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ORNL-TM-1975

PROCEEDINGS OF SECOND SOUTHEASTERN SEMINAR ON THERMAL SCIENCES

July 25-26, 1966
Oak Ridge, Tennessee

H. W. Hoffman
R. I. Vachon

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PROCEEDINGS

OF

SECOND SOUTHEASTERN SEMINAR ON THERMAL SCIENCES

July 25-26, 1966

Oak Ridge, Tennessee

Editors

H. W. Hoffman, Oak Ridge National Laboratory

R. I. Vachon, Auburn University

OCTOBER 1967

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ORGANIZING COMMITTEE

SECOND SOUTHEASTERN SEMINAR ON THERMAL SCIENCES

Herbert W. Hoffman, Chairman
Oak Ridge National Laboratory

J. Edward Sunderland
Georgia Institute of Technology

Gerald E. Tanger
Auburn University

Reginald I. Vachon
Auburn University

Robert L. Young
University of Tennessee Space Institute

PREFACE

The idea for a regional seminar on heat transfer and related topics can be credited to Professor J. Edward Sunderland of the Georgia Institute of Technology, who had the initiative — and courage — to organize the first meeting in April 1965 on the Georgia Tech campus. The stated objectives of this conference were "to stimulate research, provide a formal means for presenting research, help research workers in the Southeast to become better acquainted with work, colleagues, and resources in the geographical area, etc." To accomplish these objectives, Professor Sunderland conceived a limited number of formal lectures by prominent engineers and scientists, brief progress reports on current research at Southeastern universities and government laboratories, and informal group discussions on topics of significant interest. Importantly, he elicited the support of the National Aeronautics and Space Administration for the first conference.

The task of organizing and accomplishing this second seminar (held in Oak Ridge, Tennessee, under the sponsorship of the Oak Ridge National Laboratory) was made immeasurably easier by the firm foundation built by Professor Sunderland. To accommodate the interests of researchers in this region, the extent of the seminar was enlarged to include mass transfer, thermodynamics, fluid mechanics, and energy conversion along with heat transfer. Our name was also changed to "Southeastern Seminar on Thermal Sciences" to reflect the increased scope. The format was essentially unchanged. However, the number of progress reports included was substantially increased; and abstracts of these reports were distributed at the meeting.

This volume records the technical proceedings of this second seminar and includes the full texts of the invited lectures and the abstracts of the progress reports. The group discussions have not been included, since these were of a very informal nature and were not recorded. Unfortunately, the introductory remarks by Dr. Alvin M. Weinberg, Director of the Oak Ridge National Laboratory, was also not taped. I regret this omission and would like to express to Dr. Weinberg, on behalf of the Seminar, our appreciation for his pertinent welcoming comments.

More than a year has gone by since this meeting was held; and, while I apologize for the delay in issuing these proceedings, the passage of time has occasioned one gain. With the permission of our four lecturers, we have assigned an identification number to this report; thus, these proceedings may be referenced in your future publications.

The response of the Southeastern region to these seminars has been highly satisfactory, and it was agreed at the conclusion of this second seminar to continue on an annual basis. With this decision, the Southeastern Seminar on Thermal Sciences enters a second phase in its growth. Plans which provide for a more formal business structure, while retaining the informality of our technical sessions, are in progress.

The success of any meeting derives from the efforts of many; and so I would like to acknowledge the support of all those in the ORNL organization who worked in our behalf. Special commendation should be given to the secretaries — Jane Craig and Dolores Eden — who labored diligently in transcribing the tapes of the lectures and in typing these proceedings. Our thanks are also due to our invited lecturers — Professor Stuart W. Churchill, Dr. R. N. Lyon, Professor Warren M. Rohsenow, and Dr. Frank A. Gifford — whose enthusiasms and contributions added distinctive flavor to our sessions and to Mr. John W. Landry for his stimulating and entertaining discussion on one aspect of the peaceful use of atomic energy. My personal thanks goes to Dr. J. J. Keyes (ORNL) who served with me as a session chairman and to all of the members of the organizing committee for the Second Southeastern Seminar on Thermal Sciences. Finally, my appreciation goes to all those attending for their contributions to the fulfillment of the purposes of this seminar.

Herbert W. Hoffman
General Chairman, Second SESTS

PROGRAM

SECOND SOUTHEASTERN SEMINAR ON THERMAL SCIENCES

Monday, July 25, 1966

9:00 AM – Welcome: A. M. Weinberg, Director, Oak Ridge National Laboratory

SPECIAL LECTURES

9:15 AM – S. W. Churchill, University of Michigan
"The Present Limits on Our Ability to Predict Natural Convection and Radiation"

10:30 AM – R. N. Lyon, Oak Ridge National Laboratory
"The Fictive Film Concept"

11:45 AM – Lunch

PROGRESS REPORTS

1:00 PM – T. S. Chang, Virginia Polytechnic Institute
"Studies in Radiation Gas Dynamics"

1:15 PM – K. G. Soderstrom & C. E. Reoyo-Sanchez, University of Puerto Rico
"Free Convection Heat Transfer to Air Between Narrow Channels"

1:30 PM – J. J. Keyes, Jr., Oak Ridge National Laboratory
"Experiments on the Stabilization of Rotating Flows by Intense Magnetic Fields"

1:45 PM – F. N. Peebles & J. O. Nelson, University of Tennessee
"Turbulent Transport in a Wall-Jet Flow"

2:00 PM – C. W. Bouchillon & C. T. Carley, Mississippi State University
"A Study of Pressure Losses in Tubing and Fittings"

2:15 PM – R. W. Guy, NASA – Langley Research Center
"An Arc Heated Wind Tunnel Investigation of the Effect of Heat Transfer Rate, Low Density, and Orifice Size on Measured Static Pressures"

2:30 PM – Coffee

2:45 PM – J. T. Beard, University of Virginia
J. A. Wiebelt, Oklahoma State University
"Reflectance Studies for Film-Covered Water Surfaces"

3:00 PM – D. R. Scholz, Louisiana State University
"On the Solution of Nonlinear Equations"

PROGRESS REPORTS (Continued)

3:15 PM – D. F. Dyer, T. C. Hardin, & J. E. Sunderland, Georgia Institute of Technology
"A Quasi-Steady State Solution for Unidirectional Freeze-Drying"

GROUP DISCUSSIONS

3:40 PM – I: Desalination Heat Transfer; Evaporation-Condensation
Leader – L. G. Alexander, Oak Ridge National Laboratory

3:40 PM – II: Two-Phase Nonboiling Flows
Leader – D. G. Thomas, Oak Ridge National Laboratory

5:00 PM – Adjourn

7:00 PM – Reception

8:00 PM – Dinner Session
Special Lecture: J. W. Landry, Oak Ridge National Laboratory
"Tapping the Earth's Steam Kettle with Nuclear Explosives"

Tuesday, July 26, 1966

SPECIAL LECTURES

9:00 AM – W. M. Rohsenow, Massachusetts Institute of Technology
"Some Recent Results in Boiling and Condensing Research"

10:20 AM – F. A. Gifford, Jr., Experimental Science Services Administration, Department of Commerce
"Atomospheric Turbulence and Diffusion"

11:45 AM – Lunch

PROGRESS REPORTS

1:00 PM – G. E. Tanger & R. I. Vachon, Auburn University
"Research on Pool Boiling Response to Pressure Decay"

1:15 PM – R. I. Vachon & G. E. Tanger, Auburn University
"Research on Pool Boiling on Prepared Stainless Steel Surfaces"

1:00 PM – H. C. Roland, University of Tennessee
"Analytical and Experimental Studies of Film and Transpiration Cooling of Hypersonic Nozzle Throats"

1:45 PM – R. L. Young, University of Tennesse Space Institute
"Progress Report on Application of Similarity Techniques in Space Vehicle Testing"

2:00 PM – H. L. Atkins, NASA – Marshall Space Flight Center
"Some Areas of Thermal Physics Research in the Research Projects Laboratory, NASA-MSFC"

2:15 PM – S. S. Chehl, Southern University
D. R. Scholz, Louisiana State University
"Transient Thermal Stresses in a Cylinder with Internal Heat Sources"

2:30 PM – Coffee

GROUP DISCUSSIONS

2:45 PM – III: Combined Radiation and Convection
Leader – F. Shahrokhi, University of Tennessee Space Institute

2:45 PM – IV: Advanced Energy Conversion Systems
Leader – A. P. Fraas, Oak Ridge National Laboratory

3:45 PM – General Session: Discussion of Format and Plans for Future Seminars

5:00 PM – Adjourn

SPECIAL LECTURE I

THE PRESENT LIMITS OF OUR ABILITY TO PREDICT NATURAL CIRCULATION AND RADIATION

Professor S. W. Churchill, Chairman
Department of Chemical and Metallurgical Engineering
University of Michigan
Ann Arbor, Michigan

INTRODUCTION OF SPEAKER

J. J. Keyes (ORNL), Chairman: To lead off our seminar this morning, we are fortunate to have Professor Stuart W. Churchill, Chairman, Department of Chemical and Metallurgical Engineering, University of Michigan, and currently President of the American Institute of Chemical Engineers. Professor Churchill has been at the University of Michigan since 1947, having obtained his doctorate there in chemical engineering in 1952. Incidentally, he has an undergraduate degree in mathematics which may account for the emphasis on analytical techniques you will find in many of his publications. He has also had experience in the chemical industry and he has distinguished himself in research, as an educator, and as an active spokesman for his profession through responsible duties, positions, and editorial responsibilites in the American Institute of Chemical Engineers. As a matter of fact, he was recipient of the 1964 Professional Progress Award of that Institute. He has contributed significantly in the areas of combustion, radiation, and natural-convection heat transfer, mass and momentum transfer, high-temperature chemical equilibrium, two-phase flow, and mathematical techniques for the solution of problems in these particular areas. His subject this morning, "The Present Limits of Our Ability to Predict Natural Convection and Radiation," is certainly timely. I wish to introduce now Professor Stuart Churchill.

THE PRESENT LIMITS ON OUR ABILITY TO PREDICT NATURAL CONVECTION AND RADIATION

S. W. Churchill

Thank you, Dr. Keyes. Dr. Keyes and I were fellow graduate students at the University of Michigan, and so this is a very pleasant personal reunion. I am also pleased to be with the rest of you this morning.

The importance of free and natural convection in space heating and meteorology has long been recognized. Nevertheless, it has been a strangely neglected field until perhaps the last decade. The recent expansion in research on natural-convection phenomena is apparently a result of new applications in fuel cells, thermoelectric devices, nuclear reactors, gas turbines, polymeric processing, and other developing fields. I believe that this expansion, however, has been prompted at least as much by improvement in analytical techniques as it has been by the press of practical problems.

An important factor has been the development of high-speed computers, and this will be a subject of particular emphasis in my talk this morning. The rapid and continuing development of computing machinery is, I believe, the most important single development in engineering in this century. The potentialities of machine computation are sill not fully recognized by many engineers. Modern computers plus numerical methods and the relevant development of efficient techniques of programming have suddenly given us great new power; in particular, the power to solve those partial differential equations which we have always presumed to describe the real world. These equations were previously insoluble except for very idealized conditions. I will illustrate this growing capability of computation in the field of natural convection and, as time permits, in the field of radiation. I will do this in terms of some of our work at the University of Michigan, although clearly it could be done in terms of work being done in other laboratories as well. Our research has closely paralleled the development of computers in the past 15 years. We have always been rather frustrated and limited by the capacity of existing computers despite their rapid improvement. I believe that there is an analogy to Parkinson's law

here; namely, that graduate students can always pose a problem for a computer that exceeds the capability of any existing computer. We have tried to make a challenge of this limitation, however, and make our objective to find more efficient ways of carrying out numerical calculations. I believe that such a limitation is desirable; otherwise, one would be tempted to use hammer and tong methods rather than ingenuity.

In 1952, William R. Martini came to the University of Michigan. He was an unusual student and had a subject for his doctoral thesis in mind when he arrived; namely, the behavior of a chemical reactor driven by natural convection. His interest developed out of a water-boiler-type nuclear reactor in which the decomposition of water to hydrogen and oxygen occurred. He had been considering the possibility of a reactor to reunite the hydrogen and oxygen without any mechanical motion, and therefore, with a reduced hazard of detonation. In due course we decided to idealize the problem somewhat. We chose to look at the behavior of such a device without the chemical reaction; I am somewhat embarrassed to say that, 15 years later, we still have not returned to the problem of combined natural convection and chemical reaction. The geometry we chose, however, was closely related to the practical one; namely, a long, horizontal cylinder, was heated on one side and cooled on the other, as indicated in Fig. 1. The left half of the cylinder was a cooled metal wall and the right half a heated metal wall. Insulation minimized conduction from the hot right half to the cooler left half. The heated fluid rose along the right side and descended on the left side. The actual reactor would have hydrogen and oxygen reacting on a platinum catalyst to produce water; the heat of reaction would sustain the hot-wall temperature. The water vapor would then condense on the cold wall and drain out of the reactor at the bottom. The radial lines in Fig. 1 indicate the traverses made with thermocouples. We measured the velocity by introducing particles in the flow and photographing their tracks; we carried out fairly successful experiments of this kind.

We also decided to try to solve the equations describing the conservation of mass and momentum and energy in this region. The partial differential equations for this situation are coupled. We had available

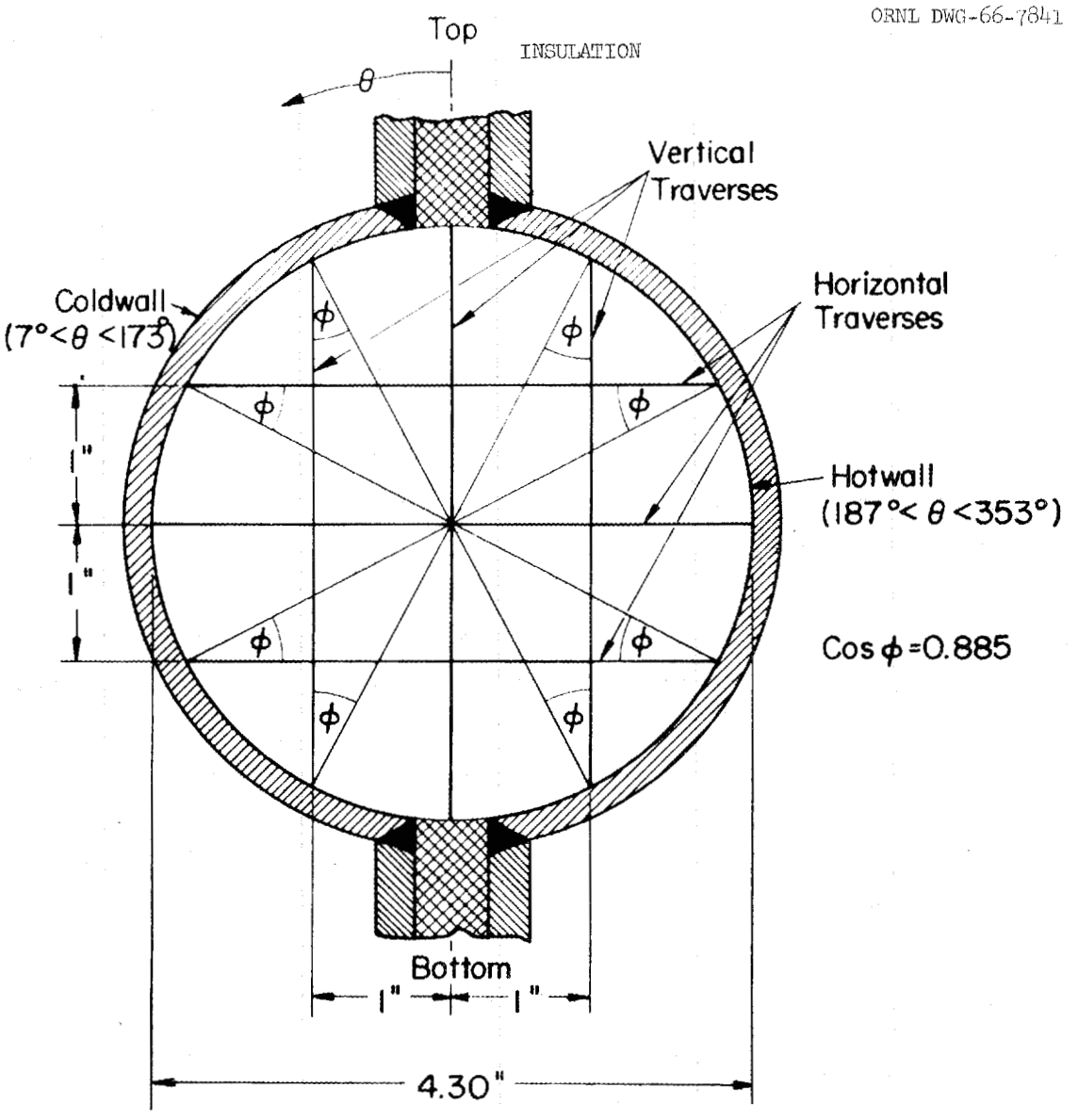

Fig. 1. Reactor Model — Long Horizontal Cylinder with Heating of Half the Lateral Surface Area and Cooling of the Other Half.

a computer known as the MIDAC; the initials officially stand for Michigan Digital Automatic Computer. However, it was nicknamed "Machine Is Down Almost Constantly" – appropriately – we never did succeed in using that computer. It had capabilities (when running) comparable to an IBM 701. We then turned to an IBM 650 which was available and operable. We concluded that the IBM 650 did not have the capacity necessary for the problem. (I have often wondered if we could not go back and do it with our improved knowledge of computing and numerical analysis.) We did succeed in solving the energy equation alone with the experimental velocities. This gave us confidence that, with a better computer, we might have been successful with the whole problem.

Figure 2 shows some of the results obtained by solving the energy equation. This figure is a plot of the local temperature gradient around the wall as a function of angle. The circles are the experimental points and the triangles represent the computed points. The agreement is really remarkable for a number of reasons. First, there was some difference between the theoretical and the experimental conditions. The insulation strips, which were rather broad in the experiment, were represented by a step function in temperature in the theory. Secondly, we assumed constant physical properties; and as I recall, the viscosity in some of these measurements varied by a factor of 4 from one wall to another. There is another factor to be considered. This figure is a graph of the temperature gradient, and the temperature gradient is a much more critical test of the data than the temperature itself. So actually, this comparison confirmed that we could solve the energy equation successfully and predict the temperature field from the velocity field.

We had an IBM 704 when J. D. Hellums came to work with us on his doctorate. Hellums succeeded in solving the general set of partial differential equations which describe Martini's problem. The method that he used involved division of the space into a set of elemental volumes and approximation of the derivatives in the partial differential equations for each elemental volume in terms of values of temperature and velocity in the adjacent elemental volumes. An alternative concept is to overlay the region with a grid and express the derivatives in terms of expansions

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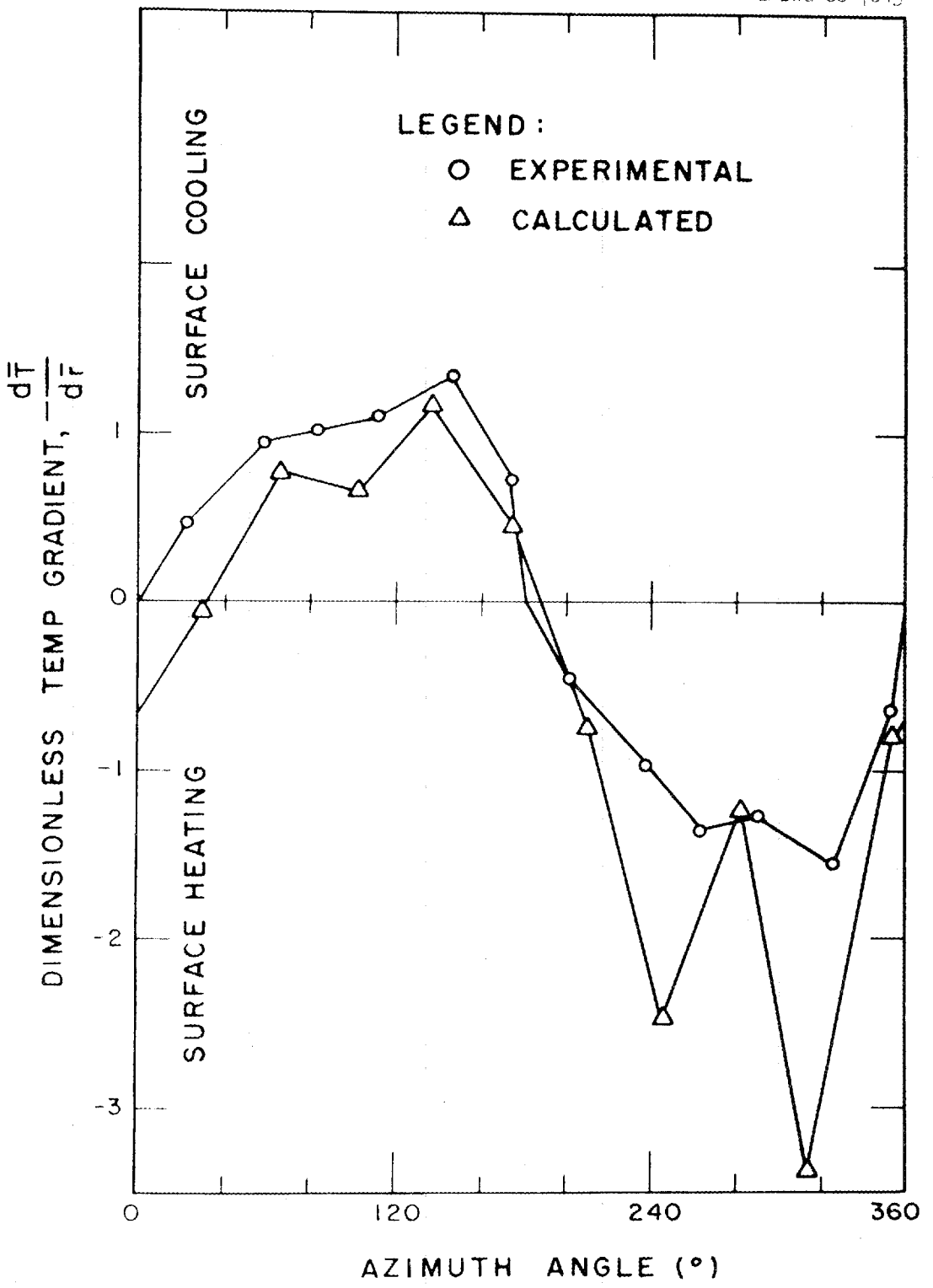

Fig. 2. Dimensionless Temperature Gradient Versus Azimuth Angle for the Assumed Reactor Geometry of Fig. 1.

around each of the grid points. Both concepts lead to exactly the same finite difference formulation.

There are many possible ways of subdividing a region. There are an even greater number of ways of approximating the derivatives at each of these points. And herein lies the engineering problem and the ingenuity; that is, one must find the most successful and efficient approximation. You must find a stable method in mathematical terms and also a rapidly convergent one.

One would think the mathematicians would help here, and indeed this problem has received a great deal of attention. But just as in other fields, there is a gap between mathematics and its use. As far as I know today, even ten years after Hellums' work, the choice of an approximation is mostly a matter of trial and error. This is because the mathematical theory is so idealized that it does not take boundary conditions into account. Actually, the boundary conditions usually cause most of the trouble in this type of representation. After much difficulty, Hellums developed an explicit unsteady-state method for making the calculations. Explicit means that he expressed all the derivatives in terms of known values at the present time and then made a calculation forward in time. This had the great advantage of simplicity, and we were looking for simplicity. Unfortunately, the forward difference technique poses a very severe restriction on the maximum size of the time step. With very small steps in time, it may take a long time to go from the initial condition to the steady-state solution. However, this method worked after we asked the computer to examine the sign of the velocity at each point and then decide which of several approximations to use.

