GENERAL DISCUSSION

MR. WILLIAM. F. ROESER[1] (*presented in written form*).—As I see it, the problem can be divided into three parts.

The first one is the temperature distribution along the test specimen. This is influenced by the design and construction of the furnace. It can be measured as accurately as desired and it can be measured throughout the entire test.

The second part is the short-time periodic variation in the temperature at any point. This depends upon the control system. With proper design, periodic variations of 5 F in the heating element will not cause changes of more than a fraction of a degree in the specimen.

The third part is the long-time change caused by a change in the calibration of the control thermocouple. It has been shown how this can be taken care of by building in a protection tube into which a new thermocouple may be inserted at regular intervals. This can be done with either platinum or Chromel-Alumel thermocouples. Chromel-Alumel thermocouples made up from one or more pound lots do not cost much per thermocouple. If a new thermocouple is used every day, you have a record of how much the control thermocouple has changed and how much adjustment is necessary to bring the specimen to the correct temperature.

The only way of determining the change in temperature of the test specimen with time is to provide an entirely independent means of measuring its temperature.

MR. P. H. DIKE.[2]—I would like to point out that a Chromel-Alumel couple is not very satisfactory when measuring temperatures precisely. When I say "precisely" I mean measuring temperatures within ±3 or 4 F.

I am not saying that Chromel-Alumel is not a good couple in its own field. Chromel-Alumel was introduced to measure temperatures, let us say, from 1500 F up to 2300 F, a range which it covers satisfactorily where platinum - platinum-rhodium is impractical in plant use.

For laboratory performance, such as we are talking about, where we are trying to measure temperatures to a high degree of precision, it seems to me that it is penny wise and pound foolish to use the Chromel-Alumel in place of the platinum - platinum - rhodium thermocouple. It is practically impossible to keep Chromel alloys free from inhomogeneity. Take a piece of No. 22 Chromel wire, and connect its end to a galvanometer. Put a hot soldering iron against the middle part of the wire and probably nothing will happen. Then take that wire and bend it into a loop where the iron touches it and the galvanometer deflects widely. You just cannot get away from the inhomogeneity. That is quite evident in Fig. 1 of the paper by Berry and Martin. Chromel was annealed as a coil at 800 C. The annealed wire was uncoiled, straightened,

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and welded to a platinum wire to form a thermocouple. In so doing, cold work was introduced causing inhomogeneity. The first part of the curve for Chromel, ascending rapidly, probably represents stress relief at that temperature. It does not represent a change in the composition.

I believe that where there is need for temperature control within the narrow limits desired for these specimens it is preferable to use noble metal thermocouples rather than Chromel-Alumel.

Mr. Wilks said the Chromel-Alumel lacks a bit in sensitivity below 1400 F. At these lower temperatures the iron-constantan thermocouple is superior to the Chromel-Alumel in homogeneity, and it has a sensitivity which is higher than the Chromel-Alumel. There is no reason why at temperatures below 750 C the iron-constantan thermocouple should not be used. However, the sensitivity of the Chromel-Alumel thermocouple is quite adequate even down to 0 C with modern instrumentation.

MR. JOHN M. THOMAS.[3]—The Hoskins Manufacturing Co. manufactures all Chromel-Alumel thermocouple wire, which in turn is closely calibrated and sold by the instrument companies to their own guarantees of accuracy.

After using Chromel-Alumel thermocouples in the University of Michigan laboratories, setting up the high-temperature test section of the Ford Motor Co. Scientific Laboratory with platinum-platinum-rhodium thermocouples throughout, and from recent experience at Hoskins, it is believed that great care will be necessary to control temperature fluctuations in creep-rupture testing above 1000 F to ±5 F, regardless of the thermocouple alloy used. There are control and measuring instrument errors, lead wire errors, and others in addition to thermocouple errors. It is my frank opinion that the gain in over-all temperature accuracies in a creep laboratory from the use of platinum-platinum-rhodium over Chromel-Alumel thermocouples, if any, would be in the order of only 1 or 2 F, at considerable added expense.

