do, but the amounts that the other two vary are quite small because most of the wire is outside the high-temperature region. As I said, we get a 3% output-voltage change between 0 and 1400°F.

R. D. WEBB: That is reproducible, isn't it? Do you need a vacuum tube to read it?

G. H. BURGER: You could use a vacuum tube, but it is not necessary; an ordinary low-impedance voltmeter would be satisfactory.

The on-off probe is essentially the same type of device (Fig. 9.2). As is the J-tube, it is made of pipe or tube. The all-welded on-off

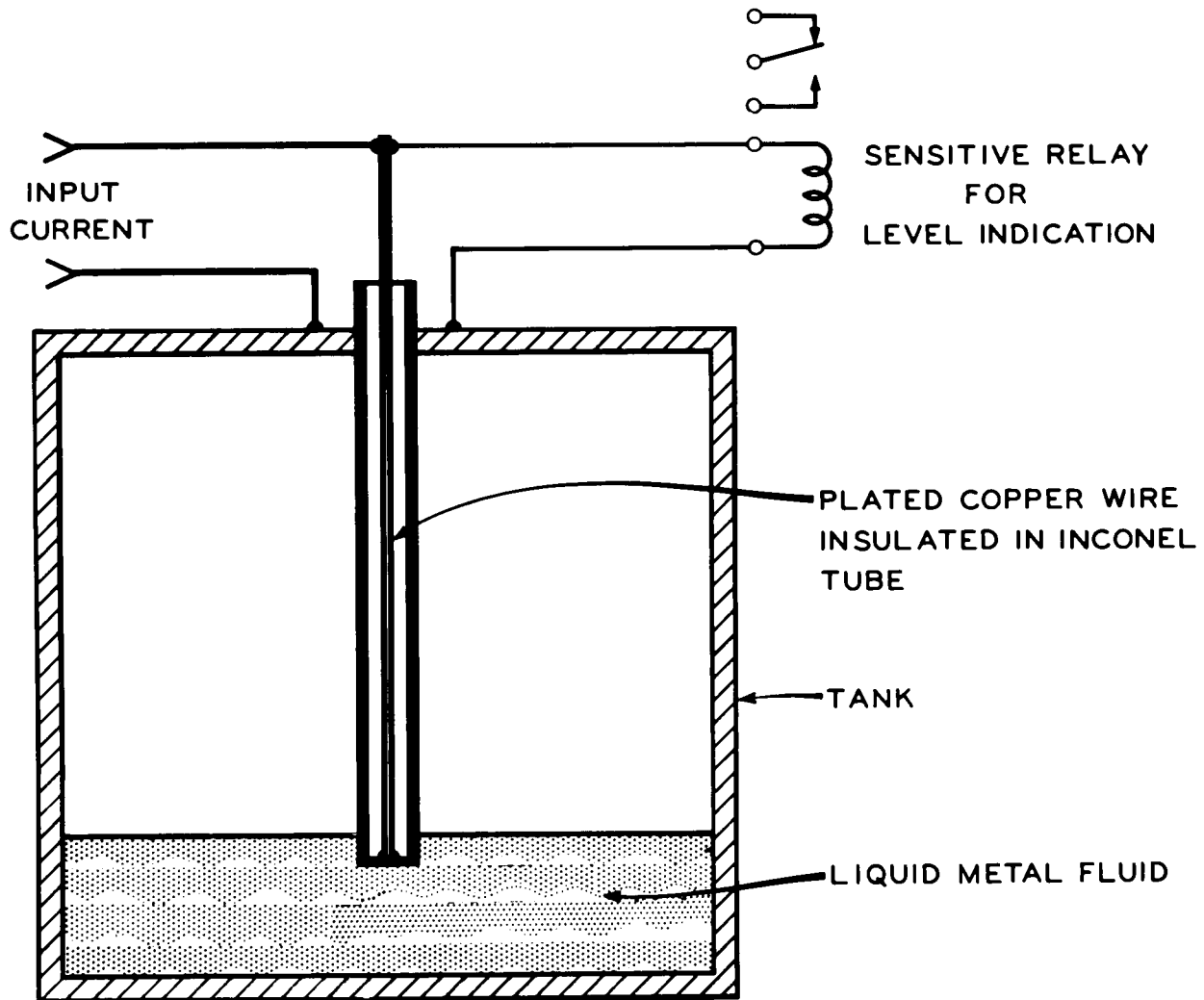


Fig. 9.2. Diagram of On-Off Level Probe.

probe is made of the material you would be interested in, maybe Inconel or stainless steel. It is welded into the tank at some point. Again it is operated by an a-c 60-cycle power source of about 3 v, which is satisfactory for our purpose. It has a single center conductor of nickel-plated copper about 1/8 in. in diameter, welded to the bottom of the tube. We had the tube capped and sealed at the bottom. As the level rises along the tube the liquid causes a change in current output which we use to operate a relay to indicate alarm. The principle is very similar to that of the continuous probe. The normal current path is down the center electrode and back up the tube wall. When the fluid touches the probe bottom the current is shunted around through the tank wall. The tube wall is

quite thin. We use, I believe, a 35-mil wall, which is, however, quite large from a current standpoint. As the liquid level comes up on the bottom of the probe, the current change will still be large, however, because the resistance is quite high through this section, compared to that of the tank wall.

W. R. MILLER: Where is the point at which you can adjust the relay on the probe? Halfway up or near the bottom?

G. H. BURGER: At the bottom. Of course, this requires a quite sensitive relay and we use a rheostat to adjust the relay current.

R. G. AFFEL: A magnetic amplifier is a natural for doing this; actually, we got by with some \$2.50 Sigma relays.

G. H. BURGER: We went to this because our experience with spark plugs was quite poor, particularly with the top of the plug above 400°F.

D. W. HUSZAGH: We had years of bad experience with spark plugs at Brookhaven. I don't know the chemical difference between sodium and bismuth as far as vapor pressure is concerned, but we had the same trouble you did. By using a side arm of very small free surface we got rid of the splashing. We have 20,000 hr on this probe. The wires are spaced 3/16 in. apart. We have never had a short, and we just use a neon light.

R. G. AFFEL: Don't you get into trouble with "crud" being conductive enough to light your neon?

D. W. HUSZAGH: No, we get a faint glow - after a long period of time. We wait until the loop is shut down and drain it all out and heat the section up to clean it for the next run. The signal from conduction by the liquid is unmistakable. On cutting the probes apart at the end of a 20,000-hr run, we found hardly any vapor condensation at the top. We thought we would get at least another 20,000 without any signs of shorting. We had exactly the same trouble as you did until we went to the sidehandle.

S. A. HLUCHAN: That sort of thing would not work on sodium. The vapor would go up as fumes even with the upper portion heated.

R. G. AFFEL: We tried that to some extent. I think this is largely a function of temperature and vapor pressure. We were interested in a system temperature perhaps as high as 1400°F. We did get some indication that this was still a problem. You get "crud" deposits of some sort on the spark plugs even with that. They light up beautifully.

P. BLISS: We get the same thing with 24-v relays.

G. H. BURGER: There is one problem associated particularly with the resistance probes. That is wetting, which is difficult below 600°F. It will eventually wet even at lower temperatures, but it is quite slow and we can never reproduce it from batch to batch of NaK. It may vary with the oxide content. It may vary with the gas purge. We may get a purge line that contains oxygen.

L. P. INGLIS: To what degree do you regulate current?

G. H. BURGER: Practically no regulation other than a regulating transformer.

We had some J's that operated around 6000 to 8000 hr without any problem. The only trouble we have ever had with them were the welds. They are welded in a corner in the horizontal section.

P. BLISS: One vendor claims that he can make that J-probe with no weld. I have not seen his product yet.

N. HUSTON: Do you feel that there are any length limitations on this?

G. H. BURGER: No, but we had planned them as long as, I believe, 36 in.

R. G. AFFEL: Mr. Edwards, do you know what KAPL has run?

R. A. EDWARDS: We had some that I think were about 48 in. long in the expansion tank. These were, as I recall, not the J type but two probes. These, I think because they were shock and vibration proof, gave accuracy within about 4%.

H. H. HENDON: I have found that the output leads of the J-tube are particularly susceptible to induced voltages since they are operating at line frequencies.

R. G. AFFEL: Their impedance, however, is thousandths of an ohm.

P. BLISS: I agree, it is very bad if you have 50,000 ohms.

H. H. HENDON: And your measuring device ordinarily is a null-balance system which has essentially infinite impedance at null; so they do show up. You have to be particularly careful about running the pairs together.

R. G. AFFEL: Have you really had a lot of trouble with that?

H. H. HENDON: Yes, it appears as zero shift which is ordinarily taken out. In connection with regulating the supply, it can be wired slightly more elegantly in the form of a Wheatstone bridge, which does

make it completely independent of regulation. In fact, one that we formerly sold is a simple unregulated transformer attachment; by rigging it in bridge fashion you have no trouble with the regulation compensation.

R. G. AFFEL: On the pickup, interestingly enough, it has not been much of a problem with us. We feel that the low source impedance of the sensing element is one advantage. Certainly had we twisted the leads around some of the extremely high current lines we were using we could have gotten into trouble. But we did handle them essentially singly. Once, in searching for a spark-plug insulator, we went to the extreme of having some beryllium oxide insulators fabricated. At least in the first test they were found poorer than the conventional aluminum silicate. Is that what it was, Mr. Leppert?

A. LEPPERT: That was about 98 or 99% alumina.

R. G. AFFEL: That was better than beryllium oxide.

A. LEPPERT: Much better. The beryllium oxide shorted out in about 2 hr, whereas from the high-purity alumina we got several thousand hours under the best operating conditions.

R. G. AFFEL: At 600°F?

A. LEPPERT: No, the temperature of the plug itself and the insulator was about 400°F and below.

W. R. MILLER: Mr. Huszagh, what accuracies did you get from the Manning, Maxwell & Moore double-diaphragm pressure transmitter?

D. W. HUSZAGH: We were using an Ashcroft test gage in the gas phase to check it. We could not detect any difference in the readings. They jumped at the same time. As far as we could tell, it was 100% accurate.

W. R. MILLER: You didn't have any of these on a test stand against high-caliber pressure standards?

D. W. HUSZAGH: We considered this a high caliber pressure standard. We didn't have a mirror scale on the test gage, of course, but the needle read right on the mark.

W. R. MILLER: This was the electric transmitter?

D. W. HUSZAGH: No, we were using a pneumatic transmitter of the type that also has the indicating needle. We didn't bother with the pneumatic part of it. We were just watching the needle.

W. R. MILLER: On your Callery transmitter, were you using the ATC readout for the transmitter?

D. W. HUSZAGH: Yes, that is the one that was sticking.

R. G. AFFEL: We had the same trouble, I think, in the ATC readout.¹ How about the flow end of the business? We have spent a lot of effort on this at ORNL. We would be interested to hear comments from others. The magnetic flow meter is widely used in small sizes, and, I gather, the venturi differential-pressure transmitter combination in large systems. We were playing with some turbine flow meters as an alternate. Mr. Smith, you folks must lean heavily on the magnetic flow meter.

F. A. SMITH: Yes. That is not the whole story, but because the flow tubes are cheap enough, and because of the inaccessibility of the submerged plumbing of the EBR-2, we feel that we can afford to duplicate flow instrumentation for a complete cross check of primary reactor flow. We will have an orifice, a flow tube, pressure taps, and a long length of pipe that can be calibrated. They are all so cheap that we are not going to rely on any one instrument. We feel that for the cost involved in the EBR-1 primary tank we can afford to duplicate flow control instrumentation.

N. HUSTON: When you go to large flow meters and have a homogeneous field, it seems to me that the location of the pole face would be extremely critical. You can take care of this in your calibration, except for any shift from high temperature, which would in effect shift the pole face by thermal expansion. Have you found this to be a problem?

F. A. SMITH: We have experimented with shifting the large magnets, and it is rather insensitive. Are you talking about linear or radial displacement of the magnet?

N. HUSTON: I was thinking of off-diameter displacement with respect to the tube.

F. A. SMITH: We have not done that. We cannot or we will not afford to calibrate the flow meter in a separate test loop before it is built into the large reactor. So we hope that it won't change. If it does we will have a cross check.

N. HUSTON: What accuracy do you need in flow measurements?

F. A. SMITH: We have diversified views on this in the EBR-2 project. Some say within 5%; others 10%.

¹W. R. Miller, "High Temperature Pressure Transmitter Evaluation," ORNL-2483, May 16, 1958.

H. H. HENDON: What do you gain from the volume flow data that you get from the turbine flow meter? And if this data is of no worth, for what other reason do you turn to this?

R. G. AFFEL: Its output, as with the magnetic flow meter, is linear with flow and has a greater rangeability over the conventional orifices. The magnetic flow meter becomes increasingly less accurate as pipe size is increased. For instance, for one small unit we can talk certainly of a few per cent. In a 12-in. unit I don't think anyone would talk a few per cent. We have reason to believe, from turbine experience in other media, that its performance in liquid metal can be reliably predicted from calibration in water.

P. BLISS: You say you don't think you can trust the large electromagnetic flow meters to 2%. Are you referring to calculated calibration or permanence of calibration?

R. G. AFFEL: I think I agree with both of your statements. Actually, the circulating eddy currents in large pipes defeat, to some extent, the efforts to calculate. I don't know what your experience is with large magnets, but we have seen rather strange happenings, as I know KAPL has on field magnets. I think G. H. Burger might fill in the recent history. They have now steadied down, have they not, from the initial change?

G. H. BURGER: Yes. We tested 1-, 2-, and 3-in. units, and again we found initial flux changes as large as 1-1/2%. We don't know why they changed. It could have been in shipment, although they were supposed to be shock stabilized as well as thermal stabilized. But we did notice this, and then after running them for a period of time they changed with temperature but they would drop right back to the same place as you lowered the temperature. It was reproducible, but the cause of the initial changes we don't know.

N. HUSTON: You may find the answer in faulty control in manufacture: a slight difference that they disregard.

R. G. AFFEL: From looking at the magnets which we have broken, I think that the quality control of the central region of the material must vary. The grain structure in some magnets appears to be considerably different from that in others. We had one that we finally pinned down. It had a very fine crack that was not at all obvious in the testing procedure

but it played havoc in the magnetic field strength with small temperature variation.

L. P. INGLIS: Has anyone tried an a-c bias similar to the bias, say, in an a-c recording? That should make it more stable.

R. G. AFFEL: As far as I know, at ORNL we have not. That was the idea of looking at the turbine. We got rid of the field. For the high-temperature operation we felt that the greater accuracy and reliability might be worth it.

P. BLISS: The point was brought up earlier that the rotameter was objectionable, because it has moving parts that might cause severe damage by getting loose. The turbine flow meter certainly has the same objection. What do you have on the area flow meter?

R. G. AFFEL: Very little. I have had sad experiences with both. With the rotameter the housing has been a problem. We have had expansion troubles with the high temperature end, transmitting the signal out.

Our instrumentation has not been plant instrumentation as such. It has been for experimental rigs where high accuracies were desired for component development. Designers were determining whether they could improve the efficiency of heat exchangers and other components a few per cent. For plant service I think my comments would be different.

S. A. HLUCHAN: The Taylor Instrument Companies have supplied flow elbow elements. Ordinarily in the piping system are one or more elbows, and an elbow made up into a metering assembly and calibrated with flow would be an inexpensive and accurate flow element.

J. N. WILSON: Is anyone working on a three-dimensional flow map inside a large tank? We have worked, of course, with the three-dimensional Pitot tube. Without internal heating we have done it with two instruments like a hot-wire anemometer. There are two probes working through a hole.

P. BLISS: We have not done any of this with liquid metals yet.

J. E. OWENS: We find it extremely difficult to find a sodium pressure meter that will read very accurately below 15 or 20 psi. Why do we have such confidence in these differential-pressure measurements around elbows and across venturi sections with pipe if we cannot measure accurately?

F. A. SMITH: We do. Sodium quality is to me a really important variable. We are running loops with ordinary steel Bourdon gages and we don't get plugs. We don't have oxygen either.

J. E. OWENS: I was thinking of some of the trouble we have had with our Callery transmitter, for example. We just do not get measurements reproducible within a half psi or so. For the D/P cell sort of device that you need for pressures of 0.1 psi, do you really have anything that will measure accurately?

F. A. SMITH: We are using a manometer.

W. R. MILLER: There are some on the market that will do it.

J. E. OWENS: If we have so much trouble with pressure why do we expect to do so much better with differential pressure?

W. R. MILLER: I think in general everybody is developing higher differential pressures to measure.

R. G. AFFEL: We have used roughly a 10-psi drop, but we have not liked to design very many systems for less than 10 until fairly recently. I don't know how low you dare go.

J. E. OWENS: Ten psi is a good measurable differential.

R. G. AFFEL: Unfortunately, the designers do shout many times about 2- or 3-psi nonrecoverable drop.

J. E. OWENS: The total drop on our loop is 25 psi through the reactor, the heat exchanger, and about 100 ft of pipe.

M. R. LANE: Has anyone here had any experience with the Fischer-Porter Magnabond rotameter for measuring sodium flow?

F. A. SMITH: We have one in operation, and it is the most sensitive that we can buy ready made. Its range is 0 to 5 gpm, and we are using it in a heat transfer study loop. We checked the water calibration of an orifice against the Fischer-Porter instrument and we found that the calibrations agreed. We are buying more for heat-transfer-study loops and for convenience. If you calibrate electromagnetic flow meters, store them for some time, then use them, you never really know how precise the calibration is. So we are doing both; we are using precalibrated electromagnetic flow meters and the Fischer-Porter flow meter.

M. R. LANE: Have you had any problem in operating?

F. A. SMITH: Only one. We had to modify it by putting a stop in so it would drain. The flow arrangement was such that, mounted in the loop, it did not drain by gravity. There was just a little pin to hold the plug off the sleeve.

M. R. LANE: I trust the larger sizes will be comparable except less accurate perhaps.

R. G. AFFEL: Is anyone exploring ultrasonic flow measurements for liquid metal systems?

M. R. LANE: I know that Stromberg-Carlson was looking into this for us at one time. They dropped it and said they were no longer interested. This was ultrasonic measurement of level with the sensor at the top of the vessel. They decided that the sodium vapor would give too great a problem.

L. P. INGLIS: I was told that General Electric tried it with the submarine, and that they could not find a transducer that would stand up to the temperature.

F. LEGLER: The Acoustica people are looking into the measurement of fluid level by ultrasonics.

R. G. AFFEL: At how high a temperature?

F. LEGLER: About 1200°F. It is a continuation of the study that has already been made.

R. A. EDWARDS: I decided to try to develop a universal pressure transmitter that could be used on water, sodium, or anything, and at any pressure, by changing materials or Bourdon tubes; one that could be used for transient or steady-state service.

We made a study of the field and with Wiancko in California we developed the twisted Bourdon. It could be used for slurries or sodium. They already have the parts developed to go up to 600°F. We would like to extend this up to the limit of the metal, such as Inconel X, to about 1200°F, where the strength curve begins to drop off. I think it can be done. The response is 10,000 cps. For a 500-psi test, the over-range is about three times the rating.

D. W. HUSZAGH: Do you think erosion might damage it?

R. A. EDWARDS: I have not worried about erosion. Swartwout makes a very good flush-diaphragm type of pressure transmitter. I have had one of those in sodium service for 1000 hr, and I have a 3-mil diaphragm at

the end of the non-clogging type. It is an excellent unit, and used in sodium at 800°F.

R. G. AFFEL: How about temperature compensation on this unit?

R. A. EDWARDS: It would probably require it. Some of these are better for temperature than for pressure.

S. A. HLUCHAN: There is very little loading there. It is just a motion device. What would be the hysteresis of an Inconel spring at the same loading at 1200°F?

R. A. EDWARDS: You would change materials with temperature, coolant and pressure.

M. R. LANE: Will this have any advantage over the volumetric NaK-filled instrument of Taylor, or that of Manning, Maxwell & Moore?

R. A. EDWARDS: This will not have a secondary containment feature, obviously, but it can be built so that, if breakage occurred, everything would be contained, welded construction.

F. A. SMITH: Why do you limit your current work to 800°F? Is there a practical limit?

R. A. EDWARDS: Well, yes, you try to put your instruments on the cold leg of the reactor system; conceivably if you have a nice pressure drop across your heat generating system, the inlet of your reactor is, say, around 800 to 900°F, then it comes out at about 1100°.

M. R. LANE: Whether the fast response that you are speaking of will be required in our system is questionable. It will take a certain amount of time to build up the reaction. It is not in the order of a fraction of a millisecond. Tenths of a second would probably be adequate.

R. A. EDWARDS: Even that is beyond the capability of so-called process instrumentation.

F. A. SMITH: There is one other reason I would stick to 1200°F if this is going to be a truly universal type of pressure indicator. I think that in the sodium business we should strive for 1200°F, and I would like to measure the pressure on the discharge high-temperature end of the reactor.

R. A. EDWARDS: Are you familiar with the work that our research laboratory has been doing with high-temperature motors? They have been successfully operating a motor, for instance, at 600°C and it looks really good.

W. R. MILLER: In working with a number of different makes of pressure transmitters, we found that success in transmission depends a good deal on how much of the spring constant of the system we could get into the cold zone.

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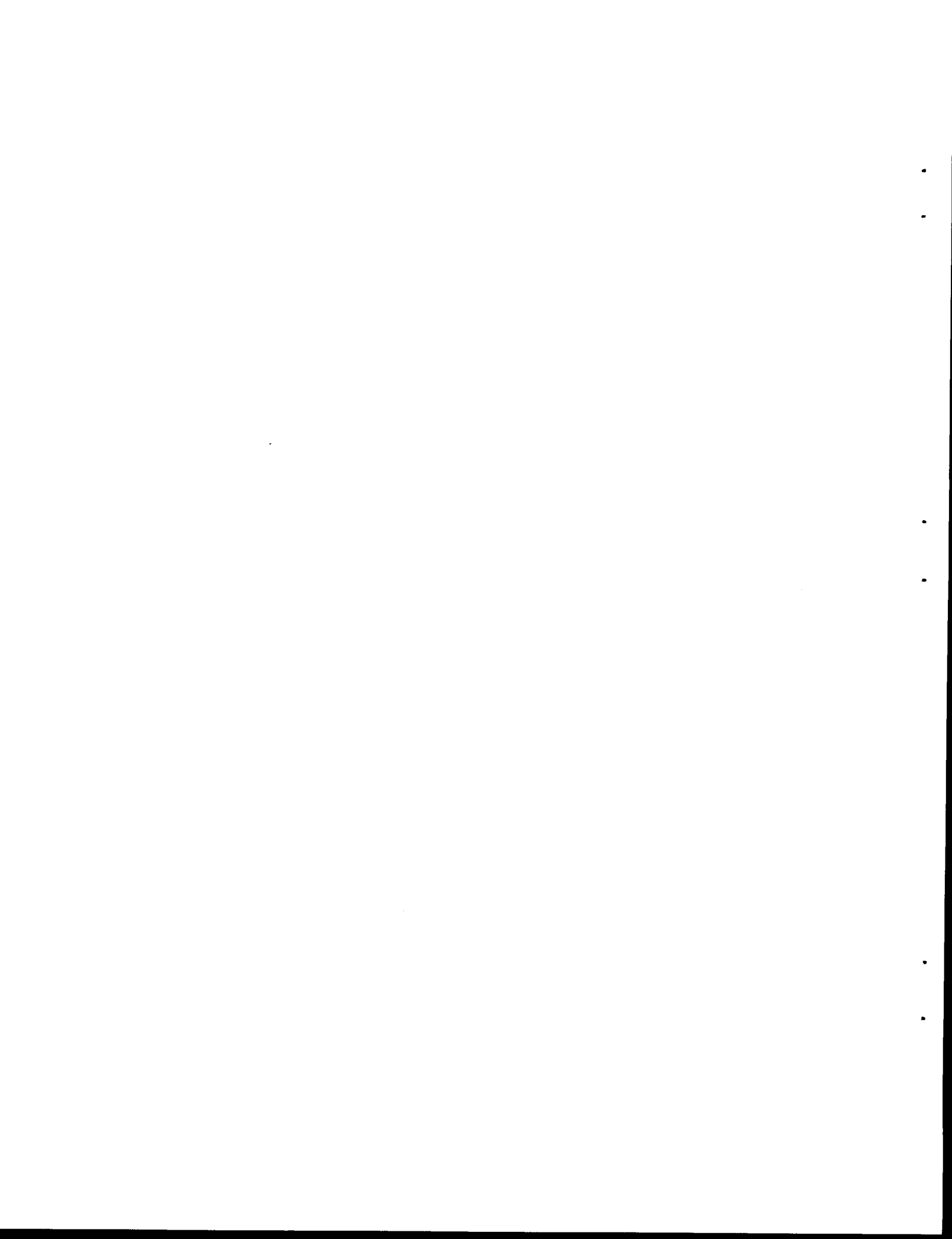
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PART III

SYSTEMS ENGINEERING OF SAFETY AND CONTROL SYSTEMS



SYSTEM DESIGN AND INSTRUMENTATION OF IN-PILE TEST LOOPS

M. A. Vogel

Westinghouse Electric Corporation

M. A. VOGEL: The most important functions of test-loop instrumentation are control of experimental conditions and maintenance of the safety and continuous operation of the reactor. This paper describes how this will be accomplished by the Westinghouse Testing Reactor (WTR) loop-control system. A test loop might be defined as "an independent mechanical system that circulates fluid through a source of neutrons." This applies to mechanical aspects of a test loop, but its instrumentation and control system is integrally related to the processes in the reactor. The basic function of a test loop is to provide a controlled experimental environment, duplicating reactor conditions.

The loops designed for the WTR are now being fabricated. The WTR is now in early stages of construction at Waltz Mill, Pennsylvania. Figure 10.1 is a picture of the model of the WTR. The test loops will be either

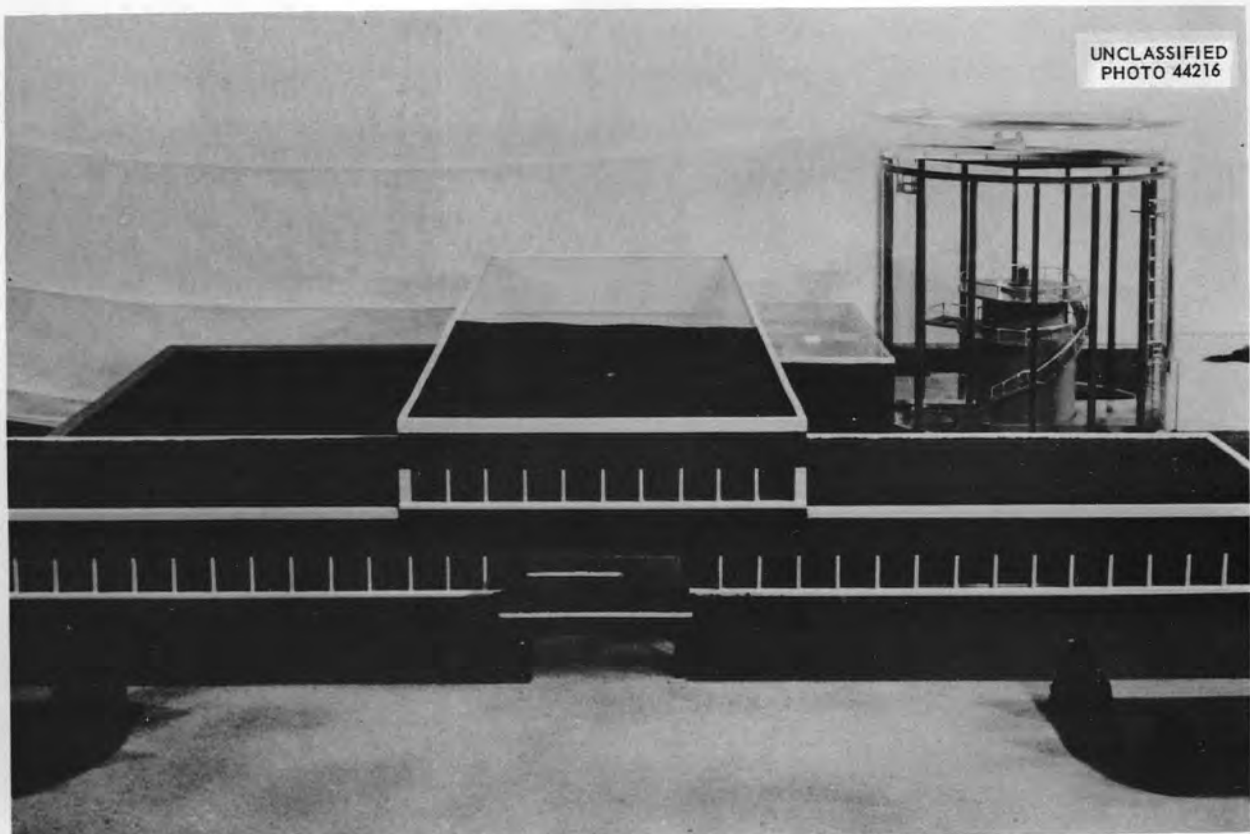


Fig. 10.1. Model of the WTR.

around the sub-pile room inside the vapor container or inside the "truck-lock" (not shown).

The WTR test loops are basically designed for pressurized-water experiments. However, with some modifications they may be utilized for boiling-water or other fluid-type experiments. The main packages of a test loop are the in-pile pressure tube (thimbles), the out-of-pile components, and the instrumentation.

Figure 10.2 shows the general relation of the test loops to the reactor proper and the various other items. The loop piping will pass through the shielding around the sub-pile room and be attached to the in-pile thimbles, which protrude from the bottom up into the core. Experimental samples may be placed in the upper end of the test thimbles for irradiation. The control of the hydraulic system associated with this irradiation space is the subject of this discussion.

Figure 10.3 shows the basic design of the in-pile pressure tube and schematic enlargements of its internal components. It is approximately 13 ft long, and contains a small concentric tube. The water flows up through the annulus, to the top, down through the middle, and out through the connecting piping to the external loop equipment. Test samples can be mounted in any desirable manner in the upper section.

Figure 10.4 is a schematic flow diagram of the test loop. It does not show the instrumentation. The maximum operating specifications for this test loop are 2000 psi and 600°F. Flow through the main piping is 45 gpm; makeup flow is approximately 1/2 gpm; purification flow is 1 gpm.

System Components

It is useful to describe (Fig. 10.4) the system and auxiliary components, so that they can be related to the control functions. The water passes along the piping from the test thimble through the cooler, which serves to dissipate the fission heat. The cooler can extract up to 250 kw of heat. The water then passes through the two primary pumps, which develop the necessary head for circulating the coolant. A standby pump provides flow in the event of failure of the primary pumps. Attached to the primary system is a combination pressurizer-surge tank, which absorbs small volume changes due to temperature changes as well as maintaining

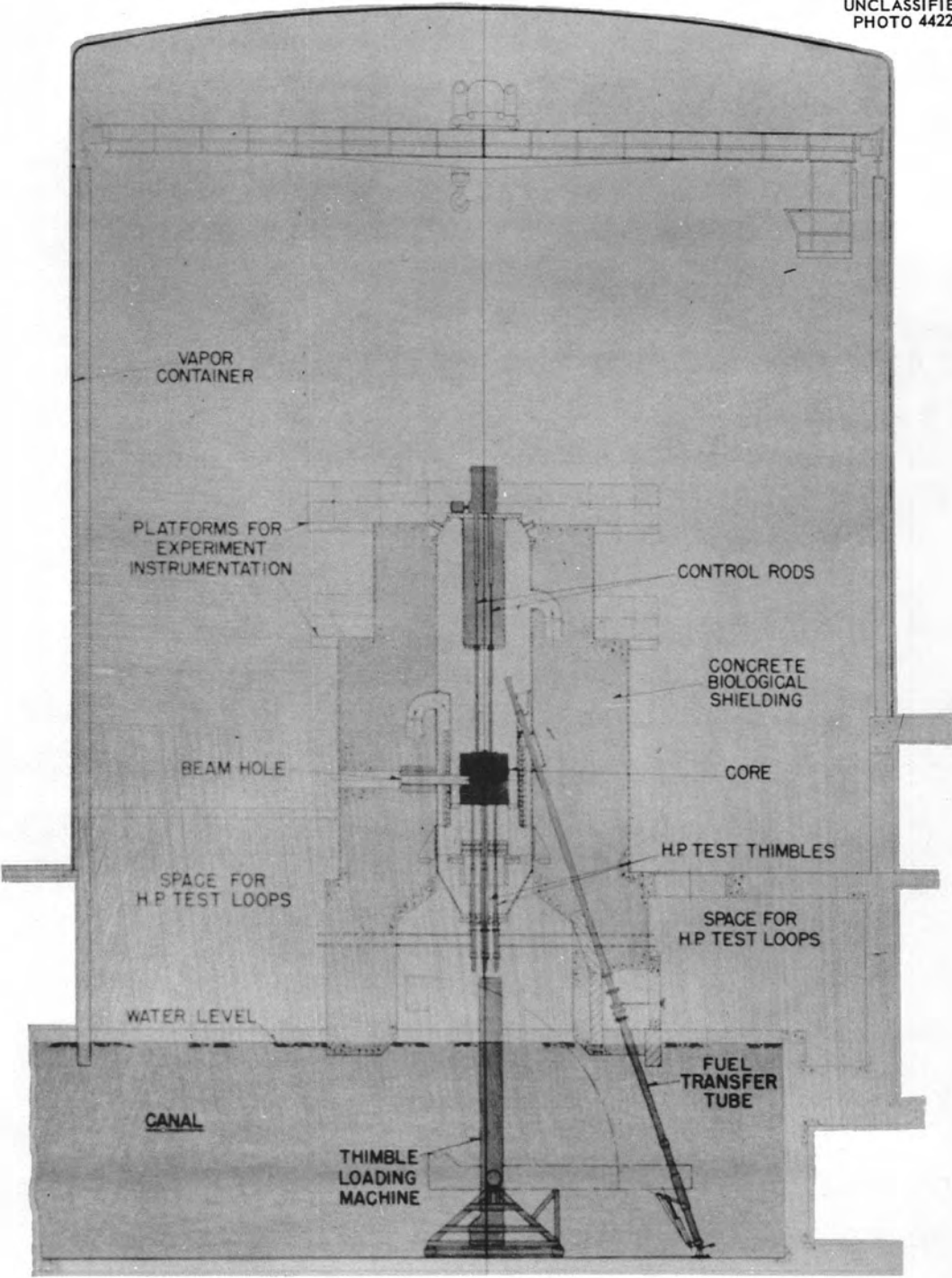


Fig. 10.2. WTR Reactor Building.

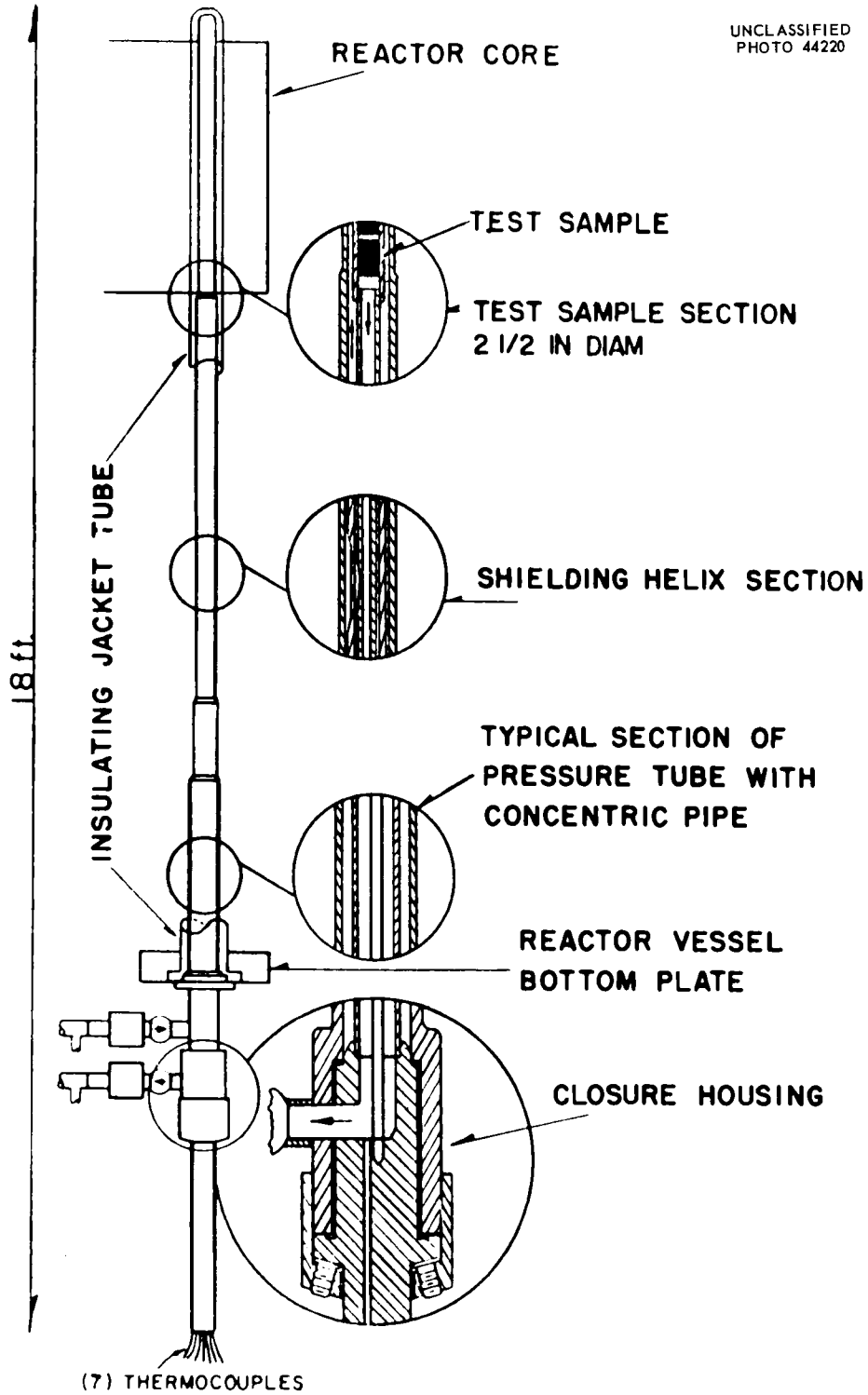


Fig. 10.3. WTR In-Pile Pressure Tube.

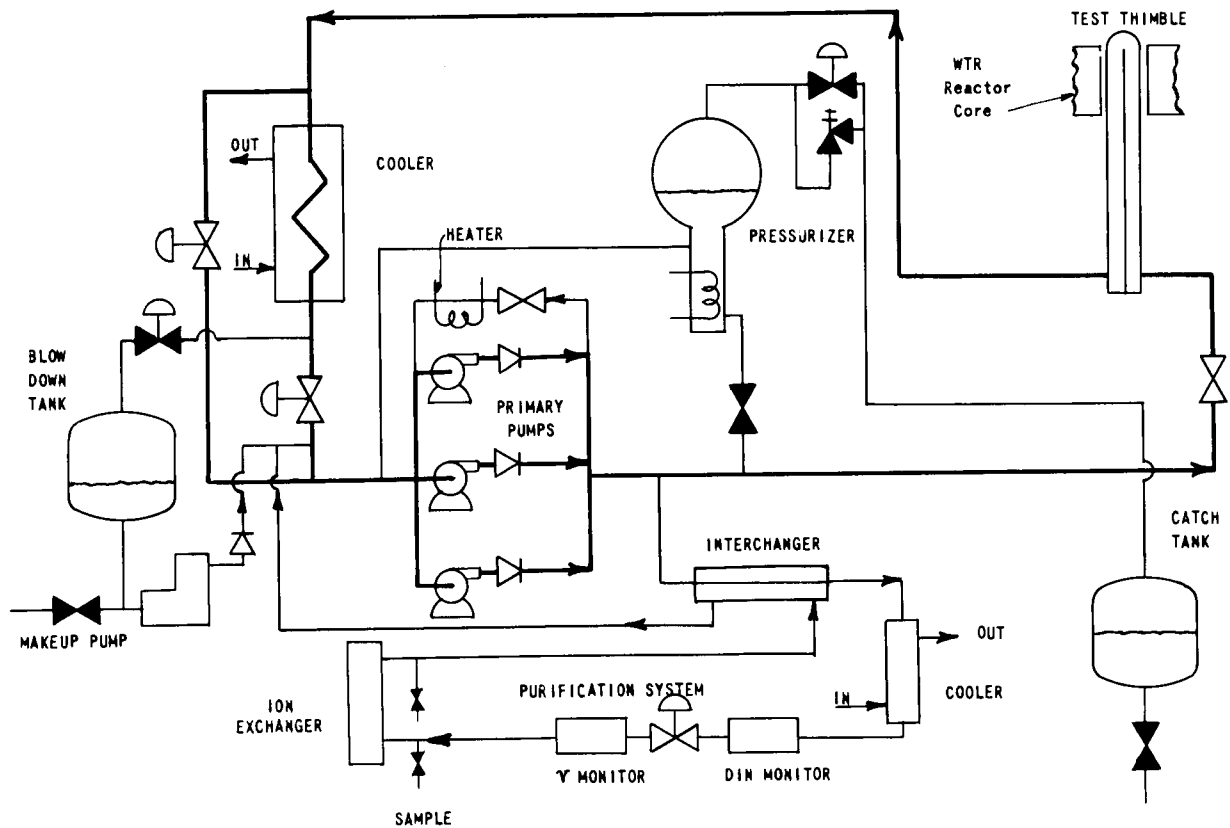


Fig. 10.4. Flow Diagram of WTR In-Pile Test Loop.

pressure. When its heaters are turned on, steam produced in the upper section maintains system pressure. The electric heaters are located in wells inside the vessel. The pressurizer is also utilized to de-gas the primary system when starting up. A loop heater is also provided to maintain system temperature while the reactor is not operating. It is a section of 1/2-in. sched-80 pipe in parallel with the circulating pumps and divided electrically into three sections, each of which becomes a resistance in each phase of a three-phase system.

Some auxiliary components are the catch tank, to receive pressurizer blowdown and loop drainage, a blowdown tank to bleed off loop fluid during temperature changes, and a makeup pump. Large volume changes are compensated by bleeding water into the blowdown tank, or by removing water from it with the makeup pump. A feature of this system is that loop fluid can be recovered. This is particularly important for heavy water experiments, where the fluid is very precious. In parallel with the pumps there is a

purification system which maintains freedom from impurities. In passing through the purification system, there are an interchanger, a cooler, a delayed neutron monitor, a gamma monitor, and an ion exchanger. There are also provisions for sampling.

Basic Controls

The instrumentation and control components, not shown in these figures, consist of instrumentation racks, control centers, a data logger, and an alarm logger. The instrumentation racks house the annunciator panels and the various indicating and controlling instruments. The control centers contain switchgear, circuit breakers, and transformers. The data logger is a recording device providing a continuous record of up to 100 points which may be printed out as frequently as once in 15 min. The data logger receives analog signals from the various primary detectors, converts them to digital form, and prints them out on an IBM typewriter. The purposes of the data logger are to replace an operator and to provide experimental data in neat concise form. In addition to this, when the data logger receives an alarm it actuates the alarm logger, which then prints out in red any variables which are not within operating limits.

The basic loop control actions initiated when the loop parameters exceed their limits are designed to meet the following important requirements:

1. Safety of the reactor. This takes precedence over all.
2. Assurance that in-pile experimental samples remain intact (do not melt down).
3. Continuity of reactor operation - assurance of a maximum number of megawatt-days of operation, regardless of malfunction of a particular experiment.

The control functions that are initiated to meet these requirements which I just mentioned are an alarm, a cutback, or a scram. An alarm is a visible and audible signal on the instrument panel. A cutback is a reduction of power to 20% by driving the control rods into the reactor. A scram drops the control rods and reduces the reactor power to zero in a very short time. An alarm always precedes a cutback, which always precedes a scram; every effort is made to let the system correct itself. The reactor is scrammed only when absolutely necessary.

Temperature Control

Figures 10.5-10.8 show how the various parameters control the loop system. Figure 10.5 shows the loop temperature control system. Only the primary system is shown. The most important variable is the temperature difference across the test thimble. This is ΔTIC , which controls the cooler-bypass valves.

The other important thermal item is the main loop temperature, TIC. This represents the temperature of the mixed fluid from the cooler and bypass. It also reflects any heat put in by the heater. The total flow for a given experiment is relatively constant. Flow control is achieved by properly proportioning the flow between the cooler and the bypass. Flow through the cooler is normally much less than bypass flow, so that a little fluid is cooled a lot. To produce a thimble inlet temperature of 500°F, for example, at a thimble outlet temperature of 575°F and a bypass rate of 75%, the cooler reduces the temperature of 25% of the fluid by 300° (to 275°F), thus effecting an over-all reduction of 75°. The loop heater comes on only to keep the loop hot when the nuclear source of heat is turned off.

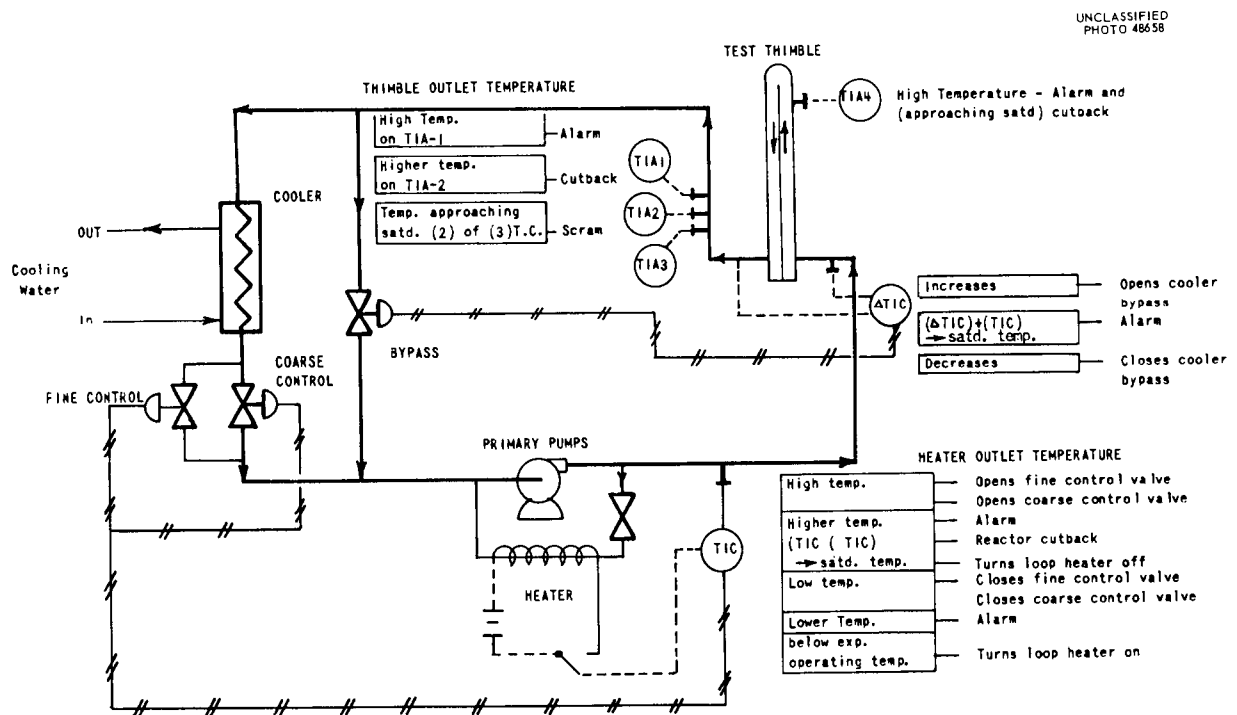


Fig. 10.5. Temperature Control System for WTR In-Pile Test Loops.

High relative temperature on TIA-1, -2, or -3 actuates an alarm. On higher temperature on TIA-2, a cutback is initiated. The temperature setting for each set point must be evaluated for each experiment.

If the temperature of any two out of three approached saturation in a pressurized water system, the reactor would scram. The object of this two-out-of-three system is to make very sure that a condition requiring scram actually exists.

As the temperature difference (ΔTIC) across the in-pile system increases, the cooler bypass is opened. This decreases the system pressure drop, and therefore the flow increases to bring the thimble-outlet fluid temperature back down. This happens almost instantaneously, because a change in flow rate is reflected very rapidly at the thimble outlet. Opening of the bypass increases the mixed-fluid temperature (TIC), and accomplishes the desired control action by opening the control valves in series with the cooler. In the event of a decrease in ΔTIC the reverse control process takes place: the bypass and the cooler valves are closed in succession.

The TIC is also used to actuate the loop heater if the thimble inlet temperature falls below desired operating temperature. It is usually desirable to operate test loops at constant temperature. Thermal cycling produces stress and corrosion-product problems.

Flow Control

Figure 10.6 shows the loop flow control system. Again we employ triplication. We consider flow the second most important variable measured. Low flow at the thimble inlet is potentially worse than high temperature at the thimble outlet. The reason for this is that low flow will result in high temperature in the thimble. Although indirect control of flow by temperature monitoring would be effective under most conditions, it is neither sufficient nor safe. If blockage stopped or sufficiently reduced the flow, the resultant excessive heat inside the thimble could not be conducted to the outlet thermocouple in time to prevent serious damage. Therefore, it is also necessary to initiate control functions by low flow measurements. Low flow on FA gives an alarm. Lower flow results in a cutback. Lower flow on any two of the three flow instruments initiates a scram.

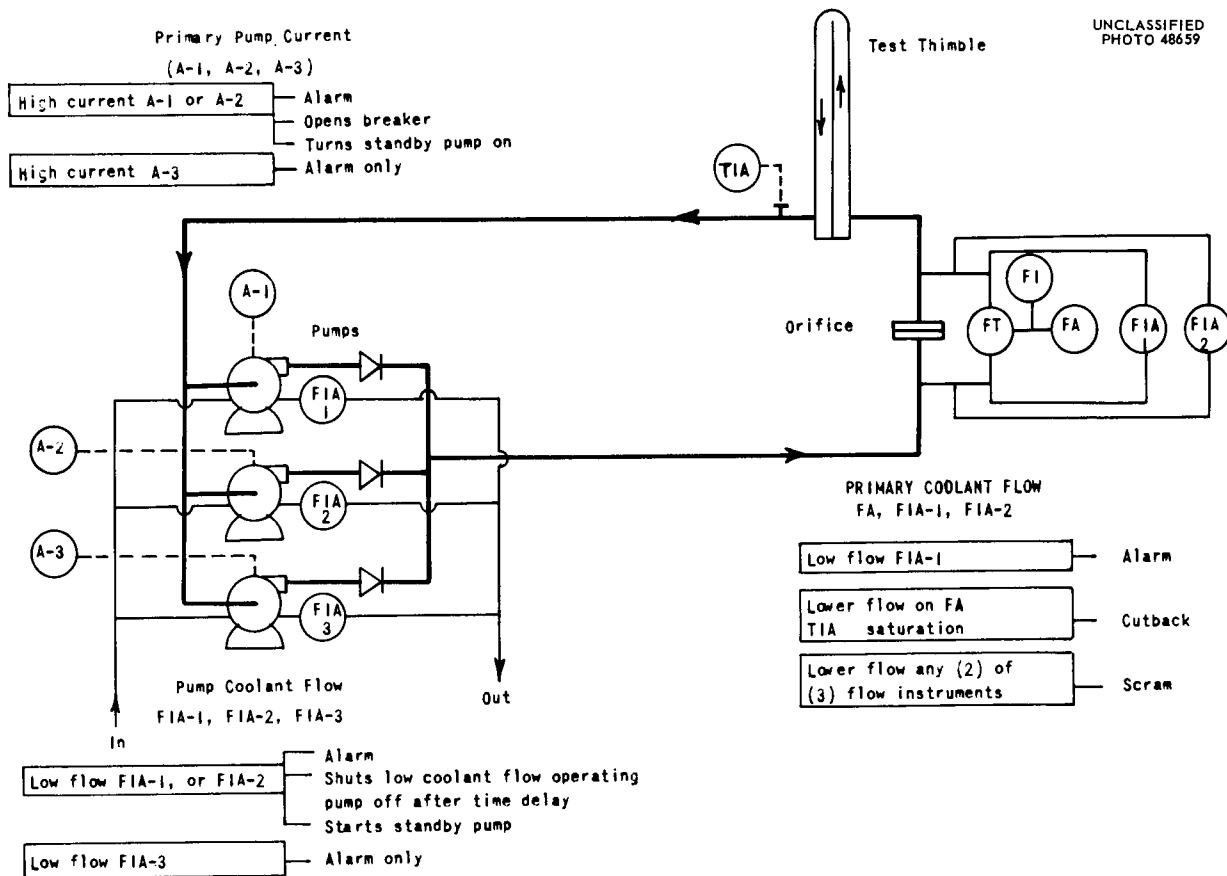


Fig. 10.6. Flow Control System for WTR In-Pile Test Loops.