The first thing that we did was to carry out the integration of the equations for a simple case for which we were very confident of the solution; namely, the isothermal vertical plate. Some of the results are shown in Figs. 3 through 6. Figure 3 shows the dimensionless heat flux versus dimensionless time; spatial dependency is also involved. This figure can be considered more simply as the heat-transfer coefficient versus time. Prior to our calculations, all that was available was the value for the steady state as calculated by Ostrach. The unsteady-state

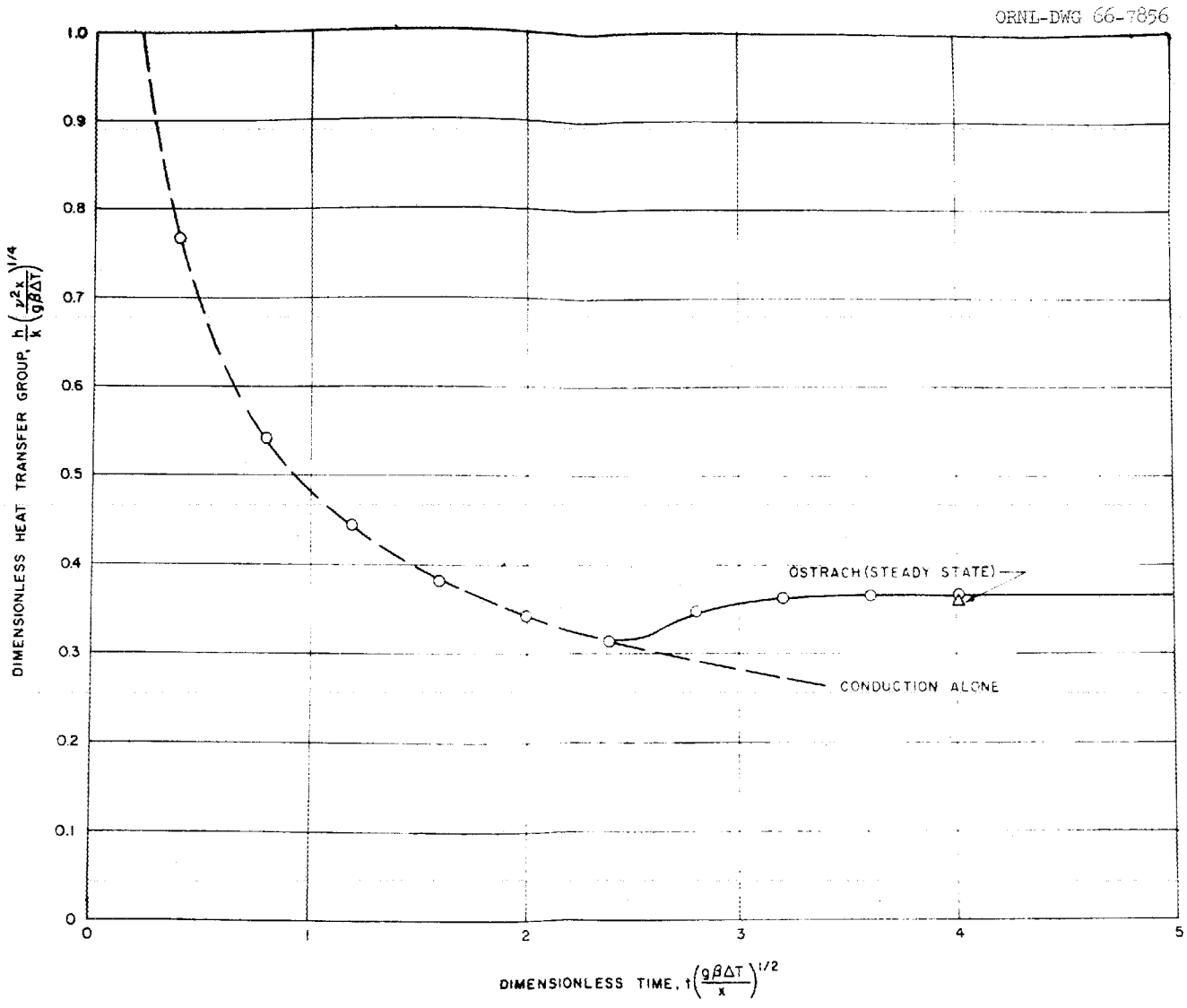

Fig. 3. Dimensionless Heat Flux Versus Dimensionless Time for Natural Convection from a Vertical Isothermal Plate.

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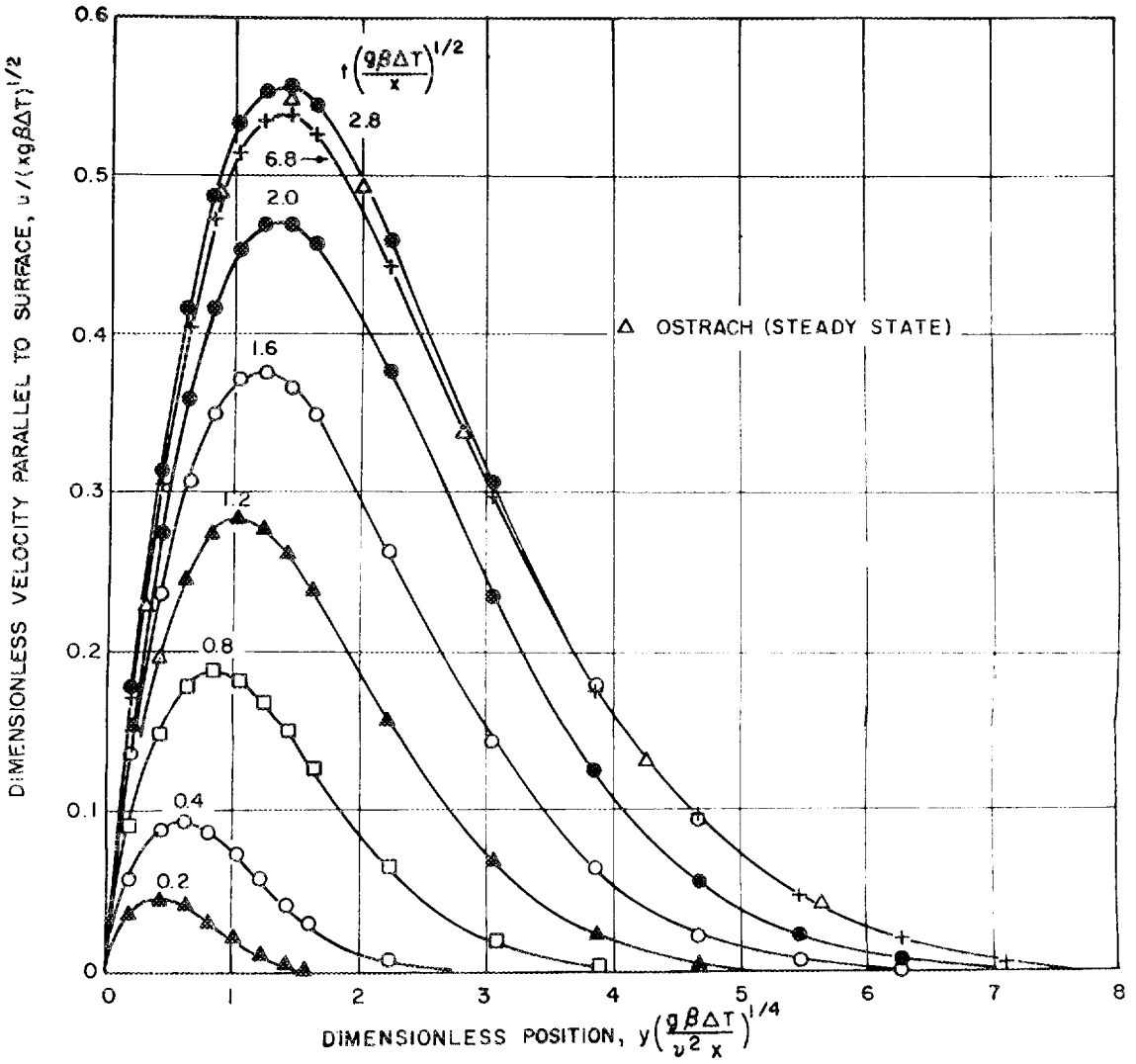

Fig. 4. Dimensionless Velocity Profiles Versus Dimensionless Position for Natural Convection from a Vertical Isothermal Plate.

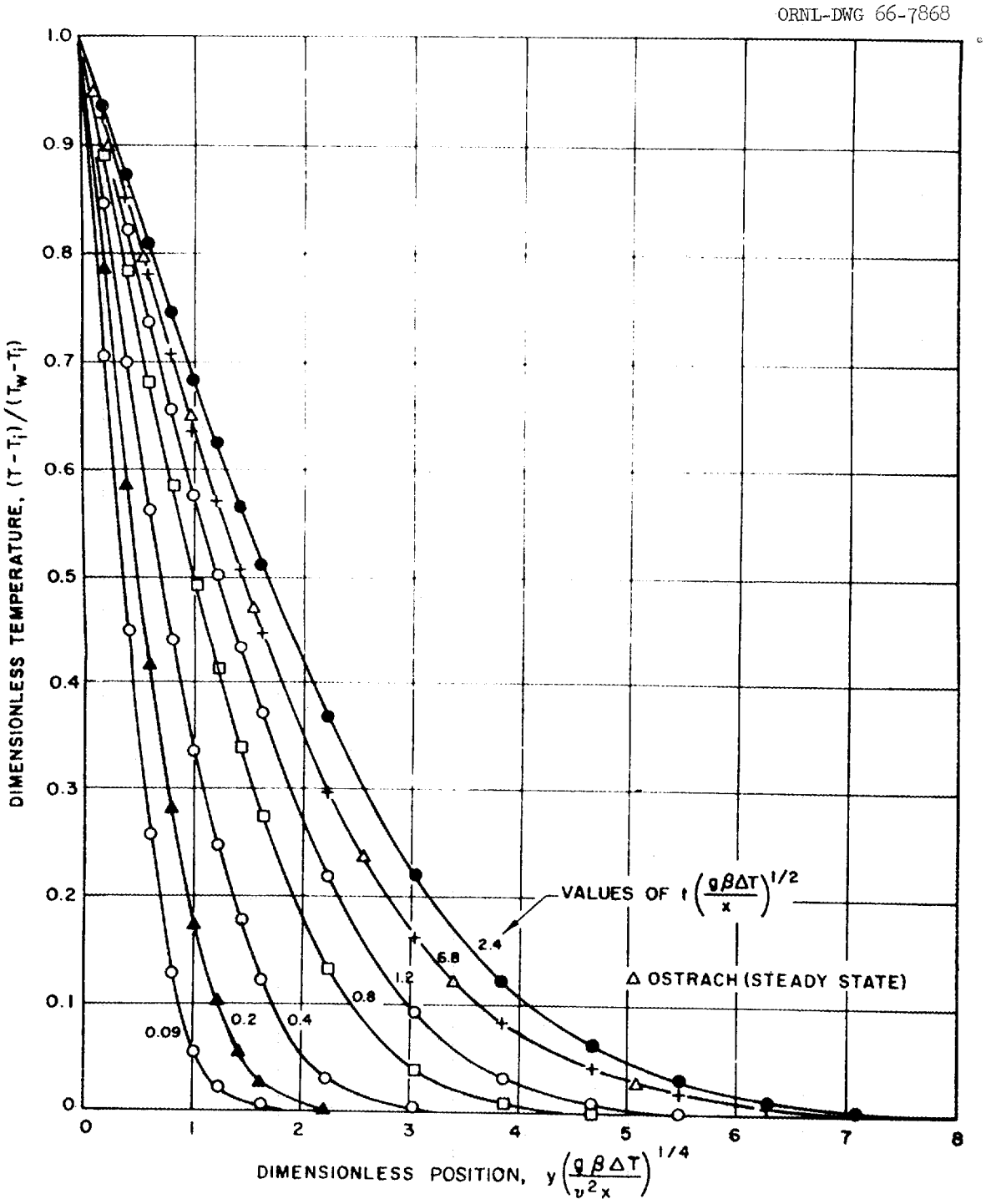

Fig. 5. Dimensionless Temperature Versus Dimensionless Position for Natural Convection from a Vertical Isothermal Plate.

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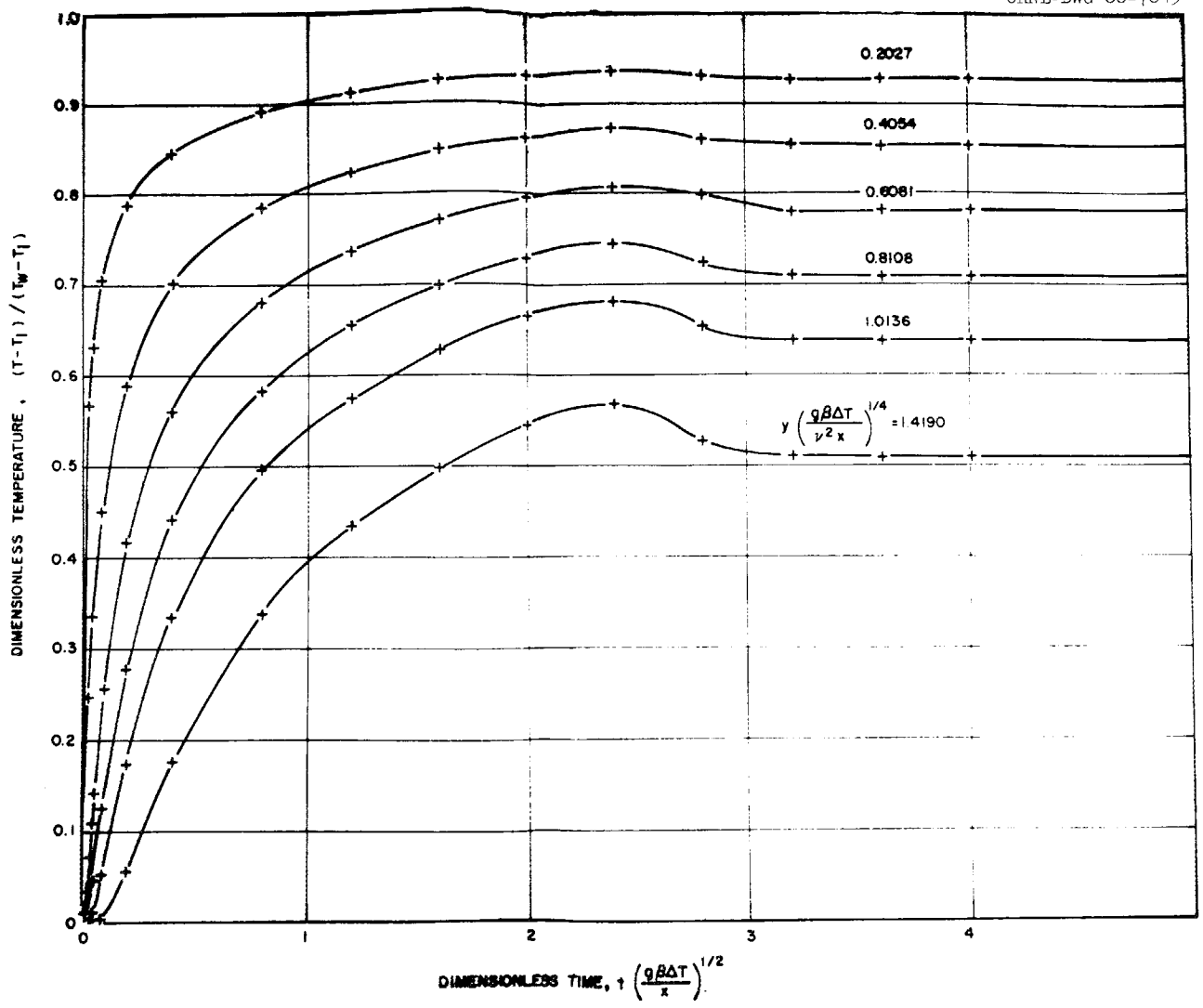

Fig. 6. Dimensionless Temperature Versus Dimensionless Time for Natural Convection from a Vertical Isothermal Plate.

solution approached this steady-state value. An interesting thing is that the simple solution for conductance from a flat plate differs negligibly from the solution for convection for a long period of time. The coefficient comes down, goes back up, and for convection oscillates toward a steady state. This oscillation occurs because the gas being heated remains stagnant initially. Movement is eventually induced by the change in gas density and the induction of fresh cool fluid causes the heat-transfer rate to increase suddenly. Eventually, a steady state is approached. This figure does not have sufficient detail to show the many oscillations. At the time we did this work, there was some controversy as to whether there was such a minimum or not in the coefficient. Eckert and co-workers had observed a minimum, while others had denied on theoretical grounds that a minimum was possible and proposed results without a minimum. The minimum actually exists, and the size of the minimum depends on the Prandtl number and becomes larger as the Prandtl number becomes smaller.

Figures 4 through 6 show some of the detailed results for the flat plate. Figure 4 shows the velocity profiles. The velocity is, of course, initially zero, reaches a maximum, and then approaches the steady-state solution. The results for steady state show very good agreement with the steady-state values of Ostrach. Figure 5 gives similar results for the temperature field. The temperature profile starts out as a step function, gradually goes through a maximum, and then decreases. This behavior is shown better in Fig. 6 in which dimensionless temperature histories are displayed versus time. The temperatures increase, decrease, increase, and finally decrease. This behavior is characteristic for all solutions for natural convection; that is, there is somewhat of an overshoot and then an oscillation into the steady-state solution.

One of the advantages of this finite difference method is that one gets the unsteady-state solution as a bonus. We were really just trying to calculate the steady-state values; but as you see, all the additional information in Figs. 3 through 6 came to us as a result of using an unsteady-state computing technique. We could have iterated using guessed values for the steady state; but instead, we iterated through real time.

We next solved the equations corresponding to the problem of Martini. Figure 7 shows the Nusselt number as a function of angle for the cylinder in Fig. 1. The Nusselt number increases gradually to infinity, comes back down, approaches negative infinity, and finally increases. The negative values result from our arbitrary definition of the local heat-transfer coefficient.

An absolute necessity in any finite difference calculation is that you prove two things. First, that the solution is convergent; that is, that the solution approaches some fixed solution as you change the size of the finite difference gird or elements. In other words, you must show that your numerical answer does not depend on how you divided the region. There are a number of papers in the literature in which people have not bothered to do this and, hence, their results are not dependable. Our proof is shown in Fig. 7; the crosses and the circles represent two different grids. We first used the grid represented by crosses, and then subdivided this grid and used the intermediate points. One sees that no change occurs when twice as many points are used. Hence, we feel confident that the solution is independent of the grid size. However, this is not a sufficient check. The only sufficient check is the demonstration that the solution agrees with experimental data. One might have a convergent solution but not the physically unique one.

Figure 8 shows our check with the experimental data. The solid line represents Hellums' calculations (as shown previously in Fig. 7); and the points represent Martini's data — the circles and the crosses are his values for the cold and hot side, respectively. We assumed constant physical properties in the theoretical calculations. We could have taken the physical property variation into account in the finite difference calculations without much trouble, but then you get results without generality. As you can see, there is really relatively good agreement with the data of Martini. Martini's data did not go to infinity, because he had an insulated section across the top — not a step function. Our theoretical solution does go to infinity and back, but in the rest of the region the agreement is really remarkable. Furthermore, this is again a plot of the temperature gradient, so it is a critical test of the data. We were now satisfied that our solution was not only convergent but also physically unique.

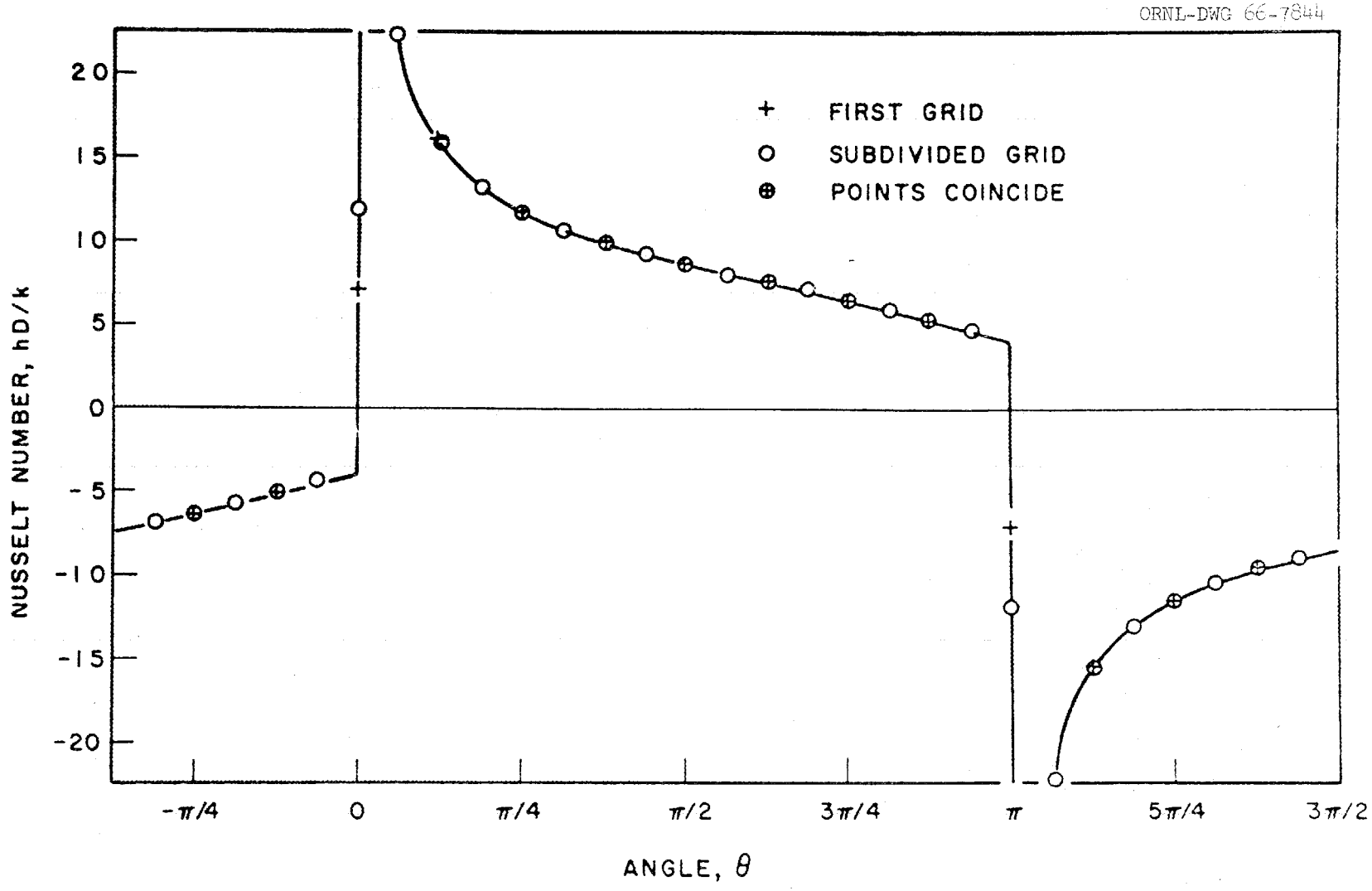

Fig. 7. Effect of Subdividing the Grid on Nusselt Number for Solution 1 ($Gr = 6.15 \times 10^5$) for the Cylinder of Fig. 1.

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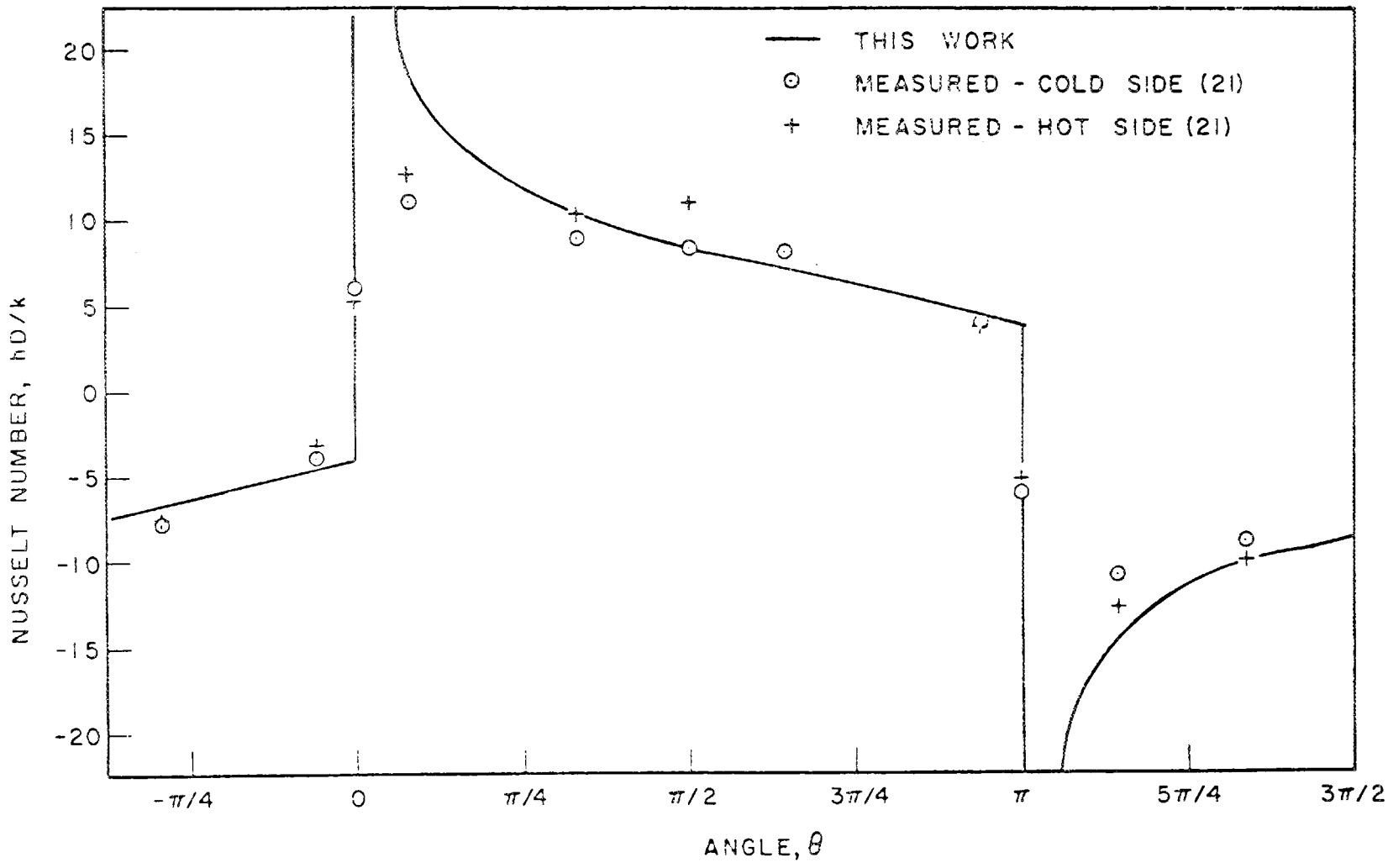

Fig. 8. Comparison of Nusselt Numbers with Measurements.