The problems first encountered at Ford Motor Co. with platinum-platinum-rhodium thermocouples were the practical problems of operation. There was frequent breakage from contamination. Because of the high cost of the platinum, the thermocouples were made as short as possible, which resulted in high lead wire errors. Also, old couples were reused beyond the point of good practice. This is a tendency in any laboratory that must control operating expenses.

No laboratory should claim the temperature accuracies required in creep testing without thoroughly examining their operating conditions and results. These proceedings have pointed out the necessity for frequent re-examination of test conditions for good temperature control, regardless of the type of thermocouples used. The summaries by Mr. Wilks of American Brake Shoe, on the use of platinum-platinum-rhodium, and by Mr. Leyda of Babcock & Wilcox on Chromel-Alumel, are excellent examples of good laboratory temperature control practices and records. Mr. Wilks offered valuable information on the magnitude of errors which could be encountered with platinum-platinum-rhodium under certain operating conditions.

In the paper presented by Messrs. Berry and Martin, some rather large errors are shown resulting from some unique exposure tests of Chromel-Alumel. These errors should be considered, as they were intended, to show large errors that might be produced under very special conditions. Since the tests showing the large errors violate a

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basic principle of thermocouple operation, that of never decreasing the depth of immersion in service, they have no direct bearing on stability obtained when good practice is used. One test described in the Berry and Martin paper (Fig. 3) was a true stability test. Exposure of a 22-gage couple for about 50 days at 1470 F showed an error of $3\frac{1}{2}$ F, measured in place, which would agree closely with the stability tests by A. I. Dahl in Bureau of Standards *Research Paper RP 1278*, which include both Chromel-Alumel and iron-constantan. Instability is usually caused by preferential oxidation of elements from the thermocouple alloys. One way of improving this stability is to use a larger wire (18-gage wire has only about half the surface-to-volume ratio as 22-gage wire).

The only safe method for controlling high temperatures over long periods of time is, as Mr. Roeser has suggested, to provide a space to insert a new thermocouple at predetermined time intervals.

It was suggested in the Berry and Martin paper that Chromel-Alumel thermocouples might be aged in a furnace to produce greater stability for special use. This could be a dangerous practice. The wire, as manufactured, is specially heat treated for the best combination of stability and uniformity. Further heat treatment is not recommended.

Mr. Leyda has described his method of buying close-limit Chromel-Alumel wire from an instrument company, and then rechecking it for calibration and uniformity. On a spool of fine wire, such as 18- or 22-gage, produced in the last three or four years, one will probably find that no thermocouple made from a single pair of spools of Chromel-P and Alumel will vary more than 1 or 2 F from the average value for those spools.

If one will then use a new thermocouple for each test (at about five cents per thermocouple), use the average calibration for that lot of wire, respect the principles of good thermocouple practice, and properly check and evaluate the stability with time at temperature, relatively good temperature measurement and control should be possible for high-temperature creep tests.

MR. ROESER.—I would like to have part of the record corrected here. Two speakers have stated that Chromel-Alumel thermocouples lack sensitivity at low temperatures.

The dE/dT of Chromel-Alumel thermocouples is practically independent of the temperature. It is slightly higher at 1000 F than at 1600 F. Therefore, there is no lack of sensitivity at low temperatures.

Mr. Dike mentioned that if a piece of Chromel wire is connected to a galvanometer, bent, and touched with a hot soldering iron at the bent portion, the galvanometer will go off scale. All that means is that he makes a very sensitive galvanometer. The same effect may be obtained with a platinum wire. It may not be so great, but we must remember that the dE/dT of a Chromel-Alumel thermocouple is about four times that of a platinum-rhodium thermocouple.

I meant it when I said that the best way of accurately measuring temperatures with base-metal thermocouples is to use a new thermocouple every day. This is good practice and I think it pays off in the long run.

If you want to make accurate measurements, do not try to use a base metal thermocouple over again.