There are only two conditions in this system that can scram the reactor: high outlet temperature from, and low flow to the test thimble.

Two auxiliary items that are measured are flow from and electric current to the primary pumps. These pumps are hermetically sealed Westinghouse pumps which require coolant to be circulated through the jackets continuously. Low flow on FIA-1 or FIA-2 can start the stand-by pump, which is on a separate water supply. If for some reason a pump impeller should lock, causing high current, the circuit breaker on the locked pump opens and the stand-by pump is turned on. Turning the stand-by pump on will usually prevent the flow from decreasing sufficiently to cause a cut-back.

Pressure Control

Figure 10.7 shows schematically the system pressure control. There is a series of control actions initiated by the instruments at the pressurizer. At high pressure on PIC-1, the heater output is reduced. At

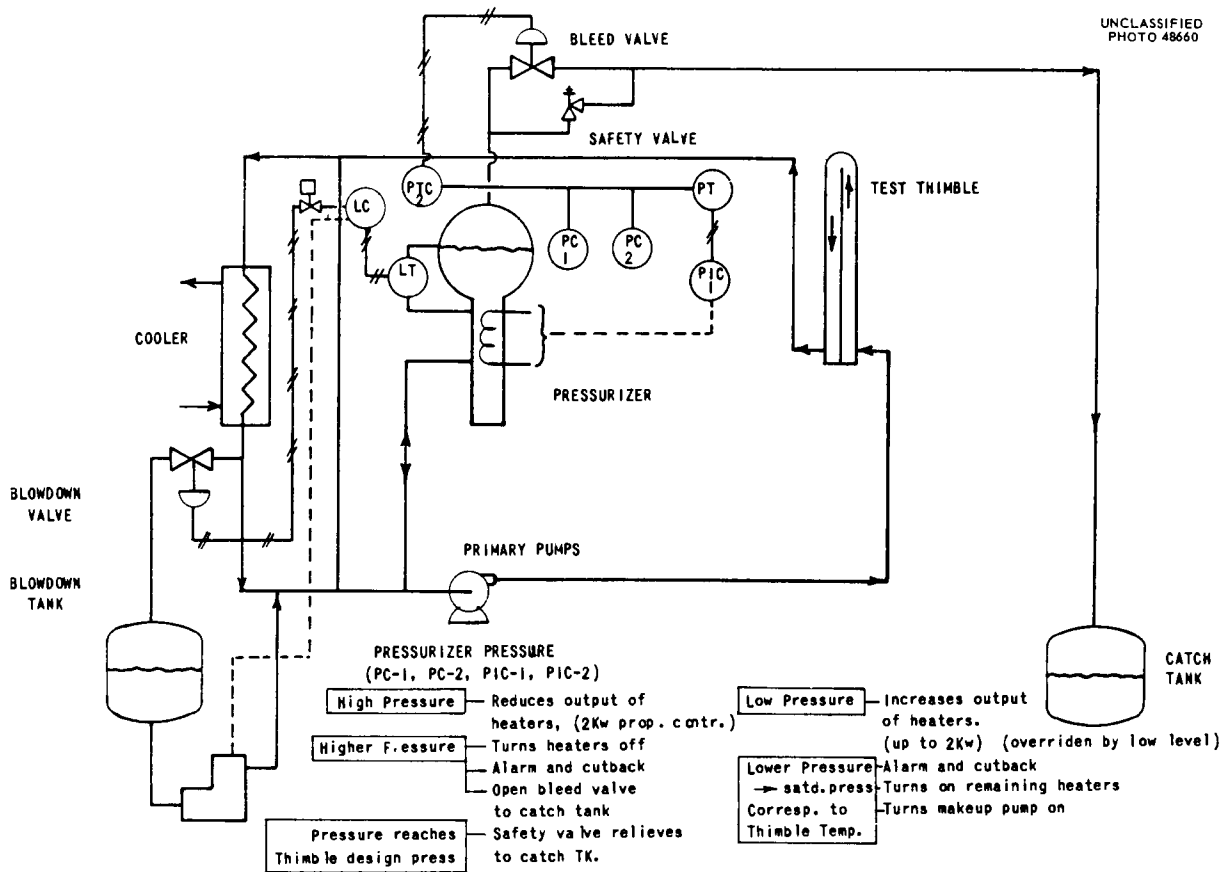


Fig. 10.7. Pressure Control System for WTR In-Pile Test Loops.

higher pressure, the heaters are turned off, and an alarm and a cutback result. If the pressure continues to rise the bleed valve to the catch tank opens. If the pressure reaches the system-design pressure, the safety relief valve opens.

At low pressure, heater output is increased. There is proportional control up to 2 kw. Additional output (12 kw) is operated by on-off control. A low-pressure signal calling for pressurizer heat is over-ridden by a low-level signal from the pressurizer; this is required to prevent burnout of pressurizer heaters. At low pressure, there is an alarm and a cutback, and the remaining heaters are turned on. The makeup pump is also turned on to maintain pressure if possible and to help reduce loop temperature.

Level Control

Figure 10.8 shows the level control system, which is related to pressurizer control. The pressurizer level has to be controlled within certain limits. High level turns the makeup pump off. Higher level opens the blowdown valve and releases fluid to the blowdown tank. Blowdown will result whenever there is a fairly large increase in system temperature. If the temperature decreases, the makeup pump will pump fluid from the blowdown tank into the loop to restore pressurizer level. Low level turns the makeup pump on. Lower level turns the heaters off, closes the loop blowdown valve, alarms, and cuts back the reactor.

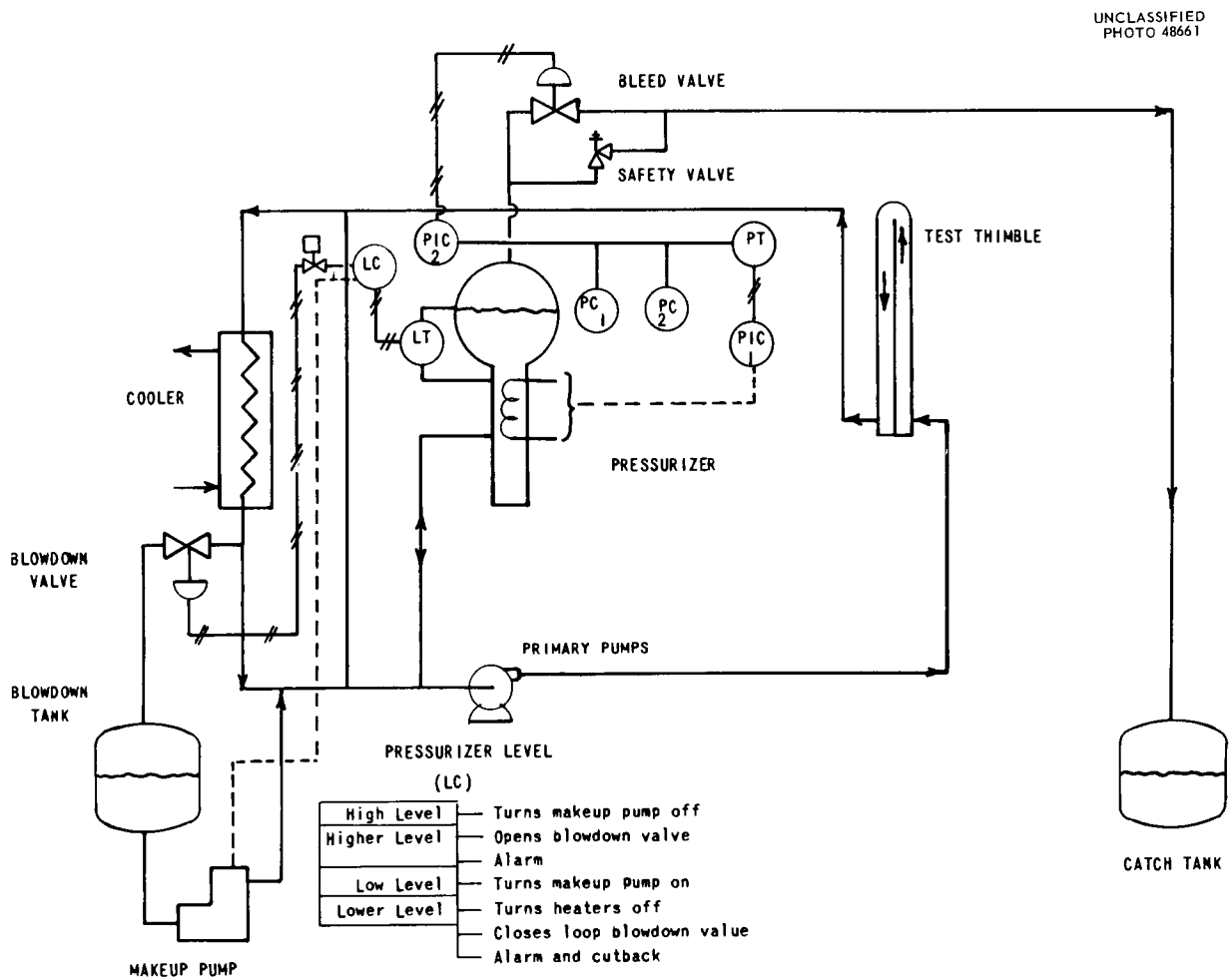


Fig. 10.8. Level Control System for WTR In-Pile Test Loops.

Summary of Logged Variables and Power Reductions

Variables recorded on the data logger are:

1. Fluid temperature inside the thimble. (In addition to the thermocouple reserved to indicate peak internal temperature for control purposes, there are six thermocouples in the test thimble that may be used to measure temperatures of the experimenter's choice.)
2. Thimble outlet fluid temperature.
3. Thimble inlet fluid temperature.
4. Temperature difference between thimble inlet and outlet fluid.
5. Flow rate to test thimble.
6. Pressurizer pressure.
7. Reactor power level.

These occupy seven of approximately ten data-logger points allotted to each loop. The remaining points may be connected to whatever an experimenter wishes.

When any one of these variables (except reactor power and temperatures measured by the experimenter) exceeds operating limits, it actuates the alarm log. This log then scans all the loop variables and prints out in red those which are not within limits. This provides a record of occurrences during any abnormal operating period.

A cutback to 20% power can be initiated by any one of nine conditions:

- 1-3. High fluid temperature: inside thimble, at thimble outlet, or at loop heater outlet.
4. Low flow to test thimble.
- 5-6. High or low pressurizer pressure.
7. Low pressurizer level.
- 8-9. Failure of electrical power or instrument air supply.

Note that items 1, 2, and 4 are conditions of the variables listed under data logging as 1, 2, and 5, respectively. Items 5 and 6 are conditions of variable 6.

Although nine conditions can cut back, only two, as stated, can scram the reactor: High temperature (on two of three instruments) of the thimble outlet fluid, and low flow (on two of three instruments) to the test thimble. Power can be restored fairly quickly after a cutback, which is therefore much better than a scram, where possible.

R. A. EDWARDS: Is there any emergency cooling on your loop?

M. A. VOGEL: Not as such. We have no separate emergency loop at present.

R. A. EDWARDS: How do you control the heaters?

M. A. VOGEL: By using saturable reactors, I believe.

R. A. EDWARDS: Is there a data logger on your main plant?

M. A. VOGEL: Not on the main plant; just for the test loops.

R. A. EDWARDS: Have you selected this logger yet?

M. A. VOGEL: Yes, it is a Giannini (Datex Corporation).

R. A. EDWARDS: Giannini is making the whole system?

M. A. VOGEL: Essentially, I believe.

D. W. HUSZAGH: What action, if any, does the operator take when an abnormal condition arises? Or, if it is automatic, aside from record purposes, what functions do the alarms have?

M. A. VOGEL: An alarm tells the operator that an abnormal condition exists. I gave the impression that it proceeds like clock work from alarm to cutback to scram. The fact is that the operator should determine the cause of an alarm and take manual action to correct it.

D. W. HUSZAGH: What kind of over-ride do you have over these automatics?

M. A. VOGEL: There are manual over-rides on practically all instruments.

D. W. HUSZAGH: Do you plan to have operators around the clock; or will this be fully automatic at any time?

M. A. VOGEL: Very definitely there will be operators around the clock, probably one at the data logger and another to go around to the various loops.

D. W. HUSZAGH: It will give every occurrence at any given time, as long as it is working.

M. A. VOGEL: This is true. However, the loop control panel is independent of the data logger and has enough indicating and controlling instruments to operate the loop without the data logger. The data logger is merely a convenience, having a recording function. It does not control.

D. W. HUSZAGH: Does the master operator at the data logger have to dictate to another, or can the other watch a light-type annunciator and take corrective action himself?

M. A. VOGEL: Each loop has its own panel. There are 40 annunciator panels. On the proper panel a light and a bell indicate that there is, for example, high temperature on the heat exchanger. Each loop has its own controls - one loop or ten - and each is operated separately. The data logger is really a convenience, partly to keep an operator from having to go around and read numbers from charts, and primarily to present data in neat concise form. At the end of the day copies of the operating data may be teletyped to an experimenter instead of being tabulated from a lot of charts.

W. E. VANNAH: Are all your logger switches operated by mechanical relays; or do you use any of the solid-state switching devices?

M. A. VOGEL: I should appreciate it if I could refer this to A. M. Yuile, of the Canadian Westinghouse Company, who are doing the design for us.

A. M. YUILE: They are all either straight electric or electric-mechanical.

P. BLISS: I thought you said there is a 100-point logger system and later showed 15. Was that for the different experiments?

M. A. VOGEL: I think I had listed 8 items. There are 100 points. How we distribute these can be decided later. But there is space for 10 loops, which would give 10 points per loop. But it may be that one loop will use only 7; another, 13.

P. BLISS: What is the speed of the logger in normal operation?

M. A. VOGEL: I will ask Mr. Yuile to answer this.

A. M. YUILE: I believe it scans all the points at one per second, then stops for a predetermined period until it scans again. The period between full scans can be adjusted from 1.5 min to 1.5 hr at will.

M. A. SCHULTZ: There is confusion relative to the data logger for these loops. The data logger actually handles the data from a multiplicity of loops; whereas the control function is handled independently for each loop. The Westinghouse reactor is capable of handling up to 15 loops at one time. We will go further, I am sure. At the moment we are constructing three loops of the type that M. Vogel described; the data logger takes the data from all three, with connections provided to handle up to 10 loops.

F. SHAFRON: I noticed that low pressure causes only a cutback. I don't know how close to burnout this may be with that kind of loop; but it would seem that low-pressure conditions would produce boiling within the loop, and that this would dictate a scram rather than a cutback. This would apply also to low level.

M. A. VOGEL: It is true that boiling might result if the pressure drops very rapidly. However, the leak would have to exceed 1/2 gpm. Reducing the reactor power to 20% decreases the test-thimble heat input to 50 kw. When the loop temperature control system is overridden, the cooler can cool the system to 400°F in approximately 5 min, which permits operation at 300 psi. Should steam nevertheless be formed, high temperature at thimble outlet will scram the reactor.

F. SHAFRON: What do you do when the pressurizer heaters malfunction?

M. A. VOGEL: In that case there is time for remedy. The pressurizer water will not cool right away, because the vessel is insulated.

F. SHAFRON: What happens if piping breaks?

M. A. VOGEL: Either low flow or high temperature would scram the reactor.

F. SHAFRON: Another question. I notice that this loop is set up so that low flow dictates the scram action. I don't know of any condition, offhand, that would cause a low flow, except a pump failure. It might be a faster operation to scram on pump-power failure as opposed to waiting for a signal from flow.

A. M. YUILLE: The low-flow scram is actuated by pump failure if all the breakers kick out. This is a little quicker.

F. SHAFRON: In that case how is operation sustained during a normal pump switchover?

M. A. VOGEL: There is no pump switchover because both pumps run continuously.

F. SHAFRON: You are saying that you are operating this loop with an excess of flow.

M. A. VOGEL: That might be a way of putting it. However, there is a manual main-loop flow-control valve, which can be set to give any desired flow rate less than the maximum pump capacity.

SAFETY AND CONTROL OF IN-PILE EXPERIMENTS

E. P. Epler

Oak Ridge National Laboratory

E. P. EPLER: I think a previous discussion very well illustrates that all experiments have some flaws in them that are not completely recognized and sometimes we run several years before we are completely aware of this.

Incidentally, I am indebted to M. A. Schultz for announcing the subject, which is that we certainly are going to go crazy when we have a dozen in-pile experiments going at once; this is very apparent to all of us. The ORR is critical and is now going up to power; there are, I think, about 24 experiments waiting, some of which will require extended shutdowns just for installation. Once we have them in, they will have ups and downs for data taking and adjustment of the experiments, and will have troubles because of reactor shutdown. Each time the reactor goes down experimenters will lose data or time.

We, therefore, have to be really critical of our system and try to discover exactly what we are trying to protect, in order that we can apply instruments intelligently and consistently. We have to find out what protection is required, and we have to try not to overdo it by providing an excessive number of scrams. We must not scram the reactor unless a scram is the only way to achieve protection. In many instances what may happen to an experiment is really not very dangerous to the reactor and to other experiments.

Protection against certain hazards must be intrinsic to installation. First, an experiment must not go into the reactor if any malfunction of the experiment can liberate the fission products in the core of the reactor. The fission products from a kilogram of spent fuel will be quite substantial, and their liberation is quite possible. We have had some experience with loops, for example, in which we circulate molten salts or liquid fuels. Our early reactor, the LTR, was not really built for experiments. It was built as a model of the MTR and was not ideally constituted for experiments. One experiment which gave us some concern involved a fuel storage tank which was outside the region of high flux. The fuel in the experiment circulated through a hairpin loop placed in a

thimble inside the reactor core. Our concern was that rupture of the hairpin loop could cause the fuel to drain into the thimble and be replenished by all of the fuel in the storage tank. We proposed to run the experiment, but we did provide a means of spreading out the fuel caught in the thimble to make the neutron economy poorer and to better dissipate the heat.

If such an experiment is permitted to go into the reactor, protection must be provided in such a way that should a failure in the experiment occur the reactor would not be in danger. The consequences of certain kinds of failure are so drastic that we cannot depend on instruments for protection. We must depend on the integrity of the installation.

A similar situation occurs in the next order of hazard, which is the liberation of the fission products of the experiment itself or the release of other types of activity in the experiment. Here we must depend on containment of some sort. We expect these things to happen; they will happen sooner or later. We will have to get rid of the fission products somehow and without too much reliance on instruments. We will try to put them up the stack as well as to trap them in filters, but we cannot depend entirely on instruments to prevent their release.

Then what do we depend on instruments for? Most important are the protection of the expensive experiment itself and the data obtained by the experiment. Experimenters can also expect protection from extended shutdown of the plant due to the loss of an experiment. After all, 20 other experiments are dependent upon the reactor, and considerable time can be wasted in shutting down, cleaning up, and starting up again after a mistake.

We have found that the principal cost of the protection that we do put in is that of false shutdown of the plant. In "Built-In Testing in Process Instrumentation" (this conference), E. Siddall will discuss a really good instrument system to protect the reactor and to provide minimum failure and maximum performance. There has been a great deal of work on this, and it will be interesting to compare two approaches to reactor safety.

M. A. Vogel stated (see "System Design and Instrumentation of In-Pile Test Loops," this conference) that he is principally worried about (1) excessively high temperature, and (2) a loss of flow which would generate excessively high temperature. Either of these would damage an experiment.

In the reactor itself we worry principally about neutron flux excursions and loss of coolant. A neutron flux excursion is a rather exotic incident. It is possible to do what we can to keep it from happening, and to correct it as fast as we can after it happens without damaging the apparatus. Are we as careful about loss of coolant from a reactor as we are about over-flux? You have probably observed that an instrument to detect too much flux has been especially built and very carefully tailored, and a great deal of ingenuity has been put into its construction. For the detection of loss of coolant, we have depended on commercial instrumentation. There are very few alarm systems. After we put in a flow meter, we consider whether a ΔT measurement might help a little; then we put in a ΔT channel, which does not work too well if there is no flow. Then we resort to other things, such as determining whether the valves are open and the pumps are running. None of these, I think, add up to the kind and quality of protection that we put on to guard against the high flux excursion.

It is true that we do not expect the same degree of performance from the instruments in the experiment safety system as from those in the reactor safety system, because we think we are guarding against less drastic consequences. Also, it is a little difficult to know just how to protect some experiments; there is no single variable that can be used as the control, like too much pressure or too much heat.

Lately we have been reexamining an old experiment. It has been in operation for five or six years. We have had a lot of experience with it, and are just beginning to appreciate its value in helping us to get the answer to this very question, "What is instrumentation for?" This is a rocking-bomb (autoclave) experiment. It is very well named because it operates at fairly high pressure and contains an explosive mixture; therefore, of course, it is a bomb. In the rocking-bomb experiment, an aqueous homogeneous fuel solution is subjected to radiolytic dissociation. We have catalysts to recombine the stuff and try to keep it below the explosive level but we don't always do this. We must protect against high

pressure, because if the thing blows up it goes, of course, through high pressure first.

We have to protect against high temperature in two ways: The thing is receiving heat both from the reactor and from a heater. We will shut the reactor down at high bomb temperatures. However, low temperature can be almost as dangerous as high temperature because of decreased catalytic activity. It is necessary to maintain the bomb temperature between high and low limits at all times. This is a little tricky if the temperature has been too high because usually we are so busy shutting the reactor down that we can't be sure that we can shut off the heater. We should put on devices to do both.

Finally, the bomb does not contain the conventional circulating machinery. It rocks. If it stops rocking for just a moment it begins to develop a hot spot. This could be particularly dangerous in the case of slurry, because a hot spot under a slurry deposit produces a local explosive region; so we feel that we will have to do something drastic if the thing stops rocking.

There is just no one handle for this experiment. We have to do something drastic for practically everything that happens. If we scam this experiment for anything that can happen to it, it is doubtful that we will ever run it. It may be interesting to know that there is apparently a bit of a struggle between those of us who are interested primarily in the safety of the reactor and the experimenter who is trying to protect his experiment. The experimenter thinks in terms of more scrams and sometimes we have to dissuade him from putting on too many.

Precisely why do we scam these reactors? Is it because it is fashionable or is it because it is necessary? I think this whole subject needs to be discussed thoroughly.

Having decided that the reactor will be shut down because of some particular malfunction of the experiment, it is your responsibility to make sure that the reactor does shut down if necessary, and furthermore to make sure that it does not shut down if it is not necessary. If the malfunction is not really dangerous, don't put the scram on. If it is dangerous, put the scram on, but reserve the scram for the fast shutdown. If you are certain that it is safe to shut the reactor down deliberately

and slowly, then do it that way. If you cannot be certain that a slow shutdown is safe then you may have to shut it down rapidly.

Once the rods are released, they should fall all the way in; thus, a scram is irretrievable. Of course, we are very much concerned with the fact that we must override the xenon buildup in the reactor. We may have only a short time in which to get all the experiments organized and the reactor up again. Failing this, we may have to replace the fuel, which we think we can do in 4 or 5 hr, but which in itself is a hazardous operation, if done rapidly under pressure. We would like to avoid the use of the scram and to restrict it to those malfunctions that actually require fast response.

With the loss of rocking motion in the rocking bomb, we just don't have any idea how much time we have, and we have to scram. There may be fuel in the bomb with such a high power density that the temperature rise on loss of circulation can be of the order of $500^{\circ}/\text{sec}$. With such a temperature rise, we obviously don't have many seconds to detect this failure and do something about it. Obviously, a scram is required. Having decided that a scram is necessary, we feel that there is an obligation not only to put it in, but to put in two methods of producing it.

We will have two independent ways ("double track") of getting into the reactor safety system. One track is operated by picking up relays, and the other by dropping out relays, so that it is hard to conceive of a single accident to the communications link which could fail to deliver a signal or which would not itself shut the reactor down. Also, we would like to have two independent sources of information about the particular accident. We would like to have two independent instruments that detect a malfunction.

If a scram is not necessary for a fast shutdown, we would like to take the slower and more retrievable method. For this we use the setback, which roughly corresponds to M. A. Vogel's cutback. In our setback, the object is to take the reactor power down to 1%. This is done by a logarithmic potentiometer such that the power will go down exponentially on a 20-sec period. Then, in 45 sec we will be down to 10% power, and in 90 sec we will be down to 1% power. The setback gets us out of the full-power region rather rapidly. It is a little difficult to hold the reactor power constant at 1% after this fast reduction, because of the delayed

neutrons and other effects. The object of the setback is to keep the reactor critical, because if the trouble is spurious or intermittent we can get up to full power with a minimum of damage to the other experiments.

The setback also suffers from lack of reliability. We have to depend on relays and motors to work. In fact, a long line of equipment must work. Most of this equipment does work most of the time. I suppose it works 999 times out of 1000, but there are occasions when it does not work. So, we put in two channels from the experiment to the reactor. One will be the setback and the other the reverse. The reverse calls for all rods to be driven in by motors, it is more drastic and faster than the setback, and it leaves the reactor subcritical, although it is interruptible and reversible.

It would be silly to send two signals and have them both produce results. We would like to have one function and the other in reserve. For the ORR we propose to provide a time delay of about one second between the setback and the reverse. If the setback is not producing a negative period after one second, the reverse will be actuated by the second channel. The two channels together give us a rather high degree of reliability, which can be approximately the same as that of a scram. This does not constitute absolute reliability. Nowhere in the tie-ins to the reactor experiments do we find the high degree of reliability that we find for reactor protection in flux excursions.

There are a few things that we should promote in order that large numbers of experiments can exist together. If possible we should design into the experiment alternative supplies of coolant and power, so that on failure of one we can pick up another source rapidly, and keep the plant running. We must not scram or shut down the plant on trivial or fancied trouble if we can avoid it. We should promote the ability to retract from the high-flux region an experiment that is in trouble and reinstate the others.

The responsibility of having sufficient instruments on the experiment is one that we should seriously consider. It is hard to say how many to use, but we should never depend on one. We should if possible have at least one of some sort in reserve. Another thing that has given us a great deal of trouble, and that seems to recur, is the question of duality in the purpose of the instruments. The experimenter would like to have an

instrument of considerable accuracy for experimental purposes, and we will have the same instrument for protective purposes. We say these are incompatible objectives. Everybody has experienced the situation in which there is a protective device operated by an instrument that is bypassed by a pushbutton in order that the instrument may be calibrated without scrambling the reactor. The experimenter will push that button to calibrate his instruments, and he will push it all day if he wants to. We have no protection against that.

We must divorce the two purposes, I am sure, and specify that the experiment must have one instrument for protection and another for data. The instrument provided for data can act as backup protection, because it is available most of the time -- thus giving us more than our minimum of one.

This again does not compare with the order of protection that we give to the reactor for nuclear excursions, but it is certainly better than the protection that we shall have if we don't do this. Many experiments are running today without protection because the instrument is bypassed by a push button held down with a stick.

Now I think that those of us who are responsible for instrumentation have a further responsibility and a function that we can offer: to develop a carryover of experience from one set of conditions to another. I am sure that all of you have experienced the situation of working on your third or fourth experiment having similar problems. The experimenter may be assembling apparatus for his first; therefore, I think that this group can be very effective in exerting some leadership to get answers to these questions and to give us direction on the required instrumentation -- its responsibility, function, and minimums.

P. BLISS: In presenting a new experiment for the EBR we find that they do not consider millivoltmeter-type instruments acceptable in safety service. I would like to hear your comments on this. If you have an accurate potentiometer and want to use a millivoltmeter for additional safety, do you feel that this is sound or unsound?

E. P. EPLER: We have for the first time prepared a "preferred list" of instruments that are acceptable to us by virtue of our familiarity with them, the availability of spare parts, and the inherent reliability

of the instruments. Some of us feel that the D'Arsonval type of instrument is useful. It is cheaper than those on the preferred list and it does not have to work all the time. We will attempt to avoid the use, first of all, of several types of instruments for prime functions. We have used recorders as more reliable instruments. It is quite possible that instruments of the D'Arsonval type will be put on our list for the secondary instruments. We are working on the question right now.

L. P. INGLIS: You mentioned that an explosive mixture might be built up in the rocking bomb due to the radiolytic decomposition. On the other hand, you said that you had a catalyst in there to take care of that. How did you know the explosive mixture built up? We have the same problem in water boilers. Did you have any indication that your catalyst was not working? If the catalyst is working properly you don't get this buildup, of course.

E. P. EPLER: We have specialists calculate what the explosive mixture is; sometimes they think they know when it is explosive and sometimes they think they don't know. This brings up a very interesting point. If they think they know what the explosive mixture is and that the set point should be a little lower they are obligated to set it down. Then they may decide after five or six years that it is hardly worth while to change the point.

As to whether the catalyst is working, I shall ask D. S. Toomb.

D. S. TOOMB: That group will answer, "It is the purpose of this experiment to find the limits of the fuel stability and the explosive limits." It is a research project and we must learn from these small autoclave experiments rather than with our homogeneous reactor.

L. P. INGLIS: I thought possibly you had some way of knowing whether that catalyst was going to work the next time or not.

E. P. EPLER: E. R. Mann has the answer to this. If you can find out that you are going to be in trouble 10 min before it happens, you can shut down.

L. P. INGLIS: Since the bomb is operated at high pressure, you could not tell by a buildup of pressure due to hydrogen and oxygen, which is rather slight?

E. P. EPLER: It is alleged that you can. However, we are really afraid of being at low pressure and coming up fast, and when we are in trouble, we are in trouble fast; that is, a detonation is fast. On the

other hand, the detonation will not be too serious. It may be cheaper to have a detonation than to shut down the other experiments repeatedly. We have to find the answer to this.

E. F. WASEM: With all the shutdown circuits with such an experiment, you always have the chance, of course, of a false scram due to some malfunction or something you did not expect. In this regard you also have a choice of basic types of circuitry. Do you have any comments on energized shutdown circuitry?

E. P. EPLER: I will leave that to E. Siddall, who I am sure will give you better answers than I could. Consider the use of scram and setback on the same channel; this is a similar question. Should you pick relays up or drop relays out; should they fall to safety or to danger? A parallel question: should you scram or set back, and should these both be on the same channel? If danger builds up slowly, the setback can take care of this and get the reactor down; and if the time constant and loop response are right, it may be possible to correct the trouble before the danger level rises to the scram point. This is not always true. Sometimes the time constants are such that you have to scram anyhow.

On the other hand, if both are on the same recorder and someone slams the recorder door you are going to get the scram. Having two protections on one instrument gives two ways of falsely shutting the reactor down. Our present preference is to put only one channel on one instrument. Let's get the protection from the setback or from the scram. If we put both on one instrument we shall have too much protection.

T. M. GAYLE: One answer to the recombination problem that L. P. Inglis asked about: it is my impression, in talking with the scientists who are doing this work, that they don't know when they have an explosive mixture. They try to get an answer simply by the difference in the steam pressure and the pressure of the decomposed hydrogen and oxygen as to whether the catalytic agent is recombining faster than radiolytic decomposition is taking place.

Right now we are trying to give them accuracies of 1 psi in 1800 and $1/2^\circ$ at 400°C , but most of these people tell me that if we can give them pressure and temperature measurements accurately enough they can determine whether an explosive mixture is there.

BUILT-IN TESTING IN PROCESS INSTRUMENTATION

E. Siddall

Atomic Energy of Canada Limited

E. SIDDALL: Instrumentation is vitally important to safety and serviceability of a nuclear reactor. I think that E. P. Epler put the situation very well when he said that if there is trouble we must have a scram; if there is no trouble we must not have a scram. It is as simple as that. We have had scrams when we should not have; and occasionally we have needed one and we have not had it. This is the starting point.

We Canadians have had one of the best and biggest reactor accidents, which has obviously colored our whole approach. We don't want any more. We are quite willing to spend a lot of money and trouble to avoid another. It is this that has led to our large-scale use of redundancy and coincidence in instrumentation. Briefly, what we are aiming at is to achieve radical improvements in performance of the system by making the system as a whole more reliable than the components of which it is made. There is nothing very new about that idea, but if logically stated, it can lead to some important innovations.

Is the added expense justified by the savings elsewhere in the system or by improved performance? I believe it is, and at any rate, we are giving such schemes a good try.

The features that we have already incorporated to a considerable extent in the control and protection systems of the NRX and NRU are redundancy, coincidence, and the general principle of assembly of components into lattices instead of chains. Figure 12.1 illustrates this point. At least three instruments operating as a group are necessary to identify a fault in one of the group. Two will agree; the faulty instrument will disagree.

This principle is widely used on a small group of instruments. We carry the principle as far as we possibly can; the advantages are the same anywhere in the system. In Fig. 12.1 one group of instruments is shown covering low flow; the other, temperature. The application holds for any type of measurement. We connect one instrument in the group to each line. We have the information in the triplicated form to the end of the lines, and it is only then that we discriminate. Then we have, almost always, a

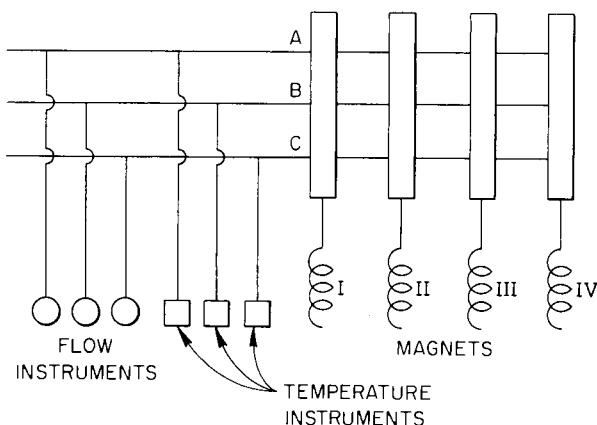


Fig. 12.1. Redundancy Circuit.

number of shutting-down devices. Each takes its own local basic information and decides whether or not to react. You can see that the lattice has emerged here. It is not a chain. The strength of a chain is the strength of its weakest link, but a lattice has good paths left, no matter where you break a single path. You can put a fault anywhere on the system and always leave some good path for information.

The simple discriminating circuit is shown in Fig. 12.2. We have a relay on each line within this rectangle, and if two of these relays are released we want to deenergize the magnet. If one releases we don't want to deenergize the magnet. That is the essence of the coincidence part of this idea. There is a small amount of resistance in series with each contact, and we put an ammeter in the magnet circuit. If one relay releases the resistance of the network increases. The ammeter shows a somewhat lower current, but not enough to deenergize the magnet; therefore we know that the single signal did get through to the last part of the system, which shows that the system is working up to that point.

It is this feature that has opened the way to built-in testing, which is my main subject. We can inject a danger signal into one instrument by a simulation as good as we can make it, and we can check the system through to all these end outlets of the lattice, and if there is anything wrong with the system — we do not have to specify what the trouble is — if that danger signal does not get through then you don't see these ammeters change.

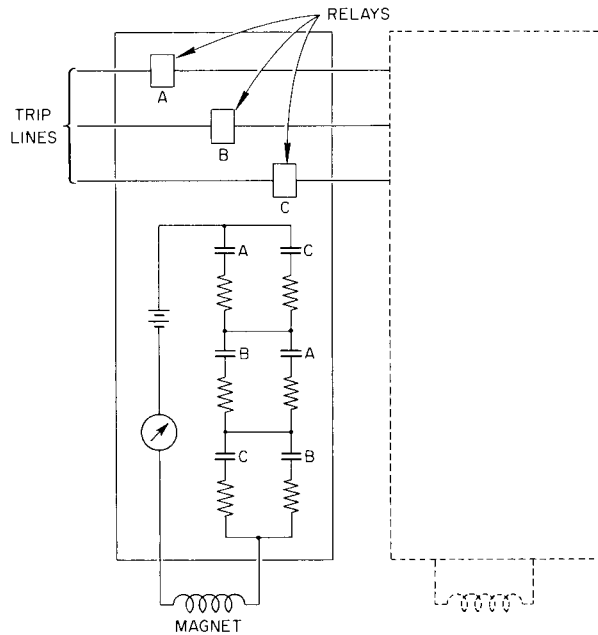


Fig. 12.2. Discriminating Circuit.

We are testing every contact in the chain if we use this particular connection. We have this type of protective system on the NRX and the NRU.

The protective system of a reactor is an inert system. When there is no danger, which is the vast majority of the time, it just indicates safe; and that is all we know. This type of system is liable to two faults which can be classified as safe and unsafe. The safe fault is a condition in which the instrument gives a danger signal although nothing is wrong with the process. The engineer curses a little and tries to do better the next time. The unsafe signal is a far more serious consideration in regard to safety. The unsafe fault paralyzes the system and prevents it from reacting to a dangerous condition. It is obvious that an unsafe fault can exist for months without detection. The system is indicating safe and the reactor is safe, so there is no lack of consistency here; but if something does go wrong with the reactor, the system can't react, and personnel don't know of the emergency until it is too late. So my main emphasis in this phase of the subject is the detection of unsafe faults.

If we could make the instrument fail-safe we would not have this problem; every fault would be a safe fault and would reveal itself by indicating a scram. However, I believe it is fundamental that we cannot make

an instrument which unconditionally fails safe. In Canada the term has been used loosely for the idea of operating a relay in a safe condition and deenergizing it by an unsafe one. Nevertheless, we have had relays stick in the energized position, and it is very difficult to find a relay that could not in any circumstances fail in this way. Contacts can be welded by discharge of capacitors through them as they close. In that case they will not operate when the relay deenergizes. Such faults can easily contribute to wrecking a reactor. There is an obvious logical gap here that we have to look at.

No matter what method we use, even in our present systems, we do testing. We don't merely allow the system to stand by for years, looking after itself. We have some sort of routine maintenance schedule and the operators are always on the watch for changes in the response of the system which might indicate trouble. However, unsafe faults can only be detected by sampling, which does not give absolute assurance. There are only degrees of security. If we tested this system by any means we like once an hour for years, we would know its past performance, but this would be no assurance of what is going to happen in the future, except by extrapolation or by prediction.

This is a process that is always with us. I believe it is quite fundamental. An example is the steering gear of an automobile. If we turn the steering wheel to the right, we absolutely assume that the front wheels will turn right, provided that they are not blocked. But there is an element of prediction there. We believe the car will turn to the right when we turn the wheel to the right because cars have always done that in the past. Even if we get into a new automobile which we have never driven before, we know that automobiles in general do respond to their steering wheels. But we don't know how that mechanism is designed. We don't know the stresses in any part of it. By what has been observed in the past on this type of system we extrapolate into the future, and unless we think pretty deeply about it we are likely to say it is dead certain that the car is going to steer correctly.

This is what we do with these systems. We predict that if we design the reactor in a certain way it will give adequate performance, and will be safe enough and serviceable enough, and it is this element of prediction that we must recognize as a link in the logical chain.

There is a formal theorem of statistics that is very controversial but relevant to testing. This is the "law of succession," which states that if an event can go two ways, and if in n previous tries it has gone one particular way r times, in the absence of all other information the probability that it will go the same way the next time is $(r + 1)/(n + 2)$. The controversy enters in going from hypothetical to real-life cases. But I think there is some justification for such an extension.

I believe that this reasoning is really the basis of all the prediction in which we get involved in designing anything. It is involved in bringing new reactor-protection systems into use, particularly if they are not of a type we have used before. In that case we will often have a testing period. Before a new reactor has to go critical, we have done what testing we can do on the instruments. If the system is relatively new, there is a fair warning to all of us that n in the law of succession is small, where r is the number of failures in n previous trials. Normally r would be very low. But n is also low. We have not thoroughly tried the system; we don't know much about it.

This is a problem throughout our industry. Changes are so fast that every new reactor has fairly large differences from the last. Often communication isn't too good. For example, we don't know in Canada your latest experiences at Oak Ridge. So if we build a new reactor we are pioneering, at least to some extent. The protective system is bound to have new features. It may have completely new types of instruments.

For instance, the trend from pneumatic to electric signaling causes immediate introduction of new types of systems for future schemes. Whether or not we accept the law of succession as being applicable, n is necessarily small. There is our warning, obvious enough, and in my opinion no different from common sense.

But what can we do about increasing n ? In the inert system n can be extremely low. If we commission a reactor and keep it running for a few weeks, the protective system is not tested for unsafe faults except when a danger signal arises; and, of course, we very seldom get real danger signals in reactors. So we may well have gone through several months of initial testing with no real input; therefore there is no output. Since n is very low, or zero, we don't really know much about our system.

What we have done in the past has been to test this system as well as we could. We have specifications on how the instrument is supposed to behave, and we have tested it on surge, and so on. But almost all these tests are logically defective. If we have to disconnect an instrument to test it, the instrument may not be put back correctly after the test, or the fault may not be in the instrument. It may be in connections between instruments. From my view of the situation, all our existing types of tests have these flaws; and it is just these flaws that may be the dominant danger. We don't know this before hand; it is too late to learn it afterwards.

The remedy for this situation is built-in testing of the type made possible by our latest system of controls, by which we can test an instrument at any time. Since only one signal out of three results, it does not effectively trip or scram the reactor, but we have now tested all the system by following the input of the system right through to the last point. If we take this seriously enough we can get tests that appear to have no logical faults in them at all.

We have only one such built-in test being installed at the moment at Chalk River. Progress is very slow with these things, but our later reactors will certainly see many more applications. What we are doing is this: The reactor neutron flux appears as three beams at the outside of the shield, and we want to measure the neutron flux in each beam. We want to get good discrimination against gamma rays, and to this end we put a lead plug in each beam and expose ion chambers to the neutrons scattered sideways from the plug. A much smaller proportion of gammas than neutrons are so scattered.

These are the mechanics (Fig. 12.3): The signal received at the ion chamber for a given flux in the reactor is a function of the position of the plug. The plug is moved by a crank with a stop to hold it in the normal operating position, which is the least sensitive position. When the crank is turned through one revolution, the neutron flux at the ion chamber goes up and then comes down again.

We can operate the period trip on this part during startup, and if the reactor reaches full power we can operate the high-neutron trip when the chamber current gets up to a high enough level. We designed the

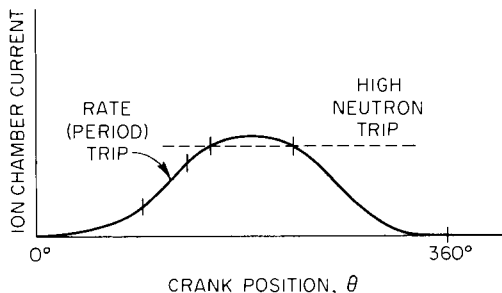
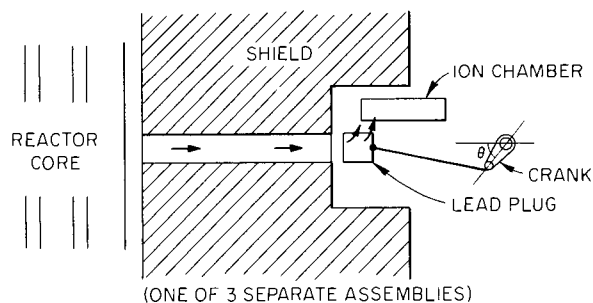


Fig. 12.3. Ion Chamber Test.

stroke of the crank and the speed of the motor to achieve these two conditions. The result is that we have imposed, as far as the ion chamber is concerned, a genuine danger signal. We have made no assumptions. We can do this as often as we like provided we don't do two channels at once.

If the test mechanism itself goes wrong, which is a perfectly fair assumption, it might, for instance, wrongly stop the crank in an intermediate position. This merely prolongs the danger signal. This, in effect, is a safe failure. Any intermediate position of the plug gives a higher sensitivity than normal; that is the element of failing safe.

This probably is not exactly a process instrument, but it does illustrate the philosophy, and it will be the first practical example we have. Our great advantage in having a heavy-water reactor with a high neutron source is that this check will be effective even with the reactor shut down. Also, we can check the visual indication during the shutdown period. The indicator will go up and down if everything is working correctly, so that, at least under manual supervision, we know that it is effective during the startup. The test will detect a saturation effect that could falsely indicate a steady signal while the reactor was running away. In our demonstration power reactor, NPD, the designers intend to enforce this

test sequence before startup. They will test the three channels and get the right answer before they go any further in the startup procedure.

A few other examples of built-in testing may not be very new, but may be worth while. In the case of temperature measurement, we put a thermocouple or temperature-measuring point in a side flow (Fig. 12.4). With a 1- or 2-kw heater ahead of the thermocouple we can readily expose it to water hotter than the main flow. So we add our testing signal to the genuine signal, and the thermocouple receives the simulated dangerous condition in this loop, even though the other loops are still monitoring the unchanged reactor signal.

In the case of flow measurements, another important class of safe measurements, we put in a solenoid valve and a regulating valve in series, both in parallel with a d/p cell. (Fig. 12.5). The regulating valve is so adjusted that when the solenoid valve is open, the pressure drops by a certain amount, depending on the pipe and valve resistances, to simulate a low flow as genuinely as we can. This test detects stoppages in the line, since the output signal will either remain unchanged or will drop, depending on the point of stoppage.

In some cases it is dangerous for the pressure to go either down, which may indicate a leakage, or up, which may indicate excessive reactor power. We take the signal through a length of pipe and connect it through regulating valves to a low- or a high-pressure point (Fig. 12.6). The instrument genuinely sees the dangerous condition.

I want to emphasize that there is great room for ingenuity in devising better methods of performing the tests. There is, for instance, the

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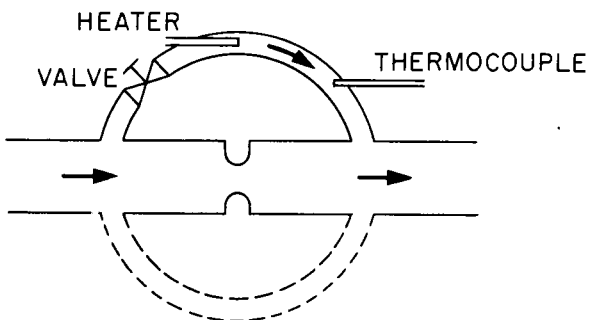


Fig. 12.4. Thermocouple Test.

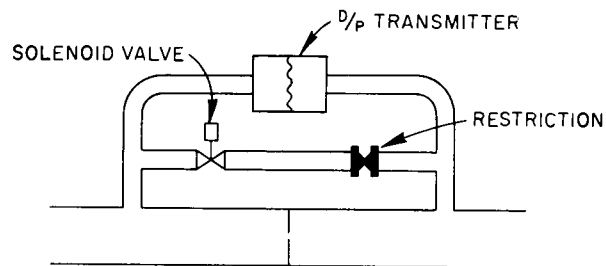


Fig. 12.5. Low-Flow Test.

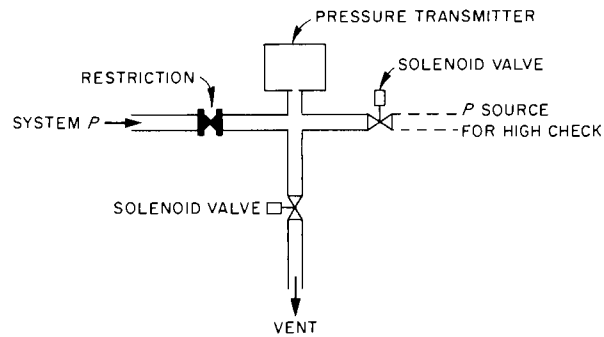


Fig. 12.6. Pressure Alarm Test.

idea of putting the heater in the thermocouple assembly, a very much simpler arrangement than the one shown in Fig. 12.4. It forces consideration of response time and similar factors, but gives a much simpler answer.

I am not saying that every instrument we put in needs this treatment. These are the vital instruments, the ones that the safety committees are interested in.

In our future power reactors, which will probably be cooled by D_2O at around $500^\circ F$, we intend to have test-signal injection into the instruments measuring coolant flow, coolant outlet temperature, and system pressure, as well as into the ion chambers. This treatment of these four parameters will be strong assurance that the reactor is all right. If these things are right, we cannot be in much trouble. The cost of these vital devices, by all the estimates we can make, is a very small portion of the total cost. By spending maybe 10% more, only 1 or 2% of the total reactor cost, for vital instrumentation we can make a radical change in the value of the protection.

This kind of system, to answer one of the questions asked by E. P. Epler in the preceding section, eases the strain of dependence on the reliability of individual components. Statistically it raises the reliability of these systems by several orders of magnitude. This produces a radical increase in immunity to safe and unsafe faults, as far as the system as a whole is concerned. There will probably be more instrument faults because there are more instruments, but they are not functional faults of the system. They are not tripping the reactor when it need not be, or failing to trip it when it needs to be. A major advantage is

that such systems permit the design assumptions to be checked in early operation without appreciable risk. Tests can be very frequent until the reliability of a device is established; and troublesome instruments can continue to have a high testing rate until redesigned or replaced.

I have been seriously concerned by some reactor hazard reports, in which the bland assumption is made, in discussing accidents, that all the control rods are being pulled out at full speed and none of the scram system works. That assumption simply is not right. Even at the moment with quite simple arrangements, we can be sure that our systems will operate correctly in 99 out of 100 accidents. With this type of approach I think we can make that figure far better. In fact solving these problems by instrumentation may be the best way.

As is said about justice, not only should justice be done but it must be clearly seen to be done. By built-in over-all testing it is possible that we can devise systems which may be clearly seen to be safe.

C. S. LISSER: Are simulated process changes fed manually by the operator or on a pre-set periodic basis?

E. SIDDALL: I should emphasize that we are designing these things, not using them at the moment. By my analysis, mechanizing them is not worth while. In fully automated systems one of the problems is to keep the operator attentive, especially during the night. If we have an operator there, it is better for him to have something to do. I put forward the suggestion that some of the most useful things he can be doing are these tests. We are putting in recorders so arranged that they will show supervision whether the test was done or not. We hope that it will be more trouble for the operator to fake the response than to do the test.

M. HEBERT: I would suggest along those lines that probably you need a triplification of your operators to decide whether to ignore the signal or what action to take on it.