Figure 9 shows data for different Prandtl and Grashof numbers plotted in such a way as to minimize the parametric variation, following an analysis developed by Hellums to find the minimum possible mathematical representation for any boundary value problem. This plot does successfully minimize the variation although as you go to small Prandtl numbers you get some variation from the idealized solution.

In carrying out an analytical solution for natural convection even for the flat plate, one must make many assumptions and discard many of the terms in the equations. With the finite difference technique, one does not need to discard any terms; the problem does not need to be idealized. A comparison of the idealized equations, the so-called boundary-layer approximation, and the exact solution with all the terms in the equation is shown in Fig. 10. The circles represent the idealized equations, and the crosses represent the exact solutions. One can see that the boundary-layer approximations are indeed valid for natural convection. I assert, however, that the only sure way one can tell whether an approximation is valid is to carry out the solution in general. One cannot say just because certain terms are small that they are negligible; since, as we will see later, small terms can have large effects. In this particular case, the boundary-layer approximations turned out to be very good. This was a surprise and a disappointment to us. The results indicated that, although we thought we were solving a two-dimensional problem, we were really solving a one-dimensional problem. That is, one could neglect the radial momentum balance, which is what the boundary-layer approximations do.

Figure 11 shows the agreement between the experimental and the computed velocities and temperatures. Simplified theory says the temperatures should lie in a single curve for all conditions; as you can see, Martini's data scatter, but they do scatter around the theoretical curve. In this instance I now trust the computations more than Martini's experimental values. The agreement with the experimental velocities is really remarkable. The results were exciting and gave us great confidence.

Figure 12 shows the overall heat-transfer coefficient or Nusselt number around the entire half of the cylinder versus the product of the Grashof and Prandtl numbers to the 1/4 power. The crosses represent

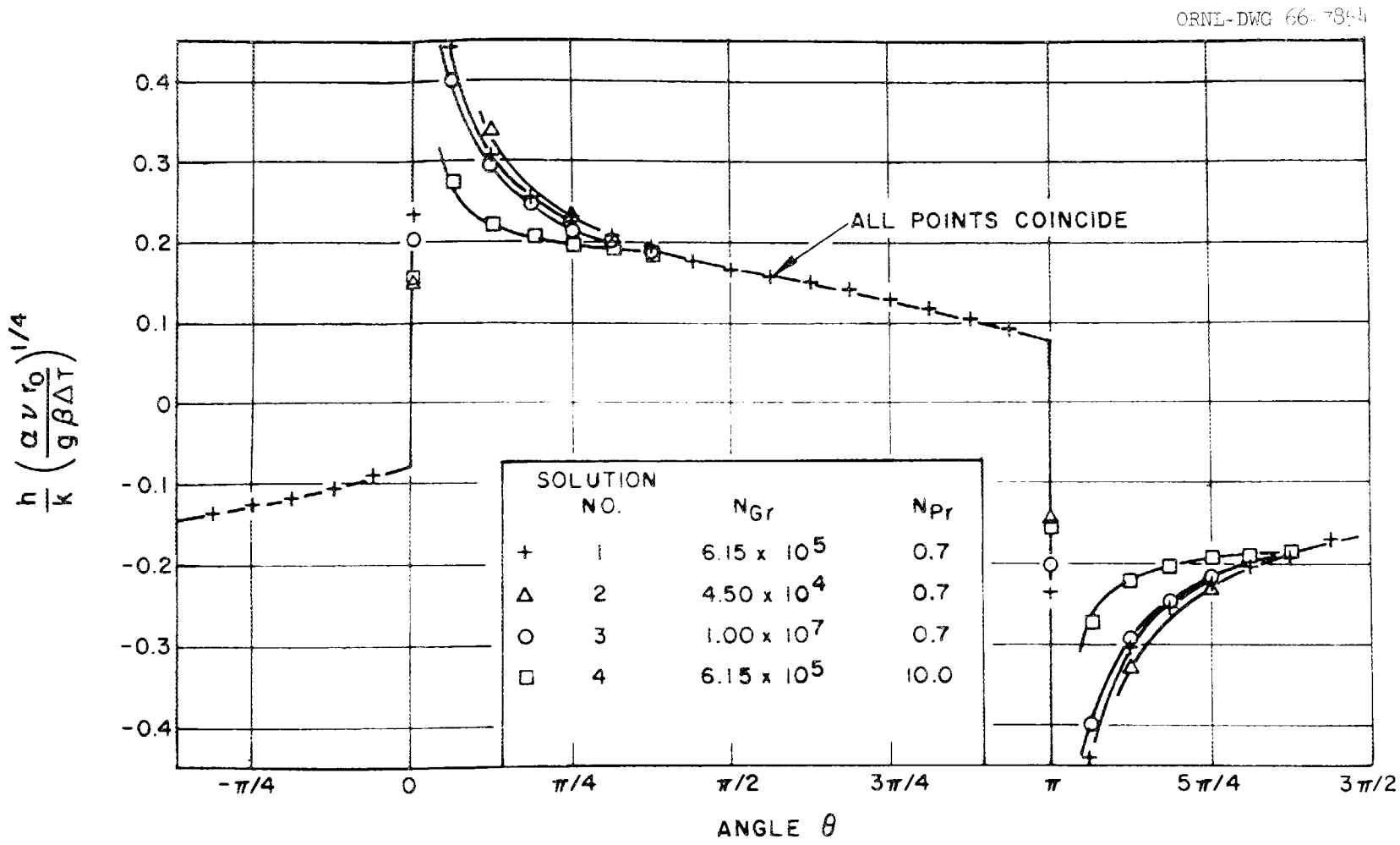

Fig. 9. Heat-Transfer Coefficient Versus Angle for the Problem Associated with Fig. 1.

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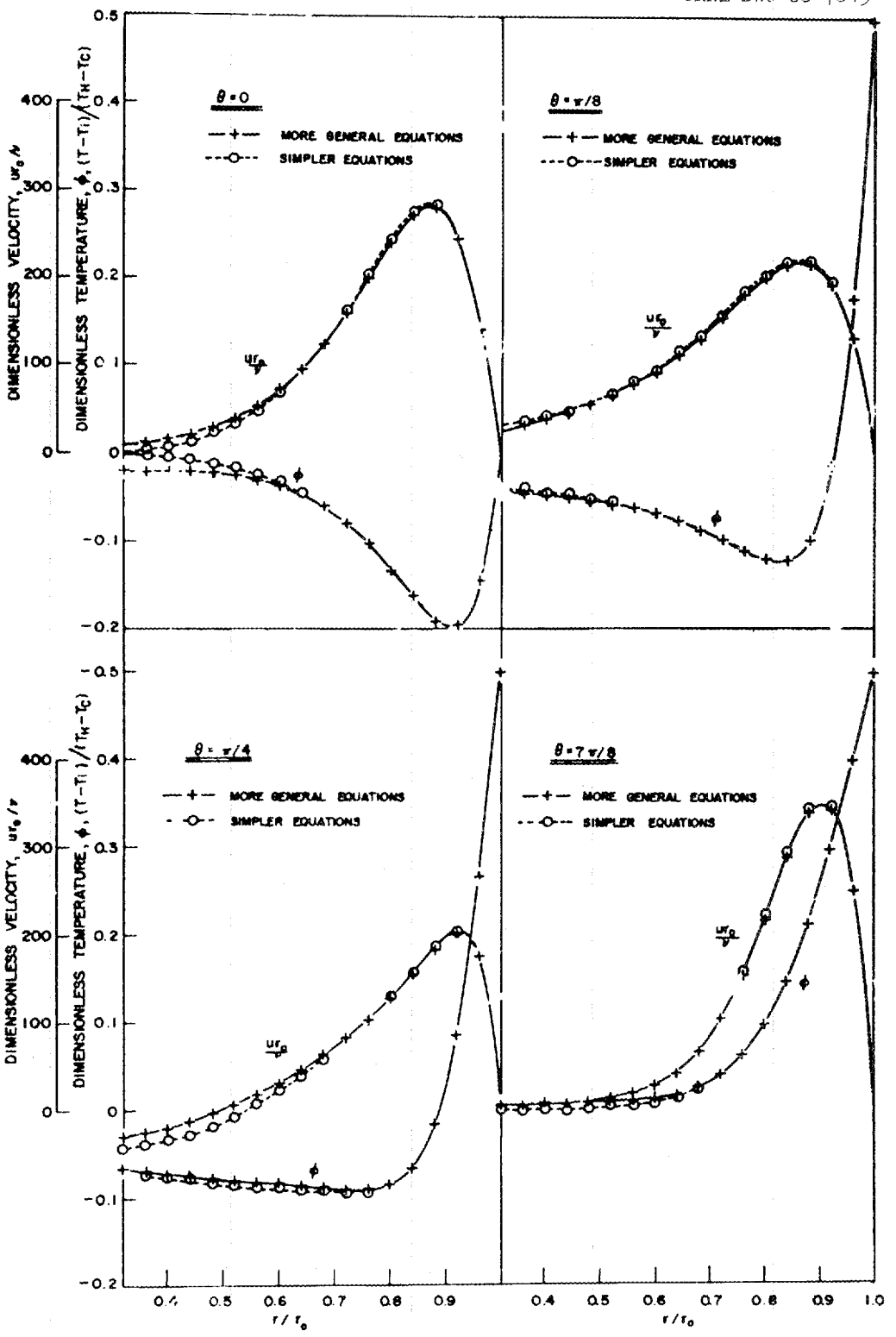

Fig. 10. Velocity and Temperature Profiles from First Grid for Problem Associated with Fig. 1.

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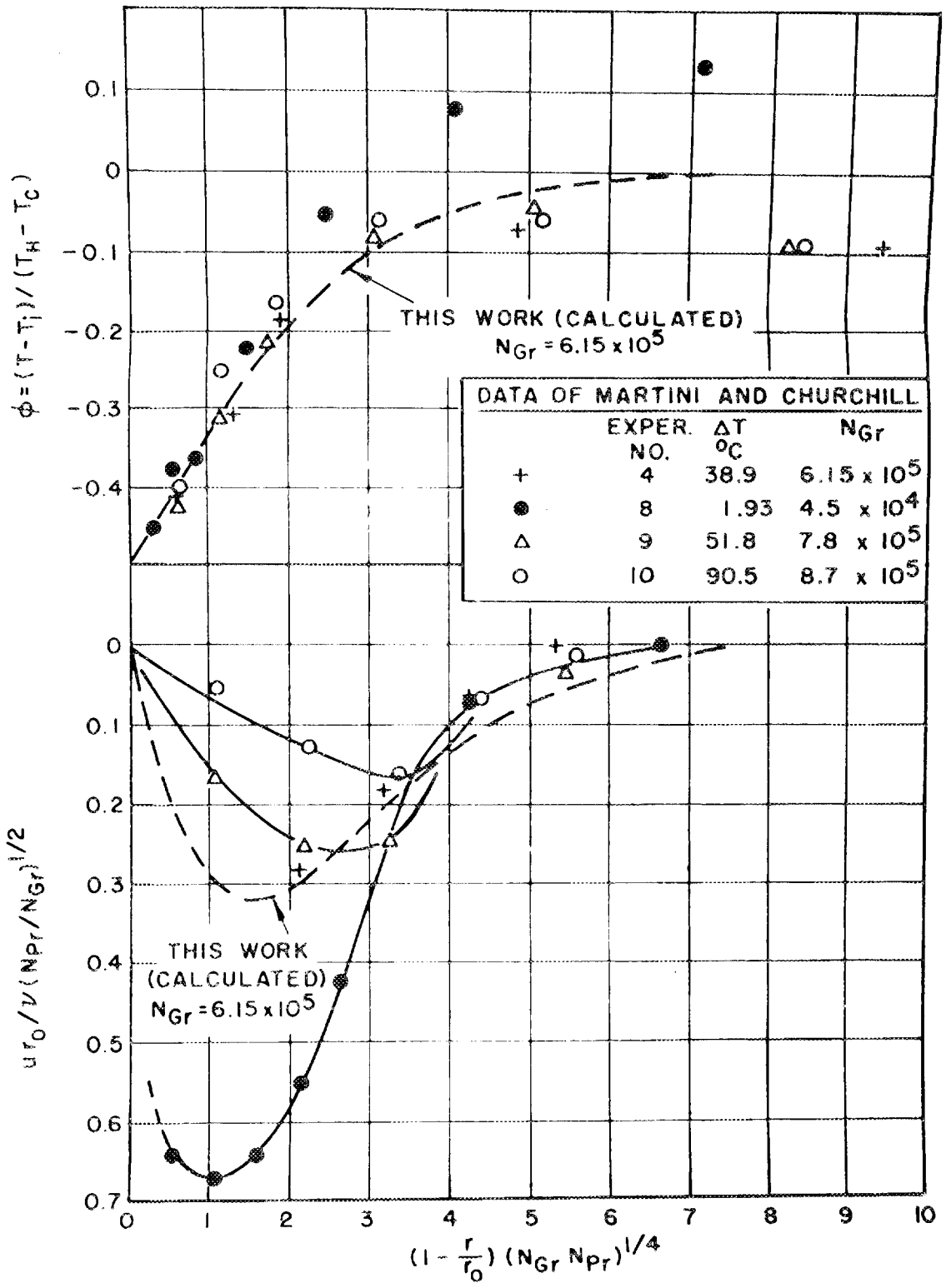

Fig. 11. Comparison of Experimental and Computed Velocities for the Problem Associated with Fig. 1.

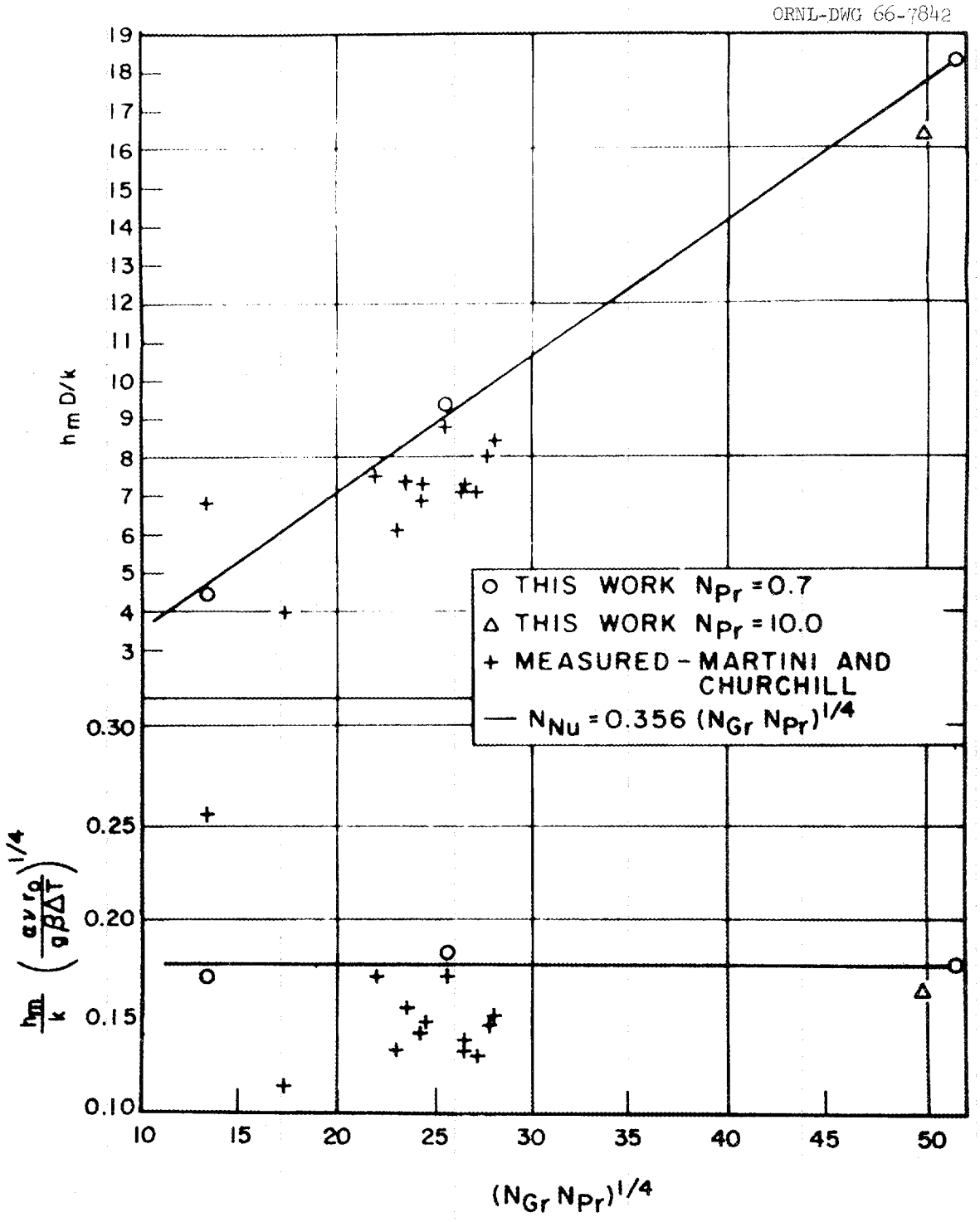

Fig. 12. Nusselt Number and Dimensionless Heat Flux Versus the Product of Grashof Prandtl to the 1/4 Power for the Problem Associated with Fig. 1.

Martini's data. Carrying out the theoretical calculations for different conditions, one gets the computed values as represented by the circles and deltas. We see that both results can be represented by a straight line in terms of either the upper or the lower ordinate. Prior to the calculations, we did not know which of the experimental data points were bad and we fitted a quite different straight line through the data using least squares. We were quite badly misled. Theory now gives us a great deal of confidence in extrapolation.

As I said, we were disappointed at this point to find that we had really solved a one-dimensional problem. Therefore, we turned to a truly two-dimensional problem. As shown in Fig. 13, we chose a rectangular region with a cold wall and a hot wall. We could separate these walls at top and bottom either by an insulated boundary or one with a linear temperature variation. We studied the behavior of the fluid in this rectangular region of general dimensions. We were pretty sure that the boundary-layer approximations wouldn't hold for this case because of the behavior in the corners. This work was done by J. O. Wilkes, who is now on our faculty.

The equations which apply to the problem are shown in Fig. 14. We have an unsteady-state term, inertial terms, a buoyant term, a pressure term, and a viscous term for the u-direction; and in the v-direction, again the unsteady-state velocity term, the inertial term, and the pressure term, and the viscous term. The energy balance has an unsteady-state temperature term, convective terms, and a conduction term. These equations are already somewhat simplified. Again, we solved the more general equations for some cases to test the validity of the simplifications. It turns out that the approximations made to this point are negligible for practical purposes. They are, primarily, that the physical properties other than density are constant and that the density variation only needs to be taken into account in the buoyant term.

We then rearranged the equations as shown in Fig. 15. First, we introduced the stream function as defined at the bottom. Then we introduced the vorticity, which is equal to minus the Laplacian of the stream function. By introducing those two terms, we reduced the four equations

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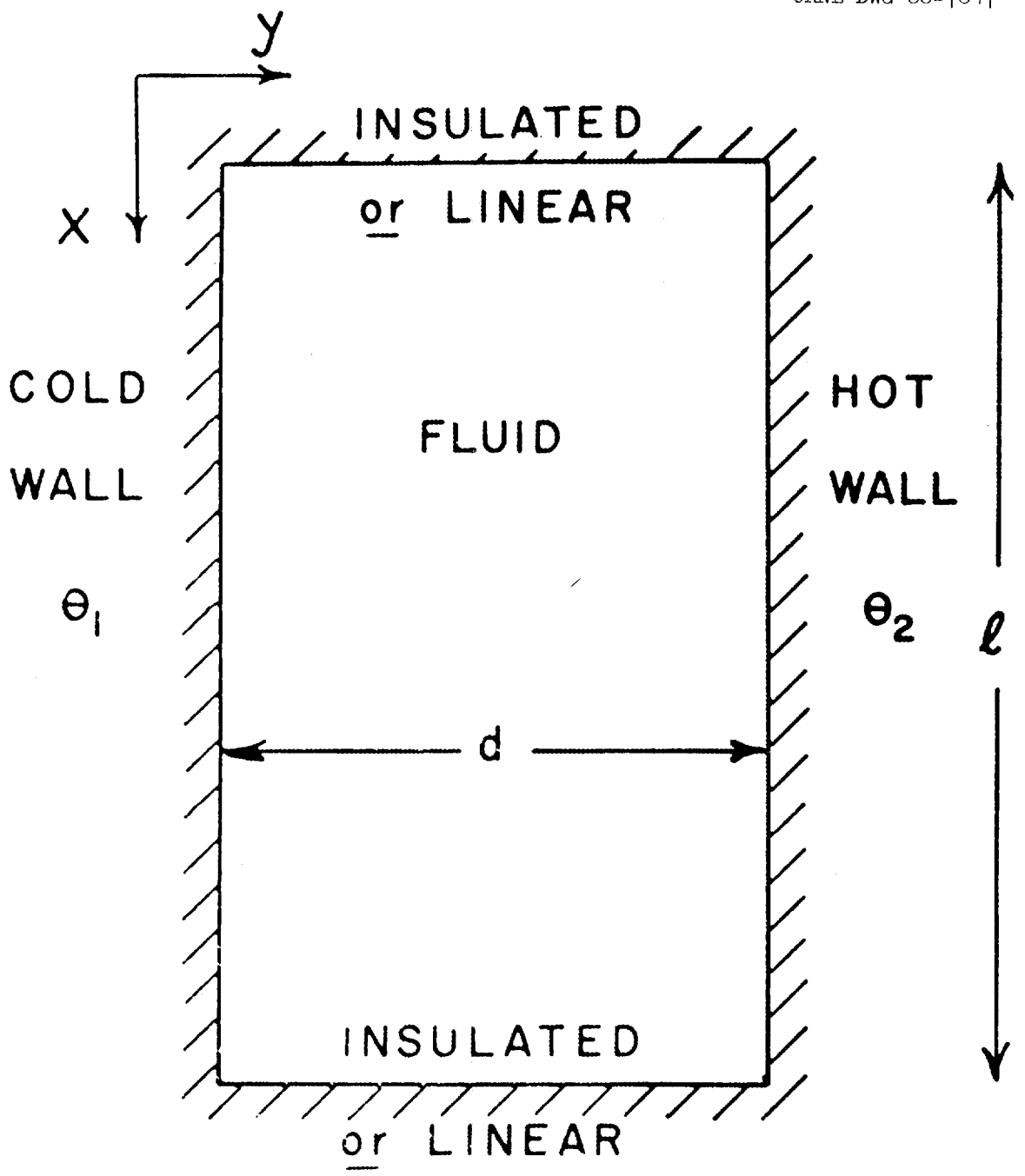

Fig. 13. Rectangular Enclosure for Two-Dimensional Problem.

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TRANSPORT EQUATIONS

MOMENTUM TRANSFER

$$\frac{\partial u}{\partial t} + u\frac{\partial u}{\partial x} + v\frac{\partial u}{\partial y} = -g\beta(\theta-\theta_0) - \frac{1}{\rho}\frac{\partial p'}{\partial x} + \nu\nabla^2 u$$

$$\frac{\partial v}{\partial t} + u\frac{\partial v}{\partial x} + v\frac{\partial v}{\partial y} = -\frac{1}{\rho}\frac{\partial p'}{\partial y} + \nu\nabla^2 v$$

CONTINUITY

$$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} = 0$$

ENERGY TRANSFER

$$\frac{\partial \theta}{\partial t} + u\frac{\partial \theta}{\partial x} + v\frac{\partial \theta}{\partial y} = \frac{k}{\rho c_p}\nabla^2\theta$$

Fig. 14. Conservation Equations Applicable to the Problem Associated with Fig. 13.