MR. EDWARD EDMUNDS.[4]—I agree that you should not use the Chromel-Alumel for long duration. However, the inquiries which we received indicate that the industry would like to have thermocouples last much longer than they do.

MR. D. L. MARTIN (*author*).—It is our

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experience that many people do misuse thermocouples. One of the purposes of our paper was to point out the order of magniture of errors that one encounters. Errors can result from inhomogeneities resulting from aging in a temperature gradient, or they may result from conductivity effects. We tried to separate these two types of errors in our study; we would not recommend the same procedure for general practice, but we conducted these tests with the idea of giving people an order of magniture of each type of error.

We feel that if you know that you might have an error of 25 C this will worry you, and we hope it does. An error of 2 C will not likely worry you, but too many metallurgists have made tests in which they think of a thermocouple as a tool that is used and reused again and again. Even with platinum couples you have to be careful, and if you are using Chromel-Alumel or any other base-metal couple, then you have to be even more careful. Certainly using a new thermocouple is a better practice than reusing old thermocouples.

MR. J. M. BERRY (*author*).—There is no question at all about the heat treatment of thermocouples prior to use. This is almost sure to be bad practice since the temperature distribution during heat treatment is most unlikely to be identical with that which will be encountered during use. However, the question of whether or not to "stabilize" the thermocouple materials prior to their assembly into thermocouples is not so clear-cut. As I understand it, the Hoskins people, who have had considerable experience with Chromel and Alumel, do not recommend that the user add his own stabilization treatment to these materials, which have been manufactured under carefully controlled conditions. I do not feel competent to discuss a general recommendation that thermocouple materials ought not to be stabilized by the user prior to use. However, I think it should be pointed out that, in the specific instance reported here, the inference is clear that the heat treatment used substantially improved the thermoelectric stability of both Chromel and Alumel. As in the case of many metallurgical phenomena, the rate of change in the property being measured diminishes with time at constant temperature. If instability is defined as the rate of change in the thermoelectric emf, these data (Fig. 1) show that the instability of these materials was roughly 15 times greater during the first 100 hr than it was during the subsequent 900 hr of aging at 800 C. Put differently, 65 to 70 per cent of the change in thermoelectric emf that eventually occurred in 1000 hr had already occurred during the first 100 hr of aging.

As Mr. Dike pointed out, the uncoiling of the wires may, as a result of a strain effect, have influenced the results of the first experiments (Fig. 1), but it could not have been a factor in the experiment with the thermocouple (Fig. 3). There are two reasons for believing that this strain effect is not of first-order importance. All of the specimens were uncoiled, each one once; when differences are being considered, it seems reasonable to suppose that effects due to uncoiling would tend to cancel one another. However, the most reassuring evidence that the uncoiling effect is not large is to be found in the comparison of the results of the two types of experiments. For the materials aged separately at 800 C, there is a combined change of emf of about 700 μv in 1000 hr when the temperature difference used for measurement is 420 C. That is, due to aging at 800 C, the materials undergo a combined change of 1.67 μv per 1 C. In the second type of experiment there was a 26 C increase in calibration value when that portion of the thermocouple aged at 800 C for 50 days was exposed to a

temperature difference of about 700 C (one end in the furnace at 800 C, the other end outside but near the furnace at an estimated 100 C). Over the range 100 to 700 C, the dE/dT for Chromel-Alumel is approximately 41.7 μv per 1 C. Thus, the 26 C change in calibration is approximately equivalent to 1085 μv for a temperature difference of 700 C, or 1.55 μv per 1 C. The two results for the change in thermoelectric emf due to aging agree within about 10 per cent.

The major objective of this paper was to point out the necessity of being able to make adequate independent measurements of temperature if close temperature control is to be realized. Although it may be easier to state the requirement than to achieve this result in a practical furnace design, I believe than an ideal furnace would have the following characteristics. The furnace should have a hole through which a standard platinum - platinum - rhodium thermocouple could be inserted so that (1) its bead is in contact with the bead of the thermocouple to be calibrated, and (2) several inches of the end of the standard thermocouple are in a zone of reasonably uniform temperature (a three- or four-element standard thermocouple would be very desirable for this). It might also be desirable to have a dummy standard thermocouple to occupy the space during normal operation of the furnace so that the occasional insertion of the standard thermocouple would not change the operating characteristics of the furnace in more than a transient manner.