E. SIDDALL: That is why we do have operators; and sometimes systems give the operators problems to which they should never be exposed. By saving money here and leaving a manual operation, we may be giving a quite junior operator formidable decisions to make as to whether he should stop the reactor because something is abnormal. What you say is absolutely right. At the end of the line, when all predicted situations have been

covered, the operator takes over and keeps the reactor out of trouble from that point on.

R. A. EDWARDS: Are you putting three d/p cells around the orifice?

E. SIDDALL: Yes, but this is the main flow stream of the reactor, of course, and that is why the cost is relatively trivial.

C. J. MADSEN: If in your operation one channel has failed, do you instruct your operator not to recheck the other channels, or do you have some provision for locking out so that the second channel doesn't trip off?

E. SIDDALL: The operator, either in this situation or if he finds an unsafe fault on a test, manually places a scram signal on that channel and suspends testing until the fault is rectified.

C. J. MADSEN: Suppose an actual fault in an instrument produced an unsafe indication on one channel. Would you bypass the instrument?

E. SIDDALL: No, we do not allow that. One of the sales points for the triplication is that bypassing becomes unnecessary. We now revert to one out of two. If either of the remaining channels signals a fault the reactor is, of course, scrambled. Typically only about 10 min elapse between emergence of the fault and its repair, surprisingly enough. Some faults take an hour or two to fix, but many can be fixed in a few minutes. The chance of a false signal occurring on one of the other channels while the fault is being corrected is quite negligible in reasonably good systems. We suspend testing on the other two channels, because a test on either of the other channels would trip the reactor. We concentrate on prompt repair of that fault and do not bypass it. This has been accepted as one of the rewards from the extra money expended on the system.

C. J. MADSEN: Have you in your studies considered the proposed Scully-Rand fail-safe system?

E. SIDDALL: I don't know that system.

C. J. MADSEN: It is a scheme in which a signal is interrupted at a periodic rate; and as long as that operation continues, safe operation is indicated.

E. SIDDALL: That I would class as a carrier technique. The operator can tell whether the channel is working, because the output shows the carrier signal. These devices improve the individual channel performance.

Let me be the first to admit that when the channel becomes completely reliable the need for these complex systems will disappear.

W. P. DiPIETRO: Upon failure of one channel, you did not change the setup. That leaves you exposed because if you fail in an unsafe direction on that one channel, a second signal coming through will not trip you. So in the event of a single channel failure I would say that you have to change from coincidence to single-channel trip. Either of the other two must force you to double your protection that way.

E. SIDDALL: That is absolutely right. The worse case is when the operator finds an unsafe fault. He presses the test button and nothing happens. He should immediately scram that channel. In other words, he has manually made it fail-safe instead of failing unsafe. The penalty is that a scram on one of the other channels will stop the reactor. But, as I stated, the chances of a second trip signal while there is trouble with the first one is very small, on a reasonably good system.

W. P. DiPIETRO: We used the multiple-channel system and have reverted to the single channel in some of our plants that are in operation. We don't go to the extreme in testing that you have presented. I think we feel in general that if the instrument is working we can assume that it will continue to work, and that we don't need, for instance, to place a heater element ahead of the thermometer. That leads to questions like, "Can you leave the heater on and get a false trip?"

E. SIDDALL: Surely, but the coincidence system can stand a reasonable number of false trip signals on single channels.

P. BLISS: There are two applications of this two-out-of-three system. In one, each temperature element, for example, is connected to your A bus, and your pressure element is connected to your A bus and you take your two-out-of-three at the end of the busses. In the other application the three temperature elements are in themselves two-out-of-three. A safe failure in the temperature signal penalizes the pressure signal to a one-out-of-two operation. Do you consider this preferable to the individual two-out-of-three scheme?

E. SIDDALL: Yes. It requires far less instruments. This is why I make the reservation that instruments need to be reasonable, though not necessarily superlative. We can see from the records of our system over a long period that one false signal a day is very poor performance on

systems of this type. If the scram signal lasts, on the average, only 10 min, the chance of another false signal while it is on is very small. If the specific two-out-of-three is used, the comprehensive testing facility is lost.

H. H. HENDON: We made a rather detailed analysis of the system you described. In two-out of-three coincidence cells of a manual testing system, assuming a rather unlikely instrument failure probability of 1000 hr, we calculated a failure probability of about one per 10^7 hr, which sounds good. However, if we introduce an automatic testing scheme with a 1-sec testing interval, which we have used, the failure probability goes up to a fantastic one per 10^{13} hr. This seems worth while even if you do let the operator sleep. Have you made an analysis of this sort?

E. SIDDALL: Yes. One of the difficulties I always find, however, is the solid body of criticism of this type of system: "Is it really necessary?" "Are our instruments so poor that you need this sort of testing?" That is the answer to your point. These techniques will go as far as desired if an organization is prepared to spend the money on them. For instance, the principle can be extended further than two out of three, but my own belief is that this instrumentation system is an enormous step forward, so that the last ounce need not be squeezed out. Just the simple step to three instruments gives all that is needed.

H. H. HENDON: Have you considered, rather than using a motor to change the imposed flux, the idea of inducing small rod motion, for example, with a solenoid, or tying the control rod motion somehow to the lead plug and thus closing the loop? This amounts to the Scully-Rand scheme.

E. SIDDALL: The trouble there is that testing the control device scrams the reactor.

H. H. HENDON: It can be done by a holding magnet.

E. SIDDALL: If that motion is enough to bring in the scram channel, it must bring it in on all channels, which will scram the reactor.

H. H. HENDON: I am not implying that this motion will cause flux change. I am implying that the motion can be tied mechanically to the plug mechanism, thus assuring that relayed motion has occurred not by change of the reactor flux.

E. SIDDALL: I didn't describe the back end of the system since we were mainly discussing the testing of instruments, but we have corresponding devices and techniques at that end. I think we have been equally successful there with certain designs. We do, in fact, periodically test any control shutdown faults, and in our own moderator-control system we have gone to the extreme of fully opening the dump valve in the test. The dump valves are so arranged, in a semi-parallel group, that opening the valves of one channel does not dump the moderator.

J. R. MAHONEY: It seems that if you make these automatic you are failing to check on the reliability of the operator, who is one of the strong links in coping with the chain of events that will occur if the reactor is in trouble. So it seems that whatever you do, you should include him in the chain so that you can test his reliability.

E. SIDDALL: Of course, you are not sufficiently testing his reliability by telling him to press a button and making sure that he has done it. The operator has to think; otherwise he is not useful to you. If we have a simple job to do, we mechanize it. The operator is there for things we have not thought of, and all that I claim this test does is give him something to do which keeps him looking at the panel.

J. E. OWENS: I agree. I don't think the operator should be a part of the safety loop. In a safety system, in general, the designer knows what he wants to do. There is no point in annoying the operator by making him do it. If there is a decision to be made or a judgment to be exercised, then you give the operator the information and ask him to exercise this judgment, but there is no point in saying, "Push the button." It is just as easy to push the button.

E. P. EPLER: I think this is a very able presentation. For the sake of completeness, I think I would like to call attention to the thing that worries some of us: Assume that we have a completely tested system that is infallible and therefore cannot fail. I want to call attention to the fact that if someone fixes the bottom of the reactor so the rod can fall clear through, and if that rod falls through, it will not scram the reactor.

I see reactor systems being built today with such booby traps built in. We have to keep our eyes on the ball and see this completely. So let's not be carried away and polish one part to perfection while neglecting this thing that can give us a great amount of trouble.

E. SIDBALL: I agree absolutely with that, Mr. Epler. I should have mentioned that our latest tests consist of dropping the rod a little way to test the release mechanism. We cannot do more; otherwise we will stop the reactor. However, we must take full advantage of the situation when we need to stop the reactor, by watching carefully what happens. We are doing that in our present reactors. If you shut down this weekend for some reason or other, don't waste that opportunity. There is a good chance to test the system fully. If you like, put in two test signals at the beginning of the system, and make sure, for example, that the rod stops and doesn't go through the bottom of the reactor.

M. A. SCHULTZ: I am greatly impressed by the ingenuity with which you designed these circuits, and you gentlemen certainly have it down as nice as one could ask. But, your emphasis on statistics bothers me. Your n equals one on your accidents, and because of this one accident you concentrated your attention essentially toward better instrumentation rather than on better reactors that possibly protect themselves.

Perhaps we should not be thinking in terms of scrambling. Perhaps we should be doing away with the scram altogether and building better reactors that don't have to be scrambled. I am concerned with this emphasis on getting ourselves out of trouble by more and more instrumentation rather than by reactors that work better in the first place.

E. SIDBALL: You produce the reactor that does not need protection and make it economically competitive with mine, and I will be very glad to go along with you.

M. A. SCHULTZ: But you see your argument has the danger that you permit any kind of reactor.

E. SIDBALL: No, that is carrying what I am saying too far. I am just saying that these are the methods by which results can be achieved on any type of reactor.

M. A. SCHULTZ: I will grant that, but a couple of years ago when we discussed this you were, for example, permit me to say, not adverse to positive temperature coefficients and you said, "My system is adequate." Whereas we said, "We will not permit this type of reactor in the first place."

Perhaps we are going against progress. Perhaps we should let the physicist design it anyway he wants it, and protect it externally. Perhaps you are right historically. An automobile is basically a very simple device. It has pistons and cylinders and all kinds of gadgets to increase its efficiency, and to protect us in turning around corners, etc. But basically the reactor and the automobile are very simple gadgets. Most devices are.

In this field, though, I think that we have not exhausted the ingenuity of our reactor designers to take a lot of pressure of this safety problem off of us. Perhaps you could have put this same kind of effort into a better reactor design.

E. SIDDALL: I believe that many of these problems are more economically solved by instrumentation.

THE ROLE OF THE INSTRUMENTATION AND CONTROL
ANALYST IN POWER REACTOR DESIGN

E. R. Mann

Oak Ridge National Laboratory

E. R. MANN: For the many things which have a bearing on evaluation of reactor instrumentation and control there just did not seem to be any one narrow category suitable and adequate. Perhaps it would be permissible to consider a title for the person who analyzes nuclear power plant instrumentation and control. The title of instrumentation and control analyst seems to fit satisfactorily.

This person has certain responsibilities, which I shall endeavor to describe in part. Twenty-five years ago there probably was no such person. I feel confident that there will be in another twenty-five years. If there now is, or soon will be, such a person, it will be worth while to describe the sort of work he might be expected to be engaged in.

Twenty-five years ago large and complex process systems were designed by first designing and building a pilot plant. The designers did the best they could with the available tools. After a program of component development and testing had indicated that the components themselves were satisfactory, the pilot plant was put together and tried out. This was intended to determine whether a system would work satisfactorily after it was known that its components had worked satisfactorily. The composite system provided interactions between components which had not been present in the earlier component developmental and test program. Eventually enough information was available to start the design of the final system.

A nuclear power plant is designed in just about the same way today. But there is one rather remarkable difference. Today the designer has at his disposal some very effective tools for determining the specifications of his plant. The large computing installations are capable of providing detailed information he could not hope for twenty-five years ago.

There was a time when the nuclear physicist working with our power-plant design group would admit that some of his data might well be in error by a factor of two. But that situation just does not exist for

much of his information now. He is pretty sure, for example, of the values he gives me for the temperature coefficients of reactivity of a particular reactor design. And he feels equally certain of the ratio he gives me of the increase in fuel to the increase in reactivity. These are only two of many instances; uncertainty about data that could have significant influence on fixing the control system specifications has to a large extent been removed. Some of the physicist's information has been accumulated from other reactor operations. But, because it is extremely unlikely that a new design is an exact copy of any other reactor, the information accumulated from other designs has to be re-evaluated.

Optimum utilization of such information would be a formidable task without the use of the large computers. The power plant design group has, besides the nuclear physicist, heat transfer analysts, stress analysts, metallurgists, chemists, mechanical engineers, and numerous other specialists. The computers are available to all of them. All the specialists named here can and do make use of these installations to determine their design specifications.

It would be naive either to expect some one who knows little about instruments and control systems to specify such equipment for the nuclear power plant, or to expect someone who knows little about nuclear power plants to specify the instruments and control systems. The proper person to take this responsibility must know enough about both. He has the large computer installations at his disposal if he needs them. In this respect he differs in no manner from any of the other specialists who are trying to make a satisfactory power plant design.

Nuclear power plants can be designed and built without the services of such a person. Somebody in the design group can come up with a process flow sheet and specify variables such as temperatures, flow and the like which he thinks should be measured and recorded. He can also provide the instrument engineer with the range, accuracy and all other information usually required to specify the instrument. However, I have seen some instrument specifications derived this way which will have to wait many years before adequate equipment will be available.

The designer or some one designated by him can also specify some of the control equipment. He can show the control engineer some holes in the general region of the reactor for fission chambers or ionization

chambers. It is almost a sure bet that if the control engineer has waited until the designer is in a position to show these holes to him that this same designer knows any number of reasons why the chamber holes cannot be moved to some other location which would make it possible to get much better information from these instruments. And the story is much the same for the location of the rod-actuating machinery.

The nuclear power plant designer is apt to do most of his thinking in terms of steady-state behavior of the system. He usually knows what his system is supposed to do when it is taken from one steady state to another provided one has in mind just the situations of the two steady states. In the past I have pointed out to a designer that during some transient some parts of his system will experience temperature overshoots for example which his specifications indicate the system simply cannot stand. He replied, "The system really can stand these high temperatures for a short time." This type of indefinite answer confuses me. Perhaps the system can stand such transient temperature overshoots for some short interval of time. But I am very much concerned with the length of the time interval in question. Also, I would like to know precisely how many such temperature overshoots the system can endure for these short time intervals before the life of the power plant is in jeopardy. Someone should make an analysis of the transient behavior of the system to determine the instrumentation and control system requirements to prevent the excessive excursion. It is not enough to know that steady-state performance is entirely acceptable. The analysis could show that no amount of instrumentation and control could prevent damage to the system by transient temperature excursions even though relatively simple devices would be adequate for steady states.

A very good illustration of a transient in the system which can lead to an excessive temperature overshoot in a power reactor is loss of coolant in a reactor using solid fuel and a pressurized gas coolant. The design may be such that adequate cooling by convection is provided to carry off the after heat some several minutes after the reactor has been shut down. This is one of the first transients I usually examine. The fuel-element temperature rise that follows operation at design point when there is a sudden loss of coolant determines the rate at which the control rods must be inserted. In one or two cases I have found that to limit the

temperatures to those specified by the designer would require that the control rods be inserted as much as 2 or 3 min before the loss of coolant occurred. And yet these same designs showed that, should the reactor survive the transient overshoot in temperatures, there was adequate cooling by convection to take care of the system several minutes after the loss of forced cooling.

This illustration demonstrates that thinking of the design in terms of steady-state performance is totally inadequate for specifying the instrumentation and control of such power plants. If no amount of improvement in the instrumentation and control for a design subject to damage in an excursion can make up for the inadequacy of the design, then the design must be changed.

Information Used in the Control System

A control system must be actuated by instruments, and if the control system is working properly its actions will be identical for a given output of an instrument whether the information that leads to this output is right or wrong. If then the control instrument that initiates the corrective action for coolant stoppage shows that the coolant loss has occurred when it has not done so, the control system will proceed to take whatever corrective action had been specified as its prime function originally. The rapid cooling of the relatively high temperature fuel elements and structure may also be something which could damage the power plant, or lead to the shortening of the life of the power plant.

The reliability of the instrument may not be the key to the solution of the problem. The quality of the information which the instrument must handle may provide part of the answer. Obviously the control action is intended to limit the temperature of the fuel elements. But there is practically no way to determine the temperatures of these elements. It would suffice to determine only the temperature of the hottest element. But it is extremely unlikely that anyone knows which element is supposed to be the hottest. If this were known then it would be equally unlikely that a good temperature sensor could be installed which could be depended upon for the life of the power plant. The gas coolant reactor outlet temperature probably would change markedly with the decrease in flow of the gas. This temperature is difficult to measure by a reliable device which

has a short response time. The gas flow in some other part of the system is another variable which can be measured and used to show the loss of coolant. And there is a tachometer on the blower shaft as well as the power input to the blower motor which could be measured. In principle all of these types of information could be used to initiate the required control action for loss of coolant to the reactor.

The instrument engineer must look at the quality of the information for each of the sources mentioned and any other sources which could be used. If a choice of the best source of information to use for this control function is left up to the designer, his criterion may be one which dictates that the sensor interfere least with some other piece of equipment for which he is desperately seeking space.

There may be other reasons than loss of coolant which could produce excessive fuel element temperatures. And the prescribed corrective action for these other causes of excessive temperatures need not be the same as that for loss of coolant. If, for example, the coolant is exactly as prescribed but the neutron flux has increased, then the fuel-element temperatures will increase. The corrective action required here is one which leads to a decrease in power production until it matches the power removal. To use the more drastic corrective action prescribed for the loss of flow situation would be very undesirable in this case.

The reliability of the instruments and the reliability of the control system must be evaluated, but even the most reliable equipment cannot be expected to provide adequate control of a system if the only information available for accomplishing this is of extremely low quality.

Almost invariably the duties and responsibilities of various specialists working on the design of a nuclear power plant overlap each other. There is nothing especially awkward about this situation, if the various specialists do not forget that someone must take the responsibility for getting a particular job done. If the instrument engineer puts an instrument wherever he is told to put one he has discharged only a part of his responsibility so long as there is no one on the job who knows the importance to the over-all plant of this particular instrument. If the information that this instrument provides is of poor quality, and if the action initiated by the information gained from this instrument is drastic, the system may be subjected unnecessarily to abuse for which it was not designed. A situation like

this leads to statements such as "the reactor was shut down some several times but most of these were brought about by instrument or control system failures." The implications are that if the instrumentation and control system had been improved the inconvenience and damage caused by shutting the reactor down so many times could have been avoided. The power-plant design itself could be at fault if it was found to be absolutely necessary to use unreliable information for such drastic control action.

Traditions Established in Research Reactor Design

It is highly desirable to have a power plant design that does not have a multitude of situations all of which require drastic control action. Some of the thinking on nuclear power plant designs has been prejudiced by the early history of the control and safety systems of the research reactors. In the research reactors, high temperatures usually did not pose problems to the control engineer. Also, early in the development stages of these reactors it was considered proper to scram the reactor for any of a number of conditions or occurrences. The control system could be so rigged that nothing but positive proof that everything about the system was working properly would allow the operation to proceed. The operator was supposed to look the situation over and determine the cause of the scram before he undertook to take the reactor back to the condition it was in before the scram was initiated.

The nuclear power plant is a large system. The nuclear characteristics of the design are almost totally different from those of the research reactor. Long life of every component in the system is a strong consideration. From the instrumentation and control standpoint the high temperatures of the system and their effect on the life of the plant pose problems totally different from those of the research reactor. Heat transfer from the fuel elements to the coolant can be just as important in determining the fuel element temperatures as is the neutron flux. Where our instrumentation and control system for the research reactor was required to keep the neutron flux under strict control at all times, we must now keep both the flux and the heat transfer under control at all times.

It could be argued that the response times of the neutron flux to control action are in general much shorter than the response times of the heat removal system, but in power reactors the fuel temperature response

times are those of concern to the control engineer. Good temperature coefficients of reactivity in the fuel elements go a long way toward limiting the temperature excursions in these fuel elements. The stabilizing effect of such coefficients can eliminate for all practical purposes the necessity for scrambling the reactor on suspicion.

One cannot more or less blindly adopt features for his control system solely because they have proved satisfactory in the past in other reactors. They may do more harm than good. An inordinate number of fast scrams of a nuclear power plant can harm the power plant by causing excessive temperature cycling of the system. Temperature cycling was hardly a problem in the low-temperature research reactor.

A proposed nuclear power plant should be considered really new. Every detail of the system should be investigated thoroughly. The instrumentation and control system should be analyzed in great detail.

Use of the Analog Computer

Before the large computing installations became available, the pilot plant was about the only means available for evaluating the behavior of the system and for determining the adequacy of the instrumentation and control. The analog computer facility is exceptionally suitable for such analyses. However, the merits of such analyses depend almost entirely on the motivation which one has in using such equipment.

If the computer is used after the entire power plant has been designed and all of the instrumentation and control for the system has been specified, it is all too likely that the computations contribute little more than a demonstration that the system should behave properly provided all of the components of the system including the instrumentation and control gear perform in accordance with the specifications. It is probably entirely too late to determine all of the details of what happens. Specification of the proper corrective action when each of a great number of components fails is of little merit when one knows that it is too late to devise a control system which will assure the proper corrective actions. The control system can usually be modified even at this late date so that those component failures which require specific corrective action will cause a reactor scram. But we are not so sure but that numerous scrams will decrease the life of the reactor. This use of the analog computer,

which requires little more than a demonstration of how the plant works when it does, probably has been responsible for the experiences of some who find little use for such equipment in designing reactors.

To a certain extent one may look upon the computer as a synthetic pilot plant, which will provide a vast amount of detailed information about the system. That was the object of the pilot plant. It was intended to disclose detailed information which could not be obtained in any other way than by operation of the pilot plant. And this detailed information was used later in perfecting the design of the final plant.

The instrumentation and control analyst finds himself looking at many small details which enter into determining the system behavior. He must find out what the system does when it works properly, find out what is likely to happen when some component fails, determine what information should show that a component has failed, determine the quality of all such information and select that which is likely to be the most reliable for his control system. These are only a few of the tasks he is responsible for.

His analysis should start almost the very day that the design starts. The designer usually begins with the preparation of a conceptual design, which is supposed to disclose the problems requiring analysis and development. If the analyses by the various specialists show that the conceptual design is feasible, work is started in developing a detailed design which will be ultimately built and tested.

The results of stress, heat-transfer, nuclear, and similar analyses contribute much in determining the feasibility of the design. The instrumentation and control analyses for the proposed system should likewise contribute much. I have only recently completed an analysis of a part of a conceptual design of a proposed nuclear power plant where the analog computer was used. This particular analysis showed many very attractive features in the design. It also showed one feature which is not so attractive. The designers have been made fully aware of this particular feature and can now proceed to change the design.

In this case there are several ways by which the problem may be solved. There are even a few ways by which the instrumentation and control system could be made to provide a solution. You can be assured that had the undesirable design feature been discovered only after the design

had been frozen the only solutions acceptable then would have been those provided by the instrumentation and control system regardless of whether this means was the best or the worst.

Whatever is proposed as a solution of this problem by the designer must be examined, to see whether in avoiding one undesirable situation he has not introduced something else equally as undesirable. This calls for further analyses on the new system. In this respect the instrument and control analyst differs little from other analysts, such as for example the stress analysts, who must look again and again at the problems that arise with each change in the original design.

The instrumentation and control analyst must make known the results of his analyses at an early stage in the design of the power plant and again and again as the design progresses. If he does this he sometimes gets the reputation of trying to dictate the design so that the power plant gets by with few if any control problems. I see no reason to apologize for such a worth while objective. After all he does not have the authority to specify the overall design. If simplification of the instrumentation and control systems of the power plant makes some of the other problems in the design totally unacceptable, the instrument engineer and the control engineer must accept penalties which ease these other difficulties.

I feel reasonably certain that the decision will not be made to build a system which the stress analyst has shown is unsound. And I see no reason why a system should be built when it is known that its performance and dependability are extremely likely to be restricted by inadequate instrumentation and control primarily because the design was such that there was practically no way by which they could be made adequate.

Conclusion

Someone must supply the instrument engineer and the control engineer with all the information needed to provide adequate instrumentation and control for a nuclear power plant. This person must know something about the limitations of instruments and control gear. He must also know something about nuclear power plants. He must be able to make the maximum use of the relatively new computer installations at his disposal, to determine what the power plant will do both in steady state and transient conditions,

and particularly to determine the behavior of all parts of the system when there is a component failure.

From his analyses come the complete performance specifications of all the instruments and all the control systems used on the power plant.

This same person must provide to the designer, at the early stages of the design, information that shows what may be expected in transient and steady-state performance from the proposed design and what may be expected after each change. He must point out conditions that indicate that changes in the design should be made, when such conditions lead to inadequate instrumentation and control.

I know of no better title to give this person than that of Instrumentation and Control Analyst.

J. N. WILSON: I certainly second many of those remarks. Two rather soul-searing experiences that we have gone through might be beneficial to apply. A long time ago at Hanford the reactor died and we suddenly discovered xenon. So even the physicists are sometimes wrong.

More to the point, at Savannah River we learned that the d/p cells had been tested nine million cycles for drift, but we forgot that under normal operation of a power reactor you don't get nine million cycles. You want to have one cycle in many months, and then the drift of the spring element is very important. Nobody makes a spring-loaded d/p cell that does not drift. So don't forget your point about each installation being different.

You can build a power reactor prototype and use it for research, but you want different instruments from those used for the production of power economically.

E. R. MANN: You are so right. Both the d/p cells and the xenon could have been detected with a small power plant if you could have gotten the flux up high enough. That is the old-fashioned way of doing it maybe, and it may take years to work yourself up to the final product.

J. A. BARRETT: From your remarks, I take it that you think our problem is more a political one than a technical one.

E. R. MANN: No, I did not imply that. It is solely technical. I go to a reactor designer to get information regarding instrumentation and control. He is the wrong man. I repeat this process several times. All of them look to me like they ought to be able to tell me. But apparently

they have other responsibilities. I cannot blame anybody for the design, either the chief designer or the man in charge of the thing, if he does not know the details of instrumentation and control. He has the over-all job to look after.

A. M. YUILE: I have found, on quite a number of projects, where the plant designer or project designer was a mechanical engineer, there was a great deal of reticence on his part to give any information out on the instrumentation and control for fear we would over instrument the plant. He wanted to wait until he was finished, and this is a political problem.

C. M. BURTON: I don't see why this could not be handled as it is in the electric power business. There they have a group that is responsible for safety and operation of the system. It may be the relay protection group, but this group holds a rather esteemed position in the design and operation of the power system. They are charged with the responsibility of the safety of the equipment in the system and continuity of service to customers.

E. R. MANN: In no case am I advocating that if I am doing the instrumentation and control analysis, and if the designer won't change the design like I tell him, I should quit and go home. My approach to him is try him again tomorrow and the next day. What I am getting at is that we cannot, as instrumentation and control men, dictate the design. If we dictated the design, we would make as big a mess of it as we accuse the designers of making of our instruments and controls. We have to yield but we have to put up a fight.

program. Also, from what we all know of the EBWR, it was designed essentially for 5 Mw, and they have already succeeded in producing two or three times this amount. These plants, apparently with the conservative designing we are using, are capable of taking a tremendous amount of overload. So, we can continue to design them cautiously, if you like, and use this overload capacity in protection.

Now I want to return to my main thesis that we have to start designing as others do.

Figure 14.1a is a schematic diagram of a gas-fired plant. A gas-fired plant consists essentially of a boiler and a load. Into this device is fed gas; control of the gas has some of the characteristics of our reactor plant. The gas is presumed to have some kind of air as a moderator or it does not go. It has a safety pilot, which is, to some extent, analogous to our scram. It has a hand-operated valve, which corresponds to some extent to the manual scram.

By way of instrumentation it has two inexpensive instruments (temperature and pressure gages). There is also a level control; this is a fancy device called a sight gage, required by code to be installed and visible. Most states require mirrors; in Tennessee television can be used to look at this sight gage; that is progress! But basically the instrumentation is extremely simple and the protection is extremely simple.

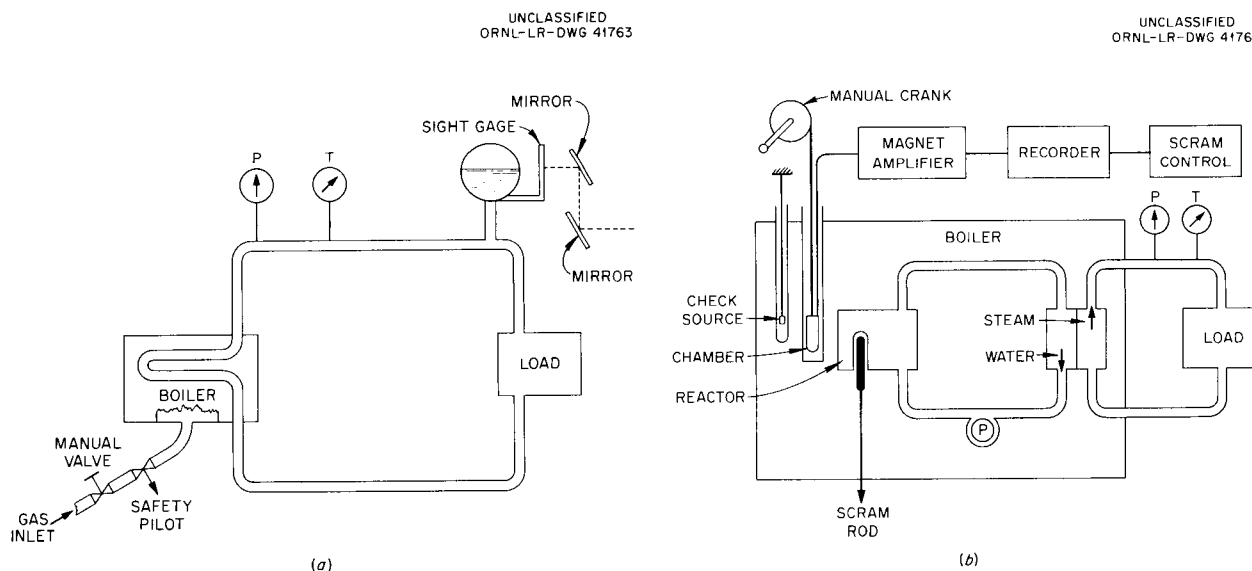


Fig. 14.1. Power Plants: (a) Gas-Fired, (b) Nuclear.

We can now design a pressurized-water reactor that will take care of itself with any reasonable kind of accident - to those of you who are not in the pressurized-water business, all I can say is that it is a question of time. To my way of thinking the designers of homogeneous and sodium reactors will perfect the art to the point where they, too, can do this.

In Fig. 14.1b a reactor and heat exchanger combination constitute the boiler. As for instrumentation, I grant that one piece of nuclear instrumentation is probably desirable. This is a really good ionization chamber, a Westinghouse fission counter, or something of that sort. To take care of the multiple ranges I will tie this instrument to a hand-crank and move it up and down; or I will put in Mr. Siddall's piece of lead, if necessary, and crank that up and down. I am going to make the range of this instrument sufficient to take care of the multiple checking. To make sure that this works before I start my reactor, I will lower a source adjacent to my instrument and check this every time I start up.

Once I get started, I don't care at all about the nuclear end of this. In the power plant I want heat, to produce power. I am perfectly willing to put a control on temperature and pressure once I get up to power. Any nuclear instrument, essentially, has to be reliable only during the start-up. I can check it before I start and I am willing to trust it for the next 30 min.

You will note that in the "boiler" part, the primary loop, I do not have a single instrument. Again I put inexpensive pressure and temperature gages in the steam loop. I argue that everything that happens in the primary loop is ultimately reflected in the steam loop. You can argue that the time constant is the weak link about the nuclear business; nevertheless, two of the time constants mentioned recently were 15 sec and 2 min. These lengths of time I can reflect over to the pressure and temperature gages. I argue that it is stupid to make measurements on radioactive water or radioactive sodium when a nice clean measurement can be obtained with an inexpensive gage.

W. H. JORDAN: As I said in introducing Mr. Schultz, his intention was to be provocative, and I am sure a lot of us reacted emphatically.

I intended to begin with a statement which I thought would be a little provocative, too, by agreeing with Mr. Siddall's remarks this morning that you cannot afford always to limit your reactor design in such a way - I

believe this is the notion that you gave -- that it will automatically be self-limiting. I think that Mr. Schultz has a good point in the case of the pressurized-water reactor or perhaps even the boiling-water reactor. Certainly the boiling-water reactor is not quite as simple, but there are some pretty safe devices. On the other hand, my feeling is that we have to look into other types of reactors. Water moderation is good for many purposes but it is perhaps a stopgap. It is certainly not the ultimate. It could be that this will be the reactor of the future, but there are some people in other countries who think that we in the United States are making a mistake in putting all our chips on the water-moderated reactor.

Mr. Siddall, will you present your opinion as to this matter?

E. SIDDALL: Mr. Jordan certainly expressed what I would initially say. As an illustration of this, I think economics is missing from Mr. Schultz' analysis. If he could design that system to be the most competitive reactor in terms of mills per kwh output I would be very much surprised. In Canada, for instance, we are following the course of high neutron economy so that we can use natural uranium. We don't have to talk about reprocessing costs. When the uranium has been through our reactor it is not used for anything for quite a long time. We burn up all the U^{235} in it. This is a starting point that looks to us extremely attractive, and we have to work on it. The type of reactor to which Mr. Schultz referred is, I am sure, a light-water-moderated reactor with enriched fuel, which alone will make it go. It must be moderated; otherwise his void coefficient isn't in the right direction, and the accidents that will be eliminated by a large void coefficient are not necessarily all the accidents. For instance, I postulate a burst in the cooling circuit or a failure of the circulating pump, and in his type of reactor the fuel can burn out.

When Mr. Schultz talked about the large margin of overload that he has available, he was assuming that a reactor can be run at 100% of load, and that if for a few seconds it goes up to 200 or 300%, it will not be damaged. That is not economics. If that reactor will run at 200 or 300% of some arbitrary level, we are going to run it at that level all the time; and even if we do that we are only just reaching economic competition. We have no great margin in this business for anything, in my opinion. In Canada every pound of heavy water has to be run as hard as we can possibly

run it. We are having to subdivide that fuel into fuel elements, because in that way alone can we evaluate the fuel to pay the capital charges on it.

A reactor is operated as near its burnup point as possible by close control. That is why we control people have nothing to be apologetic about. We have the means to run these reactors near their burnup points, or near whatever limit is set on the rating. Any highly rated reactor has a big Xe^{135} problem and this almost automatically destroys the self-regulating features.

In our Canadian reactors we have about 2.8% xenon poisoning at equilibrium. When the reactor is started up the first time this poisoning does not exist; but it builds up over a long period of hours. There is a slow diminution of activity. Something has to be done to change that. Voids or anything similar cannot be allowed, on any study that I have done, to cope with 2.8% reactivity change. If you do that, it is wrong at the end. It cannot be right both places. With instruments we can keep it right all the time. You can take out that 2.8% with absorbers or by some other method.

W. H. JORDAN: Mr. Schultz would say that the xenon time constant is so long that he has plenty of time to measure pressure and temperature and correct the rods from those.

E. SIDDALL: If he is correcting the rods he is controlling the reactor externally and not internally.

W. H. JORDAN: I don't believe he meant not to have a rod in there.

E. SIDDALL: No, I don't believe he did. That was not quite fair. However, Mr. Schultz' Fig. 14.1a is an absolute parody of present power stations. There is no resemblance to a modern gas-fired or coal-fired power station. Designers of such stations face exactly the same problem. If they have to reduce their power cost from 4 to 3.5 mills per kwh, they must start to put in the same sort of refinements that we are having to put in to reduce our cost from 20 to 18 mills.

The modern power station is very much more complicated than it was even 10 years ago. There is far more control instrumentation on it. You have only to look at the flow sheets to see that. They are working with pumps and side circuits. They are offering several stages of reheat in the turbine cycle itself, and they are squeezing the last ounce of performance out of the system. It could be that these people are wrong in going

to these complex systems, but from publications like Business Week and the Commercial World we know that these new and complex stations are replacing the older ones. The older ones are being retired to the peak load periods, and these new stations, with all their frills that we are criticizing, are the ones that are now earning money for power companies. So don't let us blind ourselves to what we are achieving by controls. These stations are being better controlled, and the last ounce is being obtained from the capital put into them. We must do the same.

I believe that when our reactors are economical they will probably be even more complicated than they are at the moment, and the instrumentation will be as discussed in "Built-In Testing in Process Instrumentation" (this conference). For instance, the containment vessel could be eliminated - this is one of the aims I always have in mind from this point - if we could convince people that our protection is good enough. By eliminating containment costs we would have saved the cost of controls several times over. The mental block that we face at the moment is that nobody dares to admit that there is an end to the line in the reactor protection business. As soon as somebody says that something could happen, safety-wise there must be somebody who will say, "This is what we will do to stop it." I postulate that with proper protection of the type I have been mentioning, which is not much more than 5% of the cost of the reactor, we stand the chance of making a reactor that will work well enough to compete, as Mr. Schultz was saying, with other power plants. There is risk involved, but we accept it because although there are dangerous materials in that reactor, we have taken enough safeguards to keep them there.

Finally, we cannot now compare reactors with that nondefinitive sort of drawing. It is not 1948; it is 1958. I want to compare, with Mr. Schultz, designs that are potentially and competitively economical.

W. H. JORDAN: The meeting is now open for discussion.

C. S. WALKER: What can be taken as a safe design margin? Suppose the metallurgists say that material can run at 1600°F. Can we design for 1600°F with no margin?

Also, I should appreciate comment from W. C. Lipinski on how they got the extra power out of the EBWR. Was it with higher pressure, higher maximum temperature, or higher flow? How close, to the upper limit that the

structural materials are able to withstand, are they able to operate, in his opinion?

W. C. LIPINSKI: The EBWR was designed for a nominal 20 Mw of thermal power, 5 Mw electrical power. The vessel was sized to hold a 5-ft-dia core, but actually it is loaded with a 4-ft-dia core, 4 ft high, and the nominal pressure is 600 lb. The vessel has a design pressure of 800 lb, working pressure of 600 lb. The 20 Mw was established on the basis of anticipated reactivity in the voids required for 20 Mw of power production based on experience with BORAX experiments.

The original approach to power was made rather carefully with respect to the stability of the reactor itself, and for any further increase above 20 Mw it was necessary to obtain approval; so the approach to higher powers was done successively by measuring the kinetic behavior of the reactor as a feedback mechanism. We installed a mechanism to oscillate the center control rod and measured transfer function at various powers and pressures. We extrapolated the data to give an indication of the maximum expected power for the particular core inside the reactor vessel. Based on burnout heat flux we had a safety factor of about 10, as I recall.

Heat flux was the criterion. It was simply a question of the void feedback mechanism. The first increase was to 33 Mw. Based on our data at lower powers and at 33 Mw, we operated at 50 Mw as the next step, and the final point was 61.7 Mw. The limit of 61.7 Mw was set by the feed-pump flow rate. We could not maintain the water level in the reactor vessels with both our feed pumps on. This is the only factor that stops us from going any higher.

Stability-wise the reactor behaved fine, and present plans are to go to 100-Mw design with the present vessel by going to a 5-ft-dia core 5 ft high. We will have to install more heat exchange equipment to dissipate this energy, but with our present plant we anticipate a 100-Mw rating.

C. S. WALKER: Then you would say this reactor was over-designed?

W. C. LIPINSKI: Yes. We had to be ultra-conservative because we are located right next to Chicago. This is one point that everybody has been making here today, and there is nothing that we can do to change it. As long as we have the fission products locked in that core and they represent a hazard, we have to go all out as far as safety is concerned. The casualties in flying airplanes today are nothing compared with casualties of a million or so.

W. H. JORDAN: I want to emphasize Mr. Lipinski's point, that reactor control people have a heavy burden. With a possible billion-dollar damage suit rather than a few million dollars, or whatever it is with an ordinary plant, we have to deal with safety of a different order of magnitude.

E. P. EPLER: I am quite sure that Mr. Schultz made several statements which were intended to be provocative. I don't think he really believes them. I think that at least one of them was aimed at me; so I will respond to it.

He illustrates a reactor of the water-moderated, pressurized type, with a temperature coefficient in the moderator and one in the fuel. The fact that he has these two temperature coefficients shows that he has a very specialized reactor. For example, if a reactor has only a moderator-temperature coefficient, then it is dependent for its stabilization on the void coefficient. On the other hand, if the reactor has a strong fuel-temperature coefficient by virtue, say, of natural-uranium or slightly-enriched-uranium fuel in place of fully enriched, then it is another kind. A reactor is capable of extremely short periods. This can cause trouble fast and something has to remedy the trouble. If the temperature coefficient is good, it will ultimately limit the trouble, but it may leave the fuel at 5000°, with no way to reduce its temperature.

The problem of fixing these things varies just on the basis of whether natural or highly enriched uranium is used. I think this is fully significant today. We have been building reactors now for about 15 years, and we are beginning to learn a little about the beasts. We keep putting together different configurations; I claim that these are different reactors, and it keeps us busy trying to keep up with the horrible things that we invent.

I think I agree that our objective should be to take the scram off the reactor but we cannot take the scram off first and see what happens next. We have to earn the right to take it off. We have to fix this reactor in such a way that we have assurance that we no longer have to drop the rod crashing into the bottom, in order to take care of accidents that we can anticipate. I think that we are in no position to earn the right to take the scram off until we have learned how to design components that won't wreck the reactor. The most dangerous thing that we can put into a reactor, I think, is a rod. It is put in there for control and safety; yet it invites an accident. I don't mean a thoughtless accident in which the rods

are withdrawn too fast at startup. This could be made fairly trivial simply by slowing the rods down. The rod is capable of coming out at any speed under certain conditions. We will fix the bottom so that the rod cannot fall through, and we will be safe from that accident. But there are other ways in which this rod can be tricky.

Slow loss of reactivity does not seem dangerous. It has a shutting-down effect on the reactor. But there are two ways that it causes trouble. First, if the operator succeeds in keeping the reactor critical by slowly withdrawing the rod and the reactivity comes back fast, he has had it. The second way of creating trouble is a circuit which has almost the same ability to keep this reactor critical. Let's say that the reactor has something less than 5% excess reactivity. If the reactivity has been lost through any mechanism which we can't always predict, this simple circuit replaces it. If it then comes back quick we have had it.

Let's allow the operator to make mistakes, and let's watch him. I don't think we can expect an operator to think quickly enough to avoid this mistake. Designers, having a whole year to think about it, have not seen it.

The constant search for more economy, including cheaper apparatus, in building reactors has made us cut some corners that we have to watch. I feel obligated to continually emphasize that this will make trouble. We have always felt that we could drop the rod into the pile by gravity. Today, in the pressurized water reactors, we hate to spend money to encapsulate our rod cells in pressure shells, so we penetrate the pressure shell with such a device that one end of the rod sticks out. This makes the rod, in effect, a piston with several hundred pounds per square inch in the reactor trying to drive it out. If you put the rod in through the bottom and push it through the core, any forces developed by the pressure will tend to push the rod back into the reactor. If you put the rod in at the top, the pressure in the reactor will try to push the rod out. Regardless of how many devices we put on to try to make this palatable, some day the rod is going to come popping out.

Can the reactor stand this accident or can't it? If it can, then the reactor does not need any scram because that is the worst thing it is ever going to get. The scram couldn't help, of course. The scram is too late.

We have not yet earned the right to say that the reactor does not need a scram, because we have not found out yet all the things that can happen to the reactor.

W. H. JORDAN: I believe that Mr. Siddall is certainly right in saying that the safety system he described is economical. It is a small fraction of the capital cost, which is the big thing we are fighting. It sounds like a good scheme, and the thing that I like most about it is the ability to check controls once an hour or whenever desired.

He has gotten around the uncertainty in the use of coincidence circuits in a very nice manner, although it still leaves me a little uneasy. On the other hand, as I said, I do buy the ability to check instrumentation, and particularly the very fine feature of checking with a signal exactly like the signal produced if the reactor is in trouble.

Do you gentlemen intend to put this on your next reactor or not? Why?

J. E. OWENS: Some variation of it, certainly. We have been giving a lot of attention to control-system reliability, and we have not devised anything as attractive as Siddall's scheme this morning. However, I agree with Mr. Schultz to a large extent. I feel that perhaps we are making our reactor-control circuits a little too complicated. I think that in a power reactor we do not need the multiplicity of reactivity controls that we have been applying to them. We must have them for startup, and perhaps for a high-level shutdown, but our control system should be based on the product that we are trying to manufacture, thermal energy. With our reactor, the time constants are long enough that we can control on temperature and on flow, and it looks as though these two will be sufficient.

We can see very little probability of slow loss of reactivity followed by its sudden recovery.

We have tried to get redundancy of protection from different types of instruments. Look at the primary loop in Mr. Schultz's figure, for example. We scram our reactor on $\pm 5\%$ variation of pump speed. We back this up with $\pm 10\%$ variation in sodium flow. We do not see in general how we can get one of these variations without the other. So we monitor at both. We back this up with our more or less ultimate hazard-detecting instrument, fuel-channel outlet temperature; and we are using two-out-of-three coincidence here. We feel that if everything else fails we still have this last-ditch measure to get the reactor shut down before it really gets into trouble.

We also monitor other temperatures throughout the loop which will shut down the reactor.

Although these individual detectors are not in themselves redundant, they constitute system redundancy, and the accessory sodium loop is monitored in the same manner. Our steam loop is also monitored for loss of feed water, turbine-throttle trip, etc. We consider not whether we have three of each particular type of detector, but whether we have at least three levels of protection for each accident that we can postulate.

W. H. JORDAN: I infer that Mr. Owens is going to consider this system very seriously in his reactor designs.

J. E. OWENS: Certainly the checking-out aspect is highly important.

W. H. JORDAN: I am not sympathetic with the thought that the instruments are so bad that they are always shutting the reactor down. I think the answer to that possibility is to make more reliable instrumentation. I have a feeling that we are approaching that now, that we know where most of the shutdowns come from, at least in experimental reactors. They come from experiments installed in the reactors.

Does anyone else have a comment?

E. P. EPLER: This is an unclassified meeting, so I cannot talk about your last reactor; but if your next is anything like it, we have a lot to decide on how we are going to protect it. We must determine what instruments we will need. I think I will have to restrict my remarks to past experience. I can borrow from Mr. Siddall on this, because he says that if other things are equal and there are no external forces, then the probability of what is going to happen next depends pretty much on what has happened in the past.

We have had a great deal of experience with small research reactors, and I will base my answer to coincidence protection on this simple reactor with which we have had the most experience. In this case I can again be extremely sympathetic in the matter of protection. I think that we have to do better. We have many ways of testing things, but we have to find a way so that I can walk up and push some kind of a button and a little cuckoo will jump out and say that this thing is all right and I don't have to ask anybody.

I think that most false scrams we have had in research reactors were due to a certain bullheadedness on our part or some other fault. They

would not necessarily have been due, to a measurable degree, to the instruments that can be put within the coincidence loop in the way that we would probably have had to put them. In order for us to avoid false scrams due to bad magnets - and we have had a lot of bad ones - we may have to put three magnets on each rod, or adopt, as Mr. Siddall has, a clutch device. In cheap pool-type reactors we are not ready to do this, because we have so many advantages that we are trying as hard as possible to exploit in the magnet that we now have. I think, therefore, that we are not prepared to go to this clutch device with which we could get greater freedom from false scrams.

At this point we are not quite ready to say that the electronics that we put into the loop is the greatest offender. One of the greatest offenders is the failing period circuit, and the only ways I know to make it less of one are to disconnect it as soon as we get through the startup - you don't like that, nor do I - or put in three. Even the latter is a little unattractive because we have not quite ruled out false scrams due to external noise that gets into the circuit; it would probably get into all three of them.

J. MacPHEE: Perhaps Mr. Siddall will comment on this problem: Suppose we have a simple system made up of a battery, a switch, and a pilot light, and we want always to be sure that when we turn on the switch the pilot light will go on. Applying his approach, we would have a test circuit which would periodically test the pilot light to see if it is working. According to the equation (see "Built-In Testing in Process Instrumentation," this conference) the more times the pilot light works when tested, the greater the probability is that the next time we press the button it is going to work.

However, there is another factor that must be considered. The life of the pilot lamp is finite; this will affect the probability that the light will go on. Have you taken that into consideration in your approach, Mr. Siddall? Perhaps you could elaborate on it.

E. SIDDALL: Just a quick answer to the fact that the life of the lamp is finite. You are not working in the absence of other information.

My impression is that the great majority of the instruments we use either don't have a fault increasing with age or use or have an increase so slow and so widely scattered that it does not seem to amount to very

much. This may not apply to every instrument you use. It does apply to the one that I described.

J. R. MAHONEY: Mr. Siddall, I have no probability on which to base my answer to Mr. Jordan's question. Can you give us an estimate of the probability of a false scram under your previous system and one for your triple circuit with coincidence? Was this 0.0001, and has it now become 0.000001?

E. SIDDALL: This will be a generalization but it is my starting point. The improvement is quite radical, from one scram per week to one in ten years or something like that. We are just getting under way with these systems, and in practice the scrams that are not random, not independent of the circuitry, are reduced. That is, if all the set points are set very close to the working point, some disturbance may be a genuine condition as far as the instrument is concerned, although, of course, it may not be really a necessary scram. That is where the operators come in.

We are fairly confident that we can reduce our false scrams by a factor of 2-5 immediately, and by proper use of the systems, after we learn more about them, I think we can perhaps do better than that. Beyond this we must determine the scrams are fundamentally necessary. It is going to be a quite difficult field.

W. H. JORDAN: It is not clear to me. Are you considering this circuit for the NRX or the NRU, or is it for the contemplated power reactor?

E. SIDDALL: All three. The NRX and the NRU now have triplicated scram systems. What we have not yet introduced is any way of comprehensive testing. We can test all the scram leads within the electrical circuitry. We can inject a test signal into the instrument by moving its pointer or by some such device, but the guard system against false scrams is not in operation. However, the NRX was completed only about six weeks ago and the NRU is in the middle of starting up. We are getting quite a number of what might be called general scrams from the instrument point of view; so it is, unfortunately, too early to get this information. However, I am quite certain that we shall soon know whether it is worth while. I think it will be.

J. E. OWENS: I should like to look into the question by Mr. Epler in "Safety and Control of In-Pile Experiments" (this conference): why do we scram a reactor? Most of it is due to our heritage from the research reactor field, I am sure. Most of us who have had anything to do with power

reactors have been through the evolution of scram circuits. In one reactor we had 100 scram contacts; and I think that when we finally got it running we had 5 or 10. Our own sodium reactor started with 30 or 40. We have it down to the order of 10 or so.

We have tried, for each condition that may arise in the sodium reactor, to ask, "Is this condition really harmful to the reactor? Will it happen fast enough to be harmful? Have we taken all other possible measures? Finally, will the scram do any good anyhow?" There are some troubles in which a scram really may not help. Particularly with high-temperature reactors, the cure may be worse than the disease. Our stress people shudder every time we talk about scrambling our reactor from full power; I think probably rightly.

So, I think that all of us could afford to look at some of Mr. Schultz's built-in self-regulating features and perhaps some of the heat capacity features of research reactors, and quit scrambling on anticipatory trouble, such as period, for example. We cut our period circuits out as soon as we get above 3 or 4% of full power. Our false scrams have been largely from period instruments and from power system troubles. We cannot divorce ourselves completely from purchased power, and, since we are on the end of a fairly long transmission line and fairly closely coupled to our neighbors who are doing things like testing rockets and running 4-Mw compressors, we have supply power troubles. Every time they turn off a pump, we see it in our period instrument circuits.

We try to get around this by separating our control circuit and putting in separate motor-generator sets. These have their own built-in troubles of which I am sure you are all well aware.

I think perhaps we need a redefinition of safety when we start to work on power reactors.

J. N. WILSON: It seems to me that all day there has been one region that we have not discussed which may be related to this last question. That is the detection of fuel element failure. One of the usual causes of a scram situation is the swelling of a fuel element, which cuts off flow, thereby raising the channel temperature. If we can detect the inception of a small rupture by detecting, for example, fission products, we can then shut down slowly before damage is done.