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DIMENSIONLESS EQUATIONS

VORTICITY TRANSFER

$$\frac{\partial \zeta}{\partial \tau} + U \frac{\partial \zeta}{\partial X} + V \frac{\partial \zeta}{\partial Y} = \frac{G_t}{2} \frac{\partial T}{\partial Y} + \frac{\partial^2 \zeta}{\partial X^2} + \frac{\partial^2 \zeta}{\partial Y^2}$$

ENERGY TRANSFER

$$\frac{\partial T}{\partial \tau} + U \frac{\partial T}{\partial X} + V \frac{\partial T}{\partial Y} = \frac{1}{P_t} \left(\frac{\partial^2 T}{\partial X^2} + \frac{\partial^2 T}{\partial Y^2} \right)$$

STREAM FUNCTION - VORTICITY

$$\frac{\partial^2 \psi}{\partial X^2} + \frac{\partial^2 \psi}{\partial Y^2} = -\zeta$$

VELOCITIES

$$U = \frac{\partial \psi}{\partial Y}, \qquad V = -\frac{\partial \psi}{\partial X}$$

Fig. 15. Dimensionless Equations Applicable to the Problem Associated with Fig. 13.

to two equations. This looks like a great gain. We have also done something else in going from Fig. 14 to Fig. 15; we have made the equations dimensionless. This step simply replaces the viscosity with the Grashof number and the conductivity with the Prandtl number. We now have two basic equations to solve instead of four. However, we still have to come back and solve the Laplacian for the stream function and then solve the bottom two equations in Fig. 15 for the velocity. In terms of the direct computations, this appeared to be of great advantage. However, there is a penalty; and this was a penalty that we didn't expect. Indeed, we are not sure anymore whether the penalty was not more severe than the gain. We know that the velocity is zero at the boundaries of the region, but we do not know the vorticity at the boundary. Thus, suddenly we have a dependent variable whose values we need to know at the boundaries; and we have no idea what they are. We end up approximating these values in terms of their values inside the region. It turns out that this introduces all kinds of mathematical complications into the calculations. We used an implicit alternating direction method for the calculations; an implicit method means that you formulate the finite difference approximations in terms of future values. In this way you overcome the problem of instability, since mathematical theory says that the equation should be stable for all time steps. The great advantage of the implicit method is thus that you eliminate the limitation on the time step and, in turn, obtain a large group of algebraic equations to solve at every single time step. Computers are very good at solving algebraic equations, and there is a gain in using the computer to do this. You solve more equations but take bigger time steps; and there results a net saving. This expected freedom from instability turned out not to be true. We are certain the reason for this discrepancy is that the approximation for the vorticity at the boundary negates the theory. We find that the equations blow up at high Grashof numbers, whereas the mathematical theory say they shouldn't.

First, let us solve the limiting case of the box in Fig. 13 — two infinite flat plates. Figures 16 and 17 give the solutions for transient temperature and velocity profiles for the limiting case of two infinite plates. The only previous solution is for the steady-state solution which is given as a linear temperature variation in Fig. 16. Figure 17 shows the velocity profiles; again the previous steady-state solution is shown.

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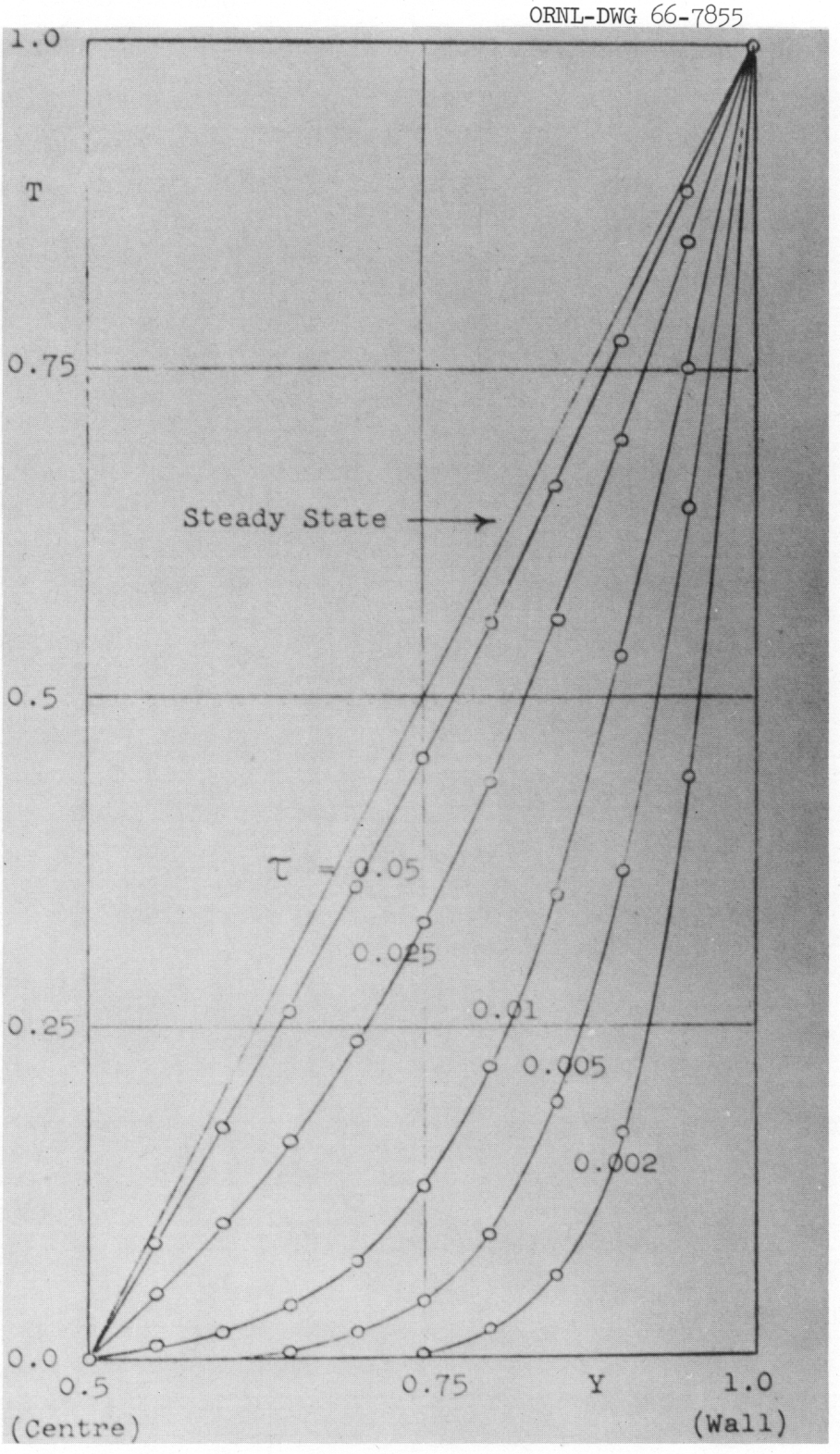

Fig. 16. Transient Temperature Profiles for Natural Convection Between Two Infinite Vertical Plates, for Pr = 0.733, Gr = 10,000, with $\Delta Y = 0.05$, $\Delta\tau = 0.001$.

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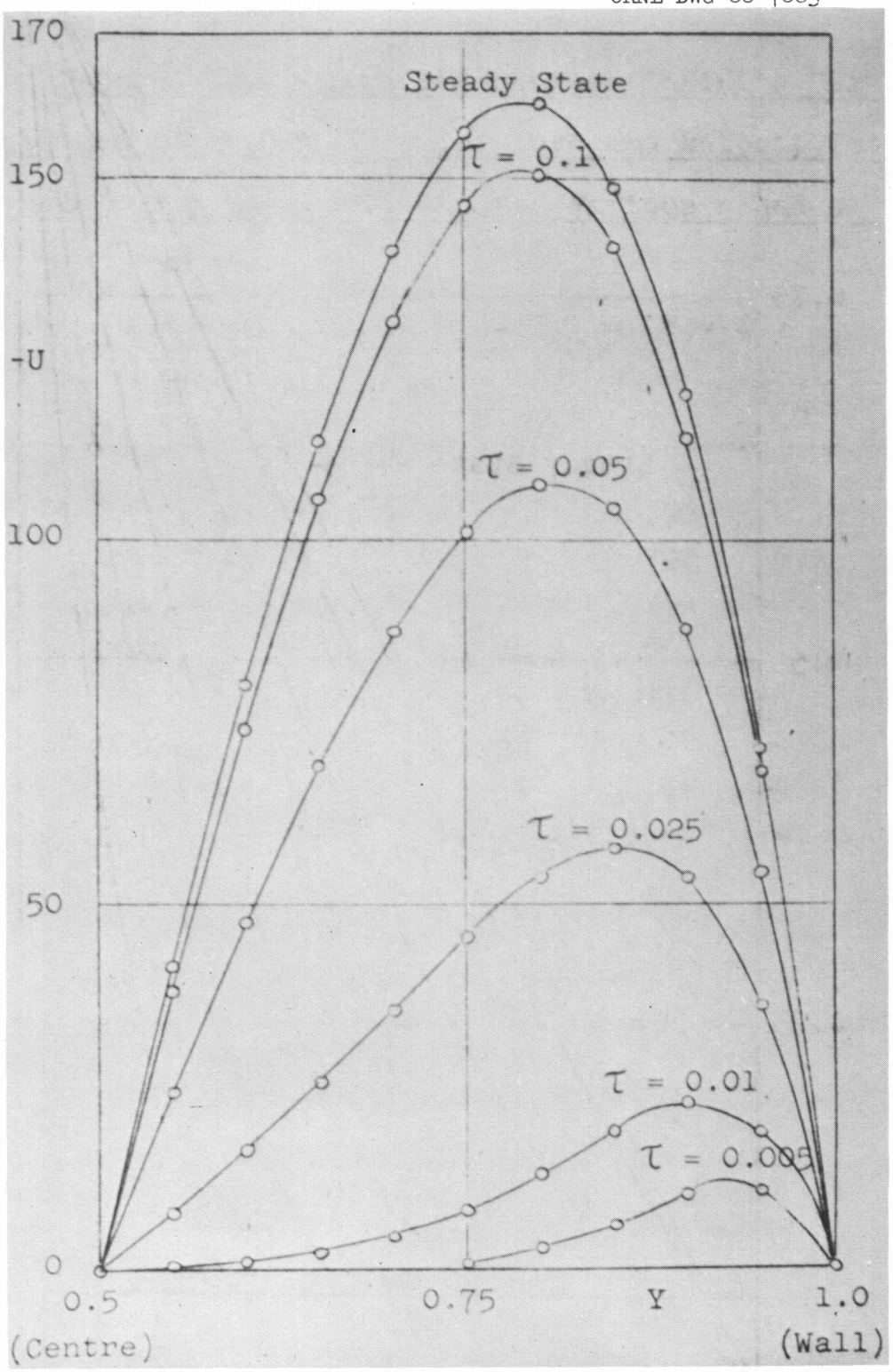

Fig. 17. Transient Velocity Profiles for Natural Convection Between Two Infinite Vertical Plates, for Pr = 0.733, Gr = 10,000, with $\Delta Y = 0.05$, $\Delta\tau = 0.001$.

Curiously enough this time we did not find the oscillations we expected. The temperatures and velocities did not seem to overshoot.

We were now ready to tackle the box – the fundamentally two-dimensional problem; and some of the results are shown in the next figures. We obtained a great surprise to begin with. This is a box that is heated on one side and cooled on the other side and insulated along the top and bottom as shown in Fig. 13. We would expect the hot air to rise along the heated side and sink along the cooled side and, thus, to circulate like a whirlpool or vortex. This is not what we found. Instead, as seen in Fig. 18, two separate circulations were generated. The two circulations in Fig. 18 gradually move toward each other (Fig. 19); and ultimately collapse into one as seen in Fig. 20. You would hardly believe how coarse a grid was used for these calculations – a 10 x 10 grid is sufficient to give the correct solution in the main or central region. To repeat, only 100 points are needed – only 10 along any boundary. A finite difference method is remarkably powerful in such problems. If you have a good representation, you do not really need a very fine grid. Figure 21 shows the steady-state isotherms.

An interesting question can now be raised. Is there a secondary flow in the corners? The answer is yes; experimentally, you can see eddies in the corners. Our grid was not fine enough, even though it was a convergent solution, to pick up these back circulations. Another order of magnitude of computing time would be required. The curious thing is that the solution in the main region does not depend on getting the solution correct in the corners. The main solution converges long before you have a fine enough grid to get the circulations in the corner.

At the time we were just completing this work, we found an analytical solution of the same problem in the literature by Professor Poots at the University of Bristol. Nothing is as jolting to a doctoral student as to find somebody has duplicated his research while he is at work on it. Wilkes is one of the calmest people I have ever known, but he was shaken that day. However, while Poots had solved the problem for the steady state, Wilkes also had the unsteady-state solution. Naturally we compared our steady-state results with those of Poots. We didn't agree at

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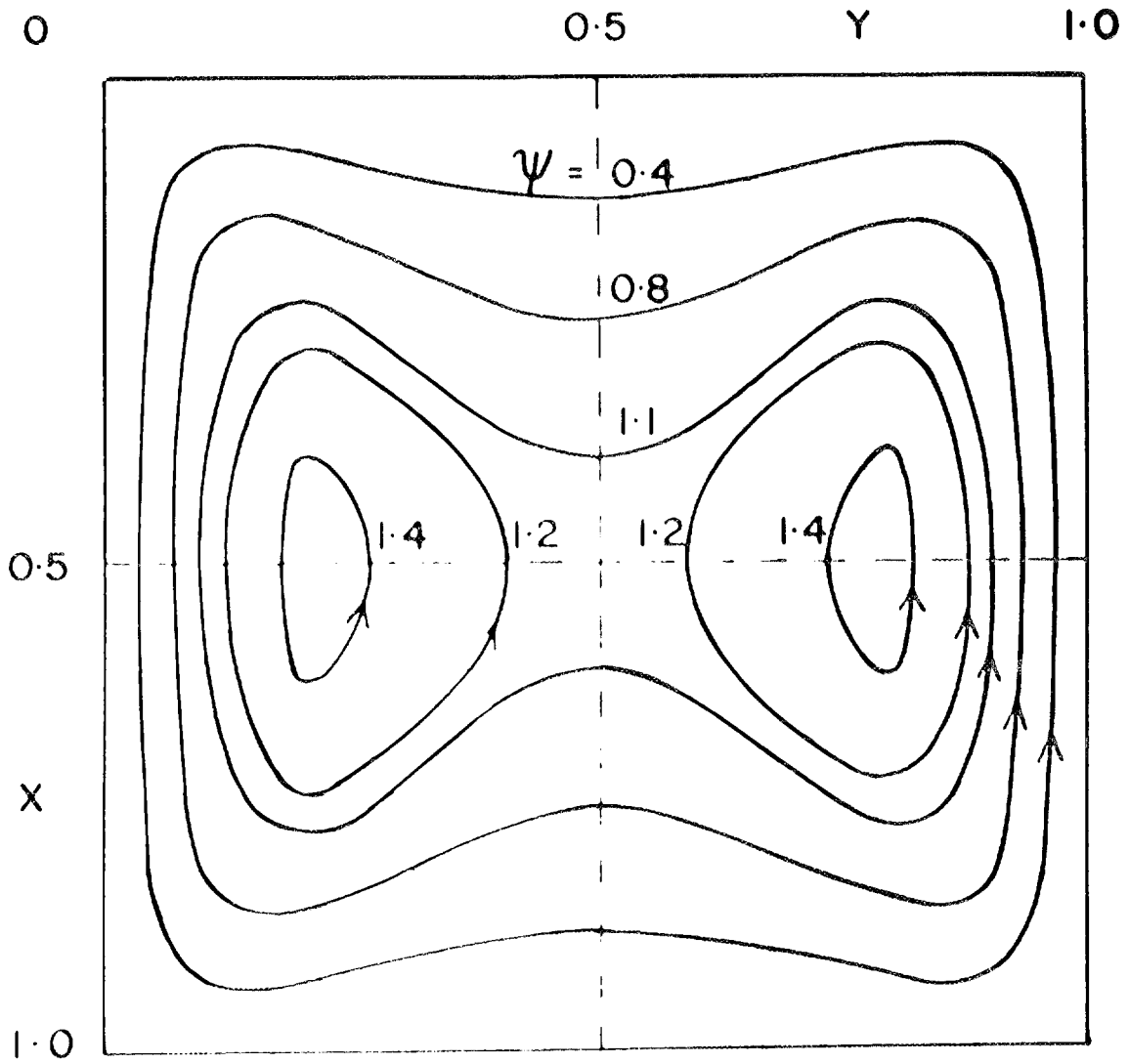

Fig. 18. Transient Streamlines (τ = 0.004) for Circulation in a Heated Enclosure with Insulated Top and Bottom as Shown in Fig. 13.

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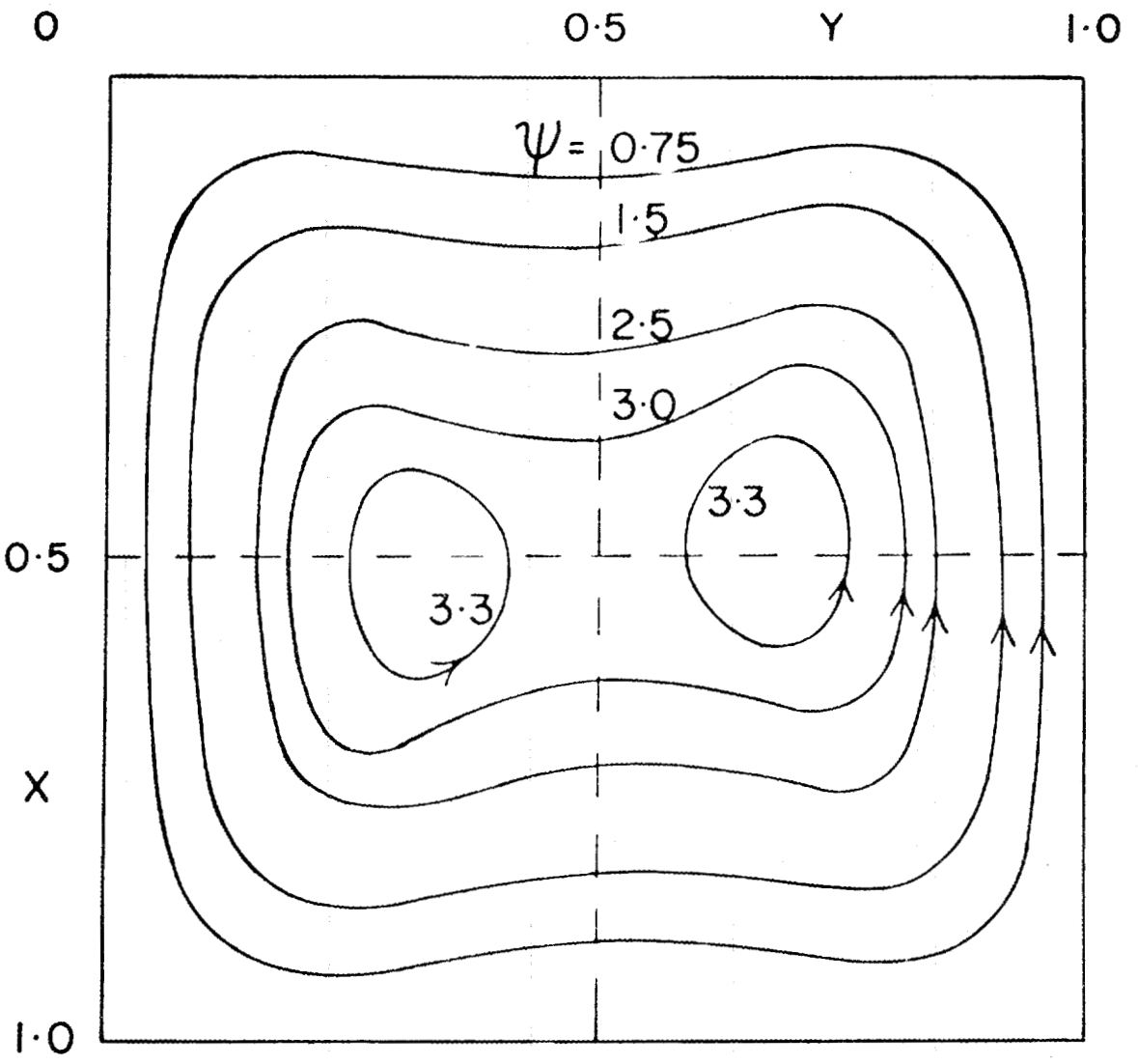

Fig. 19. Transient Streamlines (τ = 0.01) for Circulation in a Heated Enclosure with Insulated Top and Bottom as Shown in Fig. 13.

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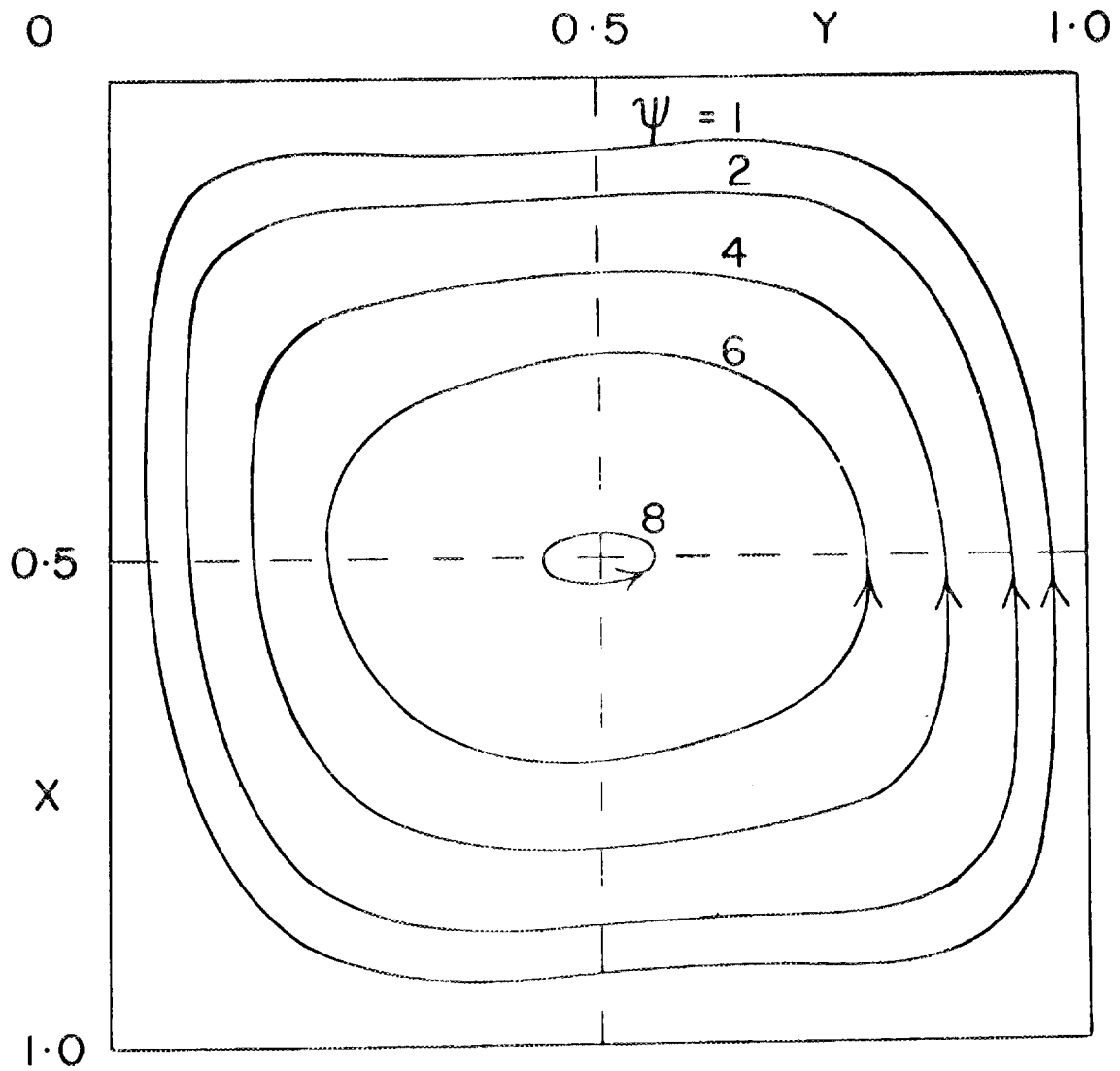

Fig. 20. Steady-State Streamlines for Circulation in a Heated Enclosure with Insulated Top and Bottom as Shown in Fig. 13.