MESSRS. W. F. BROWN, JR., AND M. H. JONES[5] (*by letter*).—Berry and Martin have drawn attention to several important phenomena which can lead to errors in temperature measurement when using Chromel-Alumel thermocouples.

Regarding the investigation of stability under a constant temperature gradient as shown in Fig. 1 of the paper by Berry and Martin, it would be of interest to know the wire size investigated and the previous thermal history of this wire. It should be borne in mind that Chromel and Alumel wires are normally annealed by the producer in order to increase stability and to provide an oxide coat. It has been our experience that this annealing practice may vary depending on both the wire gage and the supplier. For example, one company furnishes wire of gages between 18 and 30 which are continuously furnace annealed at 1900 F, being at temperature about 1 min. Gages less than 18 are batch annealed 30 min at a temperature between 1500 and 1600 F. These two treatments probably do not provide the same stability.

Berry and Martin have shown that large errors can be developed by changing the temperature gradient along aged thermocouple wires. It would be interesting to know the magnitude of the temperature gradient encountered with the experimental setup shown in Fig. 2 of their paper.

It is shown that calibration of the control couple in the setup illustrated in Fig. 5 will vary depending on the heat loss from the furnace (that is, with the end coil power). These results emphasize the inflexibility associated with the control couple location as shown in Fig. 5. Thus, calibration will be affected by the specimen type, loading arrangement, furnace temperature, room temperature, and other variables which influence heat loss from the specimen. In addition, with most control systems and specimen furnaces, a control couple located on the specimen may result in fluctuations of specimen temperature

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about the control temperature. This is due to the thermal inertia involved in transferring heat from the furnace windings to the specimen.

It would seem that a better method would locate the control couple very near the windings in order to reduce the thermal inertia effects. The true specimen temperature could then be determined by a couple or couples welded (or tied) directly to the specimen. These couples can be made to very close specification. For example, it is possible to purchase Chromel-Alumel wire matched to yield agreement with the RP-767 curve within ±0.5 per cent from 600 to 2000 F. New couples must, of course, be used for each specimen; however, the extra charge for the premium wire is small.

For extremely long-time tests, it would certainly be desirable to ensure that a stable couple was being used to measure the specimen temperature. Either suitable stabilizing treatments should be employed or the couple periodically replaced as suggested by Mr. Roeser.

MESSRS. BERRY AND MARTIN.—The data shown in Fig. 1 were obtained by testing the same size wire as used in the thermocouple aging experiment, namely, 0.025-in. diameter. All of the test specimens were taken from spools of commercial thermocouple wire. While the details of their previous thermal histories are not known, these materials had an oxidized surface and were presumed to have been manufactured according to the normal practice for such materials. That thermal history is important, as pointed out by Messrs. Jones and Brown, is evident from Fig. 1; these materials were much more stable after the first 100 hr at 780 C than they were prior to this anneal. Messrs. Brown and Jones have pointed out one reason for believing that wires of different sizes would have different stabilities, namely, that their prior thermal history may be different. In addition to this, and because the ratio of surface area to cross-sectional area is a function of wire size, different stabilities might be expected from different wire sizes even if the prior thermal histories were identical.

The temperature was quite uniform (about ±4 C) to within an inch or so from the end of the copper tube in the furnace shown in Fig. 2. The temperature gradient was quite steep from the end of the tube to the furnace port; the temperature difference over this region was in the order of 600 C. When the depth of immersion was decreased by 6 in., that portion of the thermocouple aged at 800 C (in the uniform temperature zone of the furnace) was exposed to a temperature gradient of nearly 700 C. That is, the bead was still in the hot zone and the other end of the 6-in. "uniformly aged" portion was just outside the furnace port.