Also, the economics of the power reactor that we have been studying, which is a heavy-water natural-uranium reactor, are tremendously dependent on the cost of fabricating the fuel; this is very expensive - perhaps half the cost. We can save by running very close to the fuel element limits, as Mr. Epler pointed out, but now we are in trouble with the differences in the fuel elements. If we cannot allow a rupture, absolutely cannot allow a meltdown, then we must have a lot of leeway because of the inherent differences between fuel elements. If, however, we have a very good detector that can detect, for example, the first little bubble of gas, we can afford to run closer to the edge and to get more economical power.

The same question occurred to me in regard to Mr. Vogel's "System Design and Instrumentation of In-Pile Test Loops" (this conference). One of the things that are tested in a loop, probably the most important, is the fuel-element design, and the occurrence that will cause trouble the quickest is a failure. If you have ever cleaned up after one of those messes, you know it is expensive. In the final reactor, with a multiplicity of fuel elements, we must not only detect the rupture; we must determine which one ruptured. Overhead cost is mounting all through this period. We can afford to pay for a lot of instruments to determine as soon as possible which element is giving trouble so that we can quickly get it out and avoid a scram.

I would be very interested in listening to other ideas on rupture detection.

W. H. JORDAN: We might ask Mr. Siddall. I believe that in your NRX you had a device for detecting delayed neutrons in the water downstream. Is that still in operation and are you planning to use it in future reactors?

E. SIDDALL: Yes. The NRU reactor is the latest one we have going. Dr. Pearson knows more about the immediate past of this than I do.

A. PEARSON: At Chalk River we installed 22 pairs of connected detectors, which identify each of the 200-odd fuel elements. Each pair consists of a fission-product detector and a delayed-neutron detector.

The system has become considerably contaminated during startup and background is 100 to 1000 times what we had anticipated. When this activity is removed by burnout and by ion exchange columns, we should be

back where we hoped to start. However, the GFP system, after we desensitized it, is still a very good detector. We have detected several split fuel elements.

W. H. JORDAN: Can you say a little bit more about what you use for detecting fission-product gases and about its effect?

A. PEARSON: The fission-product detector is a beta counter. The gaseous fission products are stripped out of the coolant with helium, which is then examined for beta activity.

W. H. JORDAN: Then you are also continuing to use the delayed-neutron detector?

A. PEARSON: Yes.

W. H. JORDAN: That has to be run way downstream, does it not, in order to give time for decay?

A. PEARSON: Yes, about 90 sec is allowed to elapse.

W. H. JORDAN: Then how can you use a matrix?

A. PEARSON: We have to keep the delays in the channel to the detector.

W. H. JORDAN: Do you keep the cooling channels separate for a minute or take a very small water sample out?

A. PEARSON: We take a sample of water.

J. N. WILSON: Is it true in your system, as it was a year ago, that any given rupture might be detected by an entirely different sequence of detectors? This is our experience. Sometimes you get a rupture and you see it on the gaseous detector. The next time, for no apparent reason, it is the delayed neutron detector that finds a rupture.

W. H. JORDAN: Are you using both types?

J. N. WILSON: Yes, we are using everything possible.

A. PEARSON: We haven't gotten to that stage yet.

J. N. WILSON: In the NRX this is or was true.

A. PEARSON: Yes, it was true. That is what led us to put those systems in use, of course. I suppose we put both in because there was not enough evidence to decide on either one. We thought we would try both. If we can detect an incipient failure in the NRU we can remove it and keep going.

E. SIDDALL: The NRU is not working. The NRU is a uranium-metal-fuel reactor, whereas all of our power-reactor designs are based on uranium oxide elements. Therefore, the necessity for removal of a damaged slug

will be less likely. On the other hand, we have real trouble ahead in detecting fission products in the recirculating cooling system.

Another bit of information I forgot to mention; the NRX reactor is one-pass cooled, so that anything that comes out goes in the river. A downstream detector scrams the reactor if the activity exceeds a certain level. That is one of our safety efforts. We use three radiation monitors and three ultrasonic electronic devices, which virtually eliminate false trips, whereas we had quite a lot before.

We had a similar experience with period trips. They were then put into a sequence circuit, and I think the trouble virtually disappeared.

The Brookhaven people have had some experience in two-out-of-three coincidence circuits, I believe, for quite a long time. Their approach, as I heard it, was the same, and the coincidence circuit does the job properly and eliminates the trouble.

R. L. MOORE: I have more difficulty in selling duplication than in cutting down the number of channels. Why three channels? I grant that three are better than two, and two are definitely better than one. Why aren't four better than three? Why did you pick three?

E. SIDDALL: Three is simply the smallest number with which you can separate what is right and what is wrong with instruments. If you have two, you have no means of knowing which is right and which is wrong, if one says safe and the other says not safe. With three the odd one is the wrong one. If two agree, we then accept the two that agree.

R. L. MOORE: If you use the system that you are advocating here, a primary check into the instrument system, such as the heater on the thermocouple, doesn't this tell you which instrument is right and which is wrong?

E. SIDDALL: That is a good point. As a matter of fact, two-out-of-two coincidences, where two instruments have to show the same answer before action is taken, have, I think, a real usefulness; and our safety people have perhaps emotionally jibbed at it. The chance of having a system disabled is actually twice as high as with a single instrument. If either is unsafe then the whole system is unsafe, and there is a gain in safety, but with two out of three there is a gain in both safety and reduction of false scrams. So I have subscribed to this, but when we finally begin to complete this business, I think there will be a field even for two-out-of-two coincidences. However, I don't think there is at the moment.

W. H. JORDAN: We are going to be in trouble with triplicates, not so much on the dollar side, but in finding places to put them in some of the reactor plants. This is going to be really tough.

W. P. DiPIETRO: I feel strongly that we can't sell more than three. Rather than going to four, five, or six, we had better go back to a single channel, make equipment as reliable as we can, and accept the scrams.

W. H. JORDAN: I thought Westinghouse had more than three on some of their earlier systems.

W. P. DiPIETRO: We had five channels on our earlier plans, but our current plans are for three. Our total instrumentation on nuclear is nine channels - three in the source, three in intermediate, and three in the power range. We feel that in the power range we are covered over a long term, and the duplication of channels is covered in the source and intermediate.

J. E. OWENS: The possibility of checking which instrument is wrong is, I think, probably a mistake. If you find that a flow meter reading is incorrect you check the meter. We go and look at our pump. If the pump is still running and the reactor is not over-heating, then we figure that the flow meter is no good. You almost always have a backup check in a case like this. If a thermocouple at one spot in your loop is not reading right, you probably have another one somewhere else to provide a cross check. Perhaps the trouble was not within the process instrument. Maybe it is at the preheat or some other place, but it is always there.

J. A. BARRETT: Two things are obviously at odds. I don't like to see us get into details as to the actual hardware that we are putting in because I think a philosophical question is more important. I think we have our own house to put in order before we can ask people to let us tell them what they should do.

We have heard experts in this field say they want simple control systems, yet they want to check them; they say they want simple control systems, but when they get specific it sounds complicated. I personally can't conceive of having a startup, a high level, and a shutdown channel with fewer than four separate chambers.

W. H. JORDAN: I think perhaps Mr. Siddall has made a lot of converts in this country.

E. SIDDALL: I have in fact tried to say to the reactor designers, "You design the most economical reactor you know how and pass it over to me and I will control it." I think that is the attitude that control engineers have to take. We have the cheap, plentiful methods (instruments) of doing these complicated jobs and they have not.

W. H. JORDAN: I think that the control people are going to have a great deal more to do. Actually, one of the greatest costs, as you know, Mr. Siddall, is the operating cost, and when we have calculated the operating cost perhaps we have even been a little on the optimistic side. Even with the present-day modern steam plants they are fairly high. If we can get more automation and cut the number of people from 100 or 120 or so to 40 or even fewer, it is certainly going to help a lot on power costs.

E. R. MANN: I feel that we should keep track of the designer. We should keep the pressure on the designer to do the best job he can to simplify the instrumentation and control problem. One of the things that could be very difficult is to find a place to put all this gear, because some of these reactors are very compact.

I don't like the idea of insisting that the designer must come up with a safe reactor. He has a lot of problems. But don't let him forget that it is a worthwhile objective, and, if you can, keep the pressure on him. However, it has to be a compromise.

W. H. JORDAN: The reactor-control man should be working right with the designer.

E. P. EPLER: At Oak Ridge the control man is getting the cooperation of the designer.

R. G. AFFEL: The use of coincidence assumes a random failure in the instruments. From what we saw yesterday of the state of the art, I don't feel that triplication is necessarily a safe approach.

It has been my experience in the liquid-metal area that our knowledge has progressed. Initially we had welding difficulties. The units would fail, and perhaps fail to danger in some fashion. We cured that trouble and perhaps extended the lifetime factor by three. Then, we again ran into some common symptom. Do you not assume that in order to apply your triplication we need a good background to insure this random factor? I would like to bring this out specifically in the process area, rather

than in the electronics, where we do have a reasonably good background. What is your feeling from seeing the art at present - I use the term advisedly - in the process area?

E. SIDDALL: There is a lot of information from existing instruments and existing circuits, which I think supports the general randomness that follows. If we knew where the fault was coming, which would be a real departure from randomness, we would do something about it. For instance, if an instrument is going to fail after 100 hr of service, that is no problem. We will just take it out after 90 hr or something like that.

The difficulty that arises with instruments having a normal life of 100 hr is that usually of three, one fails in 20 hr, one in 50, and the other in 500, or something like that. It is a large random problem superimposed on a systematic failure. If all I had to do was deal with unreliable instruments, I would put them in a line and test them. I have every confidence that there is a large randomness with all faults of which the coincidence would take full advantage.

R. G. AFFEL: I will speak about the slurry field - I am not in this end of the business - but plugging appears to me as though it could be a very common cause, again random in time only.

E. SIDDALL: If you put in two instruments and they both plug up, it is unsafe from then on. I would put three in, and when two of them plug up, we are out of action; it is unsafe from then on. The chance of two plugging out of three is not very different from two out of two. It is actually six times higher, but in this business a factor of six isn't very large. We usually deal with factors of two or three orders of magnitude.

I would put in three and then do routine testing with any frequency I want, because I am fearing trouble. If I am looking at these every 5 min, I am very much better off than you are. I can test these things for plugging just as often as I like. You have either to stop the operation or do no testing. If you stop operation you have lost serviceability; if you do no testing you have lost safety.

R. G. AFFEL: At this rather early stage of the art in some areas, am I not justified in selecting an auction system where I will accept one out of two for causing a scram?

E. SIDDALL: If you wish, but then you throw away the chance of testing the system. I think Mr. Jordan has made it quite clear. You can have one or the other; you cannot have both. From my analysis of most situations, the data is overwhelmingly in favor of the coincidence. Certainly you lose something with coincidence but you more than gain it on the testing.

(At this time E. P. Epler assumed the chair)

H. H. HENDON: No one seems to be troubled with the decoupled core problem in a large reactor in which a portion of the core can be supercritical, with very much higher temperature and higher flux than other core sections. This leads perhaps to a tilting of the flux or even an oscillation of the flux peak. In all of our discussions this has never come up.

I would like to explain the in-core monitoring system that we plan. Inside the core, in a matrix arrangement, interspersed between the fuel rods, are to be 64 ion chambers. The output of these will be monitored by the operator, and it will be the responsibility of the operator to control manually approximately 100 control rods to keep the flux from tilting in the reactor core. I think it is somewhat analogous to a boiler. If we have four burners and are monitoring temperature and pressure, what happens if two burners have gone out and the other two burners are melting holes in the vessel?

Of course, we have other problems that I won't get into, but again I am surprised that this has not come up because it is an economic factor with us to come out with a fuel load of uniform burnup. We are committed to a power cost (at the Dresden Plant), as you may know. I am curious to know whether anybody else is plagued with this trouble.

J. N. WILSON: It is a real problem if you have a large enough core. I think that you have to have an idea from the design of your reactor, by flux, temperature, or some other measurement, as to the three-dimensional flux inside your pot. Most of the reactors are small enough that you can do this without worrying, but a big one can very easily have one side going up and the other side going down, although the total power is constant. If you are close to the edge on fuel operating temperature then you are in real trouble. This necessitates temperature monitoring.

We have a triplicate coincidence circuit on temperature monitoring which works in a different way. If you have a multiplicity of fuel elements, which you fear will burn up or something, put temperature monitors on each one. You get into trouble if you try to put in too many thermocouples. There simply isn't room. We felt that we could not put more than one in at each point. I guess we could have, but we didn't. You gain much by having the readout at three levels of discrimination, what we call high, very high, and very very high. The very very high is the one we scram on. We demand that if a very very high signal is given, the corresponding very high and high must also be working. In the same way with the intermediate one, the very high, you have to have the corresponding high alarm going at the same time. This coincidence circuit has eliminated probably 95% of the false scrams from the temperature monitor.

We have a lot of very very high signals, which are not coincidence, due to the complexity of the monitoring system, the feedback from the switches, and what not. We take notice and correct the fault without causing any down time. So this is another way to run the coincidence circuit.

E. SIDDALL: I am very interested in that last comment. One thing that concerns me is really related to both the last points; that is, how high can you run the reactor? If you have spent the money on a certain reactor, what determines where you set the power for earning money with it? There is a possibility that the flux will be considerably different from what you think; therefore I believe we should measure the outlet temperature of every fuel element, and so run the reactor that the hottest element limits the reactor power. No matter where that falls, we will know we are doing the best we can with the reactor. We have not failed to use any of its capacity.

In reference to coincidence, these elements are partly self-checking. A large reactor simply cannot have steep gradients of flux; and if one element is genuinely reading high, the elements around it should be reading at least toward that point. We can take advantage of this without extra cost, if we discriminate in our circuit. You have taken three levels on the same one. We would only regard the highest temperature, if the element next to the highest were running fairly high also. If we can design circuits so that we can do this sort of thing, then we achieve our coincidence or multiplication without extra cost as far as the primary element

is concerned. If a thermocouple goes wrong and starts to show a very high temperature, and adjacent elements are not showing high temperatures, it is false; but if they are showing very high, it is genuine.

If one part of a large reactor is well above critical and another part very low, the reactor is not operating economically. It is wasting neutrons or something. This is, in our analysis, very expensive. In getting the reactor to run reasonably economically we almost automatically eliminate this chance of big flux absorption.

J. N. WILSON: That is a point I was going to make in a different way, but you have said it very well: we should never forget to check, especially, things like temperature and flow inside a large pot. Some things are physically impossible. We can take advantage of this in checking. We should have an intelligent operator who can look at the results and ask if there is an odd result, "Is this combination physically possible?" Especially with things like thermocouples, we can usually say, "This one is bad," and ignore it safely.

J. A. BARRETT: I hate to bring this up, but I remember a report in Nucleonics a couple of months ago about the Windscale accident. That is exactly what the operator did. He assumed that the first thermocouple was obviously bad.

J. N. WILSON: I said you have to have an intelligent operator.

J. A. BARRETT: I believe he was intelligent. I don't think you can say this (measurement) is good or bad.

J. N. WILSON: I again think that we have to have the tools to work with, and that they didn't have enough thermocouples in the right places. He was doing what he should do from the data he had, but he didn't have enough data. This is a trap we can always fall into and must guard against.

D. HOLZGRAF: I was involved in the operation of the MTR for several years, and my viewpoint is that of a research reactor operator. I am not involved in power reactors. In a research reactor like the MTR we had several unnecessary, plain, outright instrument scrams a year from the reactor and many more from the experimenters. This was mainly because the experimenters didn't take the care with the instrumentation that the reactor did, but you cannot really beat that too well in a research program. However, we had perhaps half a dozen unnecessary instrument scrams a year on the MTR, from only half of which we would recover from xenon. Each time we got caught by the xenon on the MTR, that loss meant thousands of dollars in

operating time. It took 12 hr to refuel to override the xenon, so if we saved only one scram a year on this reactor, we could pay for the whole instrument system, merely by saving the overhead for running the MTR, in dollars per minute; and this is lots of dollars per minute. This has nothing to do with the disturbance to the experimental program, and that was tremendous. A spectrometer would lose not only one or two days of data but also all the background data. The rods are going to be in a different place when the reactor gets started up again. All the loop operators shudder when there is a scram. Some of the experiments completely fail during the startup period, and we have to shut down and pull them out with a loss of several days.

On a research reactor it is really important not to shut down until it is desirable, and then to shut down gently. It is worth \$100,000 to the MTR to prevent getting caught by xenon two or three times a year. As to the ETR, I am glad that I left before they got the thing to run. I shudder to think about it. But even on a reactor that does not have a xenon problem, false shutdowns should be avoided.

C. W. RICKER: Our problem at the University of Michigan is several orders of magnitude less than that at the MTR, but we have the problem of period scrams from the period circuitry. One of our problems is faultily designed magnets. We are building our own now, which we think will probably be better from the continuity-of-operation point of view. We are not operating the reactor as an income reactor, but we have some valuable experiments in it, and we have already frustrated quite a few Ph.D. candidates and some faculty researchers just by spurious scrams. If you are exposed daily to complaints from the faculty on down, you soon start to appreciate the fact that something should be done about spurious scrams. When I first went to Michigan there was not very much support or sympathy for this opinion, but I think we can now get quite a sizable sum to improve our system to prevent them.

I am not thinking about safety figures. These things are pretty real. I have never been worried about safety in the reactor. I am not worried about the checking angle. That is the instrument man's problem again. It is just keeping the thing on the line. This is a really important part, if you have room to put enough chambers in there to do it. On the MTR we

had room. We had plenty of vertical graphite holes to put in almost anything desired.

A. PEARSON: We must have a better climate in Canada, because we don't have magnet trouble, trip shutdown, or period shutdown.

J. N. WILSON: I think that everything you say is even more important if you are trying to make power.

C. W. RICKER: This isn't just a power-reactor problem. We have a lot of research reactors that are ruining too much expensive data. Power reactors are in the future, I know.

J. N. WILSON: But I think the point is very well taken. You can pay for a lot of extra instrumentation.

D. HOLZGRAF: You don't have to wait until you get a power reactor to go into a good system to make it worthwhile. You spend an awful lot of money even on a pool reactor. If you can stop a few unnecessary scrams you save money.

J. N. WILSON: We are running a production reactor and the costs are terrific.

C. W. RICKER: Have you used a cation-anion exchange column in conjunction with measurement of the amount of fission iodine, as a method of determining that you have a fission break?

D. HOLZGRAF: We have tested it in the MTR system, but we are looking for smaller breaks than we can detect with that system.

J. N. WILSON: Of course, we are looking at fairly sizable breaks in a fantastic background from previous breaks; therefore this works very fine and we cut out an awful lot of background. The system is just as sensitive as your people say it is, but it is not nearly sensitive enough for our power.

D. HOLZGRAF: Then the only sensible thing, I think, is for you to design a reactor like the PWR.

J. N. WILSON: But again it depends on the fuel. Enriched fuel, yes; natural uranium, I think not, unless it is oxide.

R. G. AFFEL: Mr. Holzgraf, you mentioned three scrams of the MTR in a year from instruments as a reason for justification for added equipment. What number of scrams were caused by the operating people or the experimenters? Doesn't this three really become rather insignificant compared with the total number?

D. HOLZGRAF: I meant true trouble signals from experiments, and that is about what it amounts to. I have worked with all of you in your laboratories, and so I have seen a lot of different jobs going on. We have five times that many, at least, from the experiments that get out of line. I mean where the flow actually does drop or "crud" builds up. I have heard, several times, that if the pump is still running at a certain speed, the flow must be coming through. That is not true, because the loop fills up with "crud." This is a most common problem on a high-pressure loop. You cannot rely on pump speed as an indicator of what is going on in the loop. You must have a flow meter, a delta-T measurement, a pressure drop across your flow element, or some such thing.

R. G. AFFEL: It seems to me that this is where E. R. Mann and his analyst really need to come in. Weren't there quite a few more scrams due to cockpit troubles?

D. HOLZGRAF: Yes, but look at it this way: Maybe the genuine scrams are only 10% of the problems, but here (coincidence) is a way of solving the problem economically. I can show a profit. I don't know how to make the experimenters design better experiments. We tried, and it was difficult to do a job at the MTR because of the red tape and the numerous committees that you had to go through. But not much could be done about that. The experiment was not valuable enough to spend the time that was spent on the MTR control system. It ought to work! You built a pilot plant, the only pilot plant I have ever seen of a reactor.

Also, the designers of the MTR, and especially the ETR, have probably gone overboard in trying to make an all-purpose test reactor, containing something for everybody. It just won't work. Nobody is happy with it. ANP has the biggest experiment in the ETR, so you have to run the reactor around that experiment.

E. SIDDALL: There is one important group of triplicates that we are putting in. In the NRU reactor we are worried about obstruction of the cooling-water flow by a fuel failure. In the very large majority of cases the fuel element slowly distorts in some way and eventually the flow drops off. Either the element swells and reduces the channel, or it lengthens and begins to obstruct the outlet.

We have two instruments, one of which is supposed to be set ahead of the other; but they are blind instruments, and in a fair proportion of the

cases it takes a very careful check to see where the trouble is. Either instrument can cause a scram. We propose to put in triple instruments. The total cost of the new instruments is less than that of the old ones but, of course, we have to pay for the old ones as well; so there is no gain this way. Here we will probably use one as a lead instrument but this is not essential. We can see from past behavior that the big majority of cases will show up on one instrument first as an alarm. Then we can do what we presently do after a scram: look at the flow. With triplicated instruments we can do so without a scram.

This may seem rather naive, and many of us can see a way to avoid such a situation without triplication, but when we have a safety requirement plus a strong conservatism which, after all, is one of our basic defensive measures, I think the triplication is justified in any case for this purpose alone, and it is really guarding against finger trouble or cockpit trouble in setting these instruments and keeping them set at the right place.

D. HOLZGRAF: One of the big problems with experiments is, of course, that the experimenter has a tendency to over-instrument to protect his own experiment. This was brought up in "Safety and Control of In-Pile Experiments" (this conference). If you let every one of them have free rein you cannot possibly run more than one experiment at a time in the reactor. If you have 10 loops in the reactor, and if every experimenter has every setback or scram that he thinks will help to protect his loop from being stressed or strained, you have not a chance of running the reactor. One of the main things is to cut to the basic essentials; the least drastic action that will keep him out of trouble is all that should be permitted. You sometimes have to recalculate his problem and show him that he is too drastic, that you cannot tolerate it, and that you have to take a certain amount of chance or you will never finish the experimental program.

You can do that on the MTR. You may not be able to do it on the power reactor.

J. A. BARRETT: I want to get back to the question I posed. It sounds like everybody is in favor of complication. I wish Mr. Schultz were here. I think that when somebody mentions 64 ion chambers he is obviously in favor of complication. The question evidently seems to be how much complication, and we even have the experimental-reactor people now telling us

that we should put in triplicate circuits. We think we should have them in the power reactors. I think we should complicate only where necessary.

F. A. SMITH: I would like to take a compromise position in this business. I represent the fast reactor group, and I feel a little out of place in this rather unique organization. I think the difficulty is that we have too many experimental reactors. On our EBR-2 reactor we will duplicate instruments and be safe.

In my opinion it is the economics of the fuel cycle that should be emphasized, and if the chemical engineer does not do his job, I think we all will ultimately be out of business, physicists, reactor engineers, and instrument men in this field.

E. P. EPLER: I will recognize first those who want to sort out these questions of complication vs simplicity.

E. R. MANN: My attitude is this: Certainly the designer creates some designs which have to use a lot of complex instrumentation. You cannot expect the designer to solve all of his problems in 24 hr. You have to give him time. But if you can gain simplicity of a design, if you can borrow something that is simple from another design and make it applicable to your design, by all means you should do it. There are some problems left that he still has to solve, and the instrument people are going to have to live with them until he can get around to it. I don't like the idea of saying that this problem comes out either black or white. It just does not happen that way. But keep the pressure on the designer as the materials are improved, so that some day we can all go out of business. That is a nice objective. I don't think we will get around to it, but I would like to. Don't make it complex just for the sake of complexity.

F. A. SMITH: I would like to emphasize this again. In my opinion the reactor comes first. There are a lot of reactors; they have a lot of ultimate objectives. I think most of those today in the power reactor field are truly experimental by the very nature of our advertized power programs. In this sense, since these reactors have already passed the safeguards committees, we have to assume that the people responsible for the reactor design will get the cooperation of people represented in this room.

W. P. DiPIETRO: We are not getting more complicated even though we are adopting such simple techniques as the one Mr. Siddall presented as

coincidence. I think the controls are far simpler than the earlier ones. So while we may do things differently, they are certainly not more complex. We go through periods of simplification every once in a while.

I think that we in the control field at Westinghouse get in early on the planning, because we do it by a periodic review. I don't think we can all get in on the first step, but I am sure that a design started today does not follow along one track until it is completed. Periodically the plan is reviewed and altered. We get our cracks at it; we are not left out.

W. C. LIPINSKI: I should like to cite a specific example of simplification. We have been talking about trying to build self-control into a reactor. A boiling reactor is such a device. The power production is essentially proportional to the control-rod position above critical. There is a design for the ALPR reactor whereby there are simply two D'Arsonval meter movements with a contact which will be made at high flux for a flux-level trip. The ultimate in starting this reactor will be to pull control rods at a low, uniform rate to establish a reasonable period and have the reactor self-controlled at power, and simply pull the control rods further to obtain the desired power level for operation.

E. SIDBALL: You do not know what you are paying for that self-regulation. When you distort the reactor design - and I use that word intentionally - to make it self-regulating you may be playing with the most expensive part. We have been through this in Canada to some extent. We have operated our moderator at about room temperature because we get more reactivity this way. If we ran it with higher coolant temperature we would lose reactivity, and this is one of the most expensive things you can lose in the reactor.

All I am asking, in comparing the complexity of a scheme such as Mr. Schultz drew and the schemes with externally applied control systems, is that you reduce them to a common basis. Show me the self-regulating reactor for 6 mills/kwhr for comparison with a reactor with a simple imposed control system for 6 mills/kwhr; then we can talk business. But as long as people believe that self-regulation can be achieved without difficulty or complexity or altering the design of the thing, obviously we are not talking on the same basis.

E. P. EPLER: I think it is clear that if we are going to get economical production of power, we are going to get it at any cost to us. It would be paradise, I am sure, if they would design reactors the way we would like them to be, and maybe we can get away with it for a while and to some extent, but I guess in the long run we are going to have to design controls for whatever reactors will make the cheapest power. Let's face it.

H. H. HENDON: Furthermore, the system will be judged not by whether or not the control system responded to a given signal and turned the reactor down, but by the number of false scrams, any one of which could have bought the instrumentation system 10 times over if it had not happened.

J. E. OWENS: I would like to spend a little money getting rid of scrams. I have been fooling around with these reactors for some years now, and I have never seen an effective scram that was really required. We have had three situations requiring a scram. In one of these the reactor was scrammed, but it was too late to do any good; so it was sort of pointless. In another case the reactor tried like the dickens to scram and it could not; so again the circuits might as well not have been there.

In general, you get a reactor into trouble by pulling shim rods and, also in general, you can get the reactor out of trouble by pushing these same shim rods right back in. In almost every case it can be done with your normal shim speed. The foolishness of holding rods on magnets that drop the rods into the reactor seems to me somewhat outmoded. I would much rather spend our money on control-circuit design and complication, if necessary, than on safety circuits.

C. W. RICKER: You mean that you don't like the idea of throwing water into the gas-fired boiler every time the pressure gets a little high. You would just rather turn the fire back a little ways and let it float back.

E. SIDDALL: I strongly agree with that point. I don't think that we are going to be allowed to take scrams off for quite a while, but in the NRX reactor I think we had six cases where all the scrams were essential in one year. We had a rather crude and simple automatic power-regulation system. It was all electronics, a simple ion chamber, etc., and about six times in one year this thing took off - just pulled the rod out full speed. It baffled the brains of the company from the division heads down. We

fully realize that the right answer was not to scram after the control system had run away but to get at the control system. The principle of coincidence and redundancy achieved exactly the same results.

We multiply the control channels. We discriminate, in effect, between the channels, distinguishing genuine from faulty channels by agreement. We have other schemes in mind, so that the faulty signal is identified at its source and ignored, instead of being allowed to get to the stage of pulling rods. These are the techniques that I have been talking about.

J. E. OWENS: We do have a tendency to get lost behind our safety system. One fellow says, "Operate to make this better," and the other fellow says, "Don't worry about it, the scram system will save you." I think it is ridiculous.

C. W. RICKER: I cannot remember a case in five years that the scram system was necessary on the MTR reactor.

E. P. EPLER: Does anybody here have an experience to cite where a scram has been successfully operated in real trouble, due to good usage?

J. MacPHEE: I am not sure whether this was or not; so I will tell you about it and you can decide for yourselves. A technician was at the console of a pool-type reactor, and the circuits were calling for rod withdrawal. The rod was coming out. He became suspicious and walked over to the bridge. He noticed that the regulating rod was coming out, but around it was a fuel element, so he pressed the scram button. I would say that in this case the scram did help.

E. P. EPLER: Might you not also say that there was faulty design of the core that needed attention?

D. HOLZGRAF: You cannot now get a building permit from the AEC for a coolant reactor that won't take care of that problem with your rods by a hold-down mechanism. You have to have a way of mechanically preventing those controls being pulled by the shim rods or you cannot get a license.

E. P. EPLER: That is right, but there might be another one.

R. L. MOORE: One of the difficulties of saying whether you ever had a scram that was not necessary is that if it does scram you don't really know whether it was necessary or not. We had one case with the HRE in which if the thing had been left alone we would not have gotten into the difficulty we did. We had circuitry that would have dumped the reactor. We had several indications that we were in trouble. People were just tired

of dumps and held the pen up on the instrument. As a result, we got into a mess.

J. N. WILSON: I think there was a case at Savannah River that could happen to anyone. The hypothetical question was asked: If overalls got into the system and stopped the coolant from going through the fuel element would you be in trouble? Obviously the answer was: Don't let overalls get in, but nonetheless the safety is there and the flow monitor would catch it.

We had occasion to change a pump. We have a lot of rather big flows; so we are rather fussy about air and such getting in the piping. When a pump is changed, balloons are inflated in the lines on each side to seal the system when the line is open. They are counted like surgical sponges, but every now and then one is left in. It happened. We didn't know the balloon was there. It was shredded and a lot of water was required to push it through the heat exchanger; the holes are not too big. When it came into the reactor, we had a scram and we were happy to have it.

J. E. OWENS: Didn't the rods get into trouble then?

J. N. WILSON: Running?

J. E. OWENS: Shim trouble.

J. N. WILSON: We took the power load off the element which otherwise would have melted them down.

J. E. OWENS: Yes, but do you have to drop the rods? Can't you drive them in with your motors? All you are gaining here is the difference between 10 msec, if you have an extremely fast safety circuit, and perhaps 2 sec, if you have a reasonably fast shim circuit.

J. N. WILSON: It depends on when the balloon comes through. If you are very close to meltdown, it is the economical place. It is really dependent on how sure you are whether you have balloons in the system. We had one; so we are not so sure any more.

J. E. OWENS: But what is this worth to you?

J. N. WILSON: If we had a meltdown it would be worth a tremendous amount.

J. E. OWENS: Well, you are not going to melt the whole core in this case. You are going to melt two, three, or four fuel elements which cost you some down time.

J. N. WILSON: Some down time, yes.

J. E. OWENS: But every time you have a scram...

J. N. WILSON: I think, getting back to the first question, the extent of instrumentation is a matter of economics. At Savannah River the figure we like to use is fifteen hundred million dollars. That is what it costs us to build a plant. One minute down time, merely in interest on bonds, is \$300. You can buy an awful lot of instruments with the cost of a shutdown.

I think there is another place where we have a specific problem. A very simple case is the separation plant. We have a Purex system and mixer settlers. These are big, complicated gadgets for mixing organic liquids. If there is a lot of plutonium they may go critical, so we decide that we cannot put in more than a certain amount. Then someone says, "This is the bottleneck. If we could double the amount of plutonium, look at all the millions of dollars we could save by more throughput in a hundred-million-dollar plant."

We tell him that the instrument is complicated, but he says that a new mixer settler costs a million dollars. So we build him a neutron monitor, probably the fanciest thing you ever thought of, but it costs a lot less than a million dollars, and he can operate.

I think the question of instruments is one of economics.

R. L. MOORE: I think that we are all for simplification, but we get forced into making things more complicated. Usually we start with a fairly simple control circuit, but before we get through with it there have been a lot of questions we cannot answer and we have had to provide protection against these. But in this matter of duplication of instruments and adding complications, there are two things. One of them is space requirements, which spread out a control room. The other is related to this, and that is the added complexity of operation. If you add instruments indiscriminately, throw them all on the control board, and give the operator more responsibility, you can reach the point where, I believe, you are decreasing your reliability, since you confuse the operator so much he is more likely to make mistakes.

C. A. MOSSMAN: In chemical plants or process plants, where people are really watching dollars and cents, you never see this duplication and triplication of circuits.

If industry were involved I doubt that we could really justify the dollars and cents we are kicking around. For instance, assume that the MTR was down for 24 hr and we fixed it. If we had maintenance procedures like those in a chemical plant, there would be a certain number of engineers on the payroll anyway. Every time we start to figure dollars and cents, we figure the time of these people, but it really is not chargeable. I don't think that as many dollars could be saved as we are talking about.

C. W. RICKER: But you are not making a product when you are down, and you can charge that, can't you?

J. N. WILSON: I can give you a specific case of millions of dollars. I claim to know more about rayon than anybody else in the reactor business. In the spinning room rayon makes for such a very small profit margin that Du Pont is going out of the business to make nylon which is more satisfactory. A power shutdown which shuts the spinning room down for 10 or 15 sec - 1 sec is sufficient - will shut down all of the individual threads in a given spinning room. There may be 10,000 such threads. If you stop them all at one time, you lose about 3 days of operation because there is not a big enough spinning crew to start them all up. You have to do it sequentially.

In Richmond, Virginia, in the Brown plant, we have our own power plant. We didn't trust the Virginia Electric Power Company. After about 30 years some of the turbines were getting to where they needed maintenance more frequently and every now and then we would have a shutdown; an emergency shutdown which would shut down the spinning rooms. The V.E.P. Company's main transmission line now runs in front of the plant. We paid something like five million dollars to put it in. This automatically switches in now, which is critical enough. In 1 sec you can cut from the power plant to either one of the two power stations, in time to save the spinning room.

C. A. MOSSMAN: There are problems and I think it is obvious what they cost us.

J. E. OWENS: They are getting awfully close. We are building a reactor for the Consumers Public Power Company in Nebraska. It is going to be something like half of their system capacity, and the penalty they pay for purchased power on an unscheduled outage is just out of this world.

C. A. MOSSMAN: On the other hand, one of the TVA engineers took me through the Kingston steam plant. The No. 5 boiler was down because of a ruptured tube. I asked him if that was something unusual and he said, "Heck no, we have a boiler down once a week. We expect it. As long as we get it back on the line in 4, 6, or 8 hr we don't mind."

J. R. MAHONEY: The instrument engineers should be in on the design at the beginning. It is a question of the instrument engineers growing up and being recognized. It is as true in the process industry as it is in the reactor industry - perhaps even more so. There is nothing that we can do about it except to try to convince these people that we can give them a service and save them money by getting in at the beginning.

Once you do get in at the beginning, however, you don't just say, "Okay, if you will just make this thing differently I won't have to control it."

He will say, "Well, it is going to cost me so much in my process to make it self-controlling."

We have to balance these things against each other, and again it is a question of economics. He cannot afford to have a big tank and a high inventory in order to keep his time constant down to where you can get by with simple controls. If you can control the system with complicated instrumentation for less money over a ten-year period than he can by putting in a big tank, then it will be that way.

The thing which distinguishes the process industry from the reactor is the consequences of a failure. You are just orders of magnitude away from a serious failure in a process industry. In most plants, when you go down you are in trouble. When a reactor blows up it is catastrophic. I don't know whether this is an economic question or not. The only way you can make it economic is to get the insurance companies to set a premium for each type of reactor based on its probability of a catastrophic failure and then factor this into your capital cost. Then you will have the answer to whether to build reactors that are self-regulating or reactors which are non-self-regulating. But there is complicated instrumentation and attendant operating costs.

G. F. PARKER: I think that accidents in nuclear plants are not comparable to accidents in other industries. I think if we had an accident in a nuclear plant it would pose a problem other than economics. This is one

place where safety must be emphasized to the degree it is necessary. It is not a question of over-design. There is associated with it something that is quite different from other industries. If there is an automobile accident and five people are killed, you may not hear about it. Let there be an airplane accident and there are headlines at the top of the page. It is quite a different thing in that regard.

In regard to this whole subject of complexity and to Mr. Schultz' diagram, the conventional thermal plant today is really a very complex, controlled system. It isn't simple. Consider the gas plant there. You get a boiler today with a fuel in suspension. It has minimum rating control. Cut out forced draft and you cut the boiler down. It is just like scrambling a reactor. It is a complex system of controlled automation. These things must be interlocked and working. The engineer knows he cannot run it, and therefore that is why the instruments and control man should be in on the design of the system, whatever it is. I know that this varies in companies and businesses. In some, the instruments and control man is on the job and they don't put the instrument and control specifications on the tail end of the job after everything else has been designed and put in. This happens quite satisfactorily in many companies where the policies permit that.

As for instrumentation, control, and safety, it seems that in many instances the emphasis is put on the negative aspect of "Let's not put all the safety features in," rather than on the positive aspect of "Let's design more safety in the system and the system operation and in individual components and integrate them." The reaction in this whole chain from the generator back to the reactor is important in instrumentation and control. The reactor design business apparently is in a better state today than the accessory system of instrumentation is to do the job, because I think there has been some over-extrapolation in that all the problems are solved in the accessory system.

That isn't quite true. Instrumentation in a lot of accessory systems today is kind of like watchmaker's instruments that have been used in a conventional way. If you visit some of the really big commercial thermal power plants of today, you will see some really complex systems. Feedwater control was designed because it was required on the modern high-pressure high-capacity boilers.

We have talked about the economics of the MTR and shutdowns. These nuclear power plants are going to run on high load factors. Industry wants to run them on high load factors. They have tremendous investments. If you go into a power station you will learn that they have priority on what boiler they will shut down and which will run on the most economic rating. The nuclear plant is the one that will have the top priority, because they have a tremendous investment in it.

Therefore, they have instruments and controls, and the power companies are interested in safety, for which they have their own standards. I saw a turbine one day that was being shut down and during that time someone had connected the synchroscope. I saw a boiler blown up because the fuel in suspension fired in the combustion chamber and the explosion came through the wall; three men were killed. This happened in New York and there was hardly any mention of it. Let a nuclear plant do that and you will hear about it.

Don't think that we are starting a new business; that something else is not equally as complex somewhere else. We are just getting into the complexity that already exists in other fields. We are trying to substitute nuclear power for conventional power.

PART IV
DATA HANDLING SYSTEMS

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EXPERIENCE WITH LARGE-SCALE DATA HANDLING SYSTEMS

M. R. Mulkey

Arnold Engineering Development Center

M. R. MULKEY: The Arnold Engineering Development Center (AEDC) is a USAF installation operated by a contractor, ARO, Incorporated. AEDC consists of a group of wind tunnels and engine test facilities. The velocities of some of these are among the highest in the country.

One of the AEDC's most important functions is the recording and handling of data from tests performed here. After initial manual computation, data handling and computation are done on an automatic digital computer. The finished test report is the product that ARO furnishes to USAF contractors. This report contains data showing the performance of the model under test.

The AEDC facilities are available to all contractors who have Air Force, Navy, or certain other government contracts.

There are three data-handling systems at the AEDC; one in each of the three test facilities, the Propulsion Wind Tunnel, the Engine Test Facility, and the Gas Dynamics Facility. Each system has an ERA 1102 computer.

Figure 15.1 is a graphic diagram of the data-handling system. Design was started in 1951, and the system is capable of completely automatic on-line operation. The transducers are usually thermocouples, pressure transducers, and strain gage bridges, which are used to measure forces. The voltages from these transducers are fed into null-balance servopotentiometers. Shaft-rotational digital converters attached to the slidewires of the potentiometers give digital information, which is scanned at a rate of 20 channels per second into an ERA 1102 computer. The ERA 1102, a forerunner of the Remington Rand 1103-A, was especially designed for use in an on-line system. The data scanned into the computer are simultaneously recorded on punched raw-data paper tape, available for use if trouble on the computer should prevent entry directly into it.

The on-line system was felt necessary so that the test engineer could see his computed data immediately. This would allow him to make immediate decisions on the progress of his test, make changes in his test schedule, and conserve test time.

Figure 15.2 shows the Gas Dynamics Facility (GDF), a group of wind tunnels: two supersonic tunnels that cover a Mach range of 1.5 to 5; two

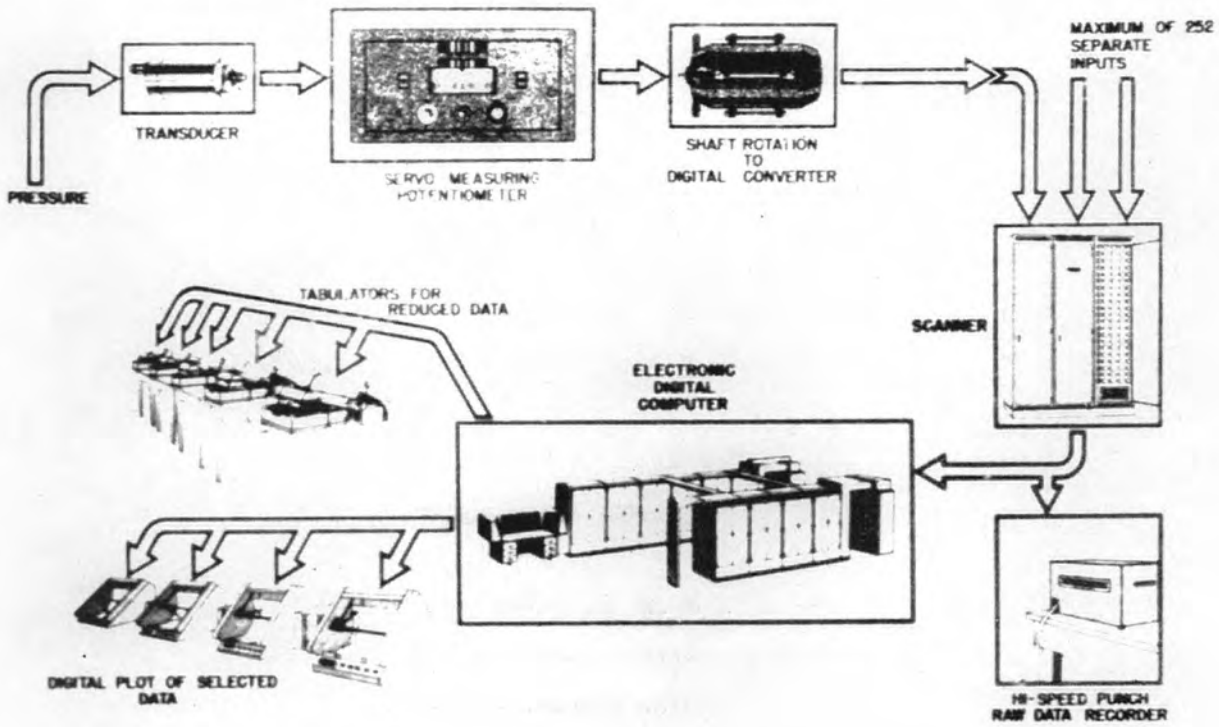


Fig. 15.1. Diagram of Automatic Data-Reduction System.

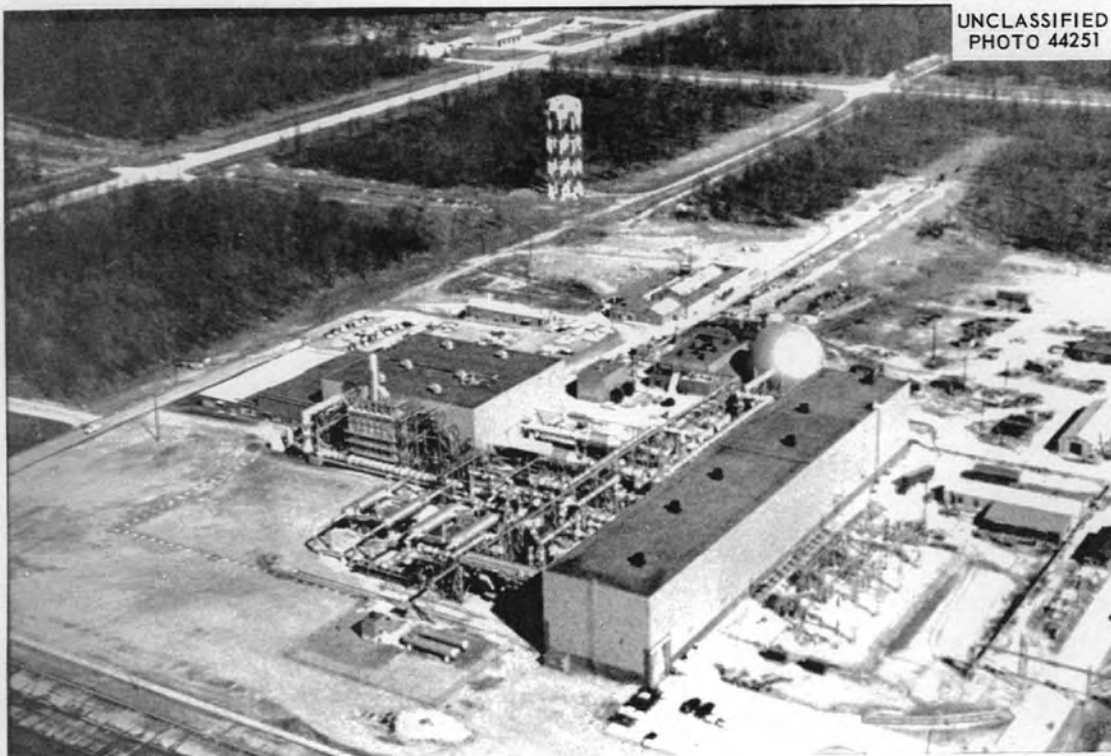


Fig. 15.2. Gas Dynamics Facility at AEDC.

hypersonic, Mach range 5 to 10; and one hypervelocity tunnel, Mach range 10 to 20.

Figure 15.3 is a block diagram of the Gas Dynamics Facility data-handling system. There are four wind tunnels connected to the computer. All the input and output lines from the computer must be switched from tunnel to tunnel. This is accomplished with the use of crossbar switches, by a control system designed to allow selection from the tunnel test area.

The scanner and recording instruments are in the tunnel area, as are the output plotters and tabulators. These are 200 to 600 ft from the computer room.

A paper-tape reader is available for feeding programs and off-line raw-data tapes into the computer. The output units of the computer are tabulators, plotters, and paper tape punches. These can be located in the tunnel test area or the computer room. Any combination of eight plotters and/or tabulators, plus a paper tape punch, can be used.

Leeds & Northrup model D servopotentiometers (Fig. 15.4) are used to record analog signals. To conserve space we decided on the small indicators and equipped them with shaft-rotational digitizers for digital read-out.

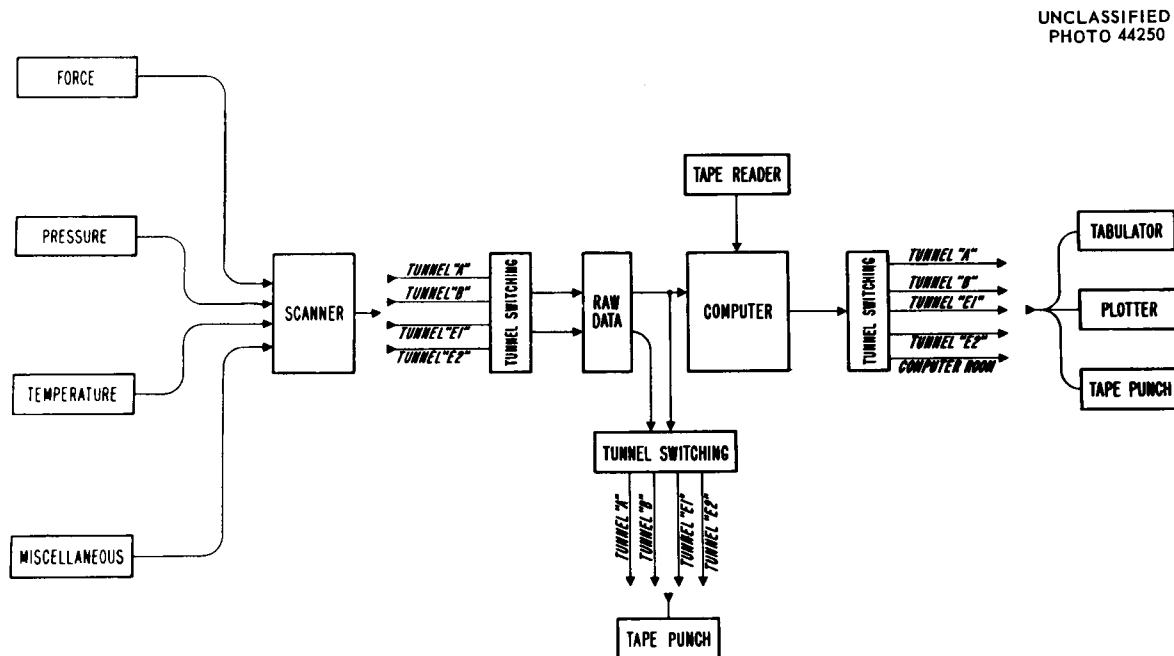


Fig. 15.3. Data-Reduction System.