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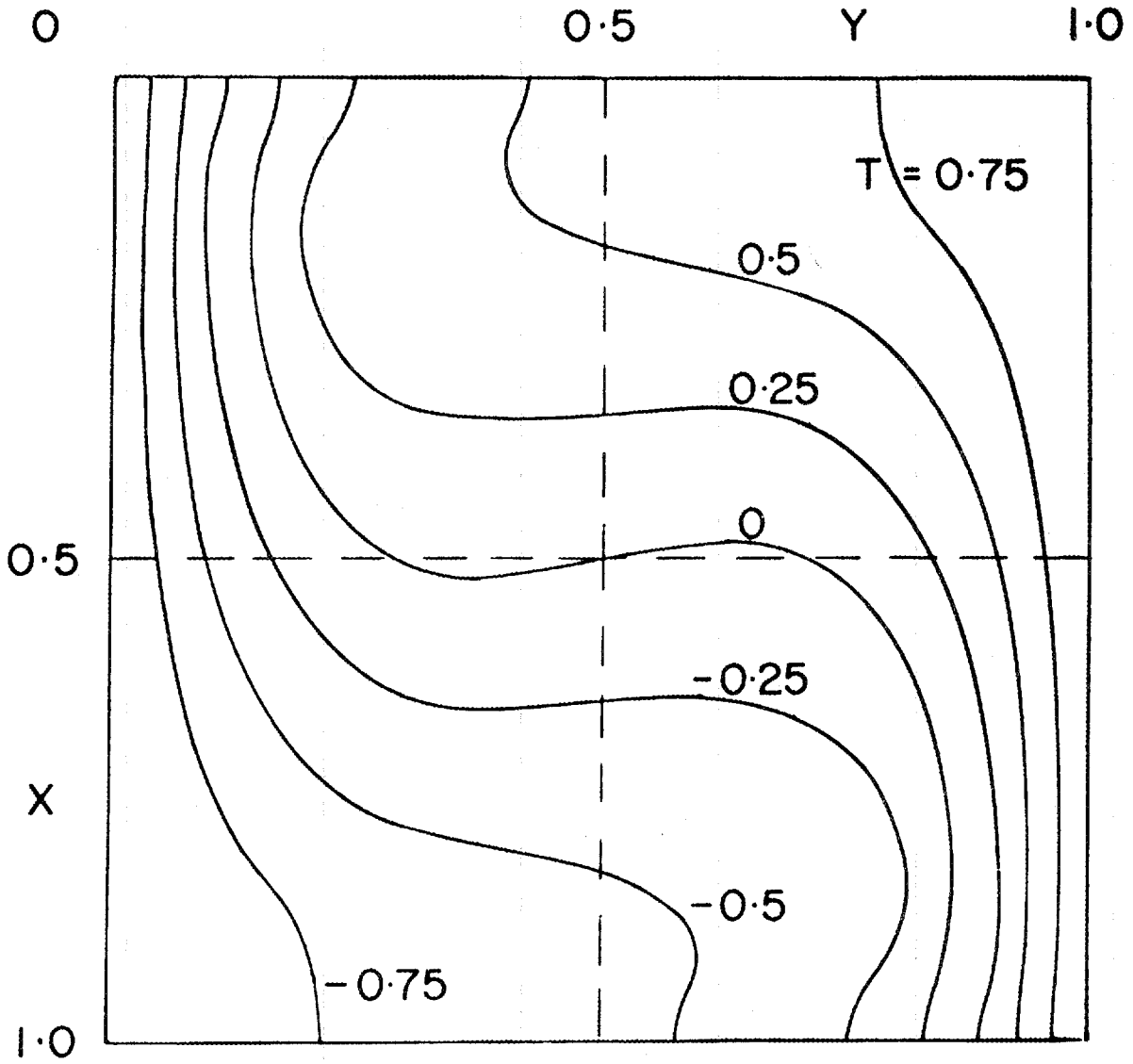

Fig. 21. Steady-State Isothermals in a Heated Enclosure with Insulated Top and Bottom as Shown in Fig. 13.

all. Poots used Fourier analysis to express the temperature field and velocity field in terms of harmonic functions. These functions were then expanded in series and the coefficients calculated to yield a double-infinite series for the temperature and velocity fields. Wilkes programmed and repeated Poots' computations on the computer and found that he had made many numerical errors, apparently, because he had used a hand computer or a desk calculator. The recomputed values agreed well with ours. Figure 22 shows this agreement for the streamlines. The open points represent Wilkes' results; the black points, Poots' results; and the half moons, the points exactly coincide. As can be seen, the agreement is excellent. I think you have to contemplate awhile before you appreciate how remarkable is this agreement. Here are two solutions to a very, very complex two-dimensional boundary value problem involving two nonlinear momentum equations, an energy equation and a continuity equation. One solution is by finite difference techniques. The other solution is by classical analysis, and yet they agree perfectly. As you can see in Fig. 23 the agreement isn't quite as good for the temperatures as for the velocities, but it is still remarkable.

I have no way of knowing which results are the best, because in a sense they are both approximations. However, there are two conclusions to be drawn here. One is that it took more computation to obtain the coefficients and to sum the series than it did to carry out the entire numerical integration. Therefore, the finite difference technique takes less work and less computation than does the "analytical" solution. Furthermore, with the finite difference technique you obtain all the unsteady-state results as a bonus. I think this work sounds the death knell of analytic methods for natural convection. The work of Poots is probably the most complicated that has been published for natural convection — it really is an accomplishment. However, I can take a senior who knows very little mathematics and quickly teach him how to use the finite difference technique; with less cost and in less time he can carry out the finite difference calculation and get Poots' solution as a limiting case of a much larger solution. I believe one cannot exaggerate the importance of this development. Analytical solutions, of course, will retain their value as a guide and test for finite difference solutions.

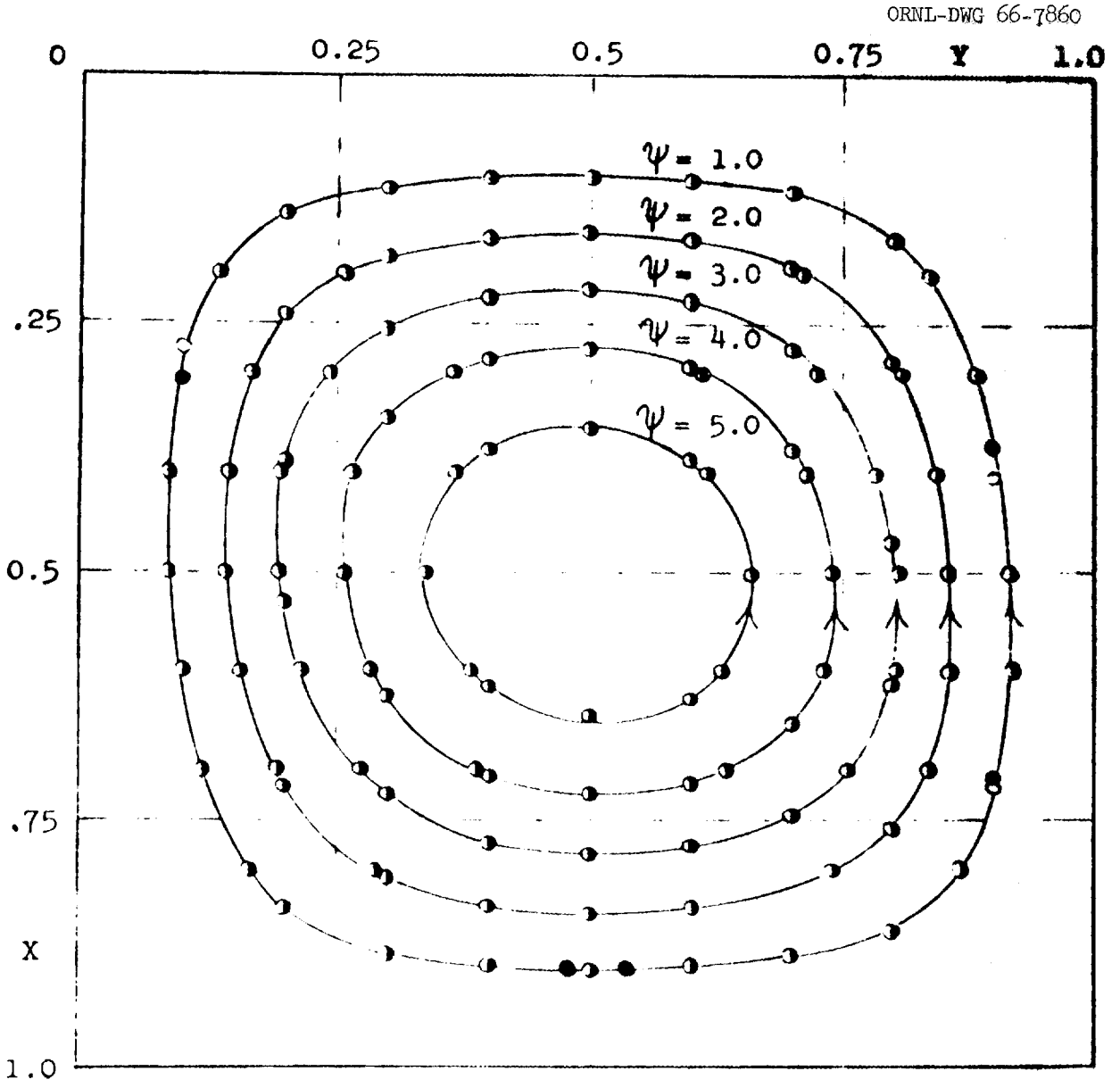

Fig. 22. Comparison of Computed Steady-State Streamlines with Poots' Analytical Solution, for L = 1.0, Pr = 0.730, Gr = 6850, Linear for the Enclosure with Insulated Top and Bottom in Fig. 13. Key to points: Computed (O), Poots (●), points too close to plot separately (◑).

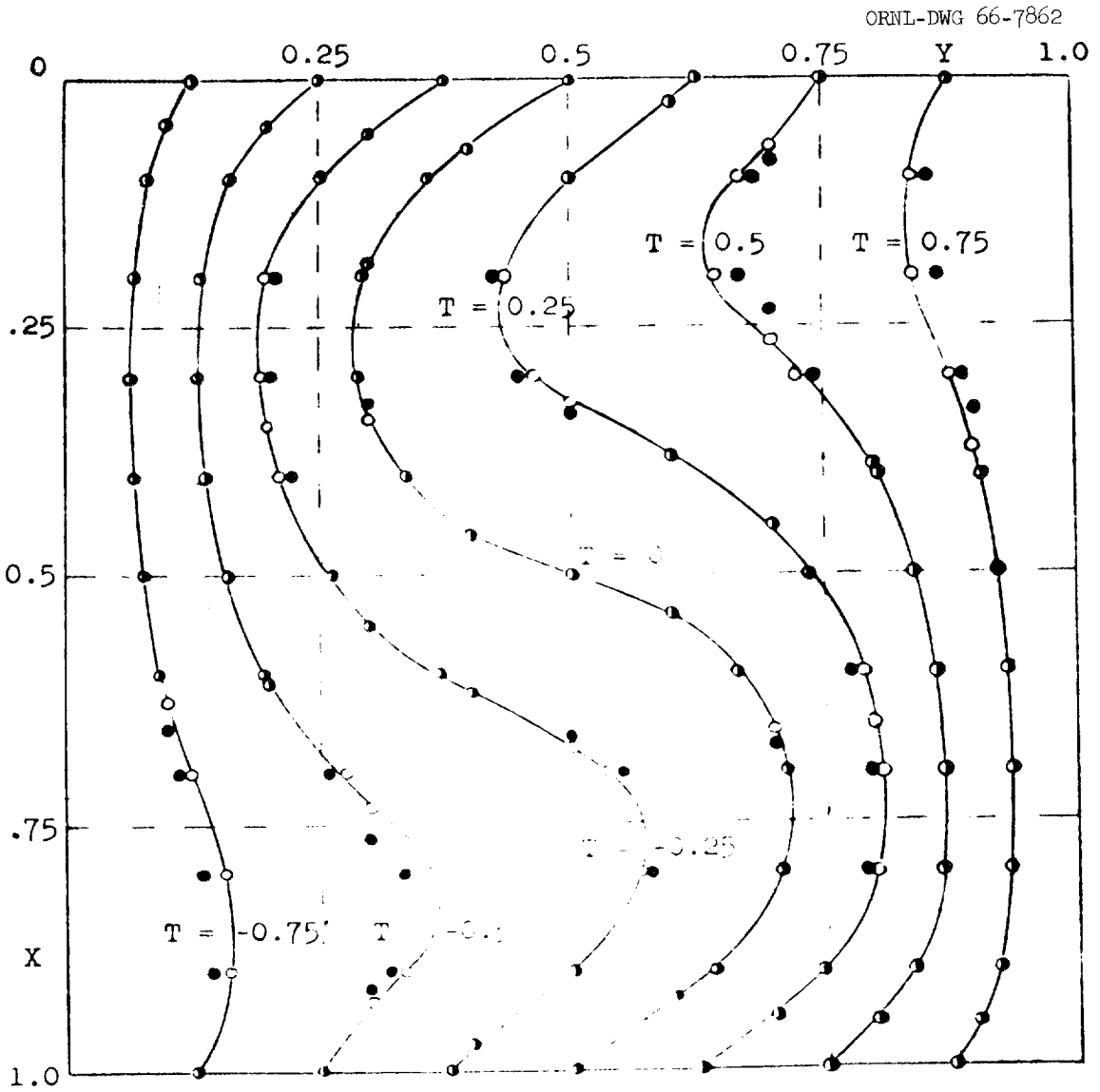

Fig. 23. Comparison on Computed Steady-State Isothermals with Poots' Analytical Solution, for L = 1.0, Pr = 0.730, Gr = 6850, Linear for the Enclosure with Insulated Top and Bottom in Fig. 13. Key to points: Computed (O), Poots (●), points too close to plot separately (◑).

Finally, Fig. 24 shows the mean Nusselt number versus time. Again, one notices the oscillatory behavior of the Nusselt number as it approaches steady-state behavior. The solution for conduction again provides an excellent bounding value and a test for the numerical results.

We next decided to look at still more complicated problem; logically, this would be a three-dimensional problem. However, after a long look, we decided to tackle something else – the problem of instability. We chose a classical problem that has been looked at for a centry and a half. It is known by various names – the Rayleigh problem or the Bernard problem – and concerns a region bounded by two horizontal plates with the bottom plate heated and the top plate cooled. For a sufficiently small temperature difference between the lower flat plate and the upper flat plate, there is no motion and heat transfer is by conduction only. If a velocity disturbance were introduced, it would be dampened out. However, above some critical temperature difference, or in dimensionless terms above some Rayleigh number, the perturbation will develop into a circulation instead of dampening out, and the heat transfer rate will be higher. This problem has been subject to analysis for a very long time; and there are indeed analytical solutions based on a linearized form of the differential equations or upon higher idealized boundary conditions – boundary conditions such as no shear at the wall. Remarkably, these very idealized solutions do predict rather closely the Rayleigh number for the onset of circulation. However, when we carried out the numerical solution of the complete equations, we found that in the linearization the effect of the Prandtl number was lost. The Rayleigh number alone does not characterize whether a fluid will start to circulate or not; the combination of the Rayleigh number and the Prandtl number does. The effect of Prandtl number becomes very important for small Prandtl numbers, as for liquid metals.

When Michael Samuels carried out finite difference calculations for this physically unstable problem, he had a great deal of difficulty with computational stability. It turned out that grid size was very, very important. The reason is that we were now dealing with the effect of a small perturbation. If you introduce a small perturbation in the fluid,

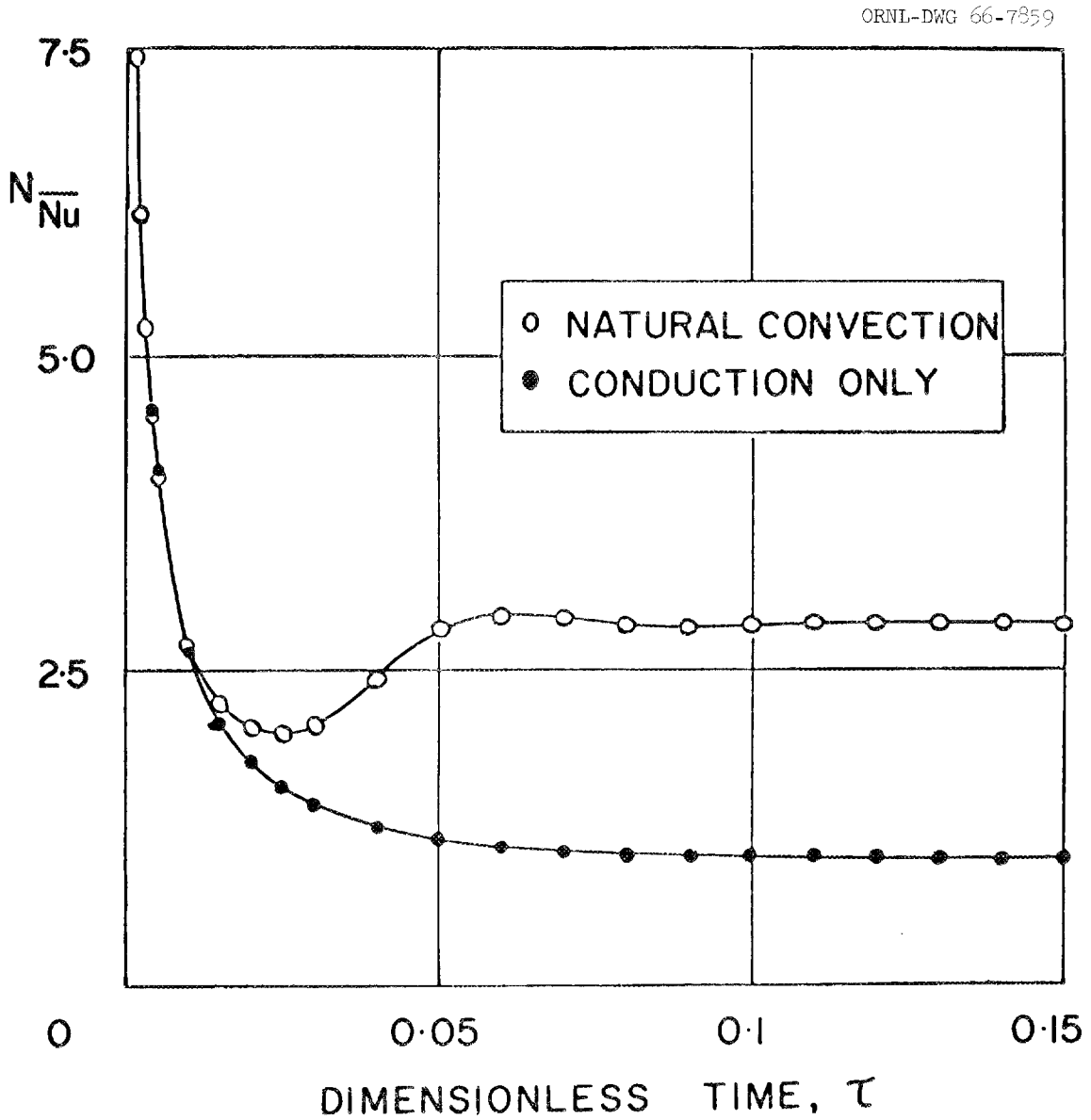

Fig. 24. Mean Nusselt Number Versus Time for the Enclosure with Insulated Top and Bottom Shown in Fig. 13.

will it dampen out or not? You must use quite a small grid size to get rid of the effect of grid size itself. Figure 25 is a plot of the effect of grid size; it shows the Nusselt number for heat transfer between the plates as a function of grid size. We want to extrapolate the curve to zero grid size. One must carry out these calculations for several grid sizes to be able to make this extrapolation. However, people don't like to do that because the expense increases rapidly with decreasing grid size. The plot of the central stream function in Fig. 25 is another check, showing also the effect of this extrapolation. I emphasize this point because I would judge that for 90% of the results in the literature the extrapolation has been neglected.

Again, in the case of Samuels' work, we found some comparable work in the literature. We didn't agree with them and ultimately showed their grid size was not small enough. Interestingly enough, in the original thesis they showed a few smaller grid values which agree with our values. Thus, although they had the right result, they didn't really carry out the extrapolation and published misleading values. It is an absolute necessity in numerical analysis that you prove that your results are not an artifact of your method of computation.

The results of Samuels' work are seen in the next figures. Figure 26 shows the square region heated from below, cooled at the top, and insulated on each side. The top and bottom are isothermal. The temperature distribution for the region is shown in the top figure and the streamlines are shown in the bottom figure. The $\psi = 0$ streamlines represent the boundary of the secondary flow.

Figure 27 shows what happens if you have a 1×2 region; that is, a region 1 high and 2 wide. At steady state, one gets two circulations; and the temperature profiles have this beautiful pattern. I think we could make art designs this way. There is no central boundary in the regions, merely a line of symmetry.

Figure 28 shows the patterns for a 1×3 box. The figure shows three circulations and the resulting temperature distribution. We have here an interesting phenomena; and I will assert a broad generality which I can't prove. The generality is that the number of circulations

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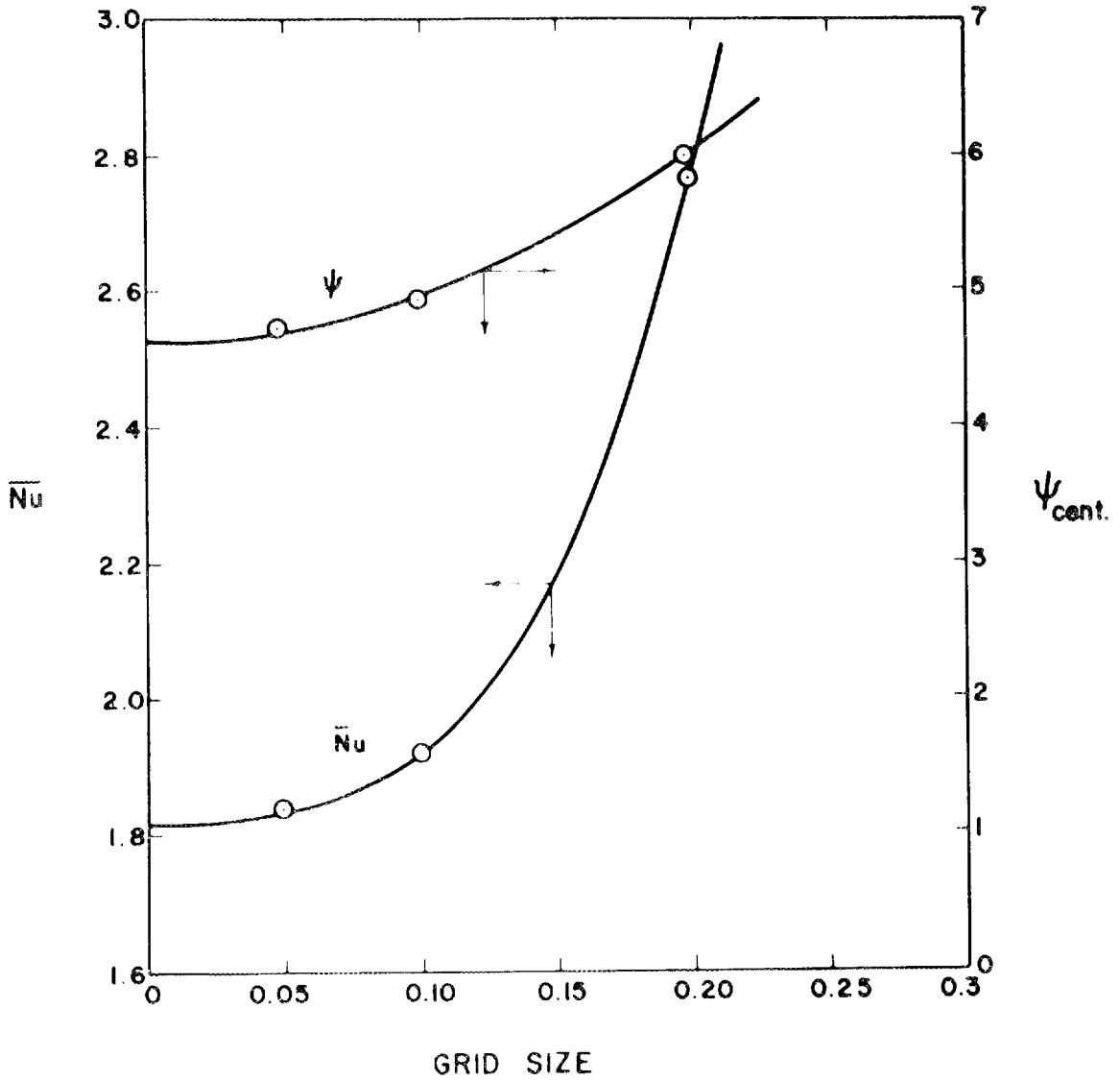

Fig. 25. Effect of Grid Size on Steady-State $\overline{Nu}$ and ψ_{cent} for a Square Cavity with Pr = 1, Gr = 3000.