When rupture tests are being made in the setup similar to that shown in Fig. 5, new thermocouples are attached to each specimen as recommended by Messrs. Jones and Brown. The procedure was described in the terms used in the paper to facilitate the presentation of an illustrative quantitative example of the conduction effect, the general implications of which are important with regard to secondary standardization. The cost of spoiled tests is high and of misleading test results even higher, and we agree that the use of premium wire may be justified. However, the fact that *initial* accuracy may be high does not allow one to draw conclusions regarding the stability of the materials.

MR. E. C. CHAPMAN.[6]—The International Pressure Vessel Code Committee made the request of the Stress Com-

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mittee of the ASME Boiler Code Committee that they determine whether or not practice in this country is uniform and if laboratories are meeting ASTM Recommended Practices E 22[8] and E 85.[10] This request had come from foreign representatives on the committee, because they are aiming toward standardization, that is, international standardization of high-temperature testing practice.

I should like some comments on what the significance of the temperature variations are in the final test results. Just what do they mean, and should the present requirements be revised; should they be made tighter than they are at present, and is that likely to be done in the near future?

MR. A. W. F. GREEN.[9]—The whole question of trying to write specifications comes up over and over again. Some of us, especially in the aircraft industry with which I have been associated for many years, who have been attempting to write specifications for control of our materials and processes have leaned heavily on such organizations as ASTM for guidance in testing procedures. Some of us have been members of ASTM for many years and have participated in some of the actions of this Society, and are cognizant of questions concerning interpretation of procedures.

But the fact remains that there must be some further definition of temperature control with reference to elevated tension testing procedures. Those of us in the aeronautical field have been utilizing Recommended Practices E 21,[7] E 22,[8] and E 85,[10] and there are differences in the plus and minus control requirements between Recommended Practices E 21 and E 22 *versus* E 85. There are those who claim that the tolerances in E 21 and E 22 are not realistic and cannot be maintained.

However, I think there is one basic fault. We have failed to indicate, too frequently, that we are talking about *indicated temperatures*. If we can get that straight, I think we will have ironed out many misconceptions that go along with elevated temperature testing.

The facts that I gleaned from these discussions are that laboratories conducting long-time tests under continuous technical surveillance and care are showing that they are controlling *indicated temperatures* within the control limits of the specifications, in fact, well within those of the Recommended Practices E 22 and E 21.

Therefore, I believe that before we cast any aspersions we should talk the same language and have our understandings basically correct. I make an earnest plea that we all support the efforts of the committee to put into words acceptable procedure controls for elevated temperature testing.

I do believe that, if all of us will start talking in terms of *indicated temperature* and then adjust things along the line with that, we will have arrived somewhere.

As I travel from coast to coast in the aircraft industry, I find more and more of the laboratories carrying on elevated temperature tension testing work. It seems that considerably more attention is being paid to relative short-time elevated temperature tension test-

[7] Recommended Practice for Short-Time Elevated-Temperature Tension Tests of Metallic Materials (E 21 – 43), 1955 Book of ASTM Standards, Part 1, p. 1605.

[8] Recommended Practice for Conducting Long-Time High-Temperature Tension Tests of Metallic Materials (E 22 – 41), 1955 Book of ASTM Standards, Part 1, p. 1612.

[9] Technical Assistant to Executive Engineer, Allison Division, General Motors Corp., Plant 2, Indianapolis, Ind.

[10] Tentative Recommended Practice for Conducting Time-to-Rupture Tension Tests of Metallic Materials (E 85 – 50 T), 1955 Book of ASTM Standards, Part 1, p. 1685.

ing than heretofore, due to the necessity to obtain quality control data quickly. The so-called close temperature controls of Recommended Practice E 21 appear to be followed.