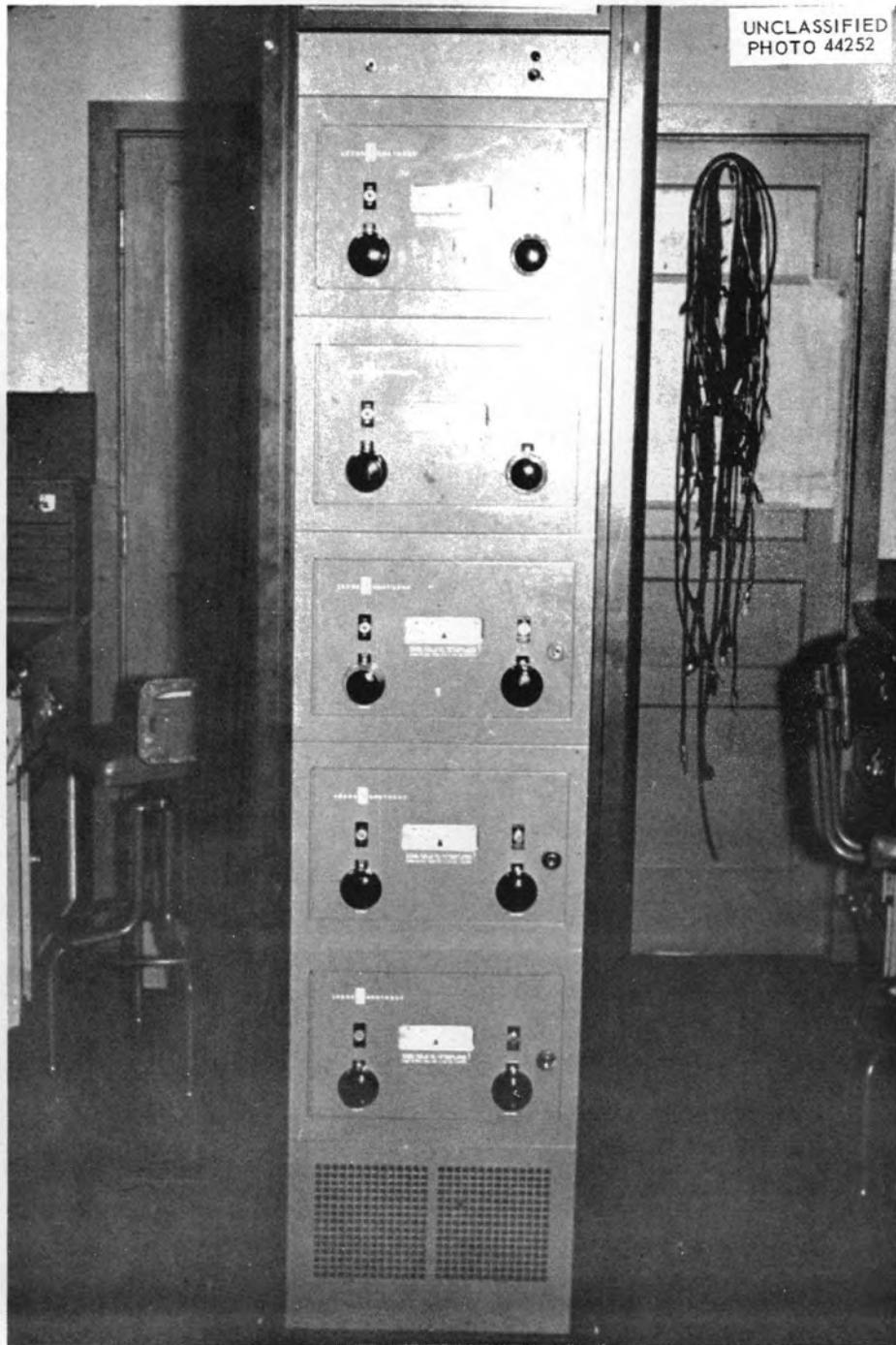


Fig. 15.4. Potentiometer Indicator.

This system is so designed that any one instrument may be used in any of the four tunnels by disconnecting only three Cannon connectors from the instrument. We have three standard connectors, one each for the digital output, the analog signal, and a-c power. This was considered necessary because we could not afford a complete set of very expensive measuring instruments for each tunnel.

Figure 15.5 shows the interior of a Leeds & Northrup model D indicator and the special gearing arrangement that attaches the shaft-rotational digitizer to the slidewire of the indicator. The digitizer is set to a full-scale reading of either 512 or 1024 counts.

The Consolidated Electromanometer (Fig. 15.6) is a high-precision instrument used to measure pressures. It has an accuracy of 0.1% of full scale pressure. It is available in ranges from 1- to 300-psi differential.

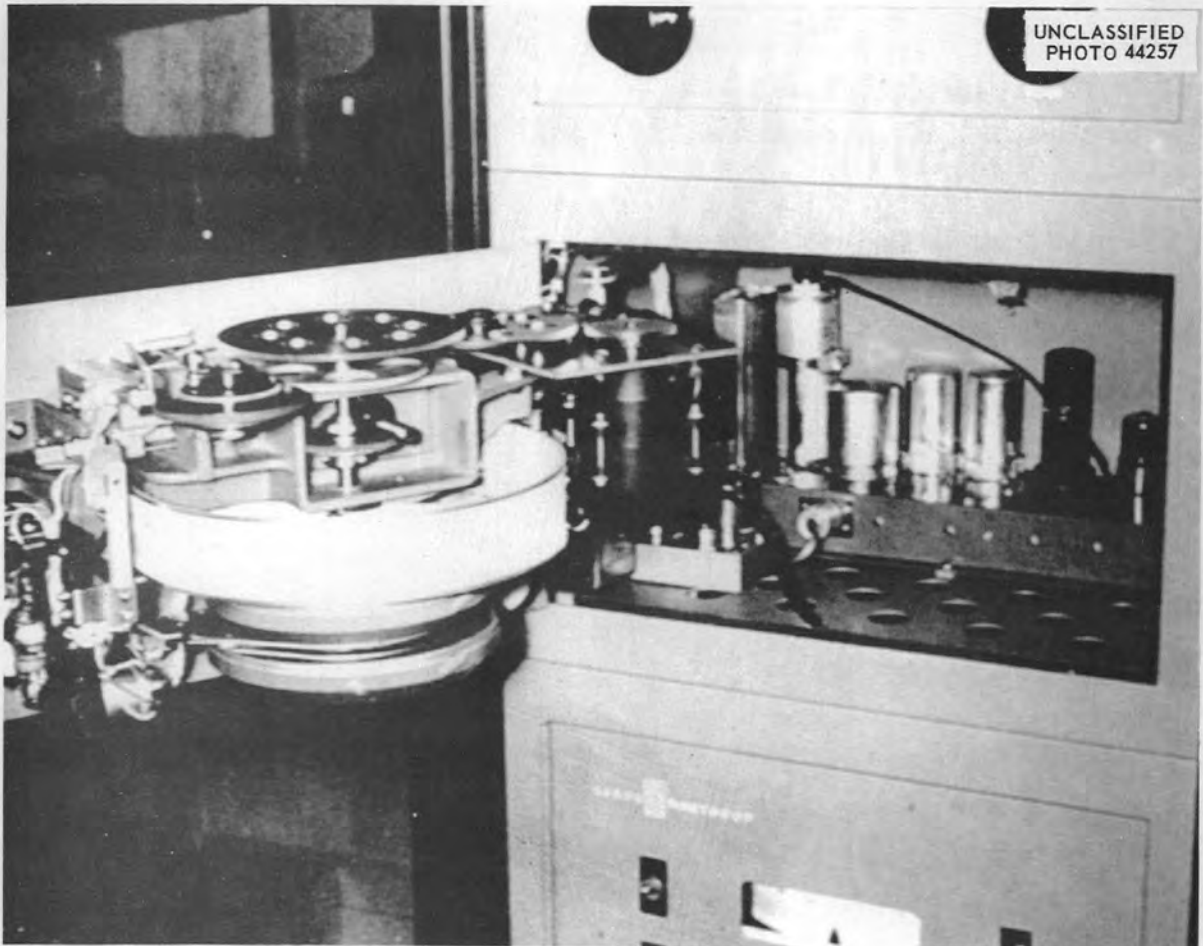


Fig. 15.5. Interior of Potentiometer-Indicator Digitizer.

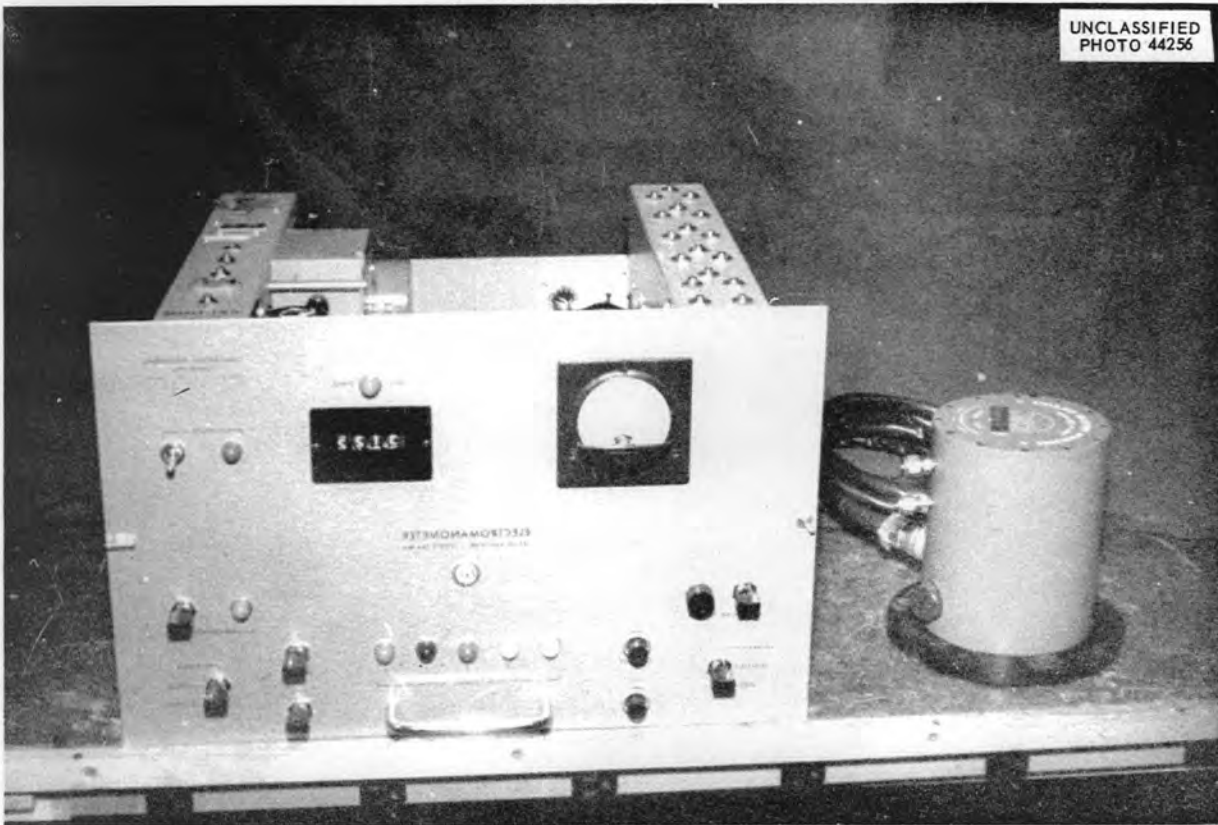


Fig. 15.6. Electromanometer.

It consists of a transducer and an amplifier-recorder. Its big disadvantages are its size and the large internal volume (0.3 in.^3) of the transducer. The amplifier-recorder is equipped with a Coleman digitizer for digital readout and a Veeder-Root counter for visual indication of reading.

Figure 15.7 is a photograph of the operating console of the ERA 1102 computer. It is a single-address machine that came equipped with one storage drum with a capacity of 8192 24-bit words.

The Gas Dynamics Facility has been on-line with this system for approximately three years. The Propulsion Wind Tunnel has been on-line for approximately one year. The Engine Test Facility has been semi-on-line for approximately three years.

In the Gas Dynamics Facility there are two continuous tunnels, one supersonic and one hypersonic, that cannot operate on the same shift; therefore, the computer operates three shifts. Two shifts are used for on-line data reduction and the third shift for program checking and scheduled maintenance.

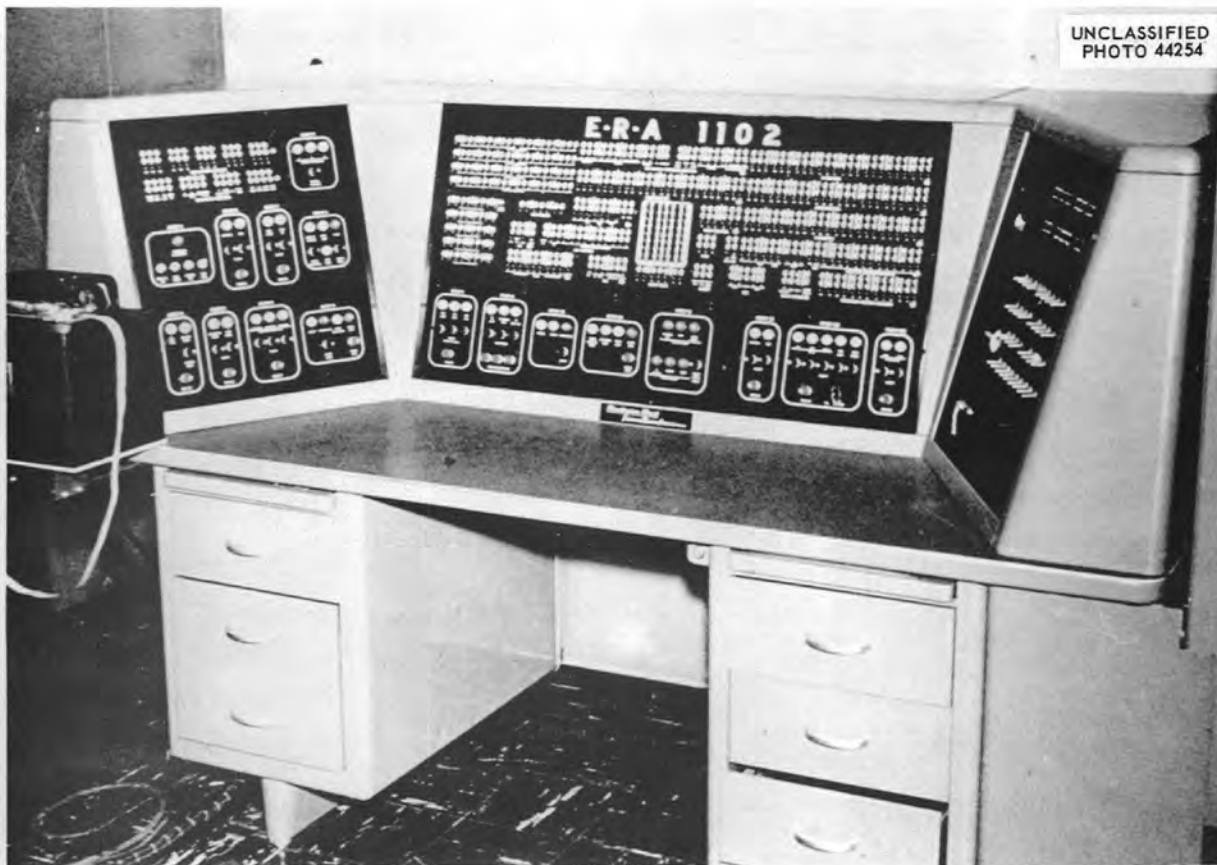


Fig. 15.7. Operating Console of Computer.

One of the difficulties with the data system is the lack of speed and storage. Up to eight tabulators can be used for output. Normally the test engineer likes to see his data tabulated on one sheet. This would require the use of a high-speed line printer.

One change made to speed data output is the use of a paper-tape punch, on the output, that can operate at about ten words per second. This tape is fed into a tape reader on a Flexowriter and tabulations can be made without the use of the computer. The data can be taken out and the machine rapidly cleared for future computations to be made. Tabulations are not normally required immediately by the testing engineer because the data cannot be evaluated that rapidly. The immediate objective is to observe an X-Y plot of two or more functions. These plots are available immediately because the plotters are capable of plotting two points per second.

An investigation is being made to determine whether it is more feasible to modify our present computer for high speed input and output or to procure a new machine. Some progress has been made in minor modifications,

such as installation of a Teletype paper-tape punch on the output of our present computer. Adding a high-speed tabulator would definitely improve the output speed.

It is difficult to speed the output of the 1102 computer without the addition of some magnetic core storage. Core storage would speed the access time to a much greater degree than is possible with the present drum storage, but would be a costly modification to the present computer. Cost of complete modification becomes quite comparable to that of a new computer, and the decision must be given considerable thought.

In the interim the GDF has installed three additional drums in the 1102 computer. This gives the advantage of having one drum for each of the four tunnels supplying data to the machine. This makes it possible to store programs on a drum that is reserved for a specific tunnel and makes it unnecessary to erase a drum when switching from one tunnel to another.

Limitations also exist in data handling and acquisition. The limitation on sampling speeds of the equipment described is 20 samples per second. It has now become necessary to sample at rates of up to 1000 samples per second. In order to obtain heat transfer rates of missiles re-entering the earth's atmosphere, it will be necessary to sample data at much higher speeds than is being done at the present. In some cases, as many as 300 thermocouples on a model must each be sampled at least two or three times per second.

Some steps have been taken to obtain equipment that will sample and record data at high speeds. There are two Consolidated Millisadics at the AEDC whose present sampling speed is 400 samples per second. These machines are now filling this gap but higher-speed digital systems will be necessary in a short time. Systems that will meet these requirements for the next two or three years are being sought, and it is hoped that one can be obtained for approximately \$250,000.

An instrumentation upheaval is taking place. In the past, strip-chart recording servopotentiometers have satisfied most requirements. Many will gradually be replaced. It is quite possible that you will be seriously considering this same problem shortly. The greatest expense of a high-speed digital system is the initial cost. Availability of

money for such equipment is always a serious matter. The cheapest high-speed digital system that will meet the present requirements costs about \$250,000. A high-speed digital converter that can sample up to 10,000 samples per second costs only \$65,000. This unit includes the converter and the magnetic-tape unit. It will accept analog signals of 1 to 200 v. Most analog signals will be approximately 5 to 20 mv. In order to increase these to voltage levels high enough to be accepted by the converter, a separate input amplifier is necessary for each channel. If it were possible to commutate low-level signals at high enough speeds, then individual amplifiers would not be required.

A 300-channel system would require 200 amplifiers at approximately \$700 each, using "leap-frog" commutation. If a voltage converter could be found that would take low-level signals, the expense would be drastically reduced.

J. N. WILSON: The Magnavox switch looked very helpful: 5 μ v noise, 2400 samples per second.

M. R. MULKEY: There are other considerations. When commutation speeds of 10,000 samples per second are used, the amplifier must be able to take a step function and have good frequency response. Normally the interest is only in static data. Two cps is considered static data and it is necessary to be down 3 db at this frequency. At 60 cps it is desirable to be down 60 db; so the amplifier serves as a filter as well as an amplifier. It may be possible to filter before the amplifier but a good method has not been found. The experts in this field agree that with a common-mode amplifier, which should be in this system, it will take at least 200 amplifiers for a 300-channel system using leapfrog commutation.

J. E. OWENS: What mechanism are you using for scanning with your present units?

M. R. MULKEY: Simple relay closure for 20 channels per second.

W. C. LIPINSKI: There is an apparent inconsistency between 10,000 samples per second and a 2 cps response requirement. That is 5000 samples per cycle.

M. R. MULKEY: That is right. Zero cycles for static work would be most desirable. Here 10,000 samples per second means that if there were

10,000 channels it would be possible to sample each channel once each second.

W. C. LIPINSKI: Not 10,000 per individual transducer?

M. R. MULKEY: No.

H. H. HENDON: It strikes me that if you simply had more of the relay scanners you would have a two-cycle filter built in by virtue of the scanning.

M. R. MULKEY: In our present system there is a simple filter, which is the servo on our recorder. It is a good electro-mechanical filter.

H. H. HENDON: I am trying to amplify Mr. Lipinski's point that it does seem unnecessary to have such a high-speed scanner for static data. More relays would still result in the 10,000 points per second and probably at a lower cost.

M. R. MULKEY: The requirement would be satisfied with 1000 samples per second. As you know, most data is not dc and is not 2 cps. There are many frequency components. The test engineer is normally not interested in this random noise. This may be correct data but not usable in the data reduction; an average value is the required result.

It appears that this needs some elaboration. A test vehicle recently tested in our tunnel B-Minor had 319 thermocouples in its skin. It would have been desirable to be able to sample all simultaneously, because the interest was in obtaining the transfer of heat, or the difference in temperature, between adjacent thermocouples. Simultaneous sampling was not feasible because the data had to be assembled onto a recording medium such as magnetic tape. The next best thing was to sample serially as rapidly as possible. If there were one recorder per channel it would be possible to record the data simultaneously by freezing the recorder at a specific time. It is essential that data be taken as fast as possible as soon as cooling is removed. This exposes the model to about 1500°F.

W. C. LIPINSKI: What is the time interval in which you take your data?

M. R. MULKEY: It varies, but in this test, data was taken for 20 sec. The 400-channel-per-second system was used. One of the problems that must be solved is the development of transducers adequate to measure these heat transfers and pressures and produce a good analog signal at a

level high enough to be distinguished from interferences. Heat transfer is one of the bigger problems, especially in our "Hot-shot" Tunnel, which has a useful run time of 25 msec. A high-speed digital system is being developed for this tunnel. The system is being built around an Epsco converter, an electronic commutator, and a core storage unit. It is planned to digitize the data from this tunnel at the rate of 30,000 samples per second. At present this data is being taken on recording oscillographs. The manual handling of data becomes quite time-consuming.

Another problem is the programing of the computer for on-line operation. Program checkout for an on-line test is more involved than that for a normal off-line program, and is more time consuming. Some tests can be programed in about two weeks. The equations and constants are requested about two weeks before test entry. These data are not always available that far in advance and sometimes it is less than a week before the data are available. A program can be checked out in about one or two days after the test vehicle is installed. This time can be considerably longer and is dependent on the difficulties and complexity of the computations required for the test.

We need new computers because the 1102 is essentially obsolete. It will continue to do a job, but a high-speed machine is needed to handle data taken on high-speed digitizers. There is no intention to tie a high-speed system on-line. The data will be recorded on magnetic tape, and transferred to an IBM-704 or -709 or a Remington Rand 1103-A by manually changing tape reels. The volume of data encountered in heat transfer tests is so great that it ties up a slow machine like the 1102 for long periods of time.

J. E. OWENS: What has been your reliability experience? How many tests have you had to repeat because the converter broke down?

M. R. MULKEY: It has been better than was expected. I expected much more trouble than we have actually had, because my previous experience has been that the more equipment one has the more trouble one may expect. The shaft-rotational converters were designed by the Coleman Engineering Company. Difficulties were encountered with the first 64 that we ordered. After a few minor design changes the converters have performed very well and we presently have approximately 250 of these in use.

P. BLISS: I can sympathize with you in regard to tests that last a few milliseconds or even a few seconds. We have seen much of the same. But in defense of automatic data systems, I might point out a type more common in nuclear work where a test will run for several hundred hours; 1000 hr in component development. There is no point in tying this into an outlying computer; there is not that much data.

The test engineer has so much to do that he cannot chart off the computers once a day. We have had some very good experience with 200-point data systems which are supervisory; that is, they have a control function. With two systems on one test we can keep track of as many as 300 or 400 temperatures, with very great saving in operator time and in supervising the system to prevent temperature distributions from going out of control. These systems cost about \$25,000 to \$30,000 each.

Nonscheduled maintenance is required once in a while, usually because of coffee spilled on the typewriter, or a similar accident. But the overall picture must be considered; so much data is obtained when the system is working that it far outweighs down time. The instrument engineer has saved the test-engineering group a tremendous amount of time. This is a simple, more antiquated, but very useful data system.

J. R. MAHONEY: What accuracy do you believe you are getting?

M. R. MULKEY: We have several systems. On our standard pressure system, we use a Wiancko transducer, the amplitude-modulated type; and we normally expect data in the range of 1%. From the Consolidated Electro-manometer we expect 0.1%. A more realistic figure would be 0.2%. Careful operation can produce 0.1% data.

For temperature measurement, when a thermocouple is used, we believe that our data system probably adds inaccuracy in the range of 1/4% to the thermocouple system. The normal limitation of accuracy is the thermocouple itself - the care given it, etc. You are probably more experienced in thermocouple work than we are. In the "Hot-shot" Tunnel we are quite happy to achieve 5 to 20% accuracy on heat-transfer measurement data. We are, of course, continually shooting for higher accuracies.

R. L. MOORE: The Magnavox switch has been mentioned in connection with sampling at low-level inputs. Your answer was that you are sampling higher rates and have a response problem with separate preamplifiers. Could the Magnavox switch be slowed down for use at the slower sampling rates

used in industrial processes? What do you think about a comparison between using a Magnavox switch for low-level sampling with a one-channel preamplifier and using the relay-type switch contact under the same conditions?

M. R. MULKEY: I am not too well acquainted with this Magnavox switch, except as to how it operates - I have seen the literature on it. But there is more than just the commutation. I have been investigating this for the Princeton Laboratory. They were interested in measuring the outputs of 10 thermocouples at a rate of 50 samples per second, which may be considered low speed; and their levels were 2 to 4 mv. What is the data recorded on at sampling speeds of over 20 samples per second? A high-speed paper-tape punch can record at 20 samples per second for 15 binary bit words. It could record 30 samples or words per second if limited to 12 binary bits or less. When this speed is exceeded, data is stored in core storage or on magnetic tape. There is a higher-speed paper-tape punch that the Soroban Company, in Florida, claims will punch at 240 lines per second. I have also heard reports that Teletype will produce a modification of its tape perforator that will go twice its present speed of 60 lines per second, which would certainly help solve this problem.

In specific answer to your question as to whether you can use one preamplifier, I feel that probably in a case like this the answer is yes, and one converter, but you have to be able to record this data on some medium. This is your limitation.

B. LIEBERMAN: Is that on-line computer used for control of your equipment?

M. R. MULKEY: No, not at all.

B. LIEBERMAN: Is this contemplated in the future?

M. R. MULKEY: No, the only control in our process-control system is an analog computer; this is used in a test facility where they have utilized some Hagen equipment. I am not well acquainted with this.

W. C. LIPINSKI: If you are feeding this computer at 10,000 samples per second, at what rate is the information coming out of the computer?

M. R. MULKEY: We will not feed the computer at 10,000 samples per second. Data at that rate will be stored on a magnetic tape, then re-run into the computer at a rate it will accept. I don't know offhand what this is. At these sampling speeds we do not contemplate tying the

system to the computer as we have the low-speed system. We feel that the cost does not justify this.

C. GOTTILLA: Do I understand correctly that all of these points are on the model rather than on the tunnel?

M. R. MULKEY: That is right.

C. GOTTILLA: Well, then the question of space limitations arises on your Mach 10-20 tunnel. I presume that you have a small throat diameter. Models are therefore about how big?

M. R. MULKEY: The test to which I refer was designed for a hypersonic continuous-flow tunnel with a 40-in. test section. We ran it at Mach 7, and the model was about 12 in. in diameter. As a matter of fact, there were 319 thermocouples and 248 pressure taps on the same model.

R. G. AFFEL: This emphasizes that perhaps we often fall into the fallacy of mistaking the acquisition of data with useful information. We have been quite guilty of applying liberal numbers of strip-chart recorders, which faithfully grind out charts. We then present these to the test engineer, who in a very short time accumulates a substantial quantity of them, and years later still find the charts in the original box. It is just too much to try to handle.

I think the philosophy of obtaining the data in a very short time to make the test as useful as possible is, indeed, a sound one. I know that in some of our experimental programs this has been a bottleneck, in that because of lag in shop work or the need for scheduling shop work in advance we have not been able to make changes which have been shown necessary by the data. It has been processed so slowly that by the time the need for modification of future test pieces is recognized, they are, perhaps, already in operation.

DATA HANDLING SYSTEMS FOR NUCLEAR POWER PLANTS

R. A. Edwards

Knolls Atomic Power Laboratory

R. A. EDWARDS: In comparison to a control system based only on temperature and pressure recordings, I feel it difficult to justify \$600,000 worth of hardware. But, for the next five years, as a minimum, I feel that every nuclear power plant that is built will be a test facility. Even before it is turned over to the customer, it will undergo rigorous performance tests as well as component tests. I feel that in order to carry out this program such a plant is a "natural" for a coordinated automatic data-reduction system. The system requires close coordination with design groups, the "system analysis" groups, as well as with management.

The factors that favor data reduction equipment no doubt have been discussed with many of you by salesmen, but in the power plant the main thing is close data coordination. Through the use of such equipment each occurrence can be correlated with time.

Let us now look at a process in the nuclear field.

The configuration shown in Fig. 16.1 can very easily be either a sodium-cooled or a pressurized-water plant merely by proper location of a pressurizer and an evaporator. The coolant will circulate through the reactor, to a steam generator, into a pump, and back to the reactor. In the secondary system, the steam will leave the steam drum, go through a turbine, down to the condensing system (water phase), through a hot-well system, through the feedwater pumps, and return to the drum.

The number of points on such a system that are of interest to the design people may be as many as 500. In order to design the data-taking operation and the recording system, it is important to avoid overextending the operators, which I think is occurring in most power plants today. Therefore, I propose the system described below.

In Fig. 16.2 approximately 500 inputs terminate at a program patch panel which is, in a sense, a glorified telephone-type routing board. Approximately 50 variables will go in a recording and monitoring panel. A recorder is necessary to allow operating personnel to observe trends as they occur. Three hundred points will be divided between two slow (1 point per second) data-logging systems. One data logger will have a fixed format

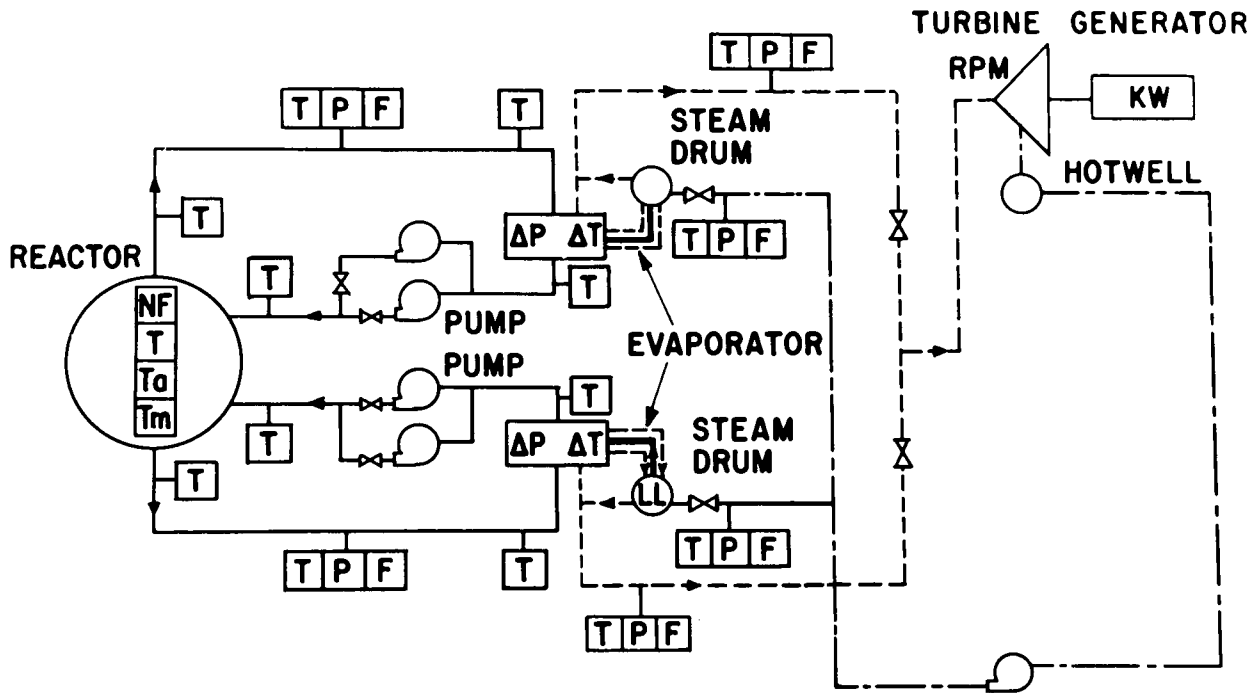
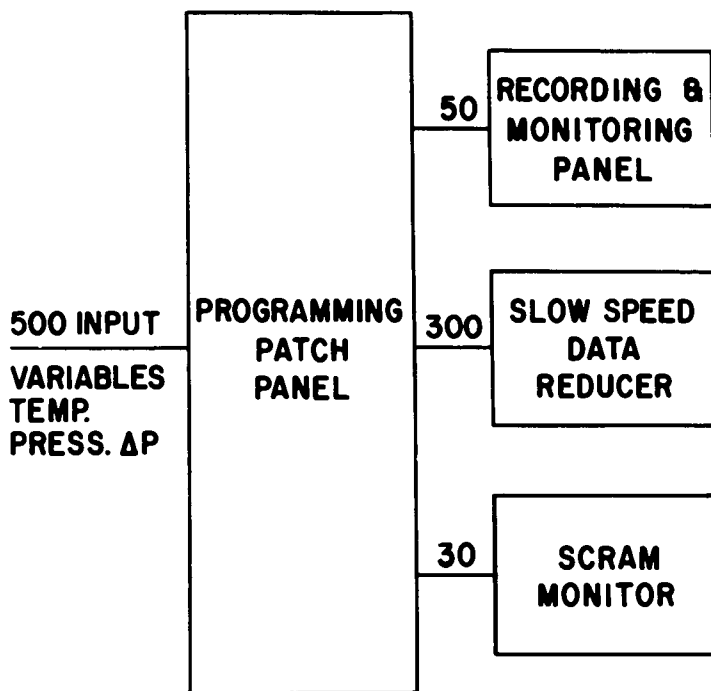


Fig. 16.1. Graphic Display of Power Plant.

PURPOSE



1. TO RECORD ON WIDE STRIP CHART RECORDERS THE TRENDS OF ALL VARIABLES OF INTEREST TO THE OPERATIONS AND DESIGN GROUPS.
2. TO PRESENT TO OPERATIONS A TYPED LOG SHEET OF 300 PLANT VARIABLES. THIS UNIT CAN ELIMINATE THE MANUAL LOGGING OF ANY DATA FOR A DAILY LOG SHEET.
3. TO CONSTANTLY SCAN AT A HIGH SPEED SIGNIFICANT SCRAM CIRCUIT SIGNALS AND COAST DOWN INFORMATION OR TO RECORD TRANSIENT DATA AND TO RECORD "INSTANT-ANEOUSLY" WHEN DESIRED THE DATA WHICH WILL BE REQUIRED FOR CALCULATIONS IN IBM COMPUTING FACILITIES.

Fig. 16.2. Data Recording System.

of about 100 points, known as plant variables; they will be parameters such as drum level, steam pressure, and steam flow. The other data will be recorded in any format that is desired. Selection of the desired data can be had by thorough use of the preprogrammed patch panel.

The third black box, which I consider extremely important, is the scram monitor. It can be used as a multipurpose unit. It is a device to record all the scram circuit channels as well as variables such as flow, coolant flow, pressure, and steam flow, so that the flow-decay curve can be observed after a scram occurs. More will be said about this unit later.

Figure 16.3 shows what I consider a sound design of a nuclear power reactor's control room.

Note the existence of a terminal board, which I consider one of the most important components in this system. All the switchings of variables can be done by the operator at that terminal board. From the terminal board a multi-channel oscillograph can be coupled to obtain an analog

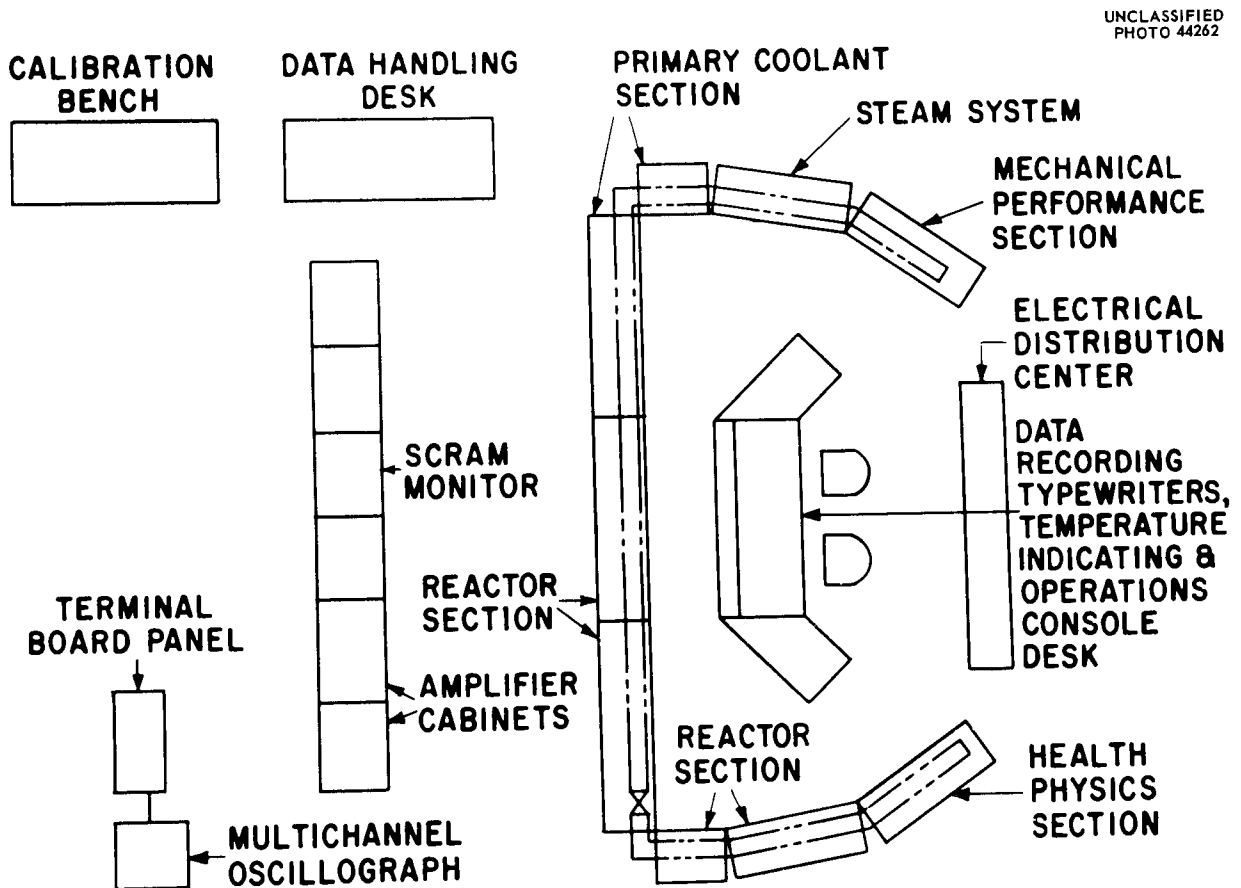


Fig. 16.3. Plan View of Recording Equipment.

record, which can be compared with simulator studies made before or during design of the plant. Next is the scram monitor section with the electronic equipment necessary for input-signal rectification, amplification, or attenuation. To the right is the main board, which can be divided into various systems. It will contain, if economically possible, a graphic section in the center portion. On the console in the center are two typewriters, one on each wing. There the operator can control the power level; he can also operate the coolant-control system and the poisoning system. Behind him will be the electric distribution center. I have found that this distribution center normally takes up as much room as the rest of this equipment. Plants are now going to miniaturized equipment (24-v switching) or are using multiple 110-v switching by interlocking many of the control functions necessary for electric distribution. I would like to see the distribution panel limited if possible to a benchboard approximately 13 ft long.

The remaining figures show closeups of some of the components.

A patch panel is shown in Fig. 16.4. On the left are some of the construction features that I would recommend for such a unit. It is an elaborate and expensive piece of hardware, but it is extremely flexible because data can be preprogrammed for the specific run. Thus it can be handled as an IBM computer is programed, by merely pushing into place these hinged patch panels.

Figure 16.5 is a block diagram of a scram monitor system in which conventional variables are fed into individual amplifiers. To gain greater reliability, commutators are not used. The data are recorded on magnetic tape. Ten seconds of current information is always recorded on the tape; if a scram occurs events during the next 290 sec will also be recorded. This information is put through a tape translator coupled with a multi-channel oscillograph or with chart readers, or from magnetic tape to paper tape and then on IBM cards for further calculations. I want to emphasize that this can be a multi-purpose unit and can be modified during test periods to record transient data.

Figure 16.6 gives an idea of the size and general appearance of the scram recorder. There is one minor error in the slide; the actual unit would show another tape transport. The following are examples of the information, found to be of interest on a typical pressurized water plant,

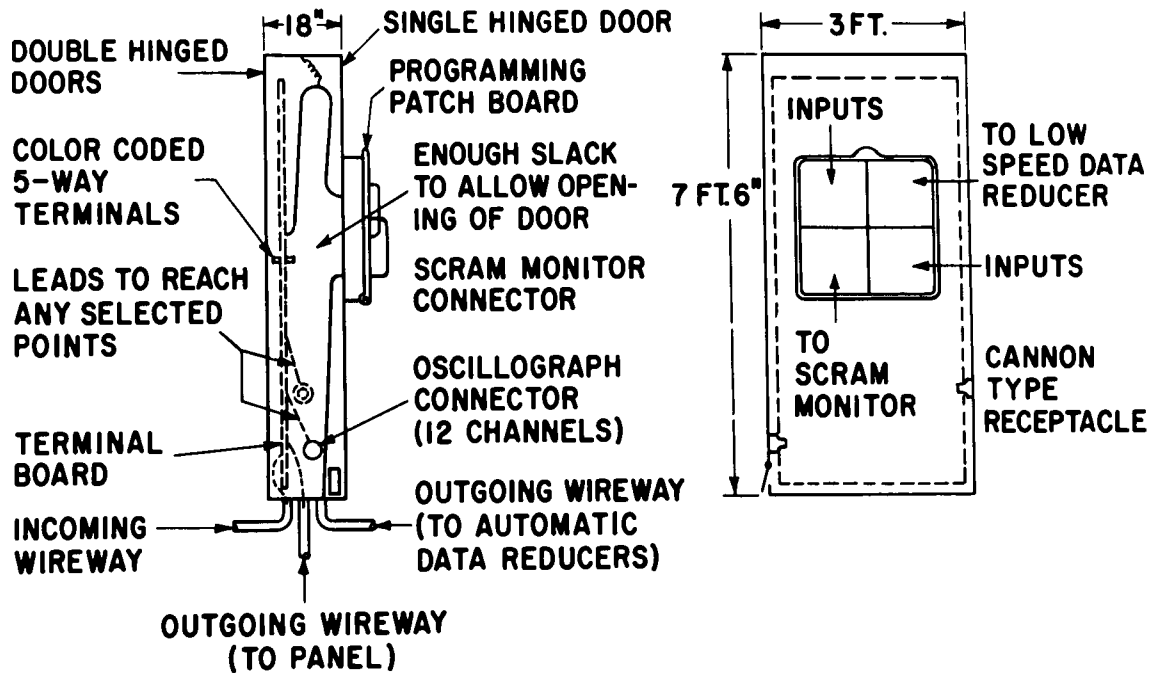


Fig. 16.4. Terminal and Programing Patch Panel.

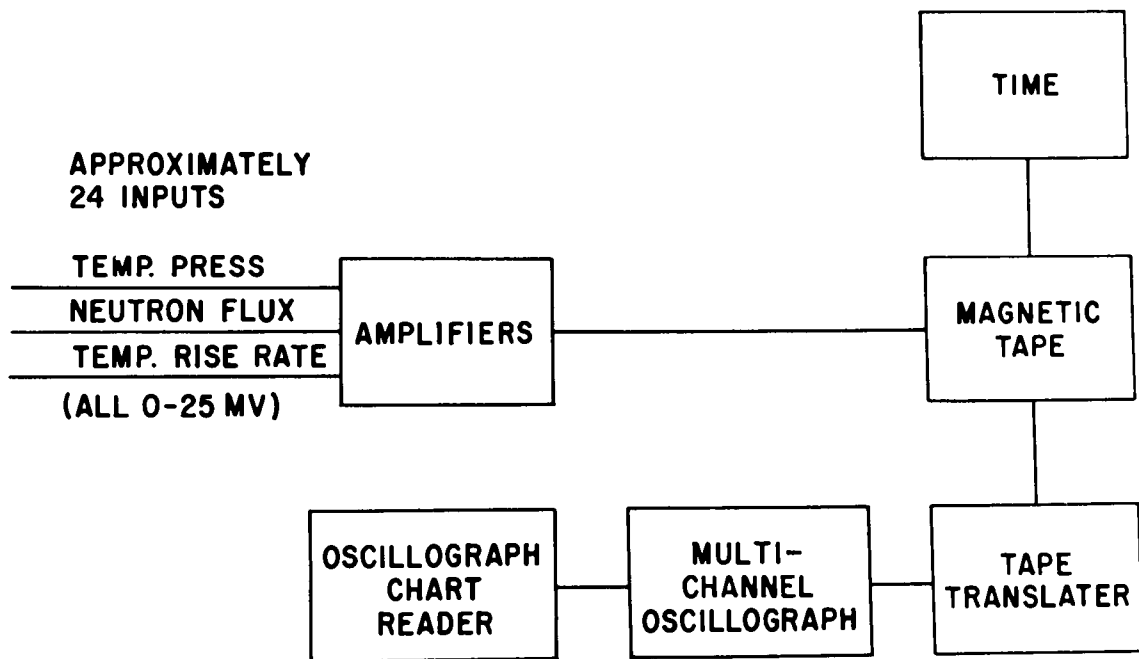


Fig. 16.5. Scram Monitor System.

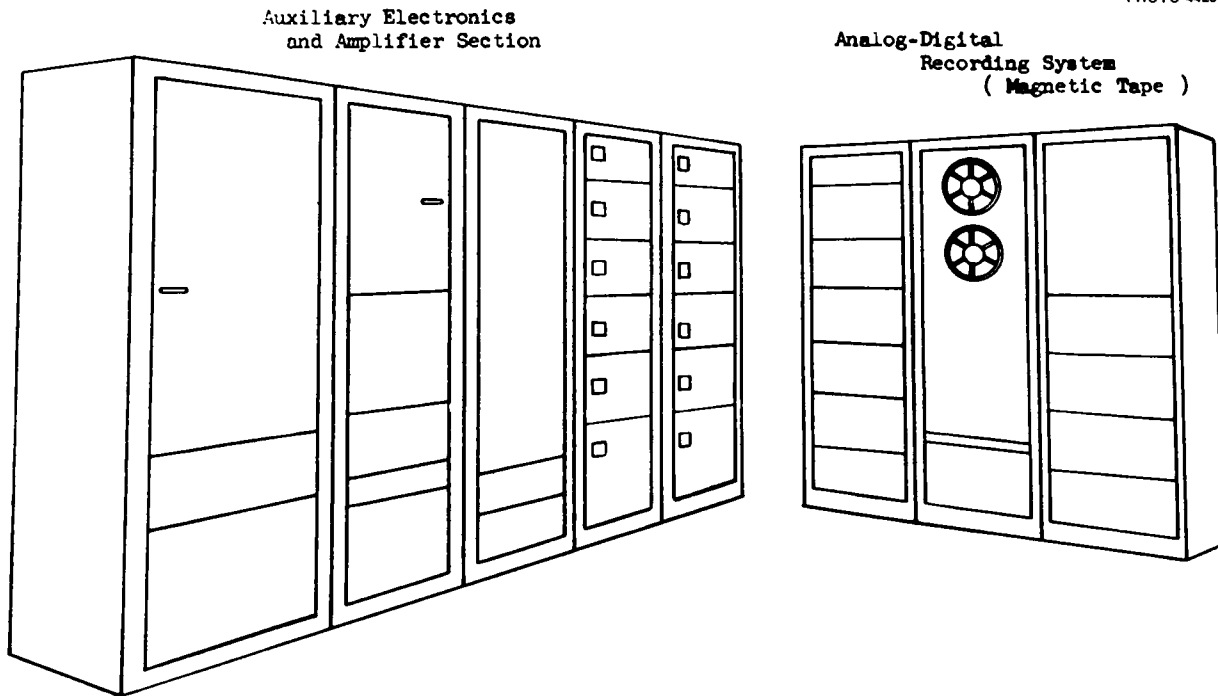


Fig. 16.6. Data-Reducer Cabinets.

that is to be recorded on the scram monitor. They are linear power, fuel-surface temperature, temperature inside the fuel, exit-water temperature, inlet-water temperature of the various passes within the reactor, signal from a microphone tied to the rod-bottoming unit, pressurizer pressure, outlet and inlet temperature of the reactor, totalized steam flow, main-coolant flow, feed-water flow, steam-drum pressure, steam-generator temperature, protective system, latch-coil current, supply voltage, drive-down in progress, and loss of pump-power scrambling signal.

Figure 16.7 shows what I would envision as the main console of the power plant (disregard the graphic panel). This may be over-simplifying the problem a little, but this is what a plant operational console should look like. If airplanes were operated along the same philosophy as power plants today, they would probably have a box in front 50 ft square by 20 ft high, for housing the controls.

Another handy device is a graphic section of the plant. The readout shown in Fig. 16.1 would be in a graphic section about 5 ft high by 10 ft wide and it would carry the various parameters, temperature, pressure, flow, etc., as shown. In front of the operator would be an additional

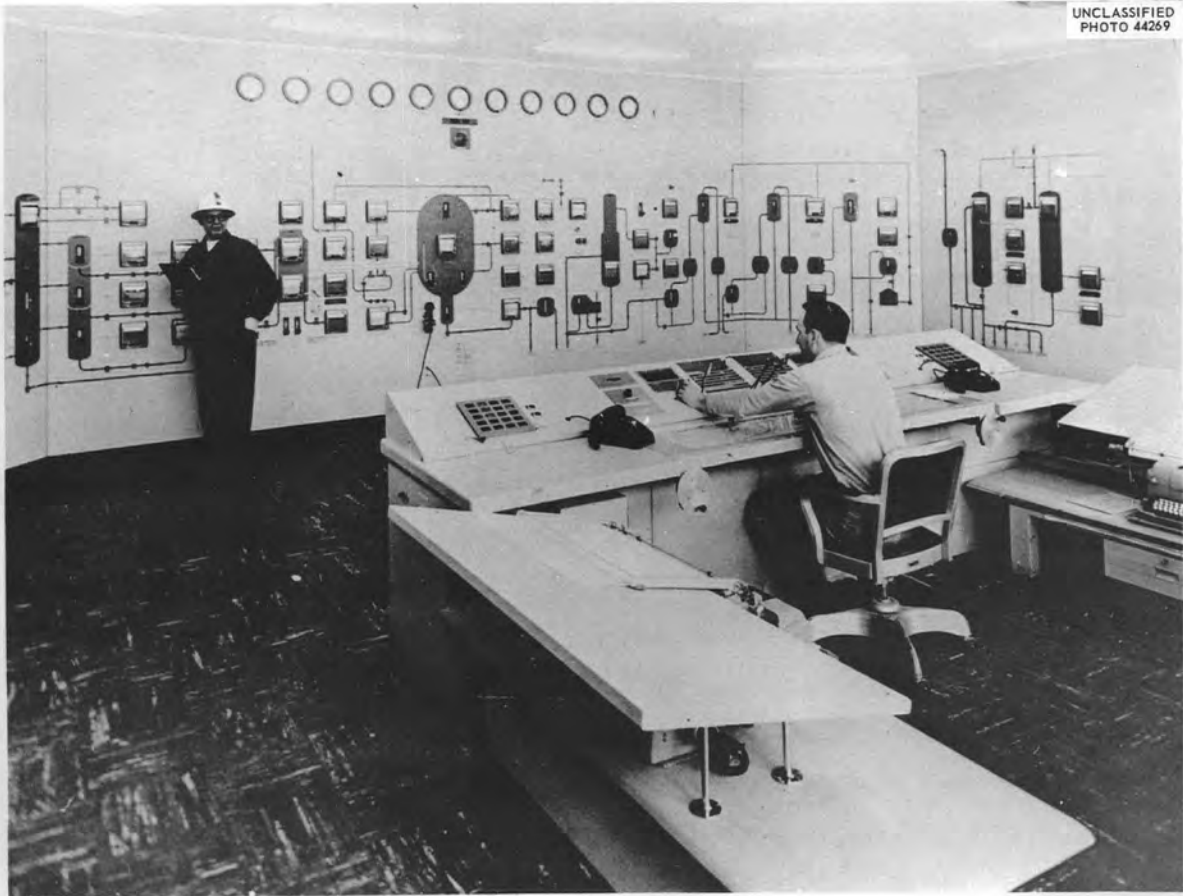


Fig. 16.7. Main Control Console.

device, giving an analog indication of rod position in such a form that the operator could see the plant condition in terms of per cent rather than be concerned with less comprehensive information.