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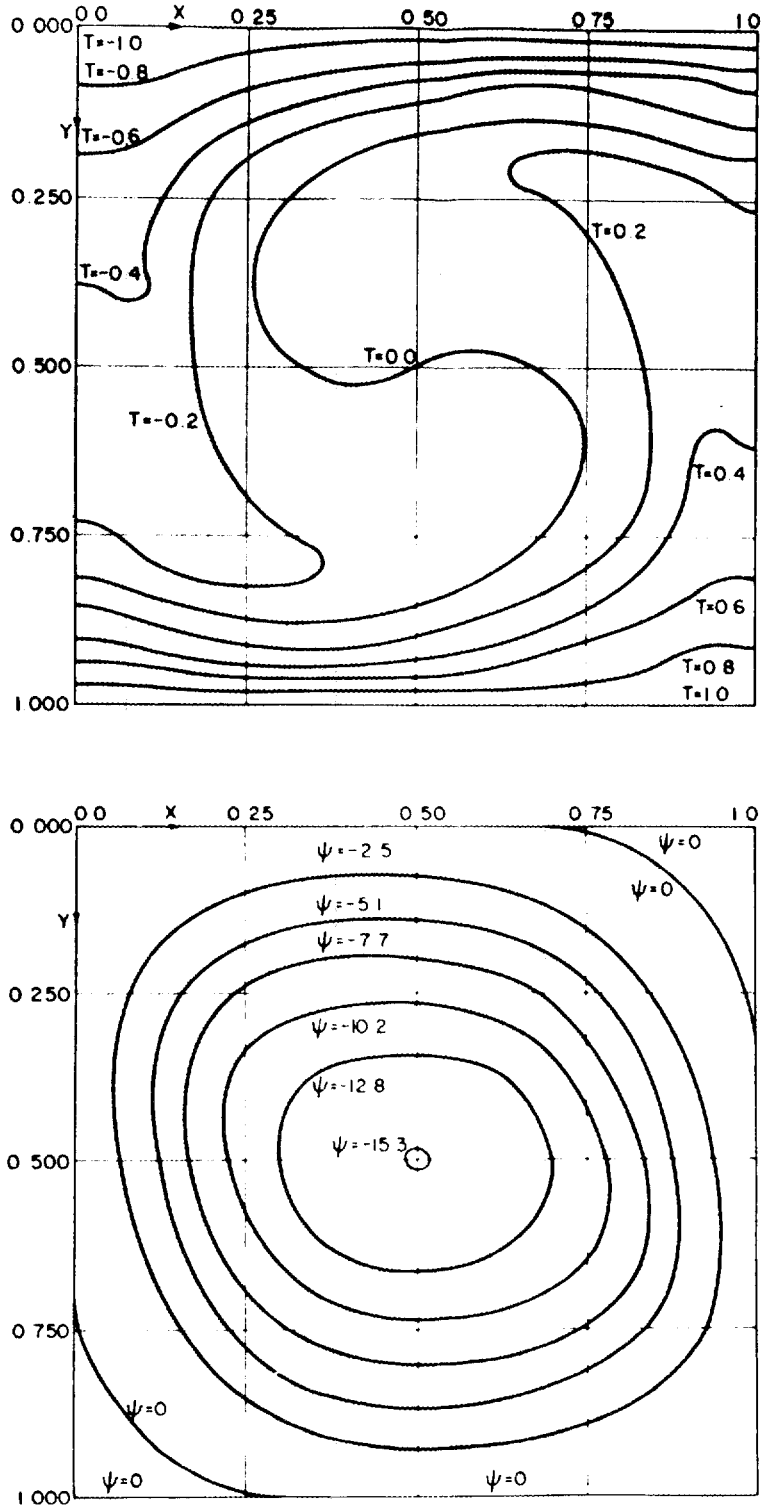

Fig. 26. Steady-State Streamlines and Isotherms for L/H = 1.0, Pr = 1.0, and Gr = 20,000 for the Enclosure with Isothermally Heated Bottom and Cooled Top with Insulated Side Walls.

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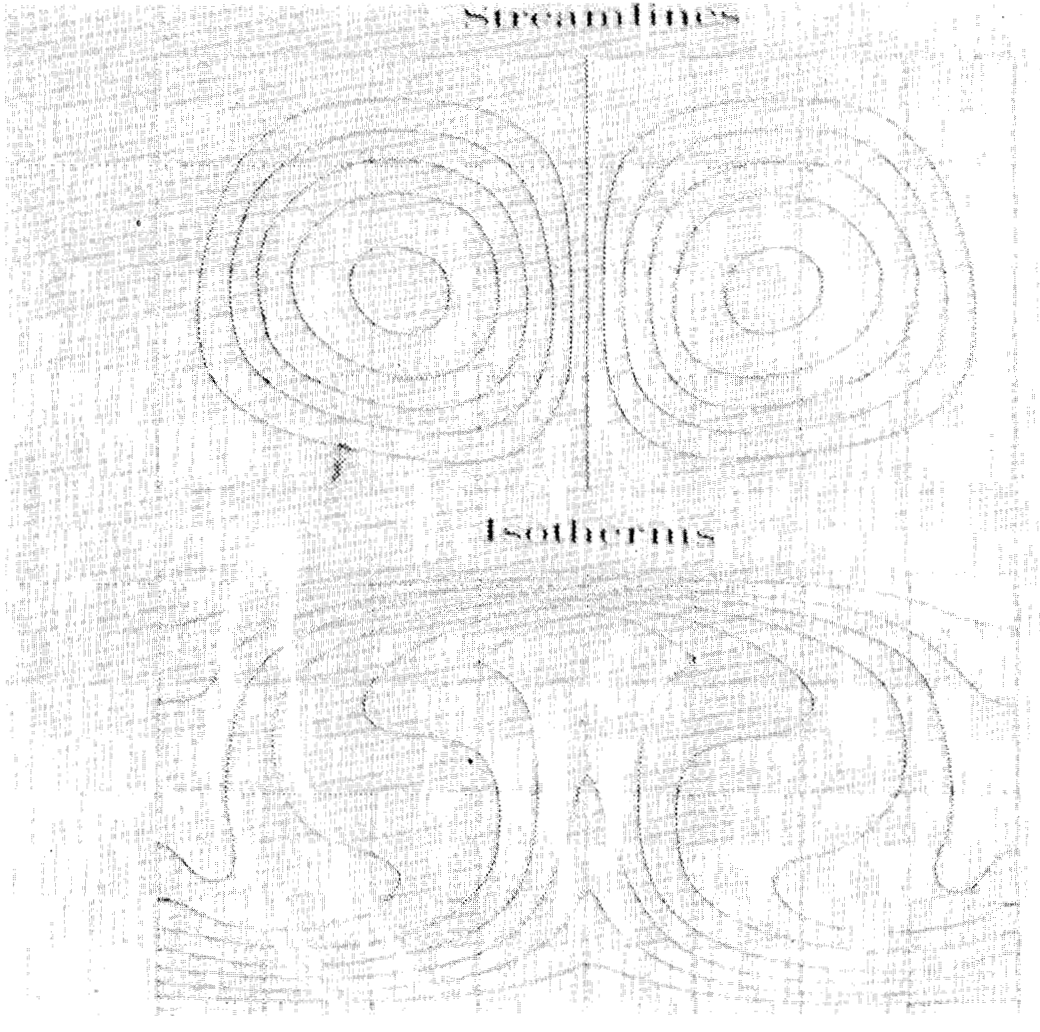

Fig. 27. Steady-State Streamlines and Isotherms for a Region with a Width-to-Height Ratio of 2 and Heating Occurring at Bottom and Cooling at Top.

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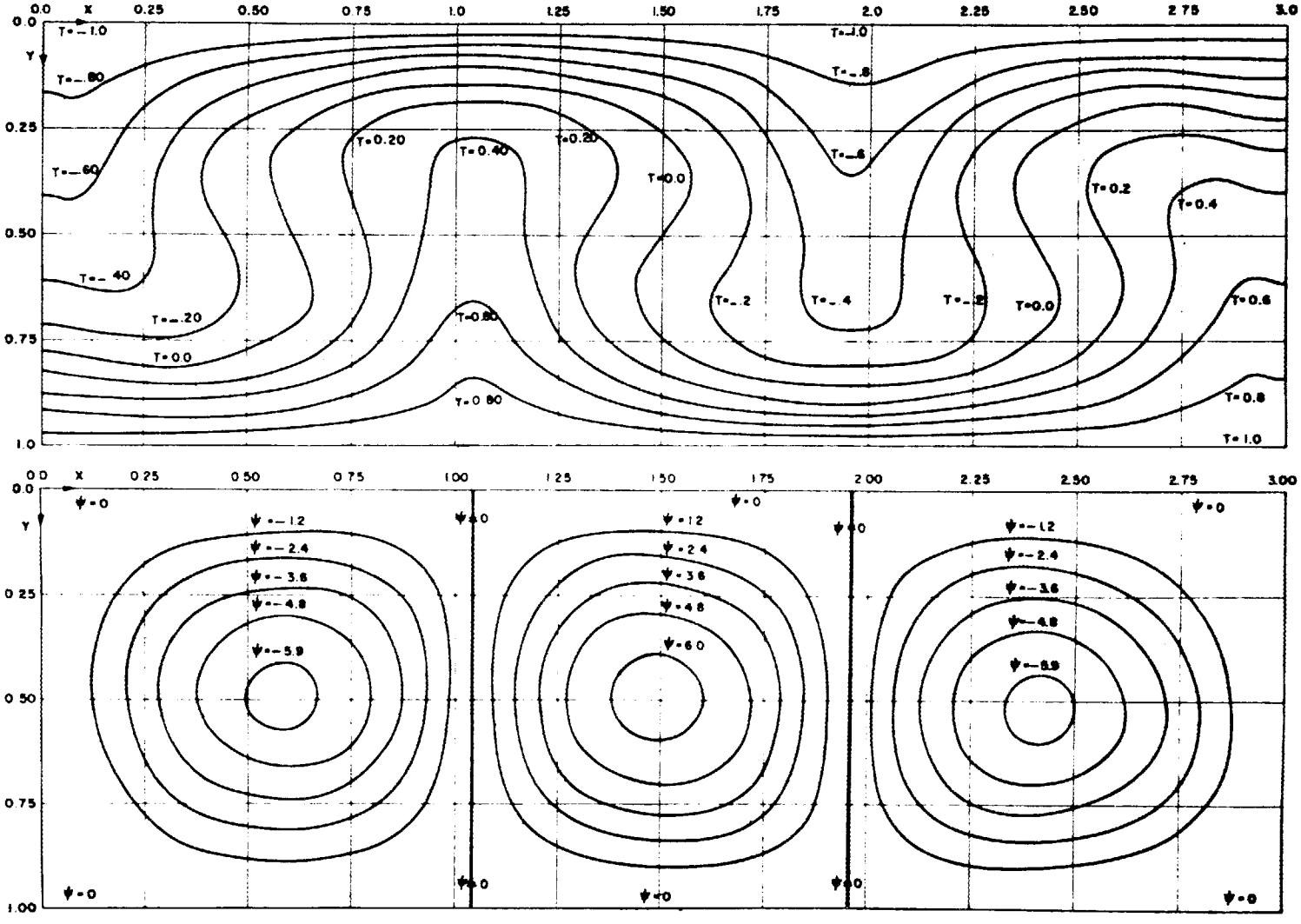

Fig. 28. Steady-State Streamlines and Isotherms for a Region L/H = 3, Pr = 1, and Gr = 10,000 with Heating at the Bottom and Cooling at the Top.

you can get is equal to the width-to-height ratio providing these are of integral size. One knows that the number of circulations must reduce from 3 to 2 as the width of the box is reduced but we didn't carry out enough calculations to define this breakpoint.

It is apparent from analysis that any number of circulations are meta-stable. In fact, Fromm at Los Alamos has carried out such calculations and produced results showing 7 different circulation patterns for this kind of a region. They are all valid solutions of the finite difference equations — which one is the unique solution? If you start the calculations and everything is perfectly symmetrical, then you get whatever solution pattern you want; that is, you can start the calculations in such a way that you will get 1, 2, 3, 4, 5, etc., circulations. However, if you introduce an assymmetric disturbance, you always get the pattern shown in Fig. 28. We started with 1, 2, 3, and 4 circulations and found that as long as we made them unsymmetrical, we always ended up with three circulations. That is what happens physically, because you always have some assymmetry physically. If nothing else, the mere variation of the physical properties with temperature provides some assymmetry. Thus, this is the physically unique solution, and can be validated experimentally. The others are merely mathematical artifacts.

I am a great believer in checking things experimentally, so we built a little apparatus (not a very good one) to test whether our computed results were correct. If we saw three circulations, as shown in Fig. 28, we would have been satisfied. You don't have to measure all of the temperatures and velocities. We had a little nagging worry in the back of our heads because we had assumed two-dimensional flow. Supposing that the flow pattern were actually three dimensional? Well, we built the box and everything looked lovely. We couldn't see very well, because we didn't have sufficient illumination; but we thought we saw a two-dimensional flow pattern. I said, "Why don't you improve the illumination so that we photograph the circulation pattern?" A few days later Samuels came into my office with a look of horror on his face and said, "Come up to the laboratory quickly." We now saw a three-dimensional circulation which suggested that none of our calculations had any validity. We had apparently made calculations for a phenomenon that didn't exist in the real world. Well, it turned out that whether you get two- or

three-dimensional flow depends on very minor perturbations. The light shining in the side to illuminate the tracer particles is enough of a perturbation to stabilize two-dimensional flow. If we had no light, we got three-dimensional flow. We discovered this by turning the light on quickly and seeing a three-dimensional pattern which then degenerated to a two-dimensional circulation. After many other experiments, we concluded that both of these circulations can exist physically and that a minor perturbation determines which you get. Thus, we have a solution to a physically existent problem; whether it exists in your laboratory or not, may depend on how hard you breathe.

This, of course, is a very unhappy situation. The literature is already filling up, both in mathematics and engineering, as to which one of these circulations is the more stable. The mathematicians have written on both sides. It turns out that it doesn't matter very much to an engineer if you are interested in heat transfer. The difference in the heat-transfer rate between two- and three-dimensional circulations is only about 5%. Why is this? Apparently because the solutions are almost equally stable. This was a grand lesson to me not to trust gross experimental work.

Three-dimensional calculations have been carried out for a cubical region by Aziz and Hellums at Rice University. However, calculations for three dimensions in an infinitely long box, would take perhaps ten thousand times as much computer time as for the two-dimensional case, using our technique. Thus, we conclude that truly, three-dimensional calculations in an unbounded region require a breakthrough in numerical methods or in the size of computers. We are trying very hard to surmount this problem, but we have no answers as yet. There are some people who have carried out some three-dimensional calculations — Deardorf at Boulder and Fromm at Los Alamos. However, we are convinced that neither of them had stable solutions. They apparently didn't bother to check stability.

I would like to tell you finally about one failure in our natural-convection analyses, because we were gaining so much confidence in our technique that we didn't think we could fail. We tackled the region

outside of a cylinder. We were going to heat a cylinder such as a wire and predict the surrounding temperature and the velocity fields. This really didn't seem any more complicated than the problems we had solved. After two years of persistent effort we did not succeed in solving the problem. The reason is as follows. You get a very narrow plume rising above the cylinder. The plume goes up, but it must come back down. It comes back down at infinity; that is, it comes back down everywhere. In order to satisfy continuity, you must take that distant flow into account. We did not succeed in finding a finite difference representation that was valid for the far field. We can get a solution that is good for the near field, but not for the far field. The boundary-layer approximations worked pretty well around the region near the cylinder. Of course, they don't do anything about the plume. We can calculate the plume, but we can't get it back down. I don't think this problem is insoluble, but it is difficult. Last week I saw a thesis in which somebody asserted that they had solved this problem. However, it only took about an hour to find that they didn't really have a valid solution. They didn't bother to check what happened to their far field. They had carried on a finite difference calculation and had not checked for convergence. Gentlemen, the literature is full of this type of nonsense. Editors shouldn't accept papers which do not include a proof of the validity of finite difference calculations. People are punching the buttons of machines and getting results and not checking them, either theoretically or experimentally.

I would like to summarize the results of what we have found for finite difference methods in natural convection as follows:

1. The problem of solving natural-convection problems is far more difficult than for conduction, for forced convection (in which you know the velocity field), or for radiation. The reason is that the nonlinear, coupled, partial differential equations which describe momentum must be solved.

2. The solution of realistic problems requires great ingenuity and trial and error because the mathematical theory of numerical stability is not applicable for practical boundary conditions. Formulation of a finite

difference representation for awkward boundary conditions, and particularly for unbounded regions, is anything but routine at the present time.

3. Cost considerations currently limit convective solutions to two-dimensional problems; further, except for very special cases, no solutions have been obtained for turbulent flow. Since such calculations would involve, by definition, three-dimensional unsteady-state random oscillations of a wide spectrum of size, they may never be practical. Better techniques and computers will come along and before long three-dimensional calculations will be practical. However, there is a valid question as to whether we will ever have solutions for turbulent flow.

4. Some experimentation is still an absolute necessity to test computational results for two reasons: (a) you must check to see whether the right model was chosen. If you put down the wrong equations or leave out a term or put in the wrong boundary conditions, you may get a solution to that mathematical problem; but it will have nothing to do with the real world. You must, therefore, check to see whether the correct model was chosen. (b) You must check to see if the solution is the physically unique one.

5. Since no one is likely to do the necessary experimental work to test theoretical results critically other than yourself, any theoretical investigation poses an ethical commitment of the same laboratory to its experimental validation. The literature is being filled with paper after paper of theoretical results which are left for somebody else to confirm.

6. Given the alternative of either experimentation or work on the computer, experimentation may well be cheaper and sufficient, even when we can carry out computations.

I would also like to compare numerical methods with classical methods of analysis. The use of analytical methods for the solution of problems in natural convection, and other mechanisms of heat transfer for that matter, is all but passé. Some time will be required for this reality to be recognized by everyone and to gain universal acceptance, since almost all of our mathematical training is in the analytical methods which were developed for the solution of the classical problems of the 19th

century. Many researchers will continue to confine their attention to those problems which are receptive to classical methods. However, the assumptions and simplifications they must make are seriously limiting and are generally not so necessary with numerical methods. The limitations I have mentioned for the numerical methods all exist for the analytical methods, but more seriously. The value of analytical solutions in the form of series for integrals or tabulated functions is often questionable. These solutions do not generally provide any more insight into the behavior of the system than a tabulation or a plotting from a finite difference calculation. Furthermore, evaluation of such solutions may well take more computer time than direct solution of the problem by finite difference methods. In summary, numerical methods in modern computing machinery offer us the opportunity to become engineers again rather than just scientists. We can turn our attention to some of the real problems that need to be solved, rather than to those problems which are receptive to classical methods of analysis. Finally, the numerical methods do not appear to eliminate the need for experimental work.

As you are aware, the title of my talk included radiation. However, I see that I have all but used up my time, so I will make only some very, very brief remarks on radiation. The field of radiation is very susceptible now to numerical methods of calculation. We can readily solve all of the classical problems. This is important, because they were so tedious that it was very discouraging to do them before. They almost all involve the evaluation of integrals or the solution of integrodifferential equations. I do not demean the difficulty of doing these problems, but they are really an order of magnitude simpler than the problems that I have illustrated. Unfortunately these classical problems invoke a couple of postulates, which throw away practicality. One such postulate is that of gray bodies and gray fluids. The fact is that all fluids have such an erratic variation of their physical properties with wave length that, if we were going to make realistic calculations for radiation, we would have to solve these equations in the wave length domain as well as the geometrical domain. This elevates the problem up to about the complexity of problems in natural convection. If that isn't bad enough, the real problem is we don't have data. The data for radiation

are in very poor shape despite all of the spectral measurements that have been made. Thus, it is difficult to justify carrying out very, very detailed calculations for radiation when data are not available as a check.

We found this area of finite difference calculations interesting and challenging. We have concluded that it takes as much ingenuity and research as any other field but that the results are proportionately rewarding. We no longer dare to carry calculations without setting up experiments to confirm their validity. I hope others head our warning.

DISCUSSION

Chairman (J. J. Keyes): We thank Professor Churchill for his very informative comments. I think we should all be challenged to go back and look at our engineering problems from the point of view of trying to use numerical solutions. I was a little worried that the experimentalist, among whom I consider myself, was becoming obsolete; but I was reassured that we are still in the picture. If we were not believers in numerical solutions, we certainly ought to be now. I believe Professor Churchill will consider questions from the floor.

W. M. Rohsenow (MIT): [Question was not clear on the tape.]

S. W. Churchill: I will summarize his comment so that it will be on the tape. Professor Rohsenow commented that I took a rather extreme position relative to the role of analytical methods. He asserted that they do indeed have a role in many problems. Furthermore, he said that they provided insight into the behavior of the different dimensionless groups as compared to simply tables of data or plots.

Of course, I agree with him about the utility of analytical solutions; and perhaps should clarify what I said. Analytical solutions will always provide for us useful results for simple problems as well as insight and bounding values for complex problems even when we have finite difference results. However, that as we try to solve more and more complex problems, the analytical methods have an uphill battle to compete with the greater generality of the finite-difference methods. As an example, to show you that I can wear either hat, I said that we had failed to solve the problem for the region outside a cylinder. We ended up doing the problem analytically. Instead, we settled for an analytical solution for that problem which is valid — for cylinders of any shape — blunt bodies, pointed bodies, etc. However we had to simplify the problem somewhat to obtain this solution. I am not opposed to analytical methods; I do believe we should recognize their limitations and the associated temptation to confine our attention to problems susceptible to analyses rather than to problems of engineering urgency.

I would like to argue a little about this matter of insight relative to the dimensionless groups. I would argue that you should always carry out an analysis just as far as you can in terms of dedimensionalizing and simplifying the equations before you go to the computer. It may be that the simplest result is not the best for computations, and I illustrated this. We made a mistake in going to the vorticity function for the problem of natural convection in the enclosure of Fig. 13. If we had it to do over again, we would just solve the original equations. Nevertheless, I would first try to simplify the representation as far as possible. Carrying out such an analysis may provide some insight. However, I do not believe that an analytical solution in the form of a series or integral provides any more insight than a numerical solution. Analysis using classical methods has a role, but I think the role is decreasing in such complicated problems as I have discussed this morning.

I might mention again the problem of convective instability. As long as you have a small Prandtl number, the very simplified analysis which was done about 20 years ago is valid. The only new things we appear to have obtained are the unsteady-state behavior and the effect of Prandtl number for small Prandtl numbers. But I assert that we obtained something more than this. We validated the assumptions made in the earlier analytical solution. The fact that this analysis existed was a great help to us.

W. T. Snyder (University of Tennessee Space Institute): I would like to comment briefly on this very interesting oscillatory behavior shown on Professor Churchill's slides during the approach to steady state in some of those transient problems. My colleagues and I have been doing some transient studies in a completely different area, in the field of magnetohydrodynamics. We have observed this same kind of oscillatory behavior as you approach the steady state and were concerned as to whether this happened physically. We had some errors in the calculation, etc., but apparently this is a characteristic of transient phenomena. There is something about the inertia of the system which tends to cause overshoot and readjustment. I was gratified to see this thing showing up in a completely different type of physical phenomena.

S. W. Churchill: Professor Snyder remarked that he had observed this oscillatory behavior in some of their calculations for magnetohydrodynamic phenomena. This behavior is, of course, characteristic of any set of equations in which you have two or three competing terms in the partial differential equation and also in the limiting case where you are going to get rid of one of these terms — the unsteady-state term. The two compete and gradually that one term drops out.

L. W. Crawford (Tennessee Technological University): I've followed your numerical studies with Martini and Hellums. I have the impression that at least in some of your earlier work you left out the pressure variations when you dealt with the velocity equations themselves. Could you clarify this?

S. W. Churchill: I was asked if we had left out the pressure term in the equations in our analysis. That is not an easy question to answer simply. In the earlier work we broke up the pressure term into the dynamic pressure term and the static pressure term and threw away the variation of pressure which results from flow. However in Wilkes' work we didn't neglect the dynamic pressure variation. We simply eliminated the pressure terms between the two equations. We didn't actually calculate the pressure variation although we could still go back and do so. This wouldn't involve anything but taking the results we have now and calculating the pressure gradients from the original equations. In the only check we made on this, the pressure variations were rather slight and not physically meaningful to us — maybe they would be to someone else.

L. W. Crawford: I have done some of these calculations myself in my own thesis, which I did not check experimentally, although there were some heat-transfer results in the literature and I checked them. However, in my thesis I used a technique which might be equivalent to making the grid finer. I used higher order terms in the difference equations to substitute for the velocity in particular, possibly third-order terms as well as first-order terms.

S. W. Churchill: Professor Crawford indicated that in his own calculations in the same general area he had used higher order of approximations for the derivatives as an alternative to making the grid finer.