The aircraft industry is a rapidly moving one, and testing accuracy whether for material control or design evaluation is essential. As one goes from laboratory to laboratory and discusses the results of many tests and materials, one cannot help coming to one conclusion: If you are discussing test results in New England, or in the Midwest or in the Far West, there is surprisingly good correlation of over-all test data on the same basic materials whether you are talking short-time elevated temperature tension tests, or stress-rupture tests, or long-time creep tests. It is of particular interest that long-time tests of 1000 to 10,000-hr duration are being conducted within indicated temperature control limits well within the confines of the ASTM procedures. This means only one thing, namely, that there are testing procedures which permit accumulation of correlative data, and we talk a relatively uniform language as to conduct of testing.

It is my observation that when you talk this relatively uniform language you are talking about indicated temperatures. This may embrace broadly some of the idiosyncrasies that have been mentioned, that is, the differences in the lead wires, the differences in couple junctions, the conditions of lead wire bending, and all the rest of the things that can upset temperature calibrations. Such things constitute a basic technological approach to any precise temperature measuring procedures.

I do think we must keep indicated temperatures basically in mind. If we can approach anything in revisions of Recommended Practices E 21, E 22, and E 85 concerning temperature control on such a basis, we can revise these requirements to read uniformly concerning limits for such control, and continue our reliance on our instrument and equipment manufacturers along with good technological approach to the whole problem of elevated temperature testing.

MR. C. R. WILKS (*author*).—The point regarding indicated temperatures is well taken and would do much to resolve the present dilemma on acceptable control limits. Strong emphasis must be placed, however, on the deviations that can occur between indicated temperature and true temperature, the sources of these errors, and precautions to be taken to minimize them.

One approach to the degree of control to be specified is to consider this in light of the effects on properties. This is difficult to answer generally as the effects will vary with material and temperature, but the following comments may be helpful in appraising the problem.

The implications of true temperature deviating from intended control temperature are self evident and manifestly favor or penalize the material under test. This will be the case whether it results from true temperature-indicated temperature deviations basically or from intended control temperature-true indicated temperature deviations due to poor control.

Uniformity of control is particularly necessary in creep testing where low rates are being measured. Readings must be made at the same temperature daily to eliminate thermal expansion effects. More serious, however, are the effects of thermal cycling on low creep rates with increasing temperature, as pointed out previously.

The thermal cycling effect is less serious generally on shorter term-higher rate tests, implying that more latitude in permissible control limits can be tolerated.

The acceptance test requirements have considerable influence on what temperature limits and control can be tolerated. The nature of the slopes of the stress-rate and stress-fracture-time plots is such that limiting creep and rupture stresses are far less sensitive to temperature than actual rates and fracture times at given stresses.

Finally, whether the manufacturer or the consumer is making the test can have considerable import. The manufacturer, under test conditions which adversely affect properties, is only penalizing himself. If the acceptance test values are still met, his results are likely to be conservative. The consumer, however, may obtain results which reject a material that possesses the required properties, if actually tested under the required conditions, and the manufacturer is accordingly unwarrantedly penalized.

MR. CHAPMAN.—The object of the International Code Committee is to standardize practices so that test results in one country are comparable with those in another country.

It seems to me that pyrometry is the core of this whole problem, and I fear that the requirements in Recommended Practices E 22 and E 85 are inadequate until standard methods of pyrometry are adopted.

MR. ROESER.—There are some limits in these recommended practices that are not consistent with good practice.

A thermocouple can be calibrated just as accurately at 1600 or 1800 F as at 1200 F. The instrument error will be almost proportional to the temperature.

Therefore, if the uncertainty is ±2 F at 1200 F, it should not be more than ±3 F at 1800 F. It is not consistent to permit a variation of ±3 F at 1200 F and ±10 F at 1600 F.

MR. WILKS.—Our experience has been that the deviations from intended control temperature are about the same at 1200 F as at 1800 F, as evidenced by Figs. 1 and 2 of my paper. It should be emphasized that these are indicated temperatures, although every effort was made to calibrate carefully and to reduce errors from contamination to a minimum. Greater uncertainty, because of increased contamination, in the true temperature as temperature is raised would appear to be the main justification for broader temperature limits at higher testing temperatures.