Such a system will cost about \$600,000 installed. I don't know how this compares with the cost of conventional recording equipment. This installed cost includes transducers, running wires, etc. I believe that instrumenting a conventional nuclear power plant would now cost at least half a million dollars.

I expect questions regarding details of this system, because I have over-simplified the problem a great deal.

C. S. WALKER: You said that Fig. 16.7 shows the console of the power plant you envision. Were you implying that a plant that delivers electric power should have a console with knobs, switches, etc., and a man sitting there?

R. A. EDWARDS: Yes; when there is a console and an operator sitting at it, he has to give his attention to some sort of process. In the monitoring and control system recommended, the operator can observe plant conditions on log sheets which are easily visible. If something is off normal, an alarm will be sounded and the off normal will be printed in red on the log sheets. The operator will have a non-standard (miniaturized) annunciator in front of him. I do not believe in the ballroom type of control room. I think they are much too large in existing power plants. Aircraft with as much horsepower and just as complicated as existing power plants have the control and equipment in a small space.

C. S. WALKER: Aircraft are not supposed to run for 20 years.

R. A. EDWARDS: The DC-3's are still flying.

C. S. WALKER: Not non-stop.

R. A. EDWARDS: I agree with your point. However, no one knows how long an aircraft can really run, because they have a rigorous maintenance program. The aircraft companies say the program is antiquated. They claim that they can run their engines a lot longer, but they are required to regularly overhaul them by the CAA.

I have made this over-simplified but I do want to get power plant people away from their old philosophy of running a plant. For instance, they cannot see an operator sitting. I have never seen an airplane pilot on a cross-country run pace up and down the control room. He can sit and be responsible. The power plant people want the operators to rove; I think that is an error if they make the control center well engineered and compact.

C. S. WALKER: Do you mean that the operator has his finger on everything from that one console?

R. A. EDWARDS: Right, if it is well interlocked.

C. S. WALKER: But does he need to? If the plant needs it, then I think a wrong kind of philosophy is being used in the design of the plant.

R. A. EDWARDS: Do you think that you could do this job properly from an auxiliary benchboard somewhere else if there were enough personnel available for operation of a multi-station control center?

C. S. WALKER: The plant should not need attention to the extent that he has to be able to reach over and turn a switch periodically.

R. A. EDWARDS: What will he do? He will probably sit there and act mainly as a communications man. This is probably his main function, but

we should not be so optimistic as to think that in the next five years a man running a nuclear power plant will be free of any action. They do have to make minor adjustments.

E. SIDBALL: If the operator is to be of any use he has to have the means to interfere with the program. He has to be able to do things; otherwise there is no use to have him there at all. So he needs the displays, the over-rides and similar things, although we can all say that if we had done our job right he would sit there doing nothing for quite a length of time. It may be the opinion of the power station folks, as it is mine, that just to sit there and do nothing is a very hard job for a man. It is better to let him walk around and do almost anything than just to sit on one chair.

The main thing I think we are discussing is the difference between data loggers and the older ad hoc recorders, and so on. How about the bottleneck? If the data-logger printer is out of action a very great deal of information is lost, and the printer, in my experience, is not a very reliable piece of equipment. It needs very specialized maintenance. The man who can handle a teletype and similar machines is much less common than the normal instrument man. He needs to have a flair, an aptitude. Have you any real figures or experience with what is being achieved with these things in terms of per cent of time they are fully operative?

R. A. EDWARDS: I can make a few comments about the components of these systems. Of the three groups shown in Fig. 16.2 (the recording and monitoring panel, the slow loggers, and the scram monitor), the first will probably give the least trouble. The slow data logger has one weak link, and I don't know how to get around it other than stocking a spare typewriter. This is true of the Underwood, Flexowriter, and IBM machines. I have not seen a typewriter rugged enough to take the beating of an automatic data-reduction application.

I have over-simplified this so much that I believe I must have certain variables constantly exhibited that are absolutely necessary for running a plant, such as steam-drum level. Enough backup instrumentation must exist to allow the plant to run even if the automatic data-reduction equipment fails. I have every bit of confidence in the slow data logger. Systems have been running successfully for about five years and only the typewriter has really caused trouble.

The scram monitor or transient-data reduction equipment is a problem to be solved. The first one is being shaken down at the West Milton site right now. The recorder seems to be satisfactory. Trouble with the choppers and the amplifier is being overcome. Once this is done, we hope that the system will be reliable.

Down time will be expensive. I understand that it costs upwards of \$5000 an hour to run such a test facility; so if such equipment holds up the plant operation there will be trouble. In recommending this equipment now I have faith that in four years from now, when the plants we are conceptually designing now get into operation, the equipment will be reliable enough to install with confidence.

There are probably going to be three systems, similar to that shown here, operating in naval nuclear power test facilities within the next year or two. Certainly this kind of equipment is not limited to test facilities. There is a difference, because power-plant use is not just a short-run application.

P. BLISS: I have done some work on typewriters, and have used several typewriters in data systems. I don't think there is a machine yet in the typewriter class that has been designed for data systems - including the line printer. I think we must have a completely different approach. Even an office typewriter gets a good deal more maintenance than the typewriter on a data system, and it does not take near the beating.

W. P. DiPIETRO: I am interested in this miniaturized approach to presenting information. Once, in the cockpit of a plane, I asked the pilot, "How do you scan all this equipment at once?" He said that he had to observe very few of the instruments and the rest were there to indicate malfunctions, which happened very rarely. So I don't put planes and reactors into the same class. I think that we have to watch more of our instruments than the pilot does.

R. A. EDWARDS: I agree. I have been in conventional power plants, and "heard" their operation. Bells, chimes, and gongs sound continually, because the plant is poorly automatically controlled. It is well known that we have pushed heat exchangers, etc., about as far as we can. I understand that power groups are now investigating the only remaining thing they can improve, the control systems. Perhaps the only way they can really

check is through coordinated data facilities such as this. I think the day will come when our power plants can reach this point.

W. P. DiPIETRO: In the power plant, I like the way buses are set up with all the instruments in the middle of the bus. We see and follow the process. I think if you squeezed the data down to tape, the chances are that you might miss quite a bit.

R. A. EDWARDS: I don't wish to imply that I will record all the data on tape. In fact, between 30 and 50 points will be so recorded. The majority is on a typewriter log sheet right in front of the operator; other information is to his left or right. He can become used to operating in a different manner.

W. P. DiPIETRO: Still, I associate the reading with the physical process as you see it in the miniature layout.

R. A. EDWARDS: I agree. There must be a compromise of existing and future operating practices.

R. J. KLEIN: Have you tested typewriters for large-volume application, such as the Teletype models 6, I believe, and 28?

R. A. EDWARDS: No, we have not. We have used only Underwood and Flexowriters. There are some interesting high-speed printers on the market. I don't know how they will perform.

J. E. OWENS: The Southwestern Louisiana Power and Light Co. is buying a data-logger system. I am not completely familiar with the conditions, but they are relying completely on their data logger. They cannot operate without it, although they have enough standard instruments on the panel to shut the plant down. The manufacturers have guaranteed them something like 99% operability on this device, and there is a penalty clause in the contract whereby they shall be billed for shutdown.

R. A. EDWARDS: I should like to cite an example of a conventional power plant. There is a logger in a Gulf States Utilities plant. Several boilers operate in this plant, and GSU has taken a systems approach to its control. They have a control room containing a logging station and a panel, about 10 ft long and 6 ft high. The panel has possibly two dozen small recorders, which record trends of steam output and the power of each turbine. From this point the shift supervisor can direct various roving operators at the boiler control panels to adjust the system. The plant is really under the supervision of this one man. This again is a compromise

between the system shown and that used at a conventional plant. However, a nuclear plant cannot have roving operators. Everything must be done from a control room.

R. E. ENGDahl: You showed a miniature graphic panel and also showed the operator at the console. It appears to me that the man at the panel, actually observing the equipment, is probably looking at recorders and observing trends and things of that sort, which will allow him to see the plant operation equally as well as the operator sitting at the console. Are you not setting up a coincidence circuit, rather than actually depending upon one or the other for the operation?

R. A. EDWARDS: I am sorry if I implied that. In the control room setup shown in Fig. 16.3, the U-shaped main panel would have its graphic section in the center. This would be visible from the console, a few feet away, at which the operator would sit. Again I may be simplifying this, but if the control philosophy of submarines is applied to power plants, one man can adequately run the plant by himself from his control station. I don't want to imply that two men run the plant.

R. G. AFFEL: In the operation of a submarine, with the very limited space, it seems to me that we are really not interested in a graphic panel. It is not needed.

R. A. EDWARDS: There are mechanical and electrical distribution boards on them to a limited extent.

R. G. AFFEL: That is right; but not the space-consuming things that we are familiar with. I think it is a function of training. I know that the military has been running quite a few studies on information presentation.

PART V

SOLID STATE COMPONENT INSTRUMENTS

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MAGNETIC AMPLIFIERS FOR RELIABLE PROCESS CONTROL

H. E. Darling

The Foxboro Company

The problem of reliability of magnetic amplifiers for process control is not one that can be covered in detail in the allotted time. To supplement this talk, I have prepared a more elaborate paper,¹ which is available, and to which I shall refer.

One of the relatively recent advances in control circuitry is the use of electric transmission systems. The system described utilizes direct current. All control systems use time constants, which are usually generated by direct current circuitry. Therefore, it becomes attractive to determine whether one can build all-d-c systems, instead of having to convert from dc to ac, then back to dc. The magnetic amplifier lends itself very nicely to this approach because it is fundamentally a d-c amplifier which is, I believe - and hope to demonstrate - potentially the most reliable of all forms of amplification. By reliability is meant a little more than the usual definition of the term. Not only must it operate without attention for an indefinite length of time, but it must maintain zero, gain, and time constant during that period.

In order to convey why magnetic amplifiers are considered so reliable, it is necessary to first review briefly some of their basic fundamentals.

Referring to Fig. 17.1a the curve represents the hysteresis loop of a type of iron which is commonly employed in magnetic amplifiers. It is a plot of flux density vs magnetizing force. Assume that a given core of a specific material is initially completely demagnetized. When a magnetizing force is applied in the positive direction, the flux in the core begins to increase (along the dotted curve) until the core will support no further flux, and has reached saturation ($+B_m$). If the magnetizing force is now removed, the flux does not return to zero along the dotted curve, but returns along the solid line. At zero magnetizing force, there is a high amount of residual flux in the core ($+B_r$). With modern core material, B_r may be as much as 98% of B_m , and for our purposes this is desirable. In order to reduce the flux back to zero, a reverse magnetizing force of

¹Author's unpublished work.

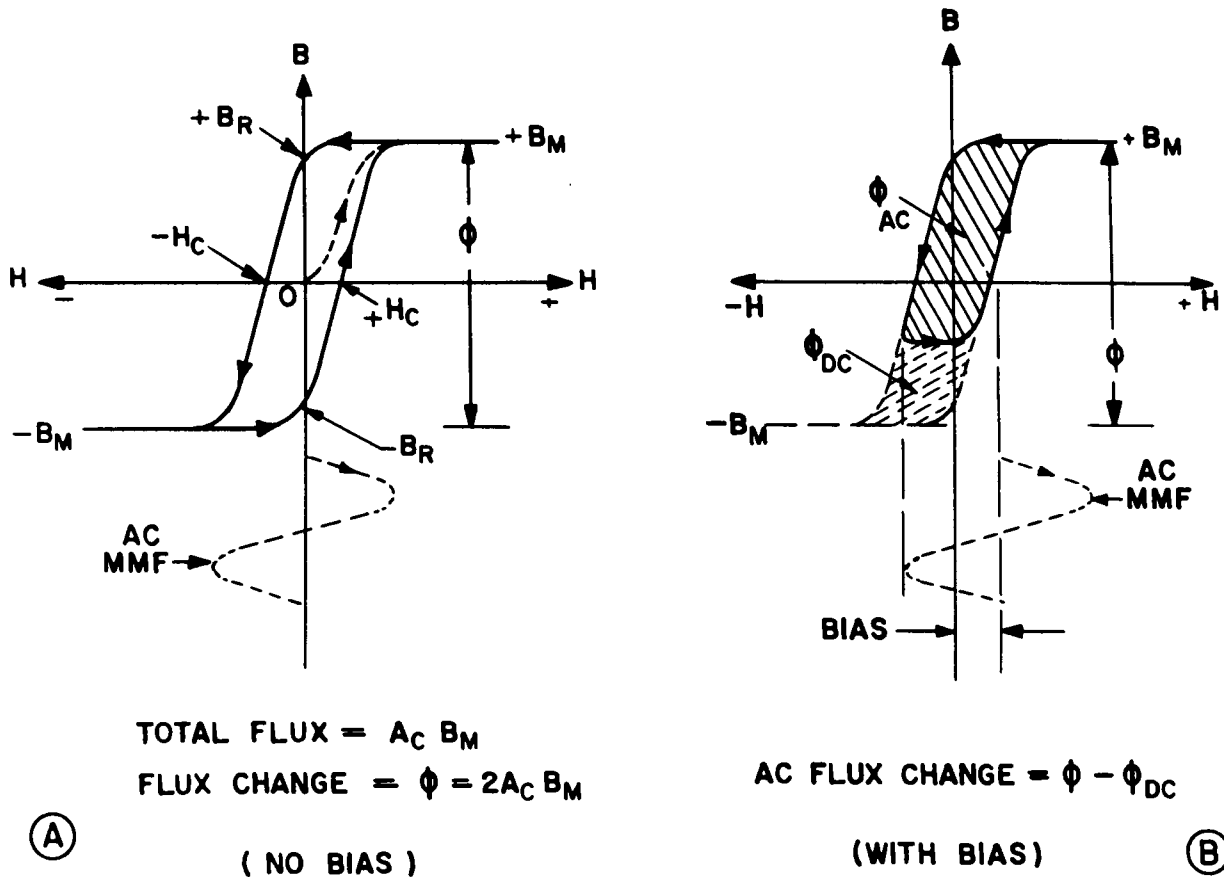


Fig. 17.1. Hysteresis Loops.

magnitude $-H_c$ must be applied. This is called the coercive force of the material. If additional force is applied in the negative direction, saturation will eventually be reached in the negative direction ($-B_m$). Upon removal of that force, the flux will return to the residual level, $-B_r$. If an alternating force is applied of such a magnitude that the flux just reaches B_m and $-B_m$, then the flux goes through an excursion represented by the over-all loop, and is the well-known hysteresis effect. In producing such an excursion of flux we have to expend a certain amount of power, which is proportional to the area of the curve. This curve is a work diagram.

A core of this type, wound with a suitable number of turns of wire, supplied with an alternating magnetizing force which does not cause the core to saturate anywhere in the cycle, will exhibit a high degree of inductance. In other words, it will allow to pass through its windings only

a very small current, the magnetizing current of the core itself. However, the core can support only a fixed amount of flux which depends upon its physical dimensions and upon the type of material used. If there is now added to this core another winding through which a direct current is allowed to flow, a portion of the total core flux is produced by the d-c signal (Fig. 17.1b). This reduces the amount of flux change which can take place when we apply the same alternating magnetizing force as before. Somewhere during the cycle the d-c flux and the a-c flux equal together the total flux that the core can support, and the core suddenly saturates. In this condition, the core has no longer a high inductance, and the current that flows through the circuit is no longer limited to the small magnetizing current, but can increase to a very large value.

The saturation of the core is somewhat analogous to opening and closing a sluiceway in a dam. It does not take very much power to raise and lower the gate compared to the enormous amount of power which can rush down the sluiceway when the gate is open. This core and its winding act as an impedance whose value can be abruptly changed from high to low.

To illustrate how we utilize this effect in the magnetic amplifier, refer to Fig. 17.2a. This is the schematic diagram of a magnetic amplifier, which contains two identical cores of square loop material. Each core has associated with it the same number of turns, and the windings are connected in series with a load across a source of a-c voltage. Common to both cores is another winding, called the control winding, through which we propose to apply a d-c signal. In the absence of a d-c signal, the core assembly represents a condition of very high inductance, because it was deliberately designed so that the voltage that is applied is insufficient to saturate the cores. The only current that flows through the windings is the small magnetizing current. The device acts very much like an unloaded transformer.

If a d-c current is applied to the control circuit, the flux produced links both cores. The polarity of the two a-c windings has been deliberately arranged so that the instantaneous fluxes passing through the center winding cancel, and no net voltage is induced into the control winding. This is done for the purpose of decoupling. If we apply a d-c signal to this control winding then, as I mentioned a moment ago, some time during the cycle the cores will saturate.

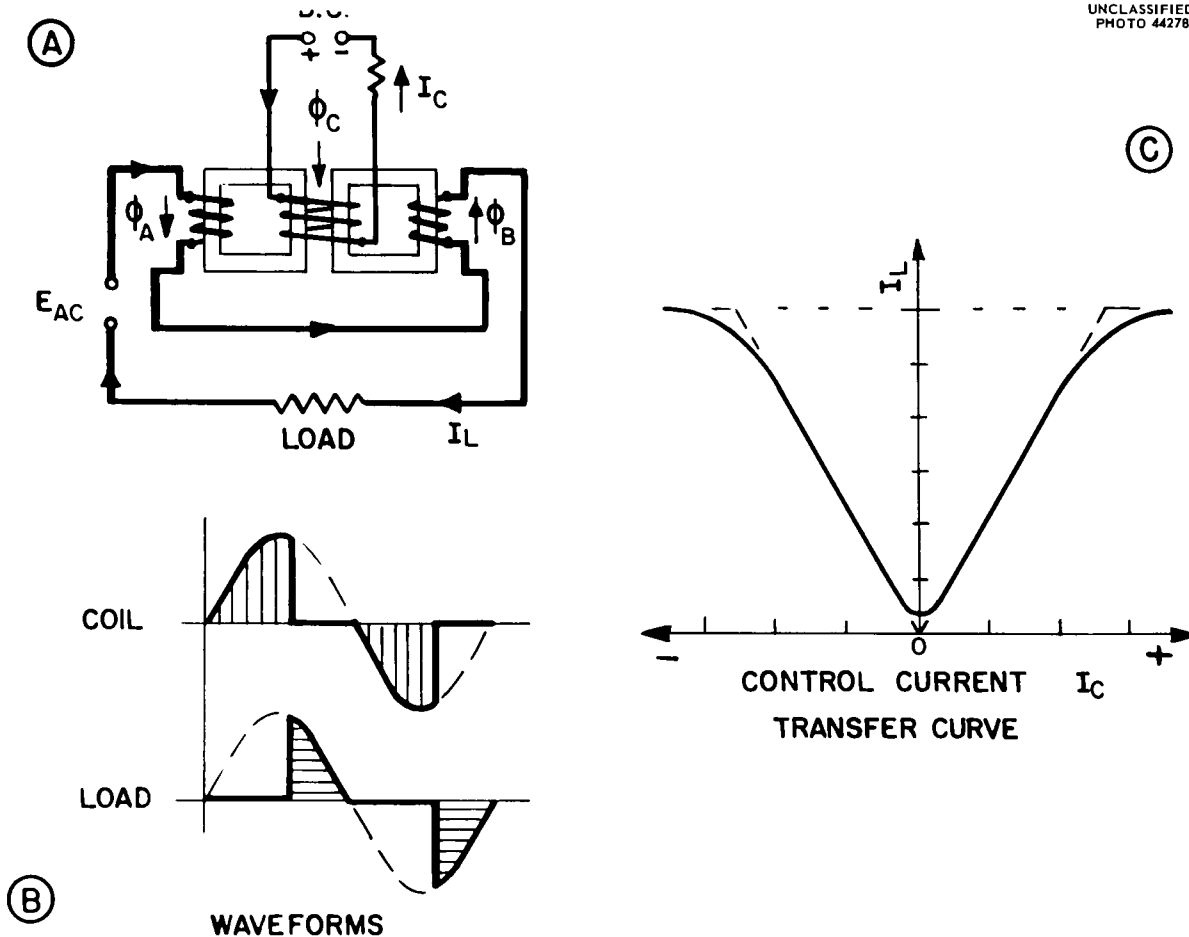


Fig. 17.2. Saturable Reactor.

If we look at the voltage developed across the coils with an oscilloscope, assuming a sine wave of voltage applied to the input, the voltage across the coils will begin to build up, and at some point in the cycle, the total of the a-c and d-c fluxes will equal all that the core can support (Fig. 17.2b). The voltage across these two windings collapses at the instant of saturation, and the supply voltage suddenly appears across the load for the remainder of the cycle. By changing the magnitude of the direct current flowing in the control circuit we can alter that point in the cycle at which saturation will occur (change the firing angle of the core). The current that flows after saturation is limited by the circuit resistance and can be many times larger than the magnetizing current. Therefore, the system is capable of operating as an amplifier.

This amplifier is called a saturable reactor, the simplest of all magnetic amplifiers. A curve of control current vs load current would show

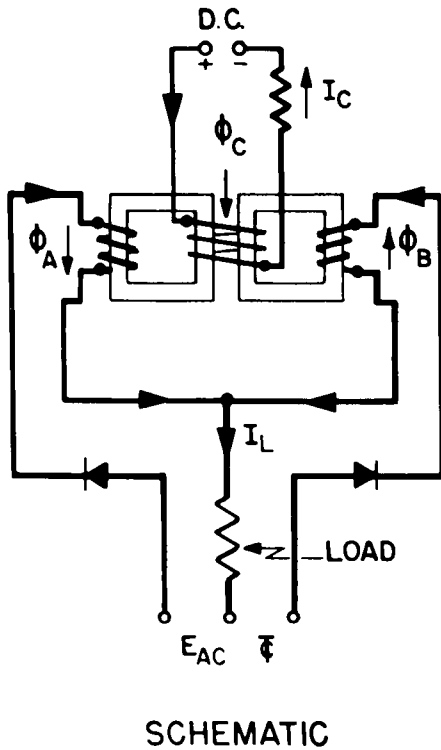
a characteristic similar to that shown in Fig. 17.2c. At zero control current there is a minimum current flow through the load; as the control current is increased in the positive direction, the output current gradually increases and finally reaches a limiting value at which the core remains completely saturated by the control signal throughout the cycle. When the polarity of the control signal is reversed, the same general characteristic appears again. The device cannot recognize the polarity of the control signal, and this is one of its basic limitations. However, consider what a simple device it is; it consists only of two cores, each with an associated winding, and one additional winding for control purposes. Obviously, if care is exercised in its design, and the applied voltage is limited to some reasonable value, there is no reason why this circuit should ever burn out or fail. Its potential life is indefinite. The device as described is, historically, the first used in the magnetic amplifier field, but has serious limitations as far as instrumentation is concerned. First, it does not recognize polarity. Second, its output is alternating current and of a distorted wave form. If we wish to utilize d-c output, we must insert in the output circuit a bridge rectifier. This can be done but at the expense of departing from the basic reliability of the saturable reactor by the degree to which this rectifier is considered reliable.

The saturable-reactor amplifier is only suited for low-current-gain applications, and it is difficult to get current gains as large as 30, although the power gains under the same conditions can be enormous. The amplifier has a moderately long time constant and is not suited for use with very-low-input-power control signals. It is the low-input-power field on which I am going to concentrate, because this is a relatively new field.

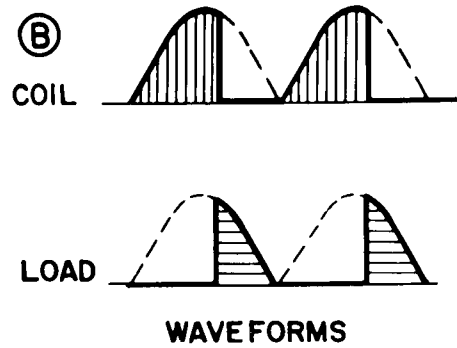
One of the reasons for not applying magnetic amplifiers more extensively to date has been the fact that, until very recently, we have not been able to make a magnetic amplifier nearly as sensitive as its competitor, the vacuum tube. We are rapidly closing that gap. Since the saturable-reactor amplifier has such limitations we will dismiss it, except for one particular application which I will mention later. At present it is used almost exclusively in the high-power field where rectifiers in the load circuit are either undesirable or impossible for one reason or another.

Referring to Fig. 17.3a, we have again the same two cores, the same gate windings associated with them, and the same control winding, but the

(A)



(B)



(C)

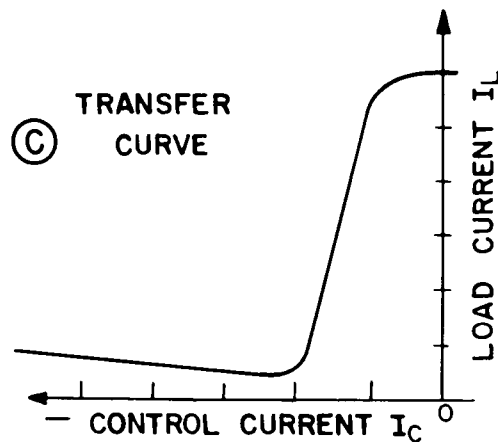


Fig. 17.3. Self-Saturating Reactor.

power circuit has been altered slightly. Instead of a single source of power supply, a center-tapped source is provided, and the core windings are now parallel connected. In other words, the load current goes to each core winding and back to the center tap through separate paths. In series with each winding is a rectifier, a circuit change which converts the amplifier to an entirely different kind of device. Obviously, from the circuit of Fig. 17.2, if a bridge rectifier were placed in the output circuit to convert some of the load signal to direct current, and if it were fed back in a separate control winding, we could apply either positive or negative feedback to the amplifier and, therefore, increase or decrease its current again. Such circuitry is in use (see complete paper¹), but this device has built into it a high degree of positive feedback. During the half cycle that the left rectifier conducts, current flows through the

core winding and load. On the next half cycle, the right hand rectifier becomes conductive, and current flows through the right core winding and load. An oscillograph shows the same type of voltage wave form generated across each core, with saturation occurring during each half cycle (Fig. 17.3b). The polarities are so arranged that the output current flowing through the load now becomes a full-wave rectified d-c signal.

The self-saturating amplifier has quite a different characteristic from the preceding one. If there is no control-current flow, after passage of the first complete cycle of applied voltage, both cores are left in a completely saturated condition and maximum current flows through the load. The transfer curve would be somewhat as shown in Fig. 17.3c. With no control signal, a large load current would flow. As the control signal is applied (this must be polarized to oppose the locked-in flux of the two cores), the output current diminishes, passes through a minimum, and then begins to increase, but at a very slow rate. If the polarity of the control signal is reversed, the core is already saturated so no change in load current occurs. This amplifier then is polarity sensitive, and the current gain can be considerably higher than that of the saturable reactor.

Because of the fact that the flux tends to be locked into the core on each half cycle (a saturating action), this amplifier is called the self-saturating reactor. The current gain can be of the order of 5000 per stage. Remember, I said gains of 10 to 30 for the saturable reactor. It is not always desirable to strive for such high current gains, but it can be done. Power gains as high as 60 db per stage can be accomplished. A very important fact is that, for some designs, this amplifier is usable down to input-power levels of 10^{-12} w, which is of the order of magnitude of core noise for this type of amplifier.

The self-saturating reactor has some disadvantages — everything always does. It has a time constant associated with it much the same as with the saturable reactor, and its power-handling capacity is dependent on the current and voltage rating of the rectifiers. This is the amplifier that is now most frequently used in general magnetic-amplifier work.

Referring to Fig. 17.4a, this circuit will be recognized as the same general type as that of the saturable reactor. It consists of two cores connected in series across a source of voltage without a series load. This device is called a second-harmonic magnetron. It is not a new device, but

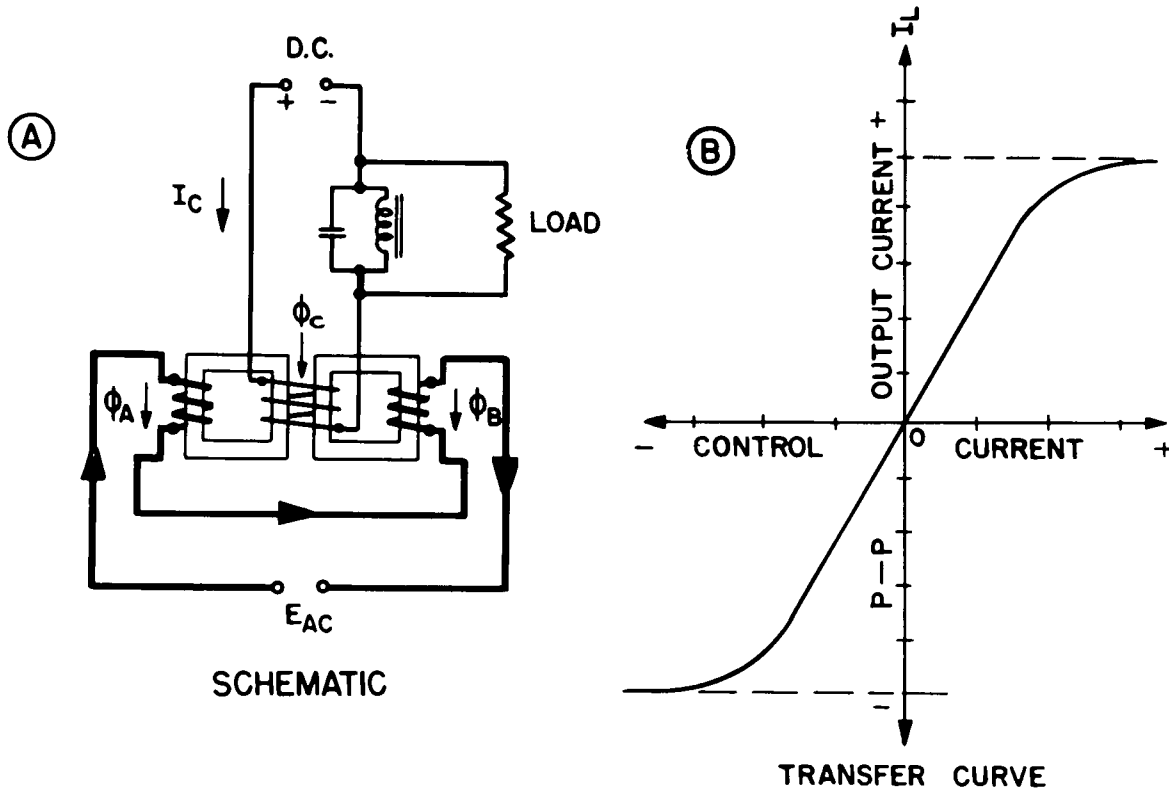


Fig. 17.4. Second-Harmonic Magnetor.

it has been developed to a high degree at our laboratory, and it has some very interesting features.

The fluxes in the two cores are so arranged that there is no signal induced in this control winding under zero-control-current conditions. If we apply a d-c signal to the control circuit a second harmonic signal is developed across the control winding. This appears only when a d-c signal is applied, so that this device becomes a d-c detector. We can accentuate the magnitude of the second harmonic signal by putting in the control circuit a trap tuned to twice the applied frequency, and can place a high-impedance load across this trap.

This magnetic device is a chopper. It converts a d-c into an a-c signal, and is capable of producing voltage amplification of 100. It has a characteristic somewhat as shown in Fig. 17.4b. If we apply an increasing d-c signal in the positive direction, we get an increasing alternating current signal as shown, eventually reaching saturation. If we reverse the polarity of the control signal we get an alternating current which has

reversed in polarity with respect to the previous signal; thus the device is a polarity-sensitive unit. It gives zero output for zero input; its principal use is then as a zero detector, and it can be used to amazingly low input-power levels.

Using cores of a design about which I am going to speak in a moment, noise level can be reduced to the equivalent of 10^{-14} w. I have a unit here, which I will show you later, which actually works at that level. This instrument has remarkable zero stability and a very simple circuit. If properly designed, nothing can go wrong; its reliability is of the utmost. It is a combined amplifier and zero detector; so that it constitutes an efficient, flexible, solid state chopper, but it does have disadvantages. It is primarily a voltage converter. It has very high voltage gain but very poor current gain. It cannot be used, for instance, in this configuration to drive another magnetic amplifier, for two reasons: First, the output is twice the supply frequency, so we would have to excite the next amplifier stage at twice the magnetor supply frequency; and second, the magnetor works best into a high-impedance load. It is primarily limited to use in combination with a vacuum-tube or transistor amplifier.

If we could develop an amplifier having these same general characteristics, but being also a power amplifier, this would be the missing link in the magnetic-amplifier picture, and would enable us to work from very low input power all the way up the chain to whatever output power we would like. An amplifier which actually accomplishes this is shown in Fig. 17.5.

Referring to Fig. 17.5a there are again two cores, each with its gate winding, a control winding, and a small additional winding which will be used for bias purposes. Due to the nonlinear characteristic of all magnetic amplifiers, it is necessary to use bias in order to limit the working region to a more nearly linear part of the transfer characteristic.

Starting at one side of the supply, current goes through the left hand rectifier, through the core winding, through the load resistance R, and back to the other side of the supply. Starting from the same supply terminal as before, current goes through the right hand rectifier, through the core winding to another load resistance R, and back to the supply.

These rectifiers are polarized in the same direction, so that both windings are conducting simultaneously on the same half cycle. This causes a pulse of current to flow through both resistors simultaneously. If we

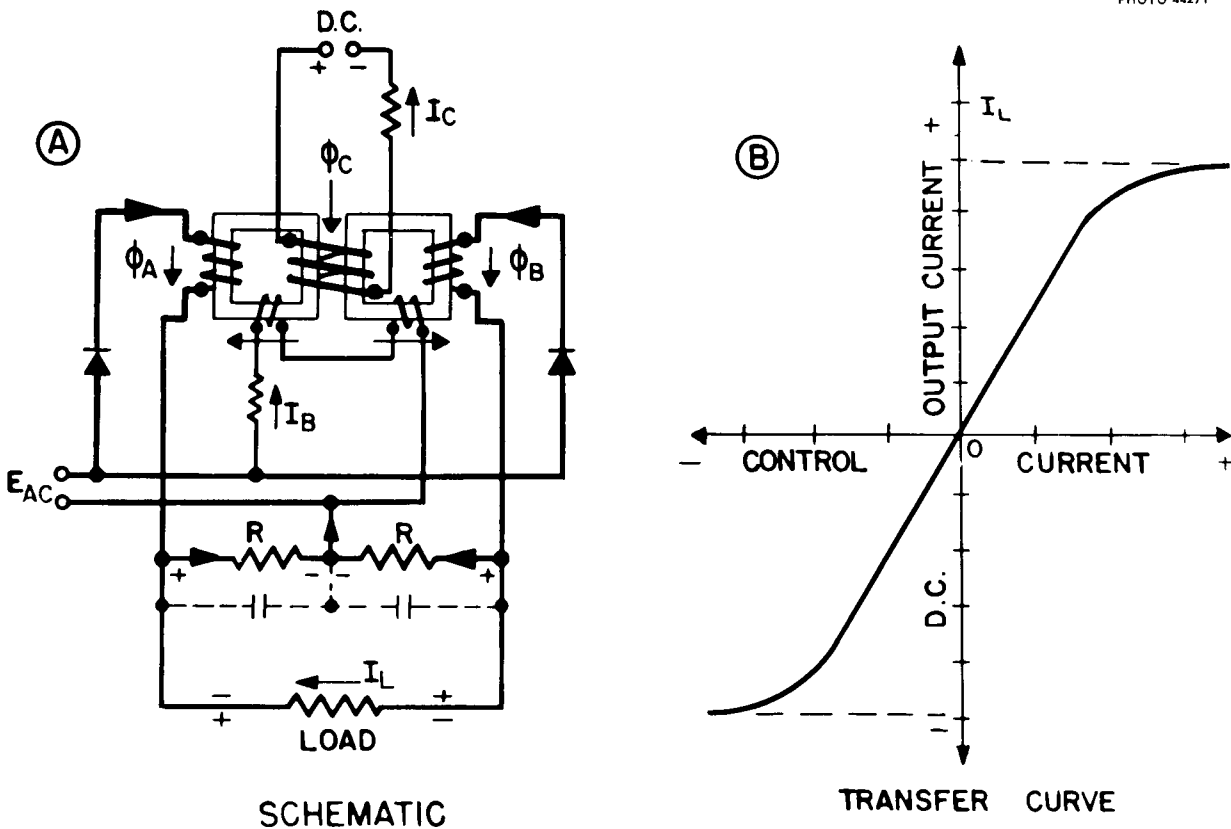


Fig. 17.5. Balanced Self-Saturating Reactor.

measure the voltage differential across the resistors R , and if everything is properly designed, there would be a zero voltage difference across the two resistors for no current flow in the control winding. The amplifier diagram looks very similar to that of a balanced modulator, and that is precisely what it is.

By virtue of the self-saturating property of the cores, at the end of the first cycle of supply voltage the cores will be completely saturated and maximum current will flow in the loads. Such a condition requires a large control signal for appreciable action, so we use a bias to partially reset the core on the next cycle. This can be done with alternating current, hence does not require the use of rectifiers. If we apply a signal to the control winding, the flux produced by this signal adds to one core flux and detracts from the other, thereby unbalancing the system. Current through one load resistor R will increase and the other current will decrease. Therefore a net current will flow in the load. This has a characteristic analogous to that of the second harmonic magnetron, in that for

zero input there will be zero output, and there is polarity sense. We have exactly the same type of characteristic (Fig. 17.5b) as in the previous amplifier, except that there is a d-c output vs a d-c input. The differential output is the difference between two chopped sine waves. As was shown in Fig. 17.2 for the saturable reactor, the differential output is a pulse that flows through the load. This pulse can be used as-is in some instances, but it is frequently necessary to apply across each resistor R an integrating action in the form of a small condenser. By so doing, we now have a power amplifier which is capable of driving another magnetic amplifier stage.

This type of amplifier has been successfully built and operated at input power levels of 10^{-12} w, which is about the noise level of this particular amplifier. It will sense input currents to the order of 10^{-9} amp, and has excellent zero stability, provided we observe certain restrictions. It can have current gains as high as 2000 per stage, and power gains as high as 50 db. Being but little different from the self-saturating reactor, it has a high degree of reliability. The only thing that detracts from its reliability is the rectifiers that we use, and we have found an answer to this problem too.

The disadvantage of the use of this type of amplifier is the fact that the system must be of balanced design, and it must be balanced! Balance is accomplished by careful core design; the same type of core design we used in the second harmonic magnetor. The rectifiers must be of the silicon fused-junction type, since nothing has been found that begins to compare with them in stability. We must balance the load! We must be careful to ensure that each core has the same number of turns, for instance. But if this is done, an amplifier results whose characteristics are similar to those of the second harmonic magnetor previously described.

The balanced amplifier is the missing link between low-input-power work and conventional magnetic amplifiers. It now becomes possible to use high sensitivity; thermocouple signals, for instance, can be accommodated, and a substantial current gain achieved. We can now cascade from this level to conventional amplifiers, and into as many stages as desired. There is now provided a means of sensing signals of very low input power.

The problem of cascading magnetic amplifiers is not a simple one. There is a great deal that is not understood about such problems. In general, it is more a matter of an art than of science to produce multiple-stage amplifiers. However, units of four stages have been built without

too much difficulty, and can be reproduced in the production line. There are many properties of multiple-stage amplifiers that must be compensated: they are temperature-sensitive; they are voltage-sensitive; they have interaction effects. Every time we apply a control signal to a control winding, there is induced in it a second harmonic signal which tends to react with the preceding stage. They also have frequency and stability limitations.

However, we handle these problems by feedback techniques. This is amplified somewhat in the supplementary paper. By the use of over-all feedback loops, it is possible to control the zero stability, gain, and time constant of a multistage amplifier. By the use of rate and integral feedback networks, we can also control the frequency response. It is possible to do much the same thing with cascaded magnetic-amplifier stages that can be done with transistors and vacuum tubes, and it is the satisfactory solution of these problems which makes possible magnetic-amplifier control systems.

The primary basis of my talk was to be reliability. The heart of the magnetic amplifier, of course, is its core. In the early days of the development of magnetic amplifiers, it was the custom to use E-I laminations. You all recognize those as the type of lamination that has been used in power transformer design for many years. They are not particularly good for magnetic-amplifier work, because no matter how carefully one assembles the core, it has an air gap. The air gap has two major faults for low-level work: first, it detracts from the square hysteresis loop that one would like to have; second, this air gap is temperature sensitive.

In recent years, a substitute for this type of lamination has appeared in the so-called DU lamination. The U-shaped punchings are so designed that when they are placed one on top of the other, the net effect is an air gap which is considerably smaller than the minimum possible with the E-I. There results a more stable core which also makes better use of grain orientation. Low-level amplifiers made with DU's are not very common. They are much more suited for medium and high power level. For further discussion of their properties, refer to the more detailed supplementary paper.

The only core types of interest in this discussion are the so-called tape-wound and the stamped-ring cores. The tape-wound core is built of

ribbon, like a tightly wound clock spring. With such a core, it is possible to utilize the grain properties of the iron to the utmost, and the effective air gap is very small. This type of core is now very commonly used in magnetic-amplifier work for all except high-power work, where it becomes expensive. The core is usually packed in an aluminum box with an insulating cover, and the ends of the case rolled over to make a sealed unit. This type of core will stand a lot of physical abuse. It is very rugged, which is one of the factors that contribute to the reliability of magnetic amplifiers using this type of core design.

Even this type of core, as good as it is, is not adequate for low-level work. It is noisy and temperature-sensitive, and although matched core pairs can be selected quite easily, it still is not suited for the low power level required in the instrument field. To overcome this difficulty, a new design has come into use in the past few years, a stamped-ring core. The ring is just a thin washer, has no air gap, and is therefore the ultimate in stability insofar as air-gap effects are concerned. The problem now is how to mount such rings in a case that can be easily handled. Such a case is just an open brass box into which these rings are placed, but between the rings are placed insulating paper spacers, which are 0.001 in. thick and ring-shaped. These provide not only lamination insulation, but also act as a cushion to prevent the laminations from moving. The box is filled, the cover put in place and sealed. This produces a core whose stability cannot be matched by any other design.

It is possible to build core pairs of this type which have phenomenal temperature stability. The noise level of the cores depends largely upon the type of material. For very low-power input work, Mumetal is used, or something of a similar nature, which has a noise level of the order of 10^{-14} watts. Magnetron units constructed in this manner are now used in production instruments, and will sense input power signals of the order of 10^{-14} w. Self-saturating balanced amplifiers, of the type described above, are made with the same type of core construction.

With the realization of the properties of the stamped ring core, we have solved one of the most difficult stumbling blocks of low-level magnetic amplifiers by the use of these small components.

In the use of these cores as a power amplifier, we are forced to use rectifiers, and in the supplementary paper I have outlined the properties

of a number of rectifiers. The only one in which I am interested — and I am sure you will be once you have used it — is the silicon fused-junction type. This device is actually one piece of metal and has potentially indefinite life. It has a very high back resistance and is a very efficient rectifier. It has a low forward resistance and, therefore, low internal power dissipation, and is remarkably stable with temperature. Its reaction to temperature is very small, but reproducible; therefore we can select pairs which vary in the same manner with temperature. When it is used in the differential amplifier, the temperature-difference effect can and must be very small. These rectifiers are, of course, very rugged in the manner in which they are built and packaged, and are very reliable.

I also mentioned, in connection with balanced amplifiers, that we are sometimes forced to use capacitors. We should use paper capacitors wherever possible because they are the best capacitors available. However, there is a certain family of tantalum electrolytics of the porous-plug design which have a number of unique properties. First of all, they can have a very large capacity per unit volume, making a small unit. This is particularly good for such things as guided missiles. They have a capacity that is virtually independent of temperature. They have a very high leakage resistance that varies only slightly with temperature. They are useful over a wide range of temperatures from -40 to $+200^{\circ}\text{C}$. They have indefinite shelf life, and under some conditions of application, their properties approach those of the best paper condenser.

Now we have the three basic components: stable cores, stable rectifiers, and stable capacitors. These are the three essential elements required in all the building blocks for our control systems.

In building multiple-stage amplifiers, or any amplifiers as far as that is concerned, an engineer must use a certain amount of common sense. We are trying to build a unit which is the ultimate in reliability. We must, therefore, be careful that we wind coils with copper of the proper size so that when the load current flows through them they won't burn up. We must be careful to choose our rectifiers so that they will not be over-rated. If we do this and are conservative in design, we produce a single-stage amplifier whose potential life is indefinite. Amplifiers of this sort have been operated for many years and we find it difficult to measure changes in their characteristics with time, so small are the changes.

As we cascade stages, the probability of failure increases with the number of stages. But if each stage is individually designed for maximum reliability, the over-all departure of the cascade from the reliability of a single stage is difficult to measure. We have actually built multistage amplifiers that have been operating for two years or more continuously, and yet we cannot measure any changes in their properties.

Another thing that we have to be careful about in the design of a magnetic amplifier is application. It is very common to short-circuit a magnetic amplifier accidentally. Again, if we design with the proper factors in mind, it is possible to build magnetic amplifiers that will work into a short-circuited load indefinitely without damage.

In practically every instance that I will discuss we use power transformers to change the supply voltage up or down to the level required for each stage. The design of these power transformers is quite important. I have stated in the paper that we nominally design them for twice the power that they will be called upon to deliver. In other words, if we have a 10-w system, we design a 20-w transformer. This is done for several reasons. First, it gives better voltage regulation, which helps a little on the voltage problem of multistage amplifiers. Second, such a transformer is little affected by short-circuiting of the magnetic amplifier. Third, but most important, is that by over-designing the transformer, we create in it a low internal impedance which helps greatly to minimize interaction between amplifier stages. Interaction arises principally from two sources: a voltage that is always induced in the control winding, and the common coupling that exists from stage to stage through the power transformer. To overcome this is largely a matter of experience. Once one understands the problem involved, it is possible to build multi-stage amplifiers which have a degree of reliability which you can't believe until you have worked with them.

A few specific applications will point out how units can be built having the characteristics necessary for use with control systems, yet using relatively few components.

A "ratio" amplifier is in effect a d-c transformer with a continuously adjustable transformation ratio. There are instances in control applications when we wish to blend two components of a product by installing a ratio amplifier between the d-c transmitter of a flow system and the electronic controller for that system, whereby we change the effective gain of

that system relative to another system so that the blending of the two components is as simple as turning a knob.

In this case, a d-c signal input is required for this amplifier, and its output will be another d-c signal which is related to the input by a fixed ratio. For instance, at one extreme a signal input of 10 to 50 ma will deliver an output of 10 to 23-1/3 ma, a 3:1 attenuation. On the other extreme, an input of 10 to 23-1/3 ma will deliver an output of 10 to 50 ma, which is a 3:1 gain. So we have a variable attenuator-amplifier. As designed, the relationship between input and output current is accurately maintained to less than 1/4% for every ratio setting.

Figure 17.6 is a block diagram of this amplifier. At the input there will be a direct current of 10 to 50 ma from the transmitter. A d-c output is required, starting at 10 ma and increasing to some higher current so that the 10 ma represents the zero of the system. In order to make the amplifier receive a zero when the 10 ma flows into the input circuit, we use a Zener reference. This device is a solid-state voltage reference source of high accuracy, and its operation is analogous to that of the familiar VR tube. It can be built to produce an output current that is remarkably accurate and stable.

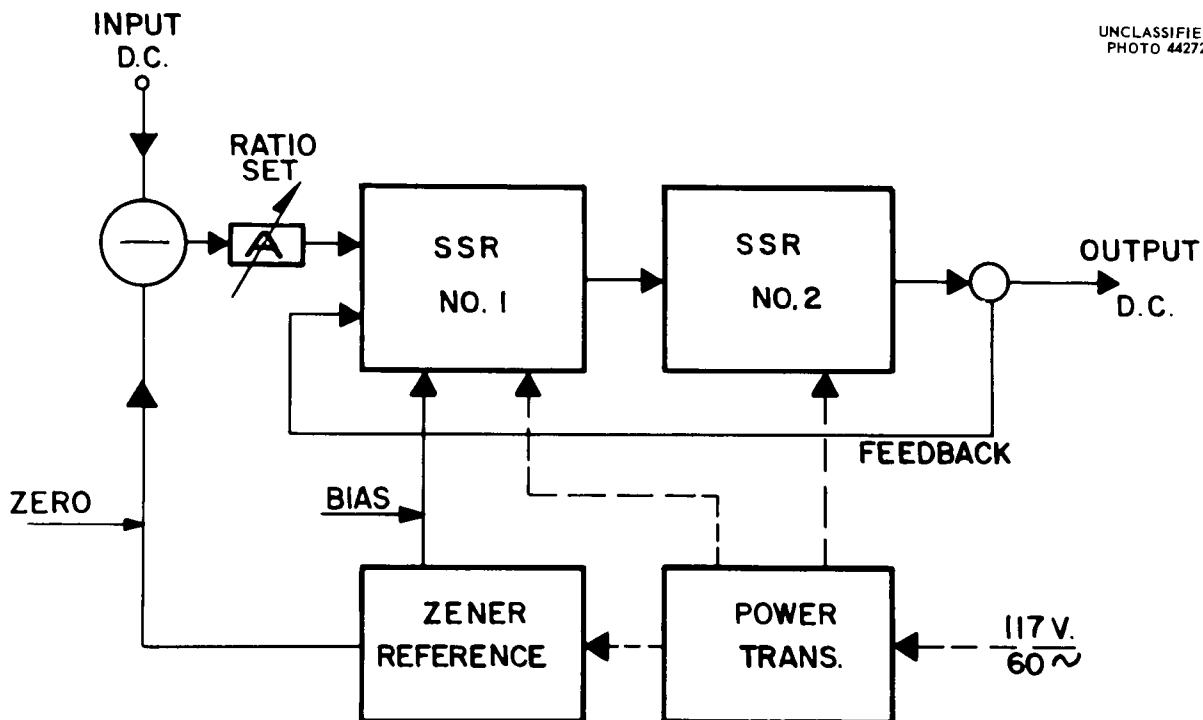


Fig. 17.6. Diagram of Ratio Amplifier.

A signal from the Zener reference is applied to the input to cancel out the transmitter input signal at the 10-ma point. A bias is applied to the first amplifier stage so that there will be a 10-ma output. Since there is no net current flow into the magnetic amplifier at this point, we can change the ratio over the entire range with no effect to the output. If the transmitter current departs from 10 ma at the input, a signal appears, which is fed into a two-stage magnetic amplifier, and appears amplified at the output. A portion is fed back around the amplifier to give 99.5% feedback, so that the internal changes of the amplifier due to temperature or any other internal effect can cancel themselves out.

This amplifier, having 99.5% feedback around it, has a net signal appearing at its input which is a very small proportion of the 10 to 50 ma from the transmitter. Actually, it is designed to deliver an output of 10 to 50 ma for a 15- μ a input change. Under these conditions the system is a highly stable amplifier, stabilized solely by feedback, and we can change the relationship between input and output current with a simple resistance attenuator.

Figure 17.7 shows the schematic of this circuit. This schematic uses a symbol for a magnetic amplifier which I have adopted, and which I hope will become popular. Each double line represents a core with its associated winding, and common to both cores are four separate control windings. The output of the first stage feeds directly into the second stage and thence into the load, with over-all current feedback around the system. All the output current is fed back through a control winding in the first stage.

At the lower left is represented the Zener reference. An a-c voltage from the transformer is rectified, filtered a little, and then applied to the Zener diode with the load on the regulator being a constant quantity. This gives our precision current reference. Compensation can be made for a very slight temperature effect in this diode by making a portion of the load resistor temperature-sensitive. A portion of this voltage output is applied in opposition to the input voltage developed across the fixed resistor in the input circuit. The net difference between these two voltages in series with a potentiometer is then applied to the control windings of the amplifier. With a small difference signal appearing at the input control winding, we get an output current flowing in the load, which is then fed back to stabilize the system.