This is indeed one of the possibilities of which I spoke, when I said that there are almost an infinite number of ways in which you can make one of these approximations. Of course, you must test for accuracy and convergence and stability when you get done. Also, the interesting question, and I think the engineering question, is which approximiation is best in terms of a given reliability and a given cost? This is a very difficult question, and I don't suppose any of us can answer it generally. We have not been very successful in using higher order derivatives. Curiously enough — not in this work but in some other work involving a chemical reactor — we encountered instability when we used higher order terms, but we obtained a good solution when we used a low-order representation. This was not a convergence problem but a stability problem.

In further answer to Professor Crawford, checking the Nusselt number is not really a critical test; and a proof of that arose relative to our multicirculations. I think this is what led Fromm astray. He found that 1, 2, 3, 4, 5, 6, or 7 circulations all give roughly the same Nusselt number. We found that two- and three-dimensional circulations also gave roughly the same Nusselt number. The critical test is simply to look at a flow pattern. This is a cheap experiment; you don't have to set up a very complicated apparatus. You can just throw some tracer particles in and photograph them and immediately see if you have the right profile. I have no question at all of the validity of the numerical results for temperature, if you have the right profiles. It is the velocity field that has all of these alternative possibilities.

L. W. Crawford: I was wondering in your heated lower plate and cooled upper plate, what sort of boundary conditions did you apply at the side walls? Did you have zero velocity there and linear temperature, or what sort of conditions?

S. W. Churchill: Professor Crawford inquired as to what boundary conditions we had at the side walls when we had a heated lower plate and a cooler upper plate. We attempted to make both our heated lower plate and our cooled upper plate isothermal and we tried to establish the condition of perfectly insulated side walls. This condition caused us great difficulty, because you can't get perfect insulation. Transparent

materials such as glass or Lucite turn out to be pretty good conductors relative to the very low rates of heat transfer by natural convection. Thus, it is very difficult to establish a sufficiently insulated side wall. There is no question whatsoever that the physical stability – i.e., whether it will circulate or not – does depend upon how well insulated the side wall is. If you have anything other than a perfectly insulated side wall or a linear temperature variation up the side wall, you will certainly have circulation at a very, very low Rayleigh number. You have the equivalent of a heated or a cooled side wall and circulation will start immediately. The quality of the insulation also determines whether you get a two- or three-dimensional circulation. If anybody has a transparent, perfect insulation I am in the market for it.

J. J. Keyes (ORNL): Has there been any success in using similarity tests in solving these convection problems: i.e., converting the partial differential equations to ordinary differential equations by the very judicious choice of the combinations of variables?

S. W. Churchill: If I had planted a loaded question, this is the one that I would have planted. I assure you I didn't, despite my old acquaintance with John.

It just happens that Dr. Hellums and I have developed a technique which we believe will find the minimum possible description of any boundary-value problem. By this I mean, if it is possible to reduce a partial differential equation to an ordinary differential equation or even to a partial of lower order, our technique will produce the necessary similarity transformation. Indeed, we have used this technique in every single one of the solutions I have discussed today; i.e., we have reduced the problem as far as it may be reduced. The technique also produces useful results in connection with repeated or alternative idealizations of the problem. If I throw away this term, do I get a different or further similarity transformation? If I also throw away this term, what happens? etc. You can see the possibilities. In most of these cases you obtain bounding, analytical solutions, or at least simplify the problem. For example, you see what happens in the limit for small Prandtl numbers or for small temperature differences. We chose the coordinates

in some of the plots I showed you in such a way that in the limiting case we would have a simple relationship. You may remember the plot of the overall Nusselt number versus the product of the Prandtl and Grashof numbers to the 1/4 power. The similarity transformation which results from throwing away all the terms that might be called second order, says that the Nusselt number should equal a constant times the product of the Prandtl and Grashof numbers to the 1/4 power. Indeed, when we plotted the data and computed values in that form, we could almost represent them by a straight line. Interestingly enough, similarity transformation says that this isn't the simplest form for the plot. I only used it because you were familiar with it. The analyses says the Nusselt number divided by Grashof number times the Prandtl number to the 1/4 power is a constant. You no longer should say Nusselt number, Grashof number, and Prandtl number; but should give a new name to that combined term, because it is the characteristic group for that natural-convection problem. We are inhibited in using these other groups. My extend answer to your question is yes, we should use similarity analysis both analytically and to help us to correlate the results for finite difference calculations.

J. W. Cooke (ORNL): In making thermal conductivity measurements, we would like to eliminate natural convection. I wonder if you would comment on the vertically heated cell, I mean the upper heated plate. How sure can we be that there is no natural convection with an upper heated plate and a lower cooled plate?

S. W. Churchill: The question is in making thermal conductivity measurements and what can one do to minimize or predict the influence of natural convection with a heated upper plate and a cooled lower plate convection would be generated only by heat flow through the side of the apparatus. There is some experimental data in the literature on the onset of natural convection in packed beds. Our solutions are all for empty regions but I suspect that the results are qualitatively valid for an insulation.

J. J. Keyes: I want to thank Professor Churchill again for a very stimulating discussion.

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SPECIAL LECTURE II

THE FICTIVE FILM CONCEPT

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INTRODUCTION OF SPEAKER

J. J. Keyes (ORNL), Chairman: Our next lecturer is Richard N. Lyon who obtained his MS and PhD degrees at the University of Michigan and then came to Oak Ridge in 1945 on the staff of the Clinton Laboratories, which was the original name of the Oak Ridge National Laboratory. He has been in the nuclear engineering business here in Oak Ridge ever since that time. He has 24 years of experience in the nuclear field, being at the Metallurgical Laboratory in Chicago prior to coming to Oak Ridge. He has been associated, as you might well imagine, with many different types of reactors, among these being the original Graphite Reactor in Oak Ridge, the Hanford production reactors, and more recently the Aqueous Homogeneous Reactor, the Experimental Gas-Cooled Reactor, the High-Flux Isotope Reactor, and the Molten Salt Reactor. As you are well aware, he has made significant contributions – both analytically and experimentally – in the fields of heat transfer and fluid mechanics. I might mention especially his work with aqueous uranium and thorium slurries. However, I think he is best known for his work in the field of liquid-metal heat transfer, having developed – what I believe I am safe in saying – the most widely used equation for predicting liquid-metal heat-transfer coefficients in round tubes and also as Editor-in-Chief of the Liquid Metals Handbook. More recently, he has turned his attention to the field of boiling and two-phase flow, especially the problems of bubble nucleation and associated superheat phenomena in liquid metals. He has done some work of a theoretical nature with regard to the capillary heat pipe device. This morning, he is going to tell us something about the frequently used but little understood fictive film concept. I would like to present Dr. Richard Lyon.

THE FICTIVE FILM CONCEPT

R. N. Lyon

Ever since I became a chemical engineer, I have heard about film resistance and film coefficients, and so on; and I gradually learned that the terms were applied in many different ways, and sometimes erroneously. Often younger engineers are confused between a boundary layer, a so-called laminar sublayer, and the fictive film. I notice Moody, for example, in his famous work* on fluid friction in smooth and rough pipes refers to a laminar sublayer as a laminar boundary layer. It is a very common slip of the tongue, or pen, which helps continue the confusion.

What I am going to do this morning is discuss some "doodling" that I have been carrying on over the last two or three years on the old concept of a fictive or fictitious laminar film, as a way of approximation in momentum, mass, and heat transfer in turbulent flow in various shaped conduits. The qualitative concept is old; but I hope to provide a solider, more quantitative, basis for it than I think you have come across before. The system under discussion this morning is one in which the flow is well established or, in the case of heat transfer, where the temperature distribution is well established. The conduit may be of any reasonable shape; later on we will see that some of these concepts can be applied to regions around solid bodies in free-stream flow as well.

In Fig. 1, I picture a cell in a conduit cross section. The apex of this cell is at the line of maximum velocity in the conduit. P_w (shown in script form in the figures), the base of the cell, is a short portion of the conduit perimeter; and the sides are two velocity gradient lines that start at either end of P_w and met at and are normal to the line of maximum velocity (we are, of course, assuming that the local velocity, being fixed in direction, can be treated as a scalar). With annular flow or flow between parallel plates, the trace of the surface of maximum velocity would appear as a line, rather than as a single point. No shear will occur across any boundary except P_w.

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*Lewis F. Moody, "Friction Factors for Pipe Flow," Trans. ASME, 63: 671–684 (1944).

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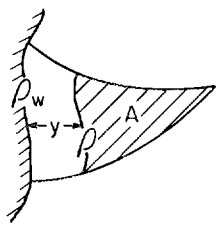

$$\tilde{\rho} \equiv \rho/\rho_w;\ \tilde{A} \equiv A/A_w;\ r_h \equiv A_w/\rho_w;\ \tilde{y} \equiv y/r_h$$

$$\tilde{u} \equiv u/u_m;\ Re \equiv \frac{4 r_h u_m \rho}{\mu};\ f \equiv \frac{2 g_c \tau_w}{\rho u_m^2}$$

$$d\tilde{A} = -\tilde{\rho}\, d\tilde{y};\ du = \frac{g_c \tau}{\mu(1+\epsilon/\nu)}\, dy$$

$$\tilde{u} = \frac{Re\, f}{8} \int_0^{\tilde{y}} \frac{\tilde{A}/\tilde{\rho}}{1+\epsilon/\nu}\, d\tilde{y}$$

$$\frac{8}{Re\, f} = \int_0^{\tilde{y}_o} \frac{\tilde{A}^2/\tilde{\rho}}{1+\epsilon/\nu}\, d\tilde{y}$$

Fig. 1. Generalized Fluid Friction Relationships for Steady-State Flow in Filled Conduit of Uniform Cross Section.

Let us consider a portion of our cell of area A, beginning at the apex and bounded by a line of constant velocity having length P. Considering the entire cell, $P = P_w$, the wetted perimeter of our cell. The total area we call A_w. We can now define a dimensionless perimeter, $\tilde{P}$, as P/P_w, and a dimensionless area, $\tilde{A}$, as A/A_w. The hydraulic radius, r_h, as usual, is the total area, A_w, of this element divided by a wetted perimeter, P_w; and the dimensionless distance from the wall, $\tilde{y}$, we will define as the distance from the wall, y, divided by the hydraulic radius. We will find later that we can use a dimensionless velocity, $\tilde{u}$, which is simply the velocity, u, divided by the mean velocity, u_m. Then, we can define in the usual way the Reynolds number and the Fanning friction factor (the ratio between the wall shear stress and the mean velocity pressure). We find that a small increase in the dimensionless area $d\tilde{A}$ equals minus the dimensionless perimeter, P, times the corresponding small increase in the dimensionless distance from the wall, $d\tilde{y}$. From the definition of viscosity, μ, and eddy diffusivity, ϵ, we obtain a general expression for the increase in velocity with increasing distance from the wall. Integrating this expression from y = 0 will give the velocity at any value of y; and weighting those velocity values by P, integrating again between y = 0 and y at the maximum velocity, y_o, and dividing by A_w gives a double integral expression for u_m. With a little appropriate juddling, we can switch around the order of integration and wind up with a single integral as shown in Fig. 1 for $\tilde{u}$ (the ratio of local velocity to mean velocity) and for the general expresssion, eight over the Reynolds number times the friction factor, for any little cell defined as I have indicated for any type of flow which is well established and where there is no net local flow in the plane of the cell.

In Fig. 2, we simply prove things out for round tubes and parallel plates. Note that $\tilde{y}_o$ is 2 in one case and 1 in the other and that, for laminar flow when ϵ is zero, Re $\times$ f = 16 for round tubes and 24 for parallel plates.

In the next figure (Fig. 3), looking back again at the final expression in Fig. 1, we see that if $\tilde{A}$ and the dimensionless perimeter, $\tilde{P}$, are very close to unity then they will have relatively little importance on

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ROUND TUBES

$\tilde{r} \equiv r/r_w$

$\tilde{y}_o = 2$

$\tilde{r} = 1 - \frac{\tilde{y}}{2}$

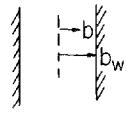
$d\tilde{y} = -2\,d\tilde{r}$

$$\frac{8}{\mathrm{Re}\ f} = 2\int_0^1 \frac{\tilde{r}^3}{1+\epsilon/\nu}\,d\tilde{r}$$

LAMINAR FLOW

$\mathrm{Re}\ f = 16$

PARALLEL PLATES



$\tilde{b} = b/b_w$

$\tilde{y}_o = 1$

$\tilde{b} = 1 - \tilde{y}$

$d\tilde{y} = -d\tilde{b}$

$$\frac{8}{\mathrm{Re}\ f} = \int_0^1 \frac{\tilde{b}^2}{1+\epsilon/\nu}\,d\tilde{b}$$

LAMINAR FLOW

$\mathrm{Re}\ f = 24$

Fig. 2. Fluid Friction Relationships for Round Tubes and Between Parallel Plates.

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$$\frac{8}{\mathrm{Re}\ f} = \int_0^{\tilde{y}_o} \frac{\tilde{A}^2/\tilde{\rho}}{1+\epsilon/\nu}\, d\tilde{y}$$

$$= \int_0^{\tilde{y}_f} d\tilde{y} + \int_{\tilde{y}_f}^{\tilde{y}_o} \frac{\tilde{A}^2/\tilde{\rho}}{\infty}\, d\tilde{y}$$

$$= \tilde{y}_f$$

IMPLICIT POSTULATES

1. $\tau = \tau_w$ THROUGHOUT THE FILM
2. $u = u_m$ EXCEPT IN THE FILM

Fig. 3. Definition of the Relative Thickness of the Fictive Film for Momentum Transfer.

that portion of the integral. If we think of a system consisting of a region very close to the wall of thickness $\tilde{y}_f$ in which only laminar flow occurs and then a region from $\tilde{y}_f$ to the center (designated as $\tilde{y}_o$, the distance to the point of maximum velocity) having a very high conductivity or diffusivity for momentum – and therefore the denominator in the integrand of the first equation approaching infinity – we have an expression that says that only what is going on in this laminar sublayer will be important. If we carry out this integration – and it is a very simple one obviously – we get that eight over the Reynolds number–friction factor product is equal to the thickness of this film divided by the hydraulic radius. In arriving at this, we have made a number of assumptions, some of which are explicit but some of which are implicit. We have – making this assumption – implied that the shear stress is uniform through that film. This is not implied in the derivation of the first equation but in winding up with this result we do imply that $\tau = \tau_w$ through the film. We also imply that the velocity in all regions of the turbulent core is equal to the mean velocity when, obviously, this cannot be so. Since the velocity in the laminar film is less than the mean velocity, there must be some region – logic tells us – in which the velocity is greater than the mean. However, for many systems the error may not be great.

In Fig. 4, we see the results of a few calculations made to see how serious are our approximations. Looking at the plot in the upper portion of this figure, if we think of this as the velocity distribution for turbulent flow (lower curve), we have first of all said that there is only laminar flow. The classical laminar flow solution is as shown by the $\epsilon/\nu = 0$ curve; it is curved because of the change in shear stress as one moves into the tube. If we keep the shear stress constant, then we move up the straight line ($\tau = \tau_w$) which, of course, is tangent to the other two lines as they approach the wall. We have then a point, $\tilde{y}_b$, which represents the thickness that would be required to actually satify the material balance in the two regions. This is perhaps less important than the thickness, $\tilde{y}_a$, that would be required if we also made allowance for the change of shear stress as we go through the film. The final point, $\tilde{y}_f$, is the – granted – erroneous film thickness arrived at if we assume

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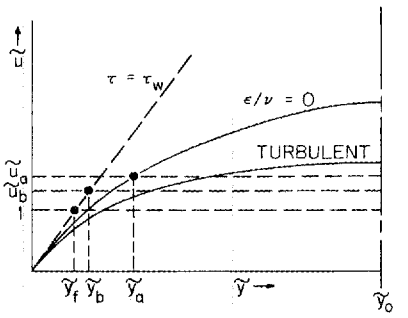

FOR ROUND TUBES

$$\tilde{y}_f = \frac{8}{Re\, f} \quad ; \tilde{u}_f = 1$$

$$\tilde{y}_f = \tilde{y}_b - 1/2\, \tilde{y}_b^2 + 1/12\, \tilde{y}_b^3 \quad ; \tilde{u}_b = \tilde{y}_b/\tilde{y}_f$$

$$\tilde{y}_f = \tilde{y}_a - 3/4\, \tilde{y}_a^2 + 1/4\, \tilde{y}_a^3 - 1/32\, \tilde{y}_a^4 \quad ; \tilde{u}_a = \tilde{y}_a/\tilde{y}_f - \frac{\tilde{y}_a^2}{4\tilde{y}_f}$$

Re	$\tilde{y}_f$	$\tilde{y}_b$	$\tilde{y}_a$	$\tilde{u}_b$	$\tilde{u}_a$
<2100	0.5	0.74	2.00	1.48	2.00
4×10^3	0.100	0.1055	0.1085	1.055	1.056
10^4	0.0536	0.0540	0.0548	1.027	1.028
10^5	0.00894	0.00898	0.00900	1.004	1.004
10^6	0.00138	0.00138	0.00138	1.000	1.000
10^7	0.00020	0.00020	0.00020	1.000	1.000

Fig. 4. A Comparison of Film Thicknesses and Central Core Velocities for Three Postulated Models in a Round Tube. (a) τ in the film = τ_w $(1 - \tilde{r})$ and $\tilde{u}$ of core is consistent with the volume of and flow in the laminar film, (b) $\tau = \tau_w$ but $\tilde{u}$ of core is consistent with the volume of and flow in the laminar film, (c) the fictive film case where $\tau = \tau_w$ in film and $\tilde{u} = 1$ in core.

we are just going up the $\tau = \tau_w$ line to the mean velocity, $\tilde{u} = 1$.

In the central region of Fig. 4 are the equations that we obtain for these three different film thicknesses. The first is the actual fictive film as usually used in people's thinking, the second is the relationship between the fictive film and the film we would get if we complete the material balance, and the third is the equation which allows for both a material balance and the change in shear stress as we go through the film.

Some sample calculations are given in the lower portion of the figure. For laminar flow – i.e., for Reynolds number less than 2100 – we see that there is a wide difference between the thicknesses of the various films. In contrast, if we go to a Reynolds number of 4000 which is just over into the turbulent region, we see that the values for the several film thicknesses are not much different. Even $\tilde{y}_a$ varies from the thickness of the fictive film by <10%. As we approach higher Reynolds number, the difference becomes even less; and at a Reynolds number of 10^6, we see that there is essentially no difference between the films whether we try to keep our material balance correct or to make allowance for the shear stress variation in the film.

It should be pointed out here, perhaps, that what we are doing – and the reason we are interested in this problem – is as follows: If we want to know what the pressure drop is in a pipe for a given mean flow or we want to know what the heat transfer is (i.e., what is the increase in mixed-mean temperature of a fluid flowing at a given mean velocity through a pipe with a given temperature at the wall and a given mixed-mean temperature of the fluid), we are not necessarily concerned with what is going on – what is the microscopic behavior or the local behavior – except as that gives us a handle on how to compute the results. Therefore, we do have the freedom of postulating this type of mechanism, if it gives us a useful engineering result; if it does not, then of course, we cannot use it. This is a freedom which we take advantage of, I think, many more times than we realize. We do know that this is not the way the material behaves; but we nevertheless find it useful to think of it this way. As long as we recognize the limitations of the system, we can use it

and get useful results. This morning we say the sun rose, but it did not rise. We find it convenient to think of the sun rising and sinking in everyday affairs, although we all know that Copernicus went to great length to explain that this all occurred because the Earth was spinning and going around the Sun, rather than the Sun going around the Earth.

I have also tabulated in Fig. 4 the values for $\tilde{u}_b$ and $\tilde{u}_a$ which, of course, are compared to the value of 1 for the mean dimensionless velocity. Again, for laminar flow, we get seriously erroneous results; but for the other models, we get quite reasonable results. This gives us confidence, then, that this model can be used in looking at turbulent flow. Turbulent flow, as you all know, is a situation which is not amenable at the present time to detailed analytical investigation.

There is another way of expressing the velocity-distance relationship and that is by means of the so-called universal velocity plot developed by Nikuradse; this is shown in Fig. 5. We define a so-called friction velocity which is the square root of the shear stress divided by the density. You are, of course, all familiar with the fact that the u^+ then is simply u divided by the friction velocity or $\tilde{u}$ divided by the square root of the friction factor over 2; this is shown by the second equation at the top of the figure. The third equation gives us the dimensionless wall distance. We all know that this velocity deviates from the constant-sheer, laminar-flow curve — i.e., the $u^+ = y^+$ curve — in the region of $y^+ = 5$ or perhaps a little lower and that there is a break somewhere in the vicinity of $y^+ = 30$. The equations approximating these results are well known; we will come back to those.

We can talk about a y^+, then, for the fictive film which turns out to be simply the square root of 2 over the friction factor (since $\tilde{u}$ is equal to 1 for the mean velocity). To the right of the plot in Fig. 5, we see an expression which is obtained for the square root of 2 over the friction factor using the actual data Nikuradse obtained; Prandtl actually did the graphical integration. In a later figure we will see a similar equation developed by von Karman using this equation. It is interesting that there is no relationship, really, between the so-called laminar sublayer, which may be down here at low values of y^+, and the fictive film, which may

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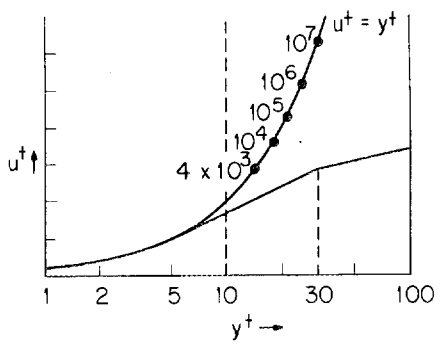
$$\frac{Re\, f}{8} = \frac{g_c \tau_w r_h}{\mu u_m} = \frac{g_c (dp/dx)}{\mu u_m} r_f^2 = \frac{1}{\tilde{y}_f}$$

$$u^+ \equiv u \Big/ \sqrt{\frac{g_c \tau_w}{\rho}} = \frac{\tilde{u}}{\sqrt{f/2}}$$

$$y^+ \equiv \frac{y \sqrt{\frac{g_c \tau_w}{\rho}}}{\nu} = \tilde{y} \frac{Re\sqrt{f/2}}{4}$$

$$y_f^+ = u_f^+ = \sqrt{2/f}$$



$$\sqrt{2/f} = u_f^+ = y_f^+ = 5.66 \text{ LOG } Re\sqrt{f} - 0.566$$
(PRANDTL)

IF $\sqrt{f} \approx a\, Re^n$

$$\sqrt{2/f} = 5.1 \text{ LOG } Re - 4.2$$

Fig. 5. The Thicknesses of the Fictive Films for Various Values of Reynolds Number Compared with the Universal Velocity Profile.

actually extend into the turbulent core way beyond the distance anyone would consider for the laminar sublayer. For example, at a Reynolds number of 10^7, $y^+ = 31$ as shown by the final point on the $u^+ = y^+$ curve. Since the friction factor does not enter this expression in a very large way, we can approximate it by the second equation shown to the right of the plot in Fig. 5; and when I plotted these data, I came out with the final equation which approximates the square root of 2 over f to within 2 or 3% of the values obtained here.