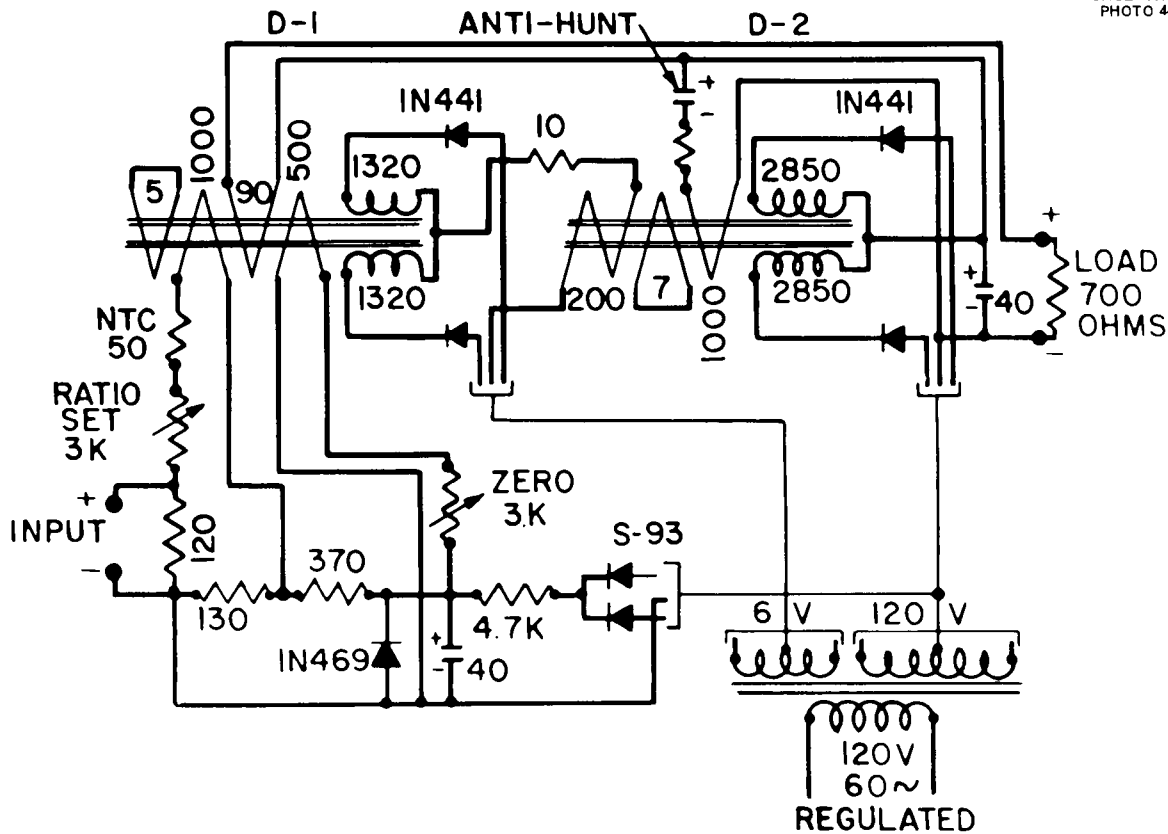


Fig. 17.7. Circuit of Ratio Amplifier.

Notice how few components are used. We have two cores and two rectifiers in each stage, and a filter. There is also an anti-hunt system which consists of a capacitor and a resistor around the two stages. This is rate feedback which is required to stabilize the amplifier once the over-all feedback loop is closed. Because of the relatively long time constant of each stage, individual loops are used. Without this stabilizing circuit, the system will hunt violently. Once it is stabilized, if we apply a step input, a step output will appear across the load. We have used two rectifiers in the Zener system, and their properties are not critical other than that they should have a long life. Precision resistors are used in the input circuit. Very few components are involved.

There is a zero shift with temperature of $+0.0047\%$ per $^{\circ}\text{F}$, or approximately $1/2\%$ per 100°F rise. There is a small span change of -0.0028% per $^{\circ}\text{F}$. It has repeatability of $\pm 0.05\%$ over a 24-hr period. This is the maximum drift we find. It is virtually independent of load changes over quite a wide range ($\pm 0.03\%$ for $600\text{ ohms} \pm 10\%$). Actually, we can change

from 200 to 1000 ohms load with less than 1/4% error. The zero stability with ratio setting (you remember that I said we must deliver 10 ma when there is a 10 ma input), is $\pm 0.05\%$. The estimated long-term stability (and these units have now been running close to two years) is 0.1%. The only penalty is that we must apply a regulated source of alternating current with a reasonably good sine-wave form. The supply is regulated to $\pm 2\%$ with 5% harmonic distortion. This 2-stage magnetic amplifier has over-all stability of the order required for instrumentation; yet we have not used a great many components.

Figure 17.8 indicates in block diagram form another system of interest in instrumentation. This is a force-to-current transmitter, and the force may be derived from a Bourdon tube, a differential-pressure cell, or what you will. We want to generate an electric current proportional to that force at all times, and we do it with magnetic amplifiers as illustrated here. First of all, we have a force motor. This is a mechanical device having the property that the force appearing at its output is directly related to the d-c current going to it. This is no simple trick in itself, and is the result of a great deal of development work, but it does exist. In other words, the force developed by the motor is proportional to the current used, with accuracy within 1/4% or better. The force motor is mechanically linked to the force to be measured, and attached to this link

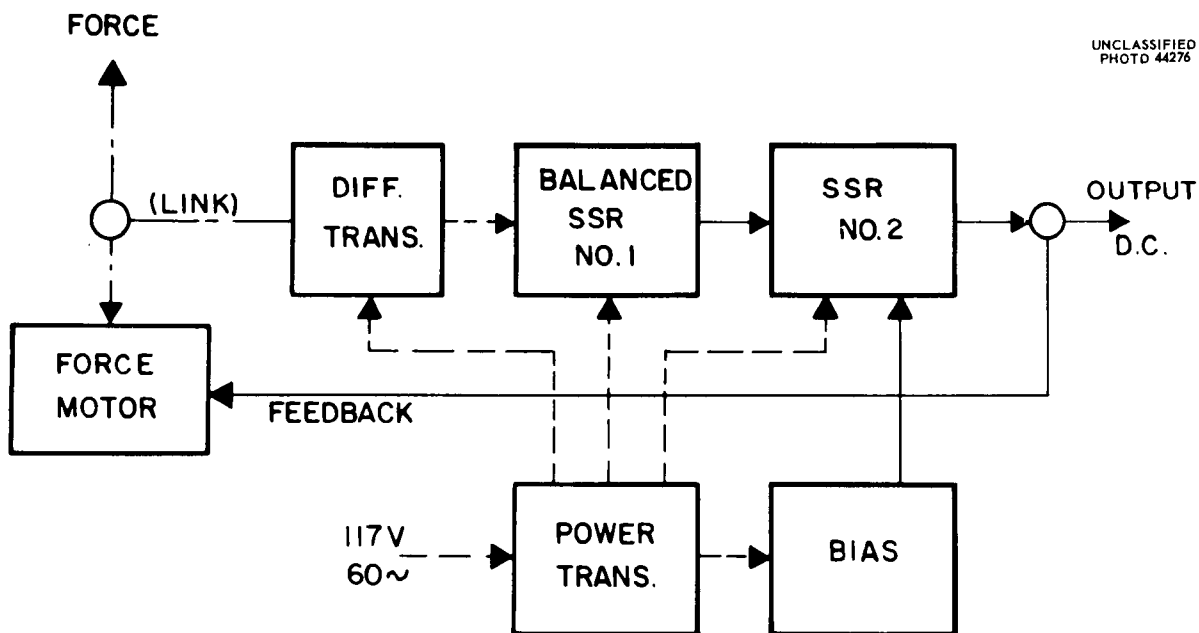


Fig. 17.8. Force-to-Current Transducer.

is a differential transformer. This may be any one of a dozen varieties, but in this case, a 60-cycle differential transformer is used such that with the movable element in the electrical center zero output is produced.

The input stage of the amplifier uses the balanced amplifier described above. This amplifier has a number of interesting properties. It will operate not only from a d-c signal, but also from a signal of a line supply frequency. In this instance, we are using it as an a-c-sensitive amplifier. When a signal appears from the differential transformer, it is amplified by the balanced reactor stage, applied to a conventional self-saturating stage to produce the output power desired, a part of the output being fed back into the force motor to produce a force balance system. If there is enough sensitivity in the amplifier, and if a high percentage of feedback is used, then we have the means of converting from a force to a current. This current then supplies a control system or other load.

The over-all system is accurate to within 1/4%. The amplifiers used are very simple and are not much different from the ratio amplifier just described, as far as number and types of components are concerned. The bias system is a very simple device requiring only a couple of rectifiers and a filter. So this is potentially an extremely stable, accurate, long-life device. It has a frequency response which is limited by the supply frequency and by the over-all feedback effect of the system; but this amplifier will respond to 8 or 10 cps, which is more than adequate for most industrial control work.

Figure 17.9 shows a slightly different type of system. This is not all-magnetic, but I am going to discuss it for a very good reason. The figure shows a block diagram of an instrument that our company has produced for several years. It is an emf-to-pneumatic converter. A thermocouple signal, or some other emf, is applied at the input terminals. Again, a Zener diode is used as a reference in the place of the familiar wet cell commonly used in emf units, and this provides a constant voltage for both zero and cold-junction compensation, if this is thermocouple application. The difference between these two signals is then fed into the second-harmonic magnetor. This is a magnetic amplifier chopper. It converts the d-c signal to ac. In this particular instance, the magnetor is excited by a 1000-cps oscillator. We obtain from its output, whenever that

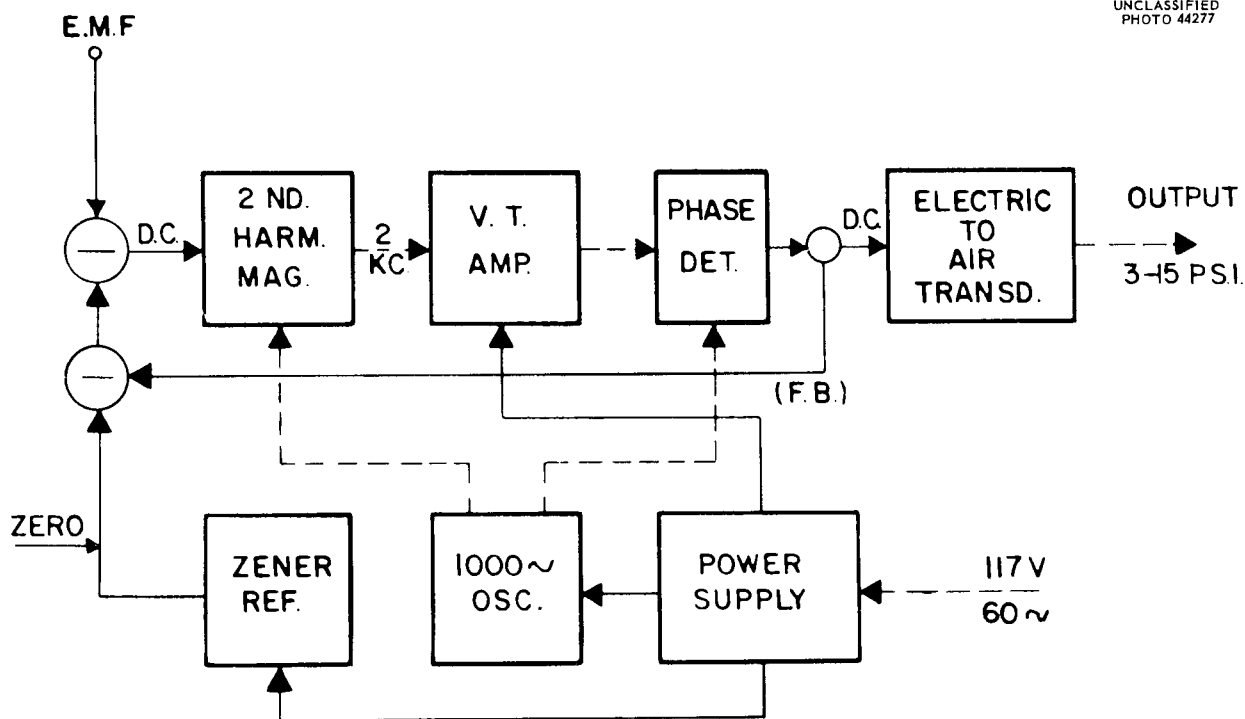


Fig. 17.9. EMF-to-Air Transducer.

d-c signal flows, a 2000-cps a-c signal, which is amplified by a conventional vacuum tube amplifier and phase detected by conventional means. The d-c output of the phase detector is then fed back around the system to produce the necessary stabilization. The input is a low-level d-c signal, and the output is a high-level signal. The output is then directed into an electric-to-air transducer, which is a combination of a magnetic amplifier and a mechanical force balance system, so that the air signal coming out is directly related to the d-c signal coming in. This produces an emf transmitter which is applicable to conventional pneumatic control systems. I showed this circuit to illustrate the use of the second harmonic magnetor. This amplifier has been in production for several years. We have never found a single instance where the zero of the second harmonic magnetor has drifted more than $1/4\%$ in years of operation. We have never found one that failed. It is one of the most reliable magnetic devices that we know about, and it should be a very good competitor for the mechanical chopper.

These are just a few of the many things that we can do with the magnetic amplifiers. In all the units that I have described with the exception

of the emf-to-air transmitter, considerable trouble is exercised to ensure the maximum possible reliability of the amplifier, and in some cases, this probably has been overdone. But we have demonstrated to our own satisfaction that we can produce magnetic-amplifier building blocks for control systems which are as reliable — we feel, much more reliable than any of their competitors performing the same function.

Unfortunately when you design a unit of this nature with reliability as the uppermost consideration, you may end up with something which is not competitive in price. This is the one disadvantage we have left to overcome.

J. F. POTTS: I notice that you have accepted a solid state device as being essentially as reliable as your magnetic device. I am wondering what your feeling is about the further development of transistors. This would permit going away from the magnetic amplifier and would gain the advantages of high impedance, frequency response and space saving.

H. E. DARLING: This is entirely a matter of what you are trying to do. We feel that a rectifier, which has only two elements, is fundamentally a more stable device than a transistor, which has three elements. Our experience with transistors, particularly of the silicon variety, indicates that they are extremely reliable devices, and I think that one of our speakers is going to have more to say about this. We are building complete control systems using transistors. We have great faith in them, don't misunderstand me, but my field is magnetic amplifiers. I am trying to sell them to you with the principal objective that the best transistor cannot touch the magnetic amplifier as a reliable solid state device. If reliability means dollars and cents, you cannot surpass the magnetic amplifier. They have their limitations — one of them is frequency response — but within the limitations of application, you cannot equal them with transistors from the standpoint of long term reliability.

J. F. POTTS: In your electric-to-pneumatic converter, I noticed you had a vacuum tube amplifier. I was wondering why you did not gain the advantage of the magnetic amplifier.

H. E. DARLING: It is not available at the moment, but we do build a complete magnetic-amplifier emf unit which will amplify the thermocouple signals to the 10- to 50-ma output current range with the degree of accuracy required. I merely gave this particular unit a little publicity because of

the remarkable record of the second harmonic magnetron. Some people are reluctant to use the magnetic amplifier, but they don't hesitate a moment to use a mechanical chopper. Here is a competitor for the mechanical chopper which has potentially indefinite life, and it should be of interest to people where reliability is important.

The vacuum tube amplifier is not a reliable unit in the terms in which I am speaking, but if you are willing to work with a vacuum tube amplifier, and to change the tubes from time to time, you can help yourself a little by not having to change the vibrator, if you use a second harmonic magnetron.

R. L. SCHIMMEL: You said early in your talk that you use aluminum or brass core boxes. Is there any particular reason for using those over, say, nylon or some nonmetallic material?

H. E. DARLING: Yes, there is a very good reason. It is one of mechanical rigidity. A nylon box will deform under temperature, especially after you have stressed it by the application of wire. A metal box, if properly designed, does not distort. It gives you a much more easily handled package, which is one of the advantages of the magnetic amplifier. It gives you a package which does not change its properties seriously with temperature. It gives you a package which you can sell, and which is extremely rugged and reliable.

R. L. SCHIMMEL: Do you experience any effect of this metal enclosure around your core?

H. E. DARLING: As far as we can see, it has no effect. We have reason to believe that it contributes to the noise level. We have not proved this yet, but as far as we can see, a magnetron of this type, made with a metal or nylon core box, will perform exactly the same as far as noise is concerned. Mechanically they are two quite different things.

P. BLISS: Does the core box constitute the insulation around the core or is it insulated?

H. E. DARLING: The cover itself is an insulator. We must be careful about that. We cannot use a shorted turn.

H. H. HENDON: I think we all certainly agree with what you say about reliability. However, on this matter of complexity, with particular reference to your ratio amplifier: the data which you gave implies that the open loop voltage gain is probably 2000, and involved in this amplifier you have 6 or 8 solid-state diode semiconductors. It is not hard with

modern silicon transistors to accomplish this same end with perhaps two transistors and one diode. You will agree, I think, that both of us are considering reliability on the order of copper wire breaking, and I submit that your system was complicated.

H. E. DARLING: The output power with which we are working in this particular case is of the order of 3 or 4 w. When you are talking of silicon transistors of this same order of power, there is a lot of difference of opinion as to their reliability.

We have done a great deal of work along this line ourselves and I agree that we can build a unit of this sort with the transistors, but we don't like to do it because we have serious questions as to their life. This does not fit into our reliability picture. We are going to build them for customers who want them, but we feel that life-wise and dependability-wise, it is at present not a good thing. We feel that the magnetic amplifier is better.

H. H. HENDON: Today the life of a solid-state semiconductor device is quite well understood to be infinite, provided you don't get dirt in the can when you put it together. It seems to me that the quality-control problem of exceedingly clean transistors vs exceedingly unstressed magnetic amplifiers is comparable, or slightly easier in the case of transistors.

H. E. DARLING: Let me put it this way: the problem in connection with magnetic amplifiers has been solved, as far as we are concerned. We are not sure that we can produce reliable transistorized systems. We have hope, we have faith, but magnetic amplifiers are in use in systems now. That is why I presented them, not that they necessarily represent the best that can be done. It is possible, heaven forbid, to combine magnetic amplifiers, transistors, and vacuum tubes into a common circuit if you wish to achieve a particularly difficult objective. But when you do this, of course, you lose some of the factors of reliability. As I said at the close of my talk, when we design an all-magnetic amplifier, with reliability as the primary objective, we always produce something that is expensive. It is not easy to sell. Transistors are cheaper, but the big question is the reliability. This we have to prove. You have one theory; I have another. Time will tell.

TRANSISTOR NUCLEAR INSTRUMENTATION FOR NAVAL APPLICATIONS

J. E. Moran

Knolls Atomic Power Laboratory

The transistor, although more than 10 years old, has come into its own as a reliable electronic device only recently. In the early years of development, manufacturers were plagued with many problems in trying to control its complicated characteristics. Today, however, they seem to have the situation well in hand. A recent publication indicated over 485 types available on the market.

However, its use in the nuclear field is just beginning. This can probably be attributed to the prime importance of reliability in this field, and to the fact that until recently the transistor had not been proved.

The transistor's biggest competitors in the nuclear field are, of course, the electron tube and the magnetic amplifier. Both of these have performed well and have demonstrated wide flexibility. The magnetic amplifier is especially useful because of its reliability.

These have a few inherent disadvantages, however, which make the transistor very attractive. The electron tube has a limited life and is relatively fragile when exposed to the unavoidable shocks in naval service. It takes considerably more power than the transistor, since it is relatively inefficient and requires filament power. The power required for electron tube equipment is perhaps three orders of magnitude higher than that for comparable transistor gear. Both the electron tube and the magnetic amplifier are relatively high heat producers, and require considerable space. This is quite significant in submarine applications where space and weight are always at a premium.

The magnetic amplifier's reliability is well proved but it is also reasonable to say that the transistor would perform with equal reliability since both devices involve solid-state materials. One advantage that the transistor has over its magnetic counterpart is response time. Magnetic amplifier response times are considered very good if they are of the order of milliseconds, while transistor circuitry operates with ease in the microsecond range. We have been burdened a lot at KAPL by the requirement that all magnetic amplifiers operate at 60 cycles. This, of course, slows

the circuit down considerably. The life of the transistor has been improved to the point where manufacturers can show test data in tens of thousands of hours without changes in characteristics. The failure rate on naval-type transistors is down to less than 0.1%.

But the picture is not all on the plus side. The transistor has some inherent disadvantages that burden the circuit designer. While most of these are not insurmountable, they must be taken into account. A reliable transistor circuit is not designed in a few days. Careful thought and a good deal of experienced trial and error are required before equipment can be said to meet the stringent requirements of nuclear applications. These are some disadvantages which must be considered:

1. Their characteristics are temperature sensitive.
2. They are damaged by radiation.
3. They are not as sensitive as electron tubes.
4. A particular type becomes obsolete rapidly.
5. Their power-handling capabilities are limited.
6. They present problems in interchangeability.

The temperature variations can be accounted for by careful design. One very successful circuit which performs within $\pm 1\%$ over a range of 0 to 65°C is the difference-type d-c amplifier. A sensitivity of less than $1\ \mu\text{a}$ has been achieved, for this temperature range, with selected transistors used in the input stage. High-quality silicon units perform well as straight d-c amplifiers for input currents of 0.01 ma or higher; a-c amplifiers perform even better. The pulse amplifier triplet is a good example of a highly stable a-c variety. This amplifier performs well in the megacycle range.

The radiation problem still exists. All transistors available on the market are affected adversely by most types of radiation. Fast neutrons create lattice defects in the material; thermal neutrons change its conductivity from an n type to a p type; and gamma irradiation creates electron hole pairs, which raise the zero signal current. The first two effects are permanent; the third is temporary. There has been considerable testing of this effect, and the evidence indicates that this is not really troublesome until an accumulated dosage of 10^{11} nvt is reached. Any area which is safe for an operator is generally good enough for the transistor. One

source estimates that the expected life, as far as radiation is concerned, is around 25 years when the equipment is used as an operating station. This does preclude using the transistor as a preamplifier in an instrument tube or any other application within the reactor shield.

Transistors are still not as sensitive as the electron tube, although one microampere or less is now achieved without difficulty over our temperature-range requirements. For very low currents, the use of the diode ring modulator, as proposed by Moody of Canada some time ago, is a good possibility. This device has worked down to 5×10^{-12} amp, and is sufficiently sensitive for most nuclear applications.

One particularly frustrating phase of transistor application is obsolescence. The field is advancing so fast that one cannot keep up with the type changes. This, of course, presents problems in the stocking of parts.

Power ratings of transistors are still in terms of watts, and they cannot compete with magnetic amplifiers or tubes on the extremes of power-handling capability. There are combinations of transistors and magnetic devices that can handle kilowatts. The new "controlled rectifier" offers many opportunities in the high-power field, since it really is a glorified transistor and can handle power in the kilowatt range. The present ratings of power transistors are generally sufficient for most nuclear applications.

The interchangeability problem, that is, the ability of the circuit to operate properly where components are replaced from a random selection, is especially troublesome with transistors. This is true because the transistor has four characteristics compared to two for the electron tube. The problem can be solved by proper design, however. A recent test on a transistorized pulse-rate computer involved changing over 30 transistors from a random selection. This was done twice with no deleterious effect on its operation.

I should like to show you some examples of transistorized gear that has been developed for the naval nuclear service. This program was started at KAPL about a year and a half ago to get experience and establish the feasibility of this type of equipment.

The Fairchild Camera and Instrument Corporation and the Stromberg-Carlson Company are furnishing service-test models. One basic requirement of this equipment is that it be electrically and physically interchangeable with operational gear on the SAR project; this is electronic equipment furnished by DuMont Laboratories.

Figure 18.1 shows a block circuit diagram of a pulse-rate computer (source range), developed by Fairchild Camera and Instrument Corporation. This is a very conventional circuitry block-wise. Pulses are received from a proportional counter, either BF_3 or B^{10} type, and amplified by the pulse amplifier, a group of 3 triplet amplifiers with a closed loop gain of 2000. The signal is then applied to the discriminator, which strips off unwanted noise and gamma pulses. The remaining neutron pulses are amplified by an additional triplet and applied to a binary counting circuit. This standard multi-vibrator circuit feeds uniform pulses that operate at half the counting rate of the input pulses. This is applied to the Cooke-Yarborough log integrator, which produces a d-c signal proportional to the log of the count rate in. The d-c signal is amplified and fed to the count-rate meter. The signal is also applied to the differentiating circuit, which yields a d-c signal inversely proportional to the period of

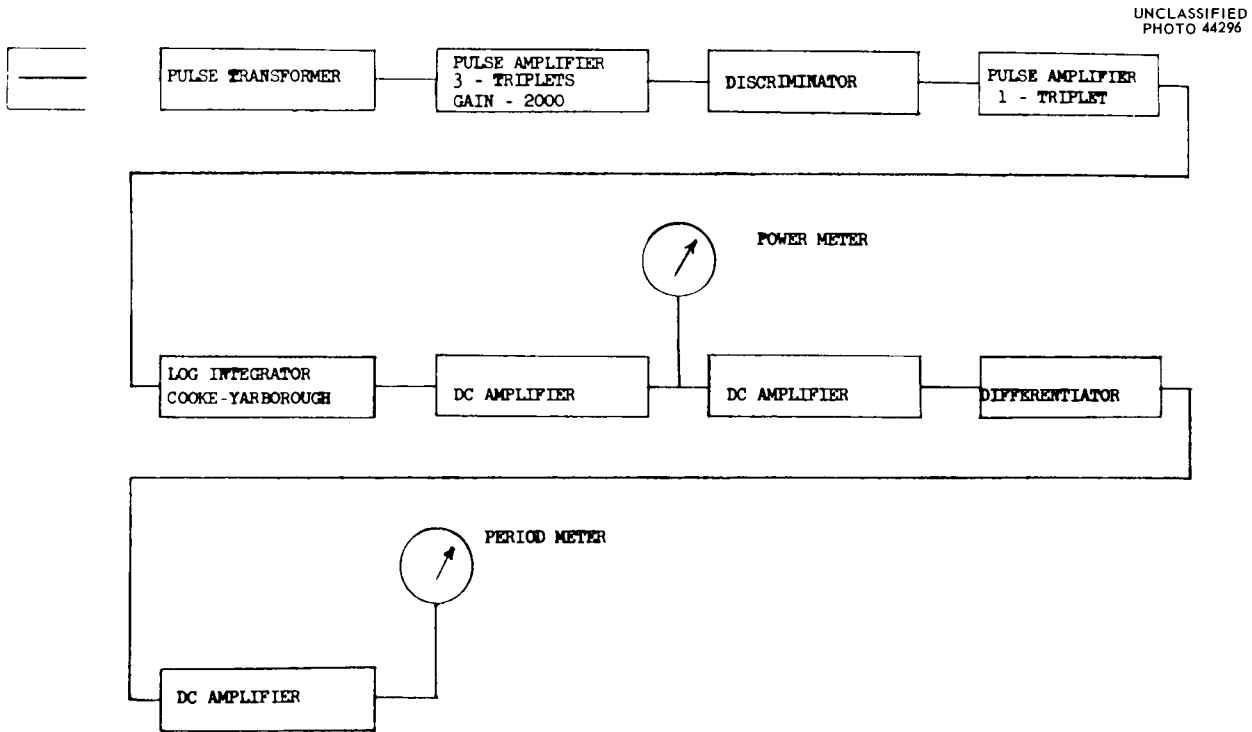


Fig. 18.1. Diagram of Transistorized Pulse-Rate Computer.

the count rate. This signal is amplified and fed to the period meter. Bottom and top views of the computer are shown in Figs. 18.2 and 18.3.

Incidentally, no attempt was made to package the components for maximum utilization of space. We had to use a drawer identical to that designed by DuMont for the tube equipment in order that the transistor gear would be mechanically interchangeable. Thus we were limited as to how we could mount components. Consequently, the emphasis was on operation rather than packaging.

The power transformers are much larger than those normally used for transistor circuits. The power supply is on the bottom side of the drawer.

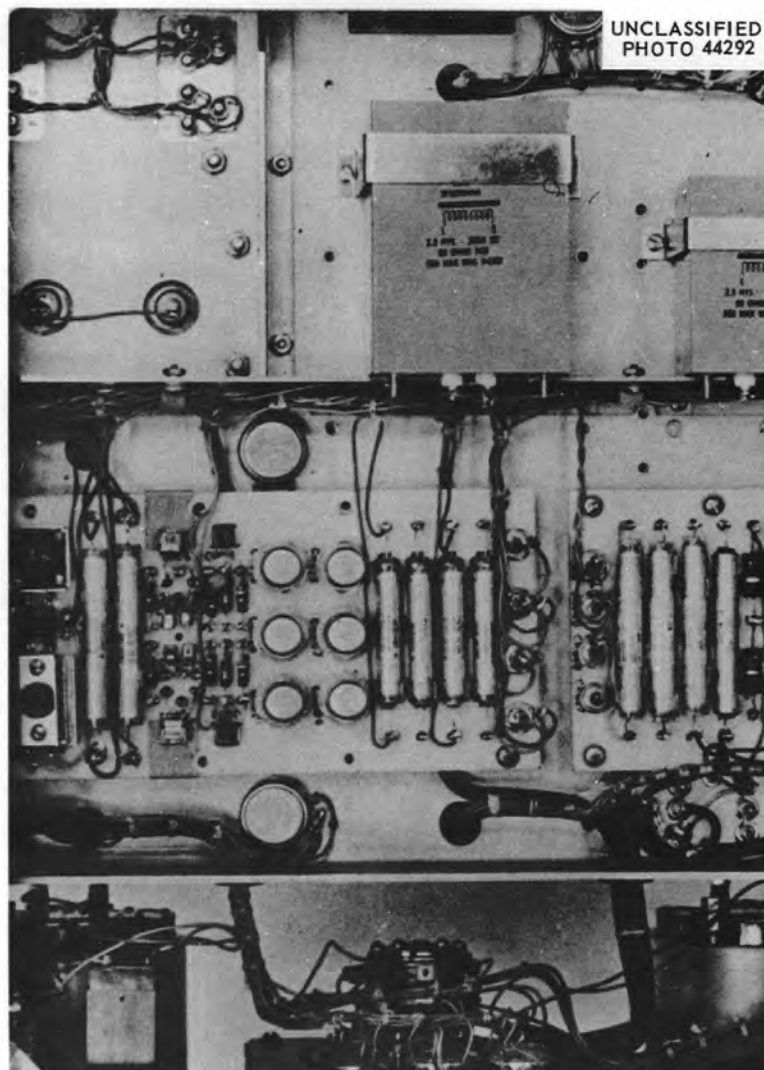


Fig. 18.2. Bottom View of Transistorized Pulse-Rate Computer.

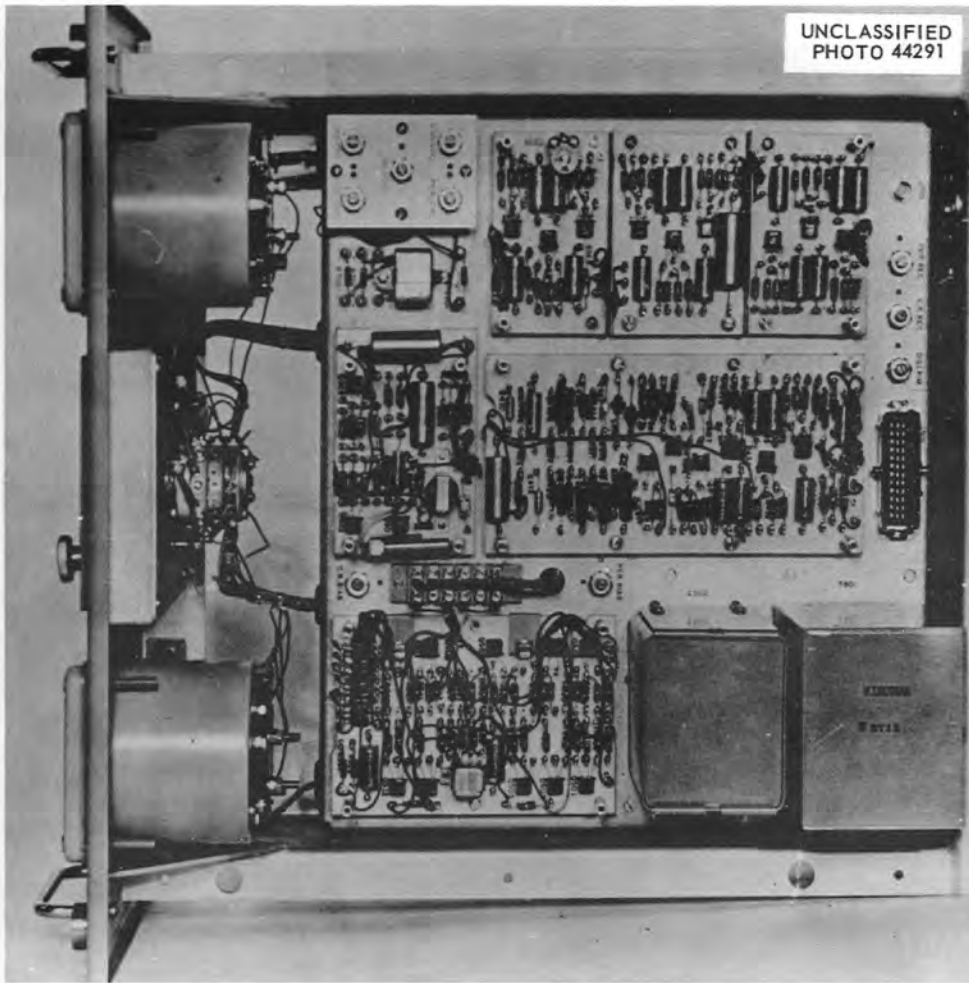


Fig. 18.3. Top View of Transistorized Pulse-Rate Computer.

The large Mylar capacitor at the upper right side of the drawer seems oversized but was needed to produce a signal strong enough to drive the period amplifier.

The equipment performs very well. It has operated approximately 800 hr. The calibration and drift stability is remarkable. I don't think we have recalibrated the period circuits once. They stay put. The other circuits have been recalibrated very slightly.

Another example of transistorized equipment is the combination electron-tube-transistor intermediate- and power-range computer, also furnished by the Fairchild Camera and Instrument Corporation. Figure 18.4 shows the intermediate-range circuit. When we started this development we did not know whether a transistor or semi-conductor circuitry could be developed which would operate at very low ion chamber currents. The currents involved are

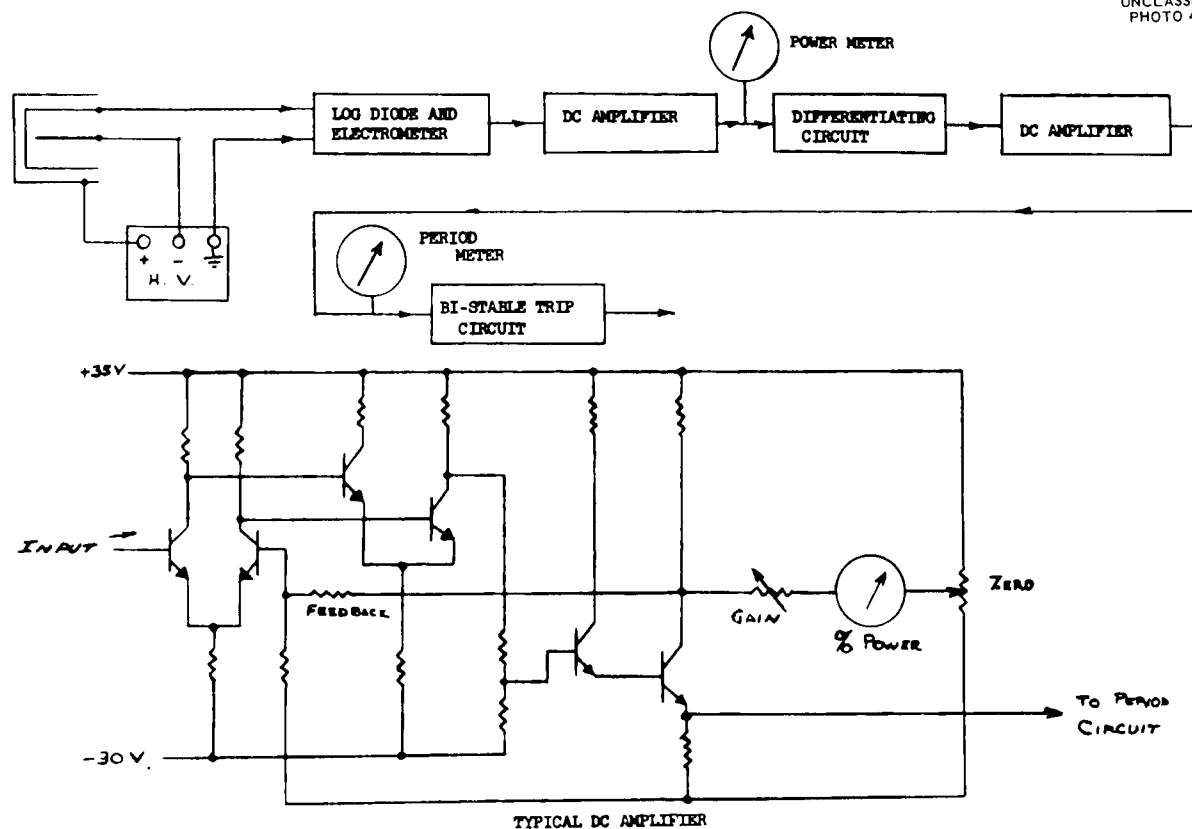


Fig. 18.4. Intermediate-Range Circuit of Combination Computer.

10^{-10} amp or less. It was decided as a backup effort to get a combination electron-tube and transistor amplifier. We used the standard log diode electrometer tube setup for the input stages. The balance of the equipment is transistorized. This is a typical d-c amplifier used in the computer. This circuitry is good to less than $1 \mu\text{a}$ from 0 to 65°C . The only requirement is that the first two transistors be selected.

Figure 18.5 illustrates the power-range circuit. Incidentally, the power-range and the intermediate-range computers are mounted on two separate drawers. We tried to illustrate the compactness of transistor circuitry by putting both channels in the same drawer. This is a d-c amplifier circuit. Extra shunting resistors are inserted in the input to provide a wide range of sensitivities. The transistor on the left produces a bias to compensate for fluctuations in thermal-induced current.

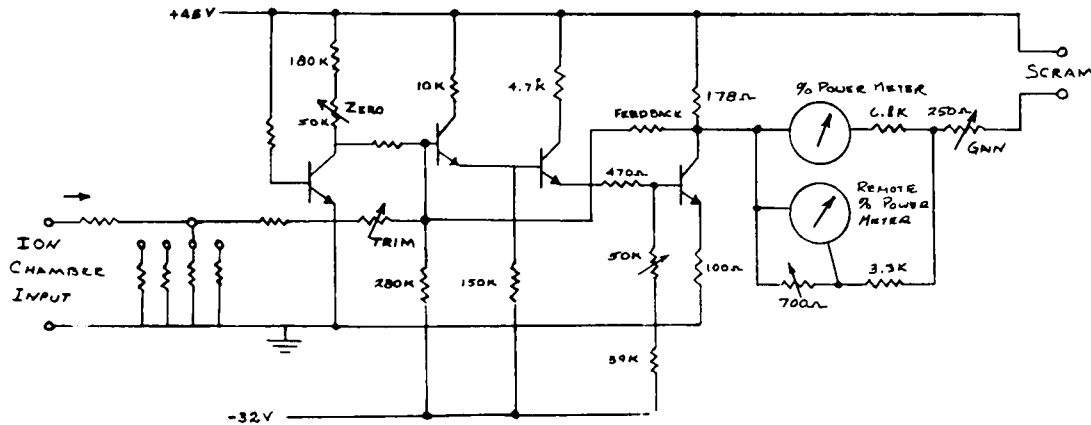


Fig. 18.5. Power-Range Circuit of Combination Computer.

The input transistor is the second transistor. Heavy feedback is used for stability and interchangeability. One of the requirements of this circuit was that any one meter could be shorted or opened without affecting the others. This was accomplished by using a combination of series and shunt resistors around the meters. An over-flux scram signal is also provided.

Figure 18.6 is a block diagram of the complete transistorized intermediate and power range computer furnished by the Stromberg-Carlson Company. The approach used by Stromberg is quite different than that used by Fairchild. This computer uses the diode ring modulator circuit (DRM). The input is fed to the DRM, which converts the signal to a-c. (The DRM includes a low noise preamplifier.) The signal is further amplified through a 5-kc a-c amplifier. A 5-kc reference oscillator supplies the DRM with a-c power. The 5-kc signal, which is proportional to the input current, then goes through the demodulator. This is a phase-sensitive demodulator rather than a straight rectifier, to ensure continuous negative feedback to the circuit. Otherwise, we might get positive feedback during transient conditions and paralyze the amplifier.

The phase sensitive demodulator is a DRM working in reverse. Current feedback is used to obtain a logarithmic response by utilizing the logarithmic characteristic of the silicon diode. The demodulator feeds the meter and the recorder, and supplies a signal to the differentiator-integrator circuitry.

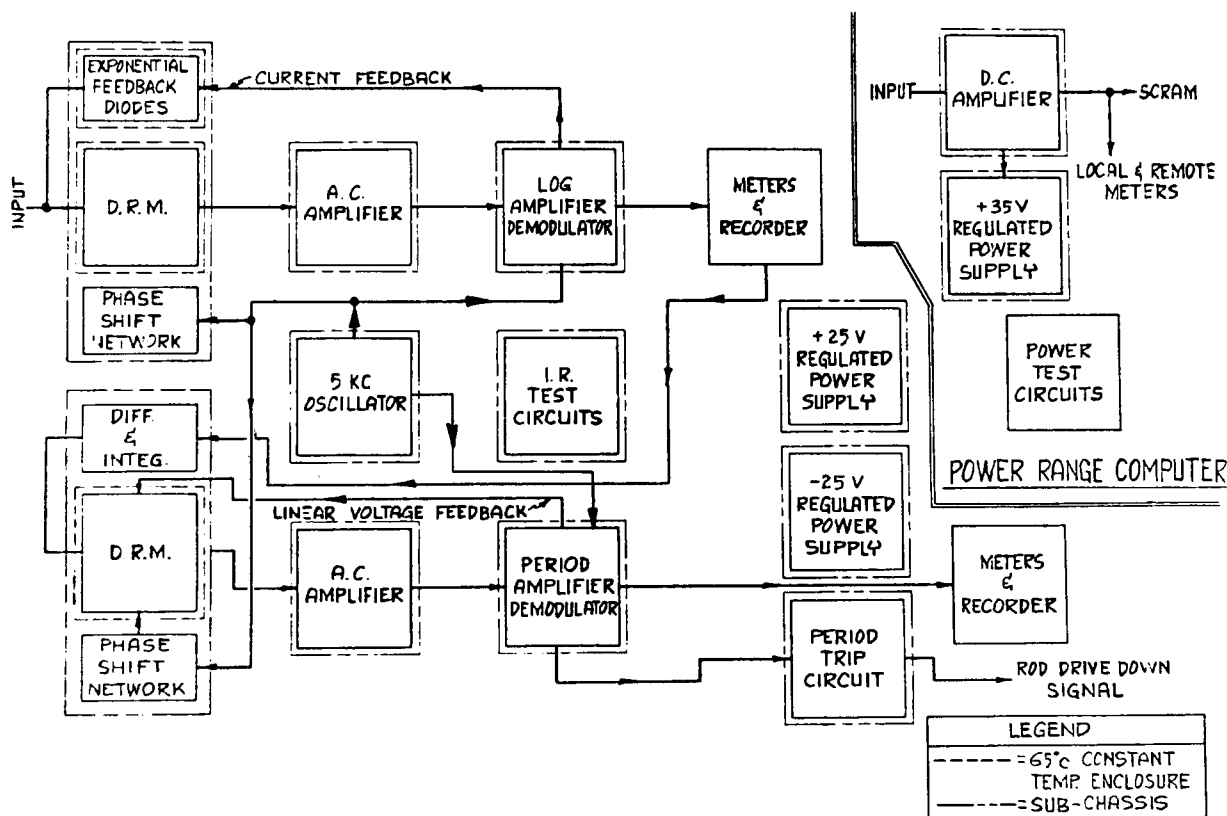


Fig. 18.6. Diagram of Intermediate- and Power-Range Computer (GETNI System).

The differentiating capacitor in the Stromberg-Carlson model is 1 μf . This relatively small value was made possible by using the highly sensitive DRM in the period amplifier. This capacitor is about 1/10 the spatial size of that used on the Fairchild model.

The 5-kc oscillator also feeds the period DRM. The period amplifier feeds the period trip circuit to the safety system, meters, and recorders. The power range is a simple d-c amplifier. Because of the relatively high currents in the power range, the required accuracy and stability were obtained by using high-quality silicon transistors in a straight compound-connected amplifier. Temperature-compensating techniques were not required. The sensitivity is around 30 ma and above.

This equipment, incidentally, is good for seven decades of input current, and its accuracy is to within $\pm 5\%$, as read on the panel meters. This includes those errors that are characteristic to the Navy ruggedized meters.

I would like to dwell a little on the diode modulator (shown in Fig. 18.7), which I think is the unique part of the Stromberg-Carlson circuit. These signals are fed into the DRM: a d-c input, the reference oscillator (5 kc at 2 v rms), and the feedback d-c signal. This is similar to the original circuit proposed by Moody. They have made refinements which make the circuits sensitive to currents of 5×10^{-12} amp at room temperature. There are two balance adjustments, a resistance and a capacitance. Stromberg-Carlson found that they did not have to use selected diodes.

Figure 18.8 shows how they package the ring modulator. It is about 3 in. long, about 1-1/2 in. wide, and 1-1/2 in. deep. The diode ring modulator is on the left, and on the right is the low-noise a-c preamplifier, which uses a Raytheon low-noise transistor.

Figure 18.9 is a closer view showing how the diodes are shielded for interference and protected against shock. All the internal parts of the DRM are potted in epoxy resin. The preamplifier is not potted. The modulator operates very well under shock and vibration and in the range of 0 to 65°C.

There is one difficult problem with this circuitry that has not been solved because of time considerations. In the higher range of temperatures (45 to 65°C) a phase shift occurs in the modulator due to a resistance

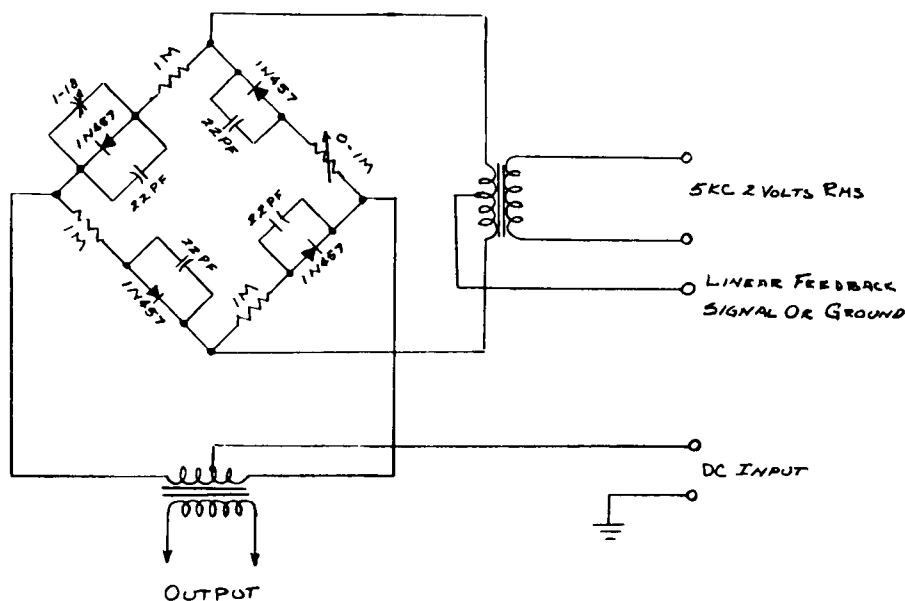


Fig. 18.7. Diagram of Diode Ring Modulator.

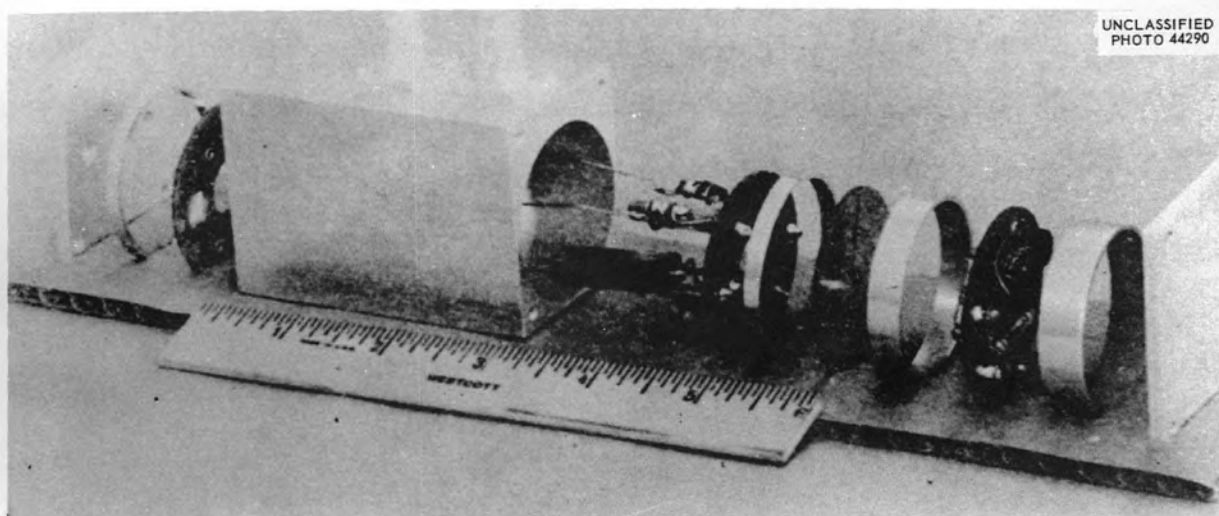


Fig. 18.8. Components of Diode Ring Modulator.