We can do another thing which I think is kind of interesting. In Fig. 6, we see first the expression for 8/Re x f which we obtained earlier (Fig. 2) and then a universal expression for the Nusselt number in round tubes which was published some years ago. Continuing, if we say that the velocity is everywhere uniform ($\tilde{u} = 1$), then this integration is easily carried out and gives us $\frac{1}{4} r^4$ (or $\frac{1}{2} r^3$ inside the integral). Further, if we say that α (simply the ratio of eddy diffusivity of heat to momentum) is unity, that the Prandtl number times the eddy diffusivity is very large, and that the eddy diffusivity over the kinematic viscosity is also very large, then the Prandtl number can be brought outside the integral; and we immediately wind up with the Reynolds analogy for heat transfer: $St = f/2$. In doing this, we make some very serious assumptions; and I think that it important to see what these are. One is that the value of the integral is actually $\frac{1}{4} r^4$. Azer and Chao have recently looked at some of the published integrations of actual data and have decided that it is much better approximated as shown in Fig. 6. A much more serious assumption, made by Reynolds of course, is that everywhere in this system ϵ/ν is very large with respect to unity; this is exactly backward from the approximation we are making in our fictive film proposal. In fact, we are saying this is so large over a part of the flow region and so unimportant over another part that we are not concerned with this. Now, neither of those is right; and of course, as Colburn has found, we can sort of lump the effect of the Prandtl number in and wind up with a Prandtl number correction for the Reynolds equation which takes into account the competition between the eddy diffusivity and the Prandtl number in the regions reasonably close to the wall and on out toward the center. This, it turns out, is a tremendously powerful approximation and is found to apply not only in straight tubes in turbulent flow

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FOR ROUND TUBES

$$\frac{8}{\mathrm{Re}\, f} = 2\int_0^1 \frac{\tilde{r}^3}{1+\epsilon/\nu}\, d\tilde{r};\ \tilde{r} = \tilde{r}/\tilde{r}_w \quad \text{(FIGURE 2)}$$

$$\text{AND } \frac{1}{\mathrm{Nu}} = 2\int_0^1 \frac{\left[\int_0^{\tilde{r}_2} u_1 r_1 d\tilde{r}_1\right]^2}{\tilde{r}_2 (1 + \alpha\, \mathrm{Pr}\, \epsilon/\nu)} \quad \text{(LYON)}$$

IF $\tilde{u} = 1$; $\alpha = 1$; $\mathrm{Pr}\ \epsilon/\nu$ AND $\epsilon/\nu >> 1$

$$\frac{1}{\mathrm{Nu}} = \frac{1}{2\,\mathrm{Pr}} \int_0^1 \frac{\tilde{r}^3}{1+\epsilon/\nu}\, d\tilde{r}$$

$$= \frac{2}{\mathrm{Re}\, f\, \mathrm{Pr}}$$

OR $\mathrm{St} = f/2$ (REYNOLDS)

$$\int_0^{\tilde{r}_2} \tilde{r}_1 \tilde{u}_1\, d\tilde{r}_1 \approx 1/2\, \tilde{r}_2^{\,1.75} \quad \text{(AZER AND CHAO)}$$

$$\text{OR } \frac{1}{\mathrm{Nu}} = 1/2 \int_0^1 \frac{\tilde{r}^{2.5}}{1 + \alpha\, \mathrm{Pr}\, \epsilon/\nu}\, d\tilde{r}$$

$$\mathrm{St} = \frac{f}{2}\, \mathrm{Pr}^{-2/3} \quad \text{(COLBURN)}$$

Fig. 6. Relationship of the Reynolds and Colburn Equations for Forced-Convection Turbulent Heat Transfer in Round Tubes, to the General Fluid Friction, and General Heat-Transfer Equations.

but also in converging and diverging systems — within experiment error or engineering error — and even for turbulent boundary-layer regions on bodies in the free stream.

Turning to Fig. 7, we see that four over the Nusselt number represents the thickness of a fictive film for heat transfer. This is not, of course, the same fictive film as for friction or for momentum transfer. If we are going to allow ourselves to think of a fictive film for momentum transfer, we can also think of one for heat transfer. Then, following on down through the equations of Fig. 7 and using the Colburn approximation, we find that the fictive film for heat transfer is equal to that for momentum transfer times the Prandtl number to the $-1/3$ power. In other words, the larger the Prandtl number, the thinner the fictive film for heat transfer, because the turbulence effects begin to become important more quickly. The fictive film for mass transfer would be the same except we now use the Schmidt number rather than the Prandtl number. It is interesting to note that, if we have a Prandtl number of 100 and a Reynolds number of 4000, y^+ of the fictive film for heat transfer is only about 3 1/2. This value is down inside the laminar sublayer which von Karman assumed to be of thickness $y^+ = 5$ in his analytical development for heat transfer. This explains, of course, why von Karman's development does not apply for Prandtl numbers above somewhere in the region of 30; he made the assumption that only conduction carried the heat up to a y^+ of 5. Of course, all of us realize now that this was an erroneous assumption, that eddying does occur clear up to the wall of the system, and only that it is small compared to the eddy diffusivity for momentum transfer led to this other erroneous idea. I might point out that Schmidt numbers of the order of 1000 are well known, which would take us down in the y^+ range of 1 or 1 1/2 for the fictive film thickness for mass transfer. This again indicated that eddying can be important way down into the region that people ordinarily think of as the laminar sublayer.

At the bottom of Fig. 7, I have put in a couple of equations to indicate what happens when the Prandtl number becomes very small. The fictive film for heat then becomes large, and we begin to approach what happens with laminar flow. The fictive film concept breaks down. Thus,

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$$Nu \equiv \frac{4\,h\,r_h}{k}$$

$$y_h \equiv \frac{k}{h}$$

$$\tilde{y}_h = \frac{4}{Nu} = \frac{4}{Re\ Pr\ St}$$

FOR TURBULENT FLOW, ORDINARY FLUIDS

$$\tilde{y}_h = \frac{8}{Re\ f\ Pr^{1/3}} = \tilde{y}_f\ Pr^{-1/3}$$

AND

$$\tilde{y}_m = \tilde{y}_f\ Sc^{-1/3}$$

IF Re = 4000 AND Pr = 100, $y_h^+ \approx 3.49$

WHEN $Pr < 1$, $\tilde{y}_h > \tilde{y}_f$

$$\tilde{y}_h = \frac{4}{7}\ \frac{\tilde{y}_f}{\tilde{y}_f + 4\,\alpha\,Pr^{0.8}} \text{ (LYON)}$$

$$= 2.64\ \tilde{y}_f^{\,0.5} \text{ (LUBARSKY AND KAUFMAN)}$$

Fig. 7. The Fictive Films for Heat and Mass Transfer.

for liquid metals, neither of these expressions has much meaning except to show that the fictive film for heat transfer will be considerably larger than that for momentum transfer and, therefore, that the fictive film concept cannot be used for liquid metals.

Carried right along with this – and I should have brought it up earlier – is the whole concept of the hydraulic diameter. We have used the hydraulic diameter all the way through this analysis. The idea that it can be carried over to another system – a system of another shape – is valid only if we have approximately uniform shear stress all the way around the walls of the system, and if the conditions we have imposed in order to develop our fictive film concept apply; namely, that the velocity is uniform over the entire cross section of the system. Again, the hydraulic diameter concept cannot be applied to concuits of different shapes for heat transfer in liquid metals.

About a month ago, I went through a little exercise to see what happens with respect to roughness. There is an equation for roughness developed by Colebrook that is actually a combination of the smooth tube relations for $\sqrt{2/f}$ developed by Prandtl (Fig. 5) and another equation for roughness also developed by Prandtl. Colebrook merely indicated how the transition occurred between these two. In Fig. 8, we see the Prandtl smooth tube equation (somewhat rewritten) and the Colebrook equation (also rewritten). I have applied a little algebra and converted the height of the roughness into y^+ terms. In other words, I have taken the height of the roughness, divided it by the kinematic viscosity of the fluid, and multiplied it by the friction velocity. Figure 8 is then essentially a y^+ plot, and we can use the final equation to see how the mean velocity is altered by the type of roughness characterized in the upper left portion of the figure. Now it is well known to all of you, I think, that if the shear stress at the wall is a constant, then, the velocity distribution is going to be a constant all the way into the center of the channel regardless of the surface which causes that shear stress. Thus, a smooth tube with a given shear stress at the wall will have the same velocity distribution as a rough tube with the exception that all of the velocities have been reduced by a fixed amount. In effect, what we have done by adding

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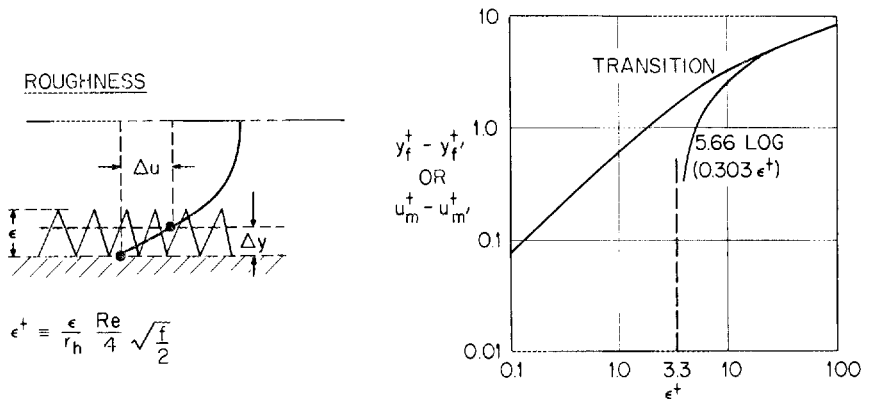


$$\epsilon^+ \equiv \frac{\epsilon}{r_h}\,\frac{Re}{4}\sqrt{\frac{f}{2}}$$

FOR SMOOTH TUBE

$$y_f^+ = \sqrt{\frac{2}{f}} = 5.66 \text{ LOG } (0.794 \text{ Re}\sqrt{f}) \qquad \text{(PRANDTL)}$$

FOR ROUGH TUBE (TRANSITION)

$$y_{f'}^+ = 5.66 \text{ LOG } \left(\frac{2.62 \text{ Re}\sqrt{f}}{\epsilon^+ + 3.3}\right) \text{ (COLEBROOK)}$$

HENCE

$$y_f^+ - y_{f'}^+ = 5.66 \text{ LOG } \left[0.303\,(3.3 + \epsilon^+)\right]$$
$$= 5.66 \text{ LOG } (3.3 + \epsilon^+) - 2.93$$

Fig. 8. The Fictive Film for Rough Tubes, and the Effective Height of Roughness.

roughness is simply to shave off a part of the universal velocity profile. You can think of this as of moving the zero-velocity point in a given y^+ distance or as shaving off a given amount of u^+. The figure indicates how much is shaved off for given values of roughness as expressed in y^+ terms. It is interesting that this can be expressed in terms which do not include the Reynolds number as a separate parameter. I have also plotted a limiting case; this is the case that Prandtl calls the fully turbulent rough situation. The Colebrook solution, of course, merges into this. Obviously, the "fully turbulent" situation cannot apply if ϵ^+ is less than 3.3, since that would give us a negative value for the limiting solution.

How one applies roughness fictive film ideas to heat transfer is a bit of a problem, since – as I am sure all of you know – in principle, if it applies, the Stanton number over the friction factor should be essentially constant. What happens with roughness is one of several things – or several of several things. Either the roughness is caused by scale building up, in which case all bets are off; or it is caused by a wavey surface, in which case we have much greater increased shear stress near the wall and greatly reduced shear stress in the central region. However, the increase in shear stress may be greater as the fluid flows over the converging part of the roughness (see diagram at upper left of Fig. 8) than in the diverging section. Therefore, the friction factor may be increased. If there is no separation in the region of the roughness, then there is some evidence that the fictive film concept can be applied. We can then say that the fictive film (which in this case would be a sort of average fictive film) times the Prandtl number to the $-1/3$ power is the fictive film for heat transfer. However, we often actually have separation on the roughness elements; and we run into trouble, because we are considering momentum which is a vector and comparing it with heat which is scalar. If we have separation and, hence, a loss of momentum which is only indirectly associated with what is going on at the wall, we do not get heat transfer at as great a rate as would be predicted by the fictive film expression.

In thinking about this and in the face of what Professor Churchill said this morning, I nevertheless have an expression that I dreamed up the other day. I have not had the chance to check this against data. Let's assume that only the part associated with unseparated flow is transferring the heat. This should be a pessimistic approach. As you know, the friction factor decreases with Reynolds number and for a given roughness tends to level off. Let's assume that only the part of the friction factor which continues to decrease is important and that any deviation from this is caused by a component due to separation which gives a friction factor independent of the Reynolds number. When we do this, using our earlier approximation for $\sqrt{2/f}$, we wind up with

$$\frac{St}{f} Pr^{2/3} = \frac{f_1 - f_2}{104\,[\log^2(1.49\ Re_2) - \log^2(1.49\ Re_1)]} \quad .$$

This says that if you know how the values of the friction factor vary over a small range of Reynolds number you may be able to compute a lower limit for the Stanton number times 2/3 power of Prandtl number over friction factor.

Another little problem which has faced people for some time — and this begins to point up some of the difficulties of the fictive film approach — is where is the region of zero shear in a turbulent flow through an annulus. In laminar flow, it can be shown that the radius of zero shear stress, r_m, is given by:

$$r_m = \sqrt{\frac{r_o^2 - r_i^2}{\ln\left(\frac{r_o}{r_i}\right)}} \quad .$$

This is a simple calculation, and the experimental results of several investigators have indicated that this is probably also the case for turbulent flow. If one uses the fictive film approach and if we say that the friction factor is equal to a constant times Reynolds number to some power, then you can show that the hydraulic radius in both directions must be the same; assuming that the mean velocity in both halves of the flow is the same. It turns out, then, for this model that r_m must be equal to the

square root of the product of the interior radius times the exterior radius. This gives you a little smaller radius than does the first expression. However, we then get into a little bit of trouble, because it turns out in arriving at the expression for friction factor as Prandtl did that u_m^+ – which is the mean velocity divided by the friction velocity – is simply the integral of the velocity distribution for the turbulent core:

$$u_m^+ = \int_0^1 (5.5 + 2.5 \ln y^+) \, d\tilde{A} \quad .$$

Again in my terminology, this can be rewritten as the relative perimeter, relative distance from the wall; but the relative distance from the wall to the center is different for different geometries. It is unity, for example, for parallel plates; it is two for tubes. We will have to write $\tilde{y}_o$; in fact, for any circular geometry – an annulus for example – the $\tilde{y}$ going in from the inside to the center would be equal (as I recall) to:

$$\tilde{y}_o = 2 \, r_i/(r_m + r_i) \quad ,$$

and going in from the outside – I guess I will have to call this double zero – to the center:

$$\tilde{y}_{oo} = 2 \, r_o/(r_m + r_o) \quad .$$

These do not give you the same answer. For parallel plates or a very thin annulus, this reduces to unity; for a circle, where r_i becomes zero, $\tilde{y}_o$ approaches 2. What happens then, if you go through von Karman's calculations for the mean velocity for the value of the above integral, is that you get:

$$u_m^+ = 1.75 + 2.5 \ln \left(\frac{Re}{2} \times \sqrt{f/2} \right) \quad ,$$

for a circular tube. This is not the same answer as for parallel plates; for parallel plates you get:

$$y_m^+ = 1.27 + \ldots \quad .$$

This means that if we are going to approach the annulus problem, we must have a mean velocity in the inside part of the annulus different from that

in the outside part. This is a variation on the fictive film concept, but it is necessary if one is to approach the right answer to what is going on in an annulus.

One practical problem in which the fictive film concept can be used involves desalting, and I have done a little analysis on salt water flowing through a tube with porous walls. The walls are an osmotic membrane that will pass water but not salt. The stream has a mean velocity and is under high pressure compared to the exterior. Thus, we have water passing through the walls at so many gallons per square foot per day – or we can express it as a velocity, since it is volume per unit time per unit area – which we can write as $\dot{V}_{\ell y}$. Now, we know that, if the fluid is going out through the wall and we are blocking the salt, we will build up a concentration gradient at the wall; and, in effect, the solute is going to be moving back through the fluid at the same rate that the fluid is moving in the y-direction in order to have no net radial salt movement at steady state. If we think of the coefficient of diffusion (D) as being equal to minus this velocity times the log gradient of the concentration, the Schmidt number is η (kinematic viscosity) over D. We know that y_m will be:

$$y_m = \frac{8}{Re\ f}\, r_h\, Sc^{-1/3} \quad .$$

Since the first part represents the fictive film for fluid friction in dimensionless terms, multiplying by the hydraulic radius gives it to us in dimensional terms and dividing by the Schmidt number to the 1/3 power converts it to the fictive film for mass transport. We then find that:

$$\ell n \frac{c_w}{c_m} = 2\ Sc^{2/3}\ \frac{\dot{V}_{\ell y}}{f\ u_m} \quad .$$

We can substitute some actual numbers into this and see what obtains. For example, let's suppose we have two parallel membranes 1/8-in. apart or a tube of 1/4-in. diameter – it doesn't make any difference in these calculations – and a Reynolds number of 10^4 (u_m = 4 ft/sec). We can use the approximate expression given previously (Fig. 5):

$$\sqrt{2/f} = 5.1 \ln \mathrm{Re} - 4.2 \quad ,$$

and find that the concentration ratio, c_w/c_m, is 1.064, which is not very great. This is for a mean velocity through the membrane of 10^{-5} ft/sec – I think they have been doing a little better than this lately. If we went up by a factor of 2 in the throughput velocity, then the concentration would begin to become important.

Interestingly enough, another thing comes into play; namely, we have been incorrect in using the fictive film concept in this way because the equation that gave us $Sc^{1/3}$ assumed the mass flow rate through the film was uniform. Here, we are talking about the linear velocity being uniform. Since we have a steady state next to the wall with a varying concentration, in effect the movement of the solid with respect to the liquid, for the case examined here, was in terms of linear velocity. In contrast, the equation by which we calculated the fictive film actually assumed that the mass velocity – so many pounds per square foot – was uniform. These cannot both be the same; they are related by the concentration. Where the concentration difference is small, this doesn't make a serious difference. Where the concentration begins to get large – and we really want some accurate results – then we have to consider the comparison between what one normally thinks of as a mass transfer coefficient and the kind of backward diffusion one gets through a region of varying concentration.

This discussion illustrates several things. One is that the fictive film concept can be used, and is very useful, for systems where one wants only order of magnitude results. This is the situation that happens regularly with technical administrators. Someone describes experimental or analytical results and the administrator needs to have his own method of checking the results. The approach is also useful in estimating whether a proposed process has a chance of being practical. In general, you cannot use the fictive film concept, or the equivalent diameter concept, if you really want to nail down the answer. There you are probably better off just going ahead and getting experimental results and relying on those where at all possible. However, you can get good order of magnitude engineering results in many cases with the fictive film concept; and

although it has been around a long time, if we understand its limitations and its strengths, it will be around for a lot longer.

DISCUSSION

Chairman (J. J. Keyes): We thank Dr. Lyon very much for sharing his "doodling" with us and giving us some insight into a concept we have all used but probably not thought about in such detail. We have about 15 minutes for questions.

R. N. Lyon: No questions; it all must have been very clear.

Chairman: We have time; so if you wish to take a few moments to formulate your questions, go right ahead. I think this expanded concept of the fictive film is indeed very educational, and something that can be brought into the undergraduate curriculum in time for the student to appreciate that it is not that simple – that universal – as I was led more or less to believe in undergraduate engineering. The student should really benefit from this sort of analysis given by Dr. Lyon.

D. G. Thomas (ORNL): Have you had any thoughts on extending what you have done on the roughness in trying to calculate the effect of roughness on Nusselt number? As the roughness height extends further and further into the turbulent core, it appears that the friction factor keeps on increasing; but it looks like the heat-transfer coefficient no longer continues to increase. Presumably, the fictive film concept would break down under such circumstances. Is this right?

R. N. Lyon: Perhaps I should have made it clearer. In a sense, the fictive film doesn't break down; because it never was there in the first place. The question is whether, I think really, it loses its universality and, therefore, its applicability to other systems. No matter what system you have, you can always dream up a fictive film which would give you the overall results. As I indicated, for example, in the liquid metal case, it doesn't have much application; because for each specific case, you have to calculate a fictive film. It doesn't do you any good to find a fictive film if you first have to answer your original question and if you can't extend it to other things. In answer to your question, I think that what is happening with roughness consisting of long spikes sticking up or the type of thing you are working on where you have wires above a surface – you get separation which increases the friction factor without increasing

the heat transfer to the same degree. Whereas, if we have even a wavey surface, on which we are building up in a sense a new boundary layer on the surface each time, it appears from the data I have seen that the fictive film concept can be applied to such a system provided you don't have separation behind the roughness elements. I think where you don't get separation, I would expect the heat transfer and friction factor results to work out pretty parallel and, therefore, the fictive film idea to work out. Where you have this kind of separation, it gets pretty hard. There is another problem, of course, and that is what happens with respect to the conductivity of the roughness element. If you have a poor conductor as the roughness, as you would if you had an oxide scale or a rust scale where that is doing a great deal of the intrusion into the liquid, then you will not get as good heat transfer up in this region as you would if you had a metal and a poor conducting liquid. So, the relationship between the conductivity of the wall and the conductivity of the liquid are, I think, going to have an important bearing again on this ratio, if this roughness becomes large enough so that you are relying on a large amount of conduction closer to the turbulent region. Does that answer your question?

D. G. Thomas: Yes, I guess what you are saying is that if you can evaluate the form drag of your element and just use the skin friction part – the total drag minus the form drag – you might come closer then to an engineering estimate of the effectiveness of the element.

R. N. Lyon: Yes, as you may remember, the equation that I put up here before said that the Stanton number times the 2/3 power of the Prandtl number over the friction factor is equal to the difference between the friction factors for two Reynolds numbers divided by 104 times the difference of the log squared of a constant times the two Reynolds numbers. What I was doing there was simply saying that in our friction factor plot, if we plot friction factor versus Reynolds number, there is the smooth tube line which comes down and actually curves a little bit – this is a log-log plot – but it will break off and become constant for various values of roughness. The parameter here would be ϵ/r or some other dimensionless term. If one says that in this region part of the friction factor is due

to skin friction and part of it is due to loss of momentum without corresponding wall shear, and if you can take out the skin friction part and use that to estimate the heat transfer, you are going to be conservative, because you don't lose all of the heat transfer when you are bringing the liquid back in recirculation; you just cut it down. Thus, some expression like this (and you remember I put it forth tentatively) would be a way perhaps of trying to estimate how much is actually due to skin friction and, therefore, will be most capable of transfering heat.

W. M. Rohsenow (MIT): You obviously find this fictive film concept useful in your own operations in life. I have been having a little difficulty clearing up in my own mind as to exactly how you use this. Let me ask you a series of perhaps disconnected questions. Would you summarize, for example, how you use this; and in particualr, is this a scheme whereby you get heat-transfer results from friction results for unusual geometries? Certainly, there is no need for it with circular tube stuff; we have most of that information anyway. Therefore, does this come down to a way of estimating heat transfer in unusual situations, if you know the pressure drop or friction effects, or just what is the major value of it? Maybe you can summarize how you use it.

R. N. Lyon: I think there are perhaps two or three places one could use it. First of all, in my own mind I combine the fictive film and the hydraulic diameter concepts; and, as you know, the hydraulic diameter concept is quite useful in looking at both friction and heat transfer in odd geometries. In doing that I have found some discrepancies. For example, the problem of the annulus in which, as we know, the shear stress is increasing on the inner surface as the ratio of inner to outer diameter becomes smaller and smaller and winds up very large for a wire in an annulus. So, I have been looking at the fictive film idea to see whether there is some way of correcting that. In heat transfer, yes I have found it useful for other types of geometries; and particularly, as I said most recently, this example of the tube with the reverse osmosis where it seemed to me it was quite useful. Another little question that I looked at was a crude estimate of the concentration on the surface of a bubble that is growing by evaporization from a solution. Again, you can make an approximation

of what the concentration variation was from the surface of the bubble to the mass of the bubble, if the bubble was growing at a certain rate. One thing that I used there was an approximation that Jakob put forth, that the bulk temperature – where you have a lot of bubbles around – is about 1 deg above the equilibrium temperature. Well armed with that you can go back and calculate whether, in a given salt solution, the concentration of the surface is really high enough so that you are far from equilibrium during growth of the bubble. In the case that I was looking at, it turned out not to be very great, which is encouraging. I use this technique to check things where it is not necessarily worth going through a formal calculation, but where I want to know the order of magnitude and what is going on.

Chairman: If there are no other questions, I would like to thank Dr. Lyon again for being with us today.

SPECIAL LECTURE III

SOME RECENT RESULTS IN BOILING AND CONDENSING RESEARCH

Professor W. M. Rohsenow
Department of Mechanical Engineering
Massachusetts Institute of Technology
Cambridge, Massachusetts

INTRODUCTION OF SPEAKER

H. W. Hoffman (ORNL), Chairman: It is my pleasure in beginning this second day of the Second Southeastern Seminar on Thermal Sciences to introduce to you Warren M. Rohsenow, Professor of Mechanical Engineering at the Massachusetts Institute of Technology. For most of you, Warren needs no introduction; his well-deserved reputation has proceeded him. He has worked for many years in the field of boiling heat transfer; and he and his students have made many significant contributions to our understanding of boiling. He is the author of numerous articles and of several books, among these being Heat, Mass, and Momentum Transfer (with Dr. Choi) and Developments in Heat Transfer (editor).

I have known Professor Rohsenow as a friend for a number of years, and I commend him to your attention this morning. He will discuss some of his recent studies in boiling and condensing heat transfer. His presentation will begin – I believe – with a very clever slide demonstrating boiling of water from a heated wire. Professor Rohsenow.

SOME RECENT RESULTS IN BOILING AND CONDENSING RESEARCH

W. M. Rohsenow

I will begin this discussion with a projected demonstration of boiling on an electrically heated wire immersed in a water pool. The apparatus is constructed so that it fits into the slot of a large lantern slide projector. A variac controls power to the wire; by varying the power, the boiling modes can be observed. Further, by turning the power on and off during boiling, the reprducibility of bubble formation at a single spot on the surface can be demonstrated; this supports the postulate that boiling takes place at cavities on the surface.

The heat flux (electric current, I^2R) is increased (B toward C of Fig. 1) to show clearly that the density of active nucleating sites (number per square foot) increases as the heat flux increases. At a high value of heat flux (Point C, Fig. 1), the boiling changes to film boiling. The vapor film around the wire can be seen to form one region and spread across the wire. If we then operate at Point E′, we observe a glowing orange color