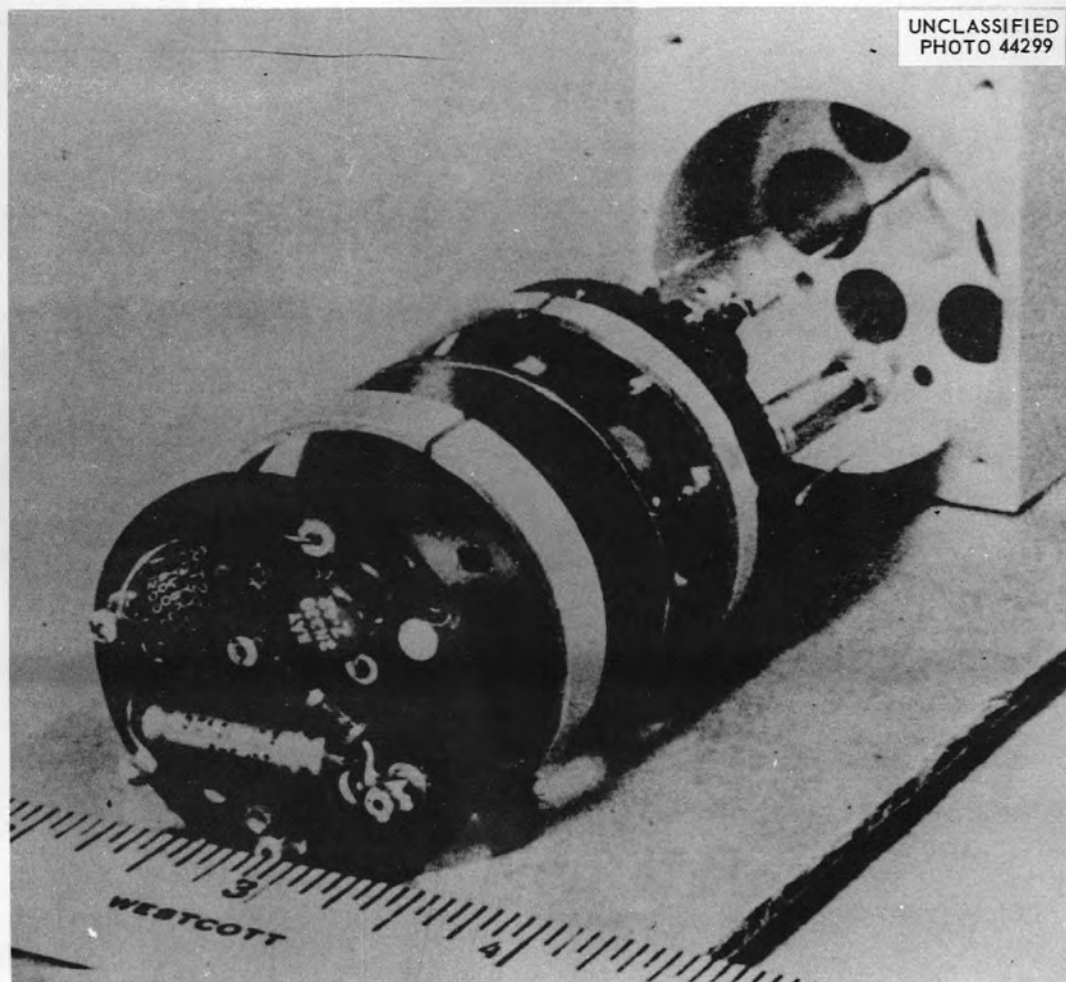


Fig. 18.9. Part of Ring Modulator, Showing Diode Shielding and Shock Protection.

change. This results in serious errors in equipment at the high temperatures. It was found necessary to keep the DRM and the log feedback diodes at 65°C by the use of electrical heaters.

The log N amplifier response is shown in Fig. 18.10. This includes all the a-c and d-c converters. The plot is for input currents from 10^{-10} to 10^{-3} amp. The threshold current is limited by the 65°C requirement. If the temperature were limited to 45°C, ten times the sensitivity could be obtained.

Figure 18.11 illustrates the temperature drift of the period circuitry of the Stromberg-Carlson intermediate range.

Figure 18.12 is a drawing of the Stromberg-Carlson intermediate and power range drawers. All of this is still in the breadboard stage. This is the same drawer that is used for the Fairchild equipment. Here we are only using 2/3 of the drawer space. If they didn't have these big Navy meters to contend with they could cut down much more. The sketch on the

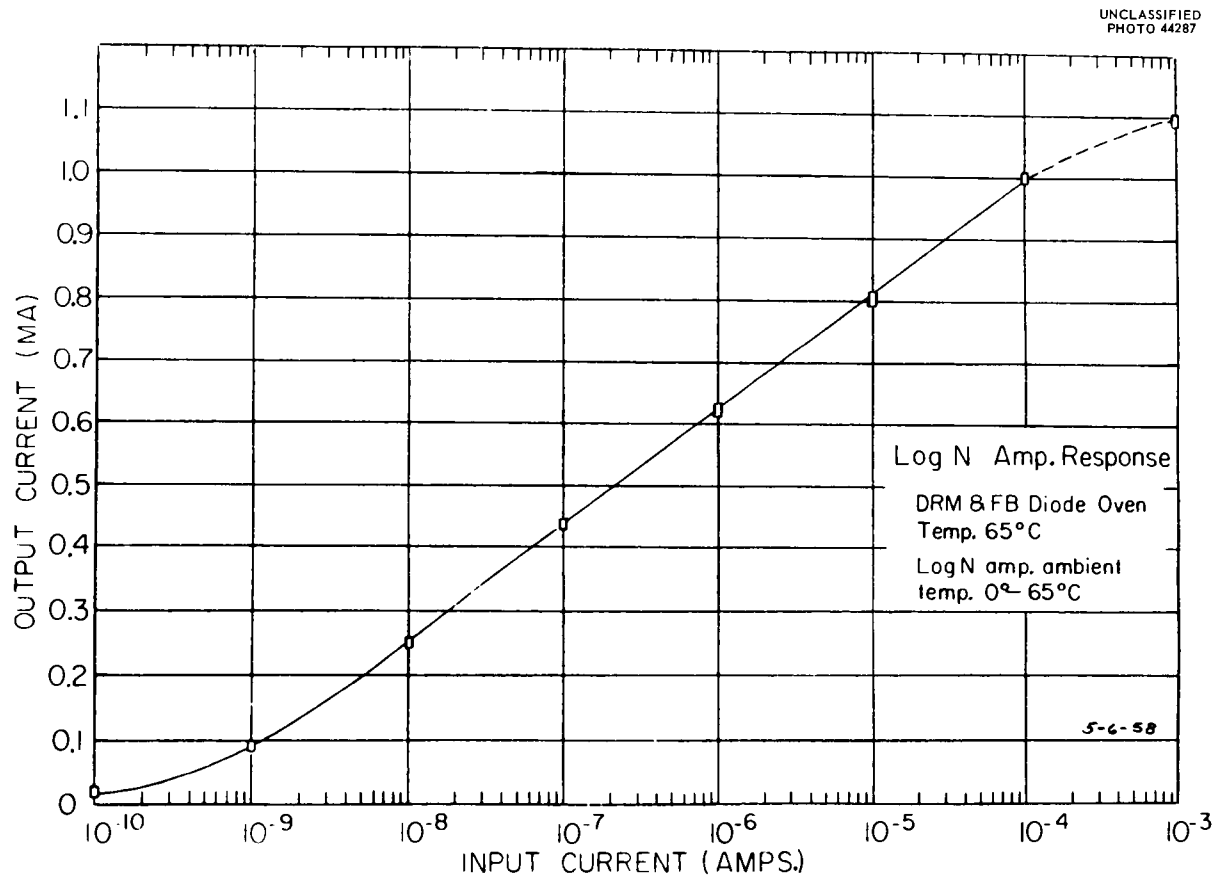


Fig. 18.10. Log N Amplifier Response.

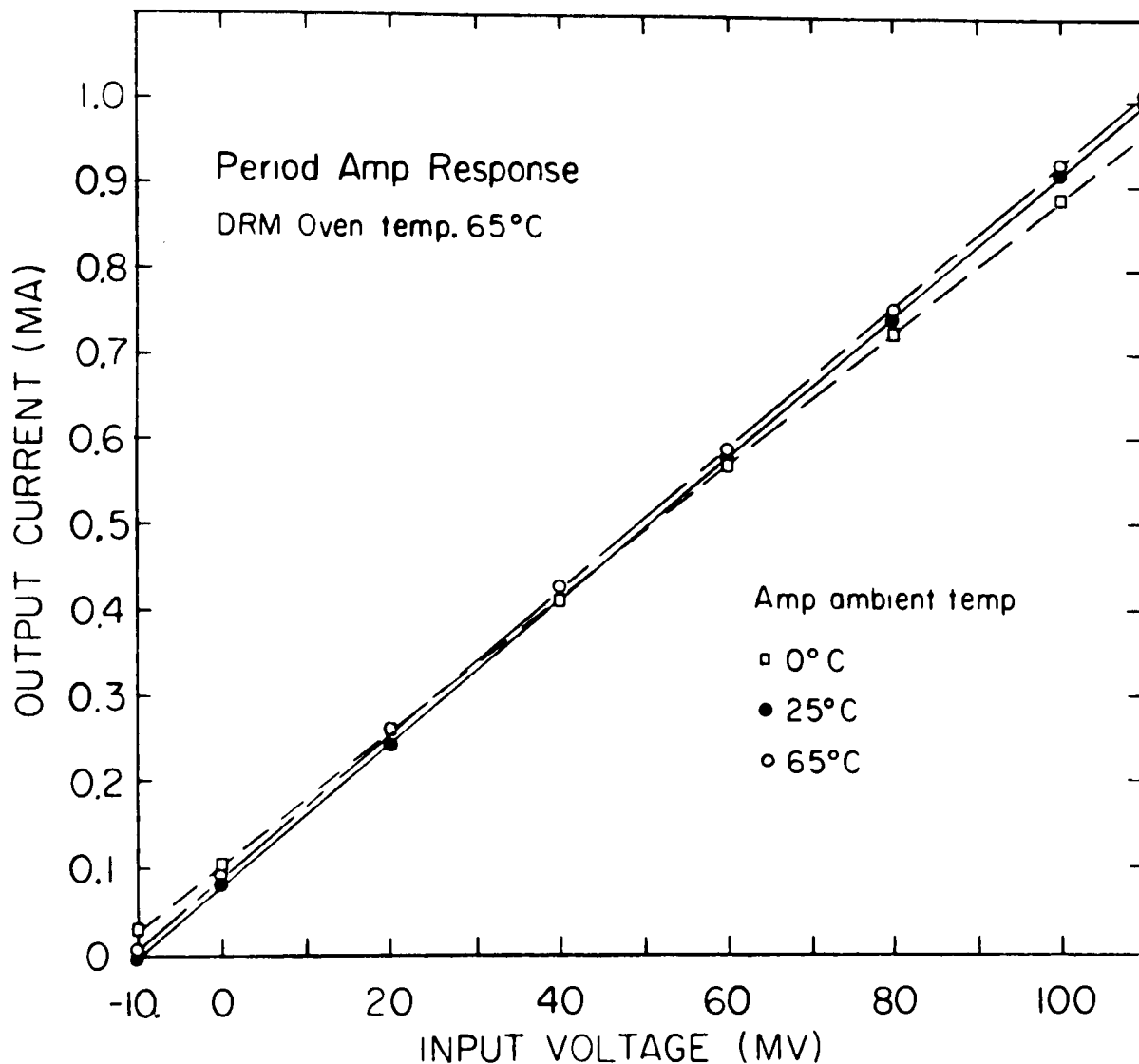


Fig. 18.11. Temperature Effects on Period Amplifier Response.

right shows how they package the individual components in aluminum cans. Inside these cans are Fiberglas boards on which the components are mounted. Printed circuits and turret lug soldering are employed. On the bottom of the sub-assemblies are regular barrier-type terminal boards. We would have liked to have used the quick-disconnect plugs, but the Navy has found this type of connection a troublemaker, and would rather spend a little more time removing leads.

Figure 18.13 illustrates rod-position indicating system components. This is a Ford Instrument Company product. It consists of a long inductance

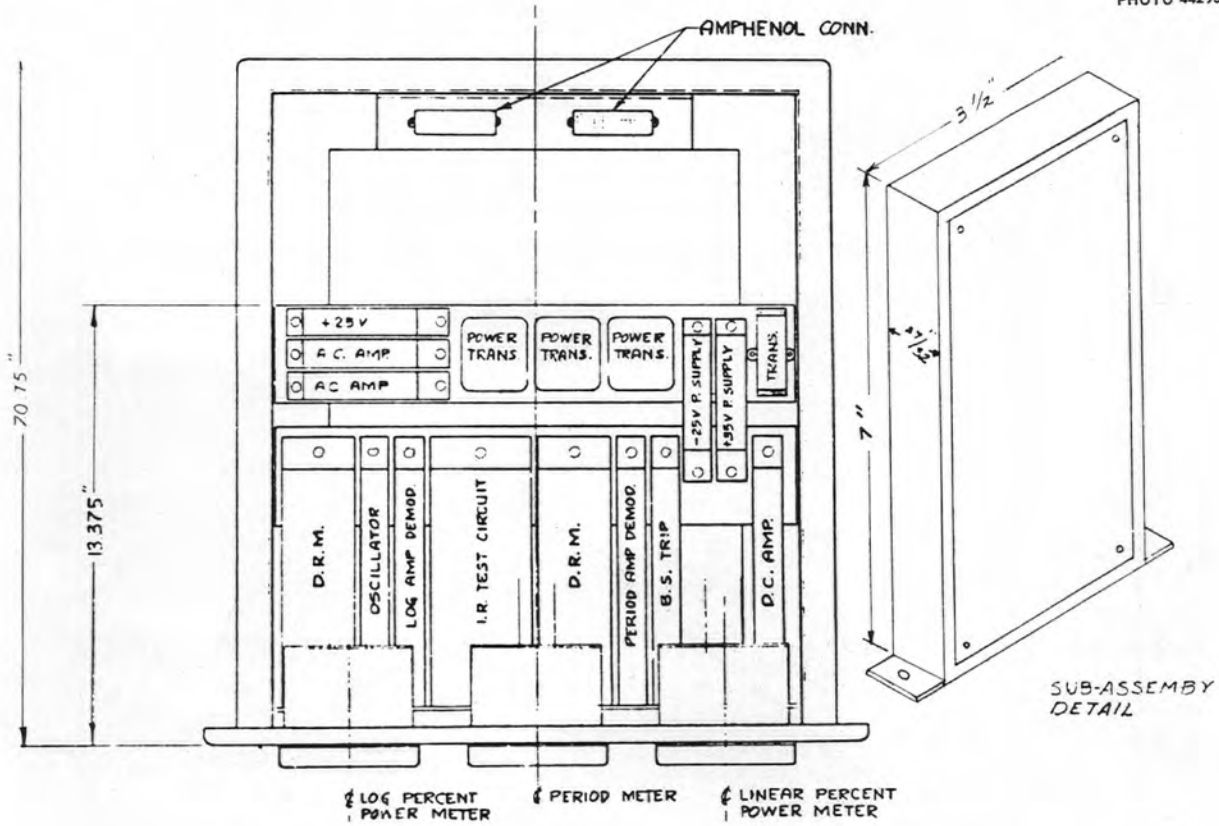


Fig. 18.12. Drawers for Intermediate- and Power-Range Computers.

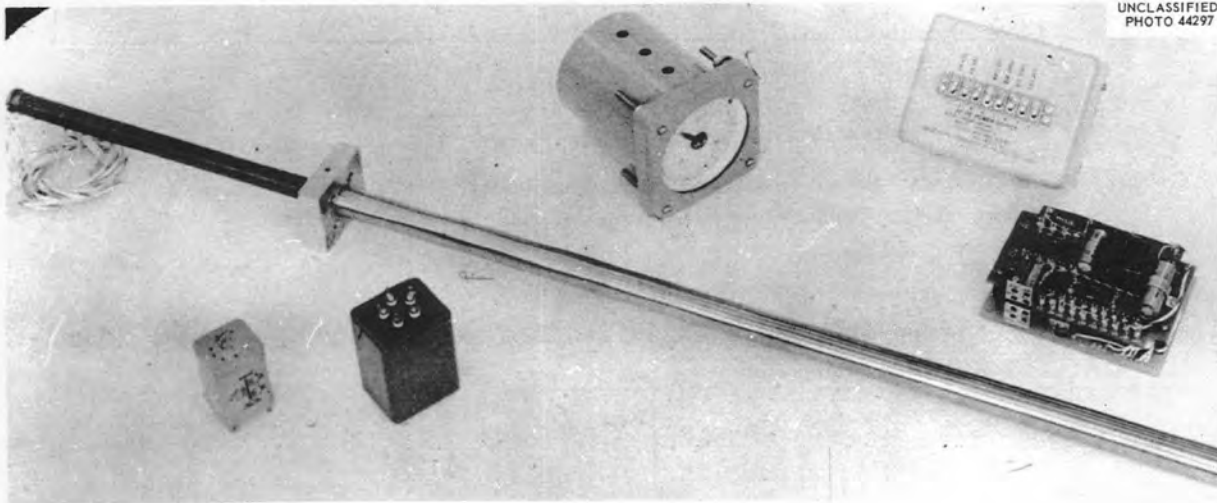


Fig. 18.13. Rod-Position Indicating Components.

probe, a servo amplifier, an a-c bridge, and a power supply. As the reactor control rod moves up and down it changes the inductance of the probe. This change in inductance unbalances the bridge, causing the readout servo motor to move and rebalance the bridge in the standard feedback configuration.

I think this is the first instance where transistors were used in naval reactor control service.

Another transistorized piece of equipment that is now a part of the naval service is shown in Fig. 18.14. This is radiation-monitoring equipment made by Fairchild Camera and Instrument Corporation. This model is an air-borne particle detector. Air is drawn by a pump through filter paper within the equipment. Photomultiplier tube-scintillation counter techniques are used for reading the amount of radioactivity in air-borne

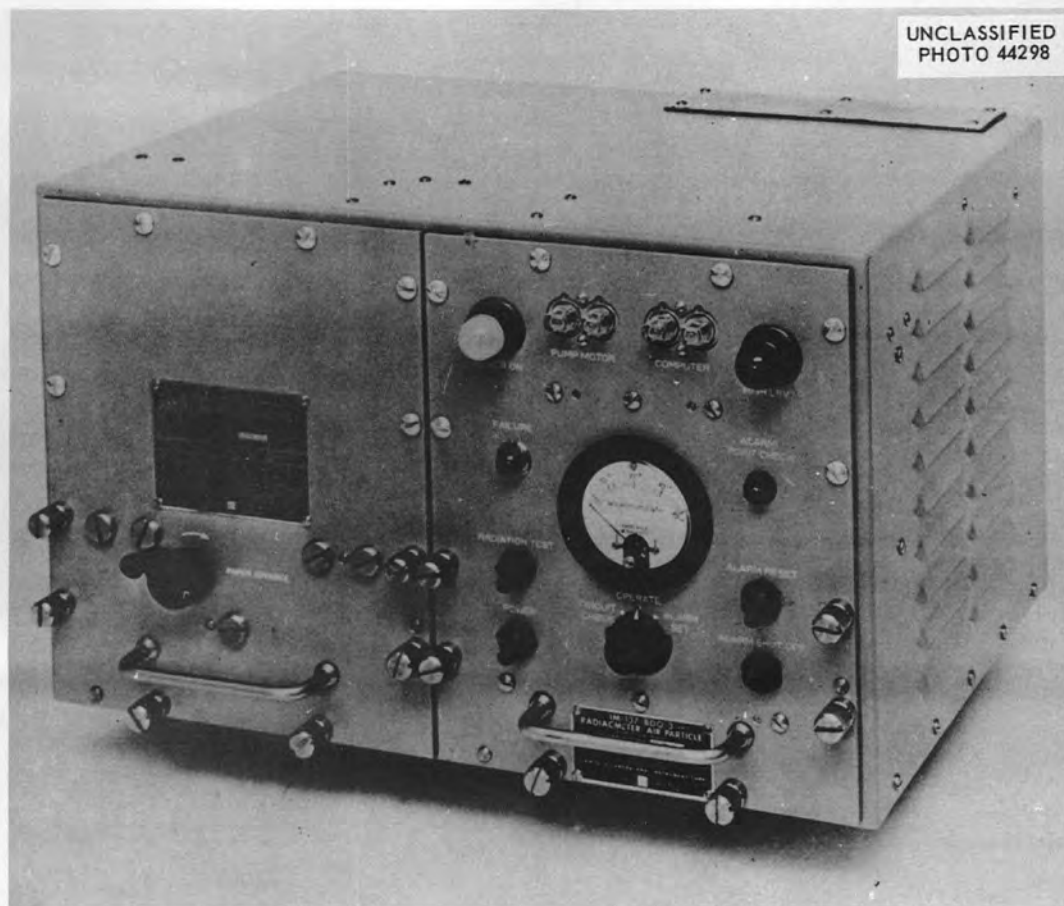


Fig. 18.14. Radiation Monitor.

particles. I would like to show you a view of the inside of this equipment but unfortunately this was the only picture that was available. Fairchild has done an excellent job of providing access to the components for maintenance purposes. We don't think that much maintenance will be required, but we can get to the parts if necessary.

The transistor seems to have a good future in the nuclear field. As transistor development improves, we can hope to see transistor circuitry applied in ever-increasing numbers to the control and instrumentation of nuclear reactors.

E. F. WASEM: You mentioned that you wanted to make the equipment electrically interchangeable with your DuMont equipment. In your control output, were there any particular problems with the magnetic amplifiers?

J. E. MORAN: We have considerable difficulty in working with magnetic amplifiers. The control windings of the amplifiers for which we are supplying signals have peak voltages as high as 8 v feeding back to the transistor equipment from the gate windings. This raised havoc with our relatively low-voltage circuits. In the case of the bistable trip on the period circuit the feedback voltage actually made the circuit trip by itself. We used clipping techniques to eliminate this. We can handle up to 3 v rms noise on the control windings very easily.

E. F. WASEM: On the Fairchild equipment you didn't get a transistorized intermediate range? Is that correct?

J. E. MORAN: In the first two stages, the electrometer tube and the logarithmic diode are tube types.

E. F. WASEM: On the rod position indicator that you showed last, what was done to minimize the effect of stray beams? I understand that it is quite a bit better than some of the previous equipment?

J. E. MORAN: I don't know what has been done on this problem. I know they have improved the equipment considerably. This is a Westinghouse Development. Maybe Mr. Madsen can help me.

C. J. MADSEN: I think that might be properly deferred until this afternoon's session. It is quite involved.

J. L. ANDERSON: What did you find as to the over-all drift stability of the Stromberg-Carlson equipment?

as long as we can to see how it operates. If we have trouble with it we will know why.

W. P. DiPIETRO: I am primarily interested in your personal experience.

J. E. MORAN: Well, as I said, I have found that it is a hard job to get a design together so that it will operate properly at the temperature variations. But the short experience that I have had with this equipment is that once you get the thing together it does very well.

J. E. MORAN: We have not had a really good test. They tested the over-all drift for two weeks, in which it stayed within its prescribed accuracy.

J. L. ANDERSON: Do you have any rule-of-thumb comparison between the diode ring circuit and the thermionic diode?

J. E. MORAN: No. I have found that the drift in all the semiconductor equipment is negligible. Do you mean time drift or temperature drift or both?

J. L. ANDERSON: Both. You mentioned that you had temperature-stabilized the feedback diode; this indicates that they are temperature-sensitive. I am wondering in terms of the time drift also.

J. E. MORAN: I didn't give you numbers but the time drift has never been a problem in any of the semiconductor equipment. Generally temperature drift is the one to worry about. One of the circuits operated 600 hr without adjustment. I don't know why the DRM-logarithmic feedback circuit would not work the same way.

C. J. MADSEN: I notice that you are using printed circuitry. Have you seen any indication, say, of resistance to the use of printed circuitry in the naval applications that you have mentioned? In some of our work we have encountered some opposition.

J. E. MORAN: No, not in the transistor work. I don't know what happened on the readout system. There may have been a problem there. In our work we have not encountered one.

W. P. DiPIETRO: We were thinking about transistors in this field a couple of years ago. The feeling I got was that the transistor was not here as a reliable, stable device, and even with the diodes, as late as two years ago, selection was necessary in order to get a reasonable product. Do you feel that you have come farther than that now, that you can design a piece of gear that will not require an awful lot of factory checkup before you produce it?

J. E. MORAN: Well, actually that is the purpose of the developmental gear, and the answer to that is, yes, we do feel that we can do this now. We have not proved it conclusively yet. We are going to operate this equipment continuously. We are going to take it into our prototype hull and operate it in conjunction with the electronic equipment, and keep it going

MAGNETIC AMPLIFIERS FOR NUCLEAR POWER PLANT SAFETY CIRCUITRY

K. H. Kline

Atomics International Division

Introduction

Much of the argument against the use of magnetic amplifiers for nuclear reactor and power plant safety circuitry has originated due to their slow response time compared to that of vacuum-tube circuits.

A couple of years ago a series of small two-winding saturable reactors (Ferristors) which use very-high-frequency carrier currents, considerably reducing the response time, were introduced on the market.

Basic Circuit Considerations

Four types of circuit configurations will be considered, all stemming from the basic amplifier circuit shown in Fig. 19.1 with a table of important characteristics.

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TYPE →	41-8, 11	41-9, 12	41-10, 13
Z _i AT 1 KCS (R=0)	(80 + j7) Ω	(220 + j12) Ω	(680 + j25) Ω
1-2 INDUCTANCE mh	2.5 UNSATURATED 1.0 SATURATED	10 5	40 18
3-4 INDUCTANCE μh	120 UNSATURATED 6 SATURATED		
3-4 RESISTANCE OHMS	4.5		
BAND PASS R = 1KΩ	D.C. TO 400 KCS	D.C. TO 65 KCS	D.C. TO 30 KCS

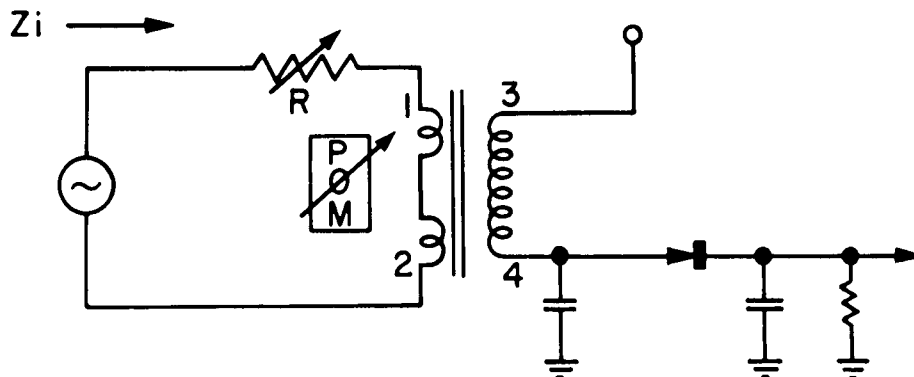


Fig. 19.1. Basic Amplifier Circuit with Table of Characteristics.

A carrier voltage is applied to a series L-C circuit, the inductance of which is the controlled winding of a saturable reactor. When no current is flowing in the control winding, the core is unsaturated and the inductive reactance of the controlled winding is high compared to the reactance of the series capacitor. Nearly all the carrier voltage appears across the inductance, and, since the output is taken across the condenser, the output is low. As current begins to flow in the control winding, the core starts to saturate, the inductance decreases and a greater portion of the carrier voltage appears across the output condenser. The voltage across the output condenser is demodulated to remove the carrier and the resultant output waveform is a replica of the input waveform impressed on the control winding. Because this resultant output voltage is greater than the input, the device amplifies.

Three types of the reactors have a small permanent magnet attached to the top of the plastic case which houses the reactor. This magnet may be adjusted spatially with respect to the saturable core, determining the quiescent operating point of the amplifier. This magnet takes the place of the usual bias winding of magnetic amplifiers.

SAFETY CIRCUIT BUILDING BLOCKS

Amplifiers

Figure 19.2 is the circuit diagram of a two-stage direct-coupled amplifier with a voltage gain of 300 and a power gain of about 20,000 with an input swing of 0 to 20 mv. The frequency response is flat within 3 db from dc to 200 kc.

Gain stability of the circuit is affected to the greatest extent by the frequency and amplitude stabilities of the carrier supply. Temperature and humidity variations of the components will also affect the gain stability to a lesser extent. Temperature variations have practically negligible effect on the saturable reactor until they are high enough to damage the magnetic properties of the core material.

Bistable Circuit

The bistable circuit is well known in vacuum-tube and transistor circuits and is used to generate sharp pulses or step outputs from slowly varying input waveforms. Figure 19.3 illustrates the use of a saturable reactor as the switching element. Triggering action is obtained by positive feedback via the feedback resistor R_{fb} and the 500-mmf condenser.

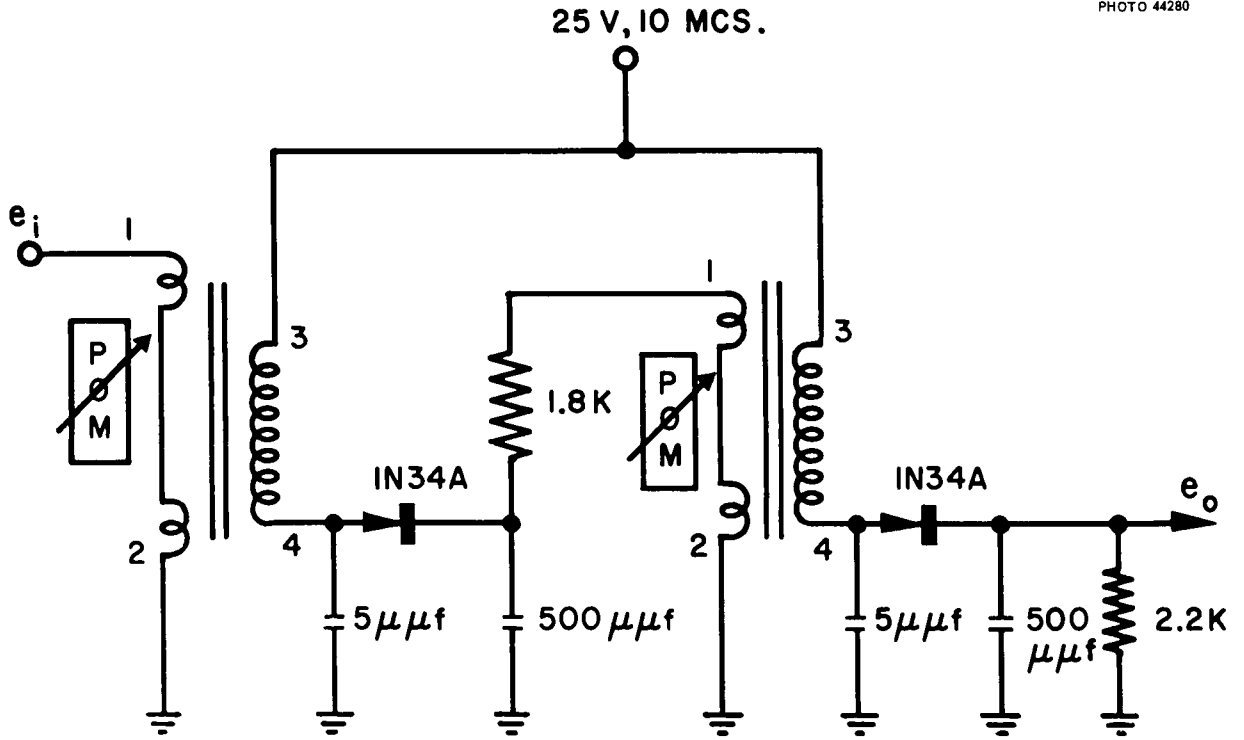


Fig. 19.2. Two-Stage Direct-Coupled Amplifier.

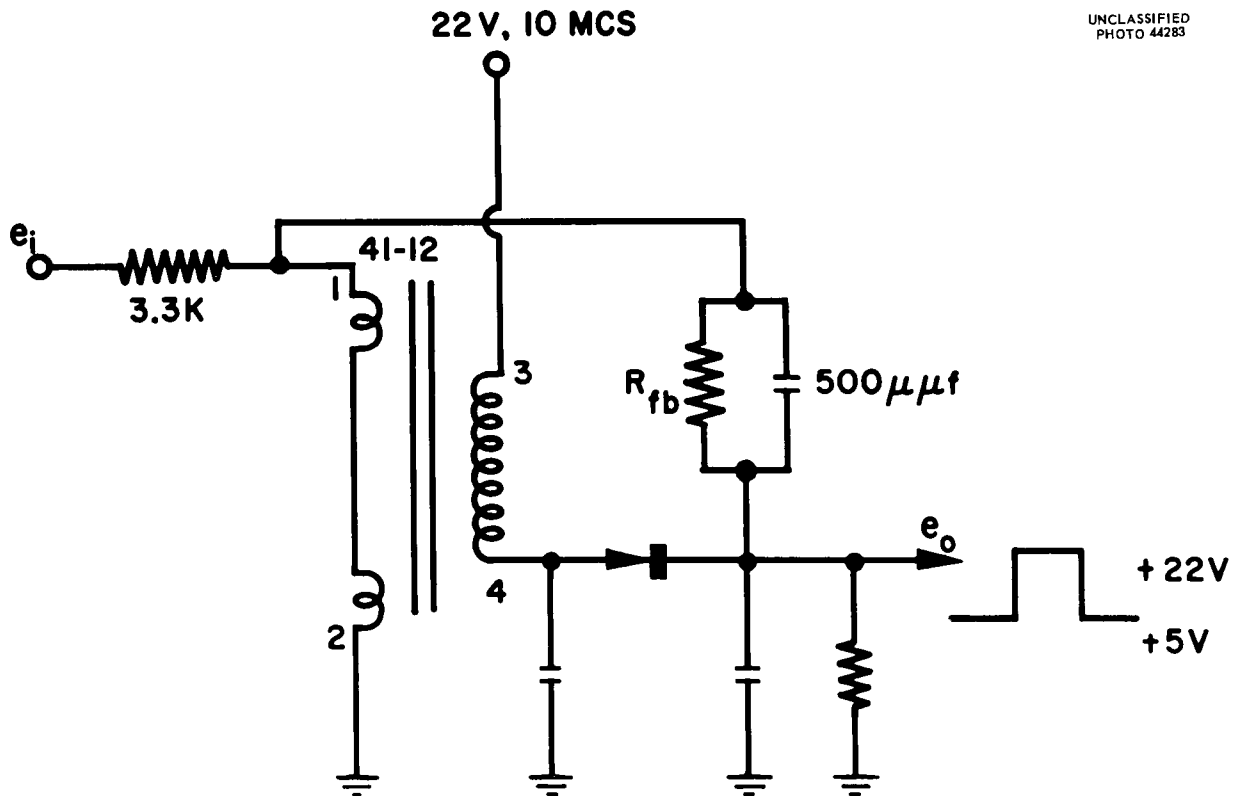


Fig. 19.3. Use of Saturable Reactor as a Switching Element.

At zero input the reactor is unsaturated and the inductance of winding 3-4 is 120 μ h. Most of the 10-Mc carrier appears across the inductance of the output winding, and the rectified output voltage is only 5 v.

As the input voltage rises, current in the control winding 1-2 increases. Output remains relatively constant until the reactor begins to saturate at approximately +5 v input. At this point, the controlled winding inductance begins to fall, and the output voltage begins to rise and further increases the control winding current by way of the positive feedback loop. This regeneration action quickly saturates the reactor and the output then becomes 22 v.

Once the output is high, signal current plus feedback current holds the reactor saturated, and reverse switching action does not occur until the input voltage drops to +3 v. When the input voltage has fallen to this value the output voltage begins to drop as the reactor becomes less saturated, regeneration rapidly returns the circuit to its original state, and the output voltage returns to the plus 5 v level.

As the value of the feedback resistor is decreased, the reverse switching point will occur at a lower voltage until a point is reached where the complete removal of input signal will not return the bistable to the off condition. This type of bistable is particularly suited to alarm circuits. Any transient of magnitude and duration suitable to trigger the bistable will then be held until manually reset. Reset action may be accomplished by opening either the feedback loop or the carrier-supply lead.

Rise and fall time of the output for the circuit shown is 2 μ sec and the circuit will respond to a maximum sinusoidal frequency of 50 kc with an R_{FB} of 8.2 kilohms.

Coincidence Circuit

The third circuit to be considered is a coincidence circuit (Fig. 19.4), which consists essentially of a bistable preceded by a diode summing device.

The signals A, B, and C are supplied by circuits with a maximum output of +22 v such as the circuit shown in Fig. 19.3. The voltage divider made from the 22-kilohm resistor and the 3.3-kilohm resistor requires that at least two inputs into the circuit shown in Fig. 19.4 have +22-v signals before the bistable trips.

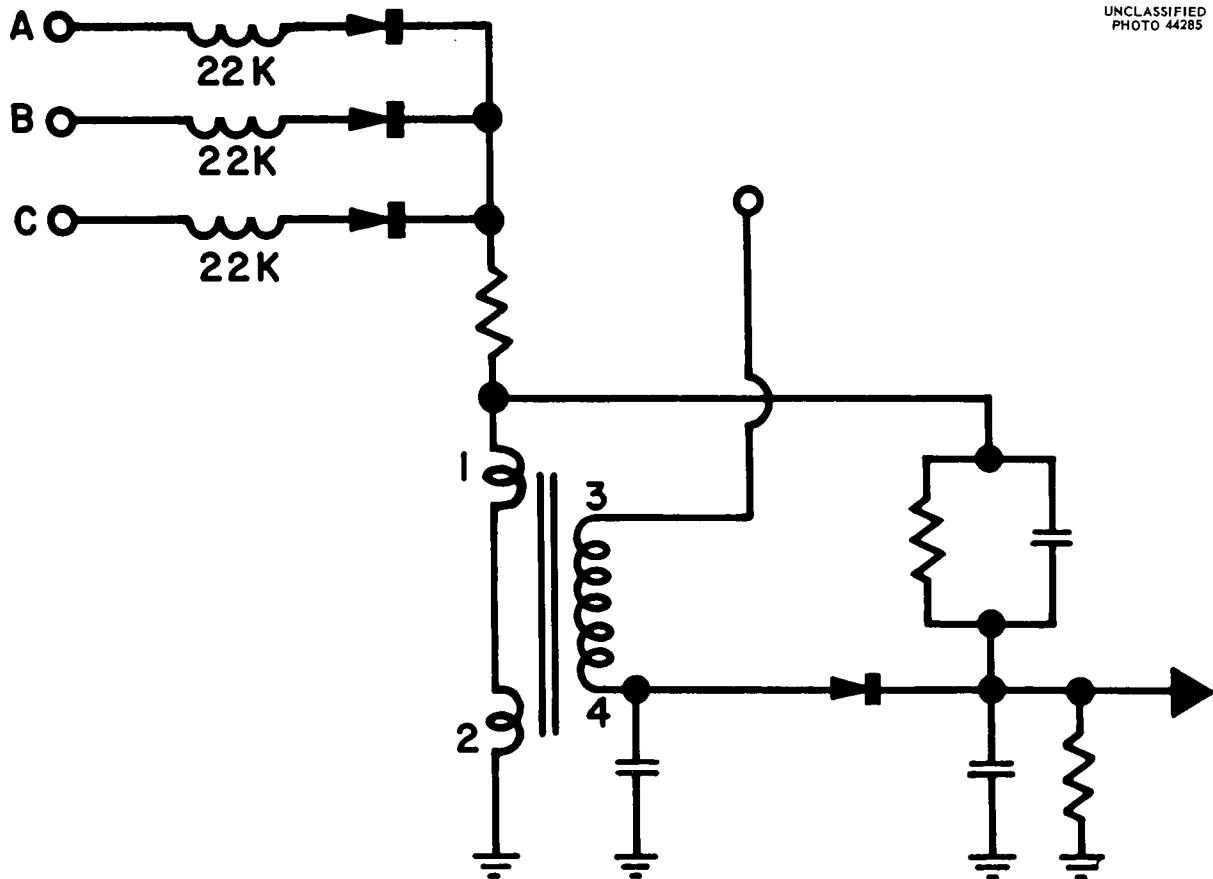


Fig. 19.4. Coincidence Circuit.

The feedback resistor of this bistable may be chosen high enough that the bistable will reset itself when the necessary input conditions for triggering have disappeared.

Differential Amplifier

The fourth small-component circuit to be considered here is a difference amplifier.

Both ends of the control winding of the basic amplifier (Fig. 19.1) are connected to signals to be compared (Fig. 19.5). If these two signals vary together in time and magnitude, the output of the amplifier will be low. However, as one signal differs from the other either negatively or positively, the output will be increased.

Systems Using These Four Basic Circuits

Now that we have four building blocks, let us look at the type of system for which they are considered, the block diagram in Fig. 19.6.

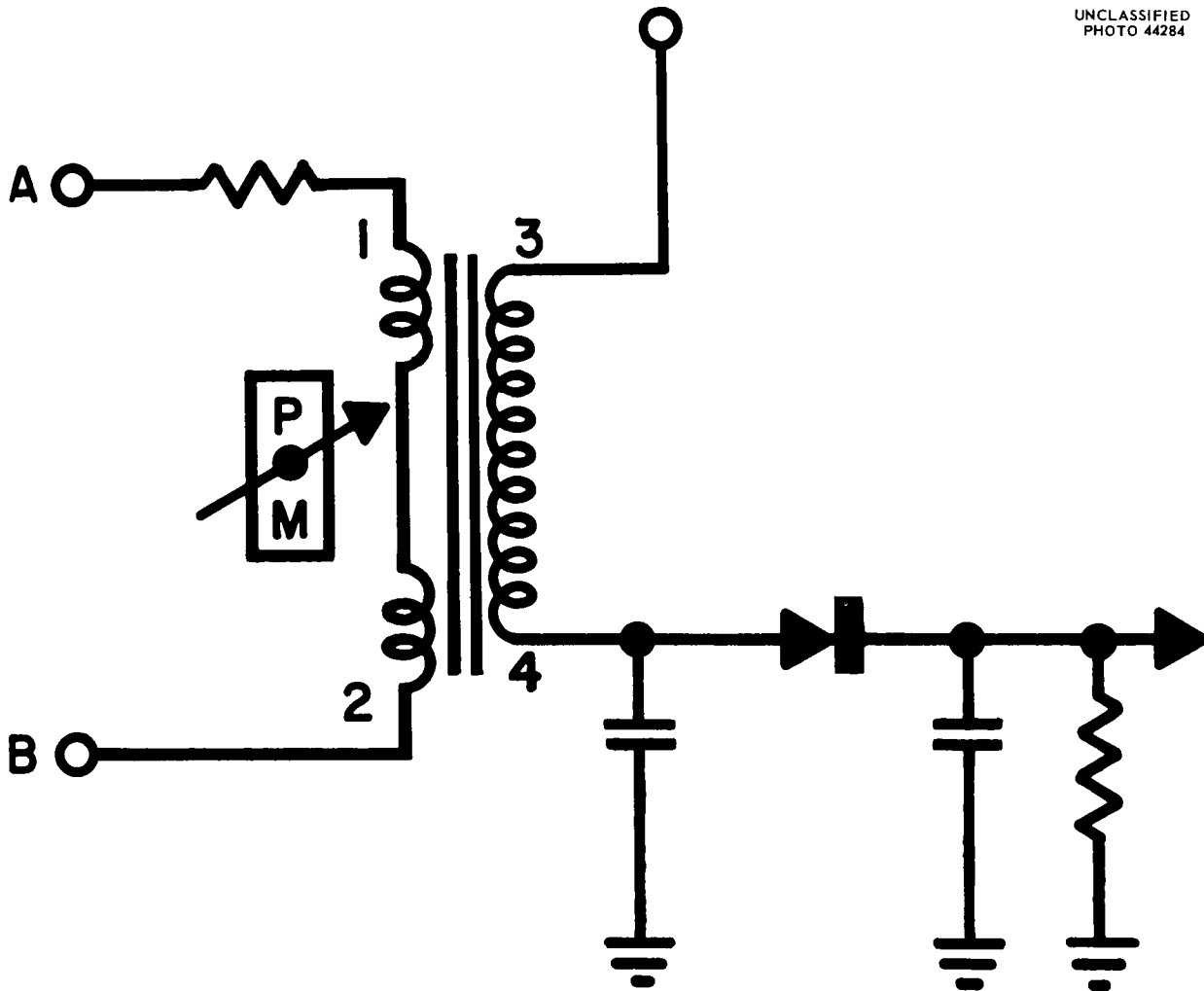


Fig. 19.5. Differential Amplifier.

The abbreviations in the blocks are as follows:

<u>Abbreviation</u>	<u>Component</u>
K	Two-stage amplifier
DK	Difference amplifier
LBS	Latching bistables
NLBS	Non-latching bistables
SK	Summation amplifier
A	Alarm or position on an annunciator
SI	Summation indicator

This is a three-channel coincidence safety circuit that requires agreement of two or more input signals that some preset danger level has been exceeded, before action is taken.

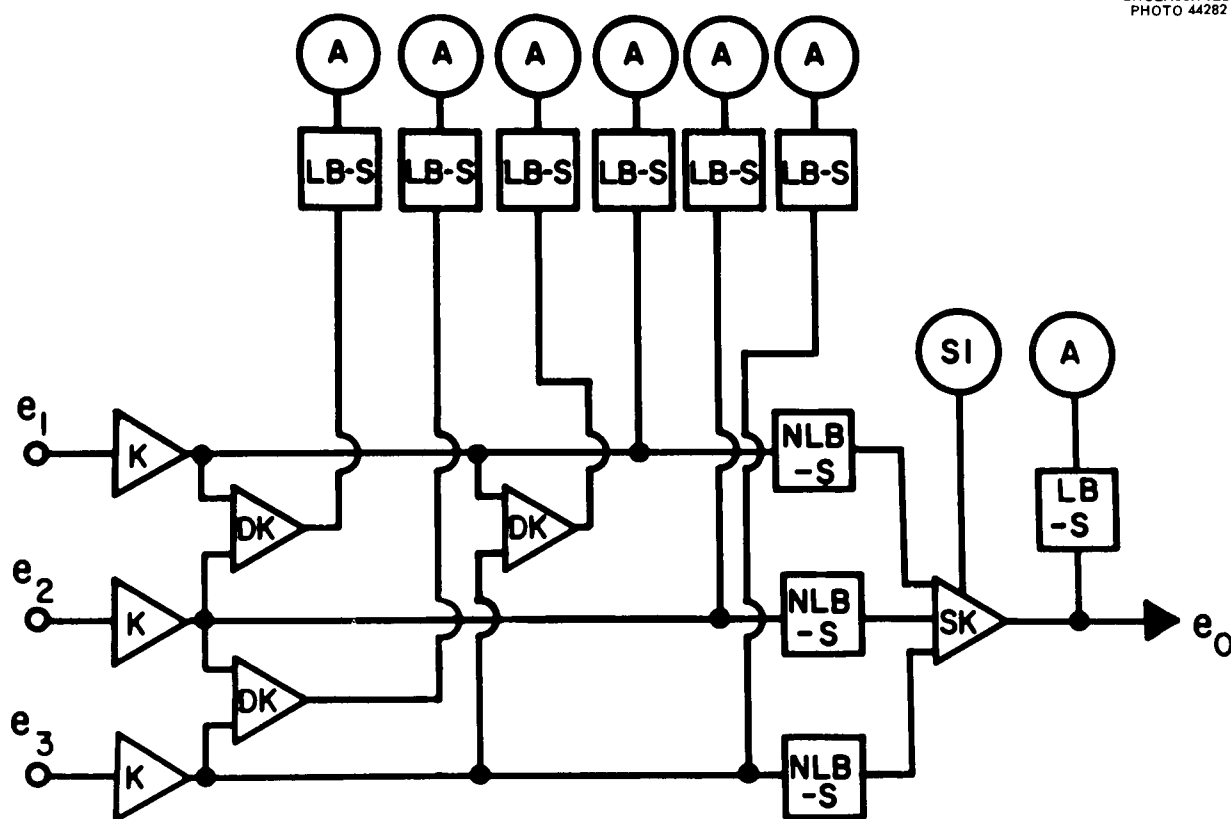


Fig. 19.6. Three-Channel Coincidence Safety Circuit.

The three input signals are from three detectors measuring the same variable. These detectors may be thermocouples, flowmeters, pressure transducers, or any device having an output in the region of 0 to 20 mv with an output impedance of 500 ohms or less.

Assume that the inputs to the channel are from thermocouples and that the temperatures they are measuring are slowly increasing.

These three input signals are first amplified by two-stage linear amplifiers like that of Fig. 19.2. Somewhere along the line as the signals increase, the set points of the non-latching bistables will be exceeded. If only one of these bistables trips, the output of the summation amplifier will jump to high. This signal may be used to initiate any corrective action which may be necessary.

So much for normal operation of the system.

Various alarm devices are placed in positions throughout the channel to indicate the following:

1. unbalance, greater than some tolerance band, between the input signals,

2. the presence of a near-action level in one branch of a channel,
3. the presence of an action (or scram) level in one branch of a channel,
4. malfunction of some portion of the system.

These alarms are of the self-locking type which, when they are actuated, require operator acknowledgement. This principle is proposed because if a self-reset bistable or indicator were used, the condition initiating the alarm may clear before the cause is determined, and therefore no knowledge would be gained. In addition, the necessity and expense of an auxiliary annunciator is eliminated with the addition of relays operated by the alarm bistables.

On the block diagram there are eight alarms and indicators. The first three alarms at the upper left of the block diagram are unbalance alarms. Since all three inputs have the same origin, as mentioned earlier, they should be equal in magnitude. If one of these signals becomes unbalanced from the remaining two, two of the difference amplifiers will operate and their bistables will trigger. The first compares e_1 and e_2 , the second e_2 and e_3 , and the third e_3 and e_1 . For example, let e_1 be the unbalanced signal; then the first and third difference amplifiers will operate and their bistables will trigger. Input e_1 will then be indicated as the faulty signal since e_1 is the only input signal common to the two bistables tripped. It is undesirable to have two indications for one fault because determination of common signal between two alarms requires the operator to make a decision that can be rendered unnecessary. Therefore, if two contacts on each of three relays, operated by the three-signal unbalance detectors, are connected in an "and" matrix (computer terminology), a single indication for a single unbalance fault is given.

The next set of three alarms are pre-scram alarms, set to operate at some level less than the scram level for the variables represented by e_1 , e_2 , and e_3 , therefore warning the operator that near-danger conditions exist.

The next alarm to the right is an alarm, set to operate when the output of the summation device of the coincidence circuit is of a magnitude that indicates two or more of the preceding signal circuit bistables have tripped. This is the channel trip alarm indicating that a coincidence of danger signals has been obtained and, therefore, that conditions exist demanding corrective action.

The remaining indicator is a summation indicator also attached to the summation network within the coincidence circuit. This indicator is essentially a voltmeter which will tell how many of the three bistables that feed into the summation device have tripped.

CONCLUSION

These circuits are self-monitoring; when they are operating properly, there is a minimum output which will disappear or be attenuated when one or more of the following happens:

1. loss of carrier,
2. reactor controlled winding open,
3. short circuit of either capacitance in shunt with output signal,
4. burn-out of rectifier.

The circuit of Fig. 19.6 is also self-monitoring from the standpoint that the unbalance detectors will indicate a fault if a primary detector, supplying an input signal to the channel, either shorts or opens. If the gain of an amplifier increases or decreases, that will also be indicated by the unbalance detectors.

The circuits may be made fail-safe if desired, by operating the system in a reverse sense. As an example, an amplifier is adjusted for maximum output with zero input by proper orientation of the small permanent bias magnet. The input signal is then applied to the control winding in such a way as to unsaturate the core; the output signal would then be a constant less the amplified input.

The one big disadvantage of the system is the requirement of a source of 10-Mc current for the saturable reactors.

The advantages of long life and immunity to all ambient variations have been mentioned.

C. J. MADSEN: Have you made any check for possible radio interference from your 10-Mc source?

K. H. KLINE: We hope that when we put a large system together - I might add that this is in the breadboard stage - we will be able to contain the 10 Mc satisfactorily so that it won't cause anybody else trouble.

C. J. MADSEN: On, say, a shipboard installation, that might be a problem.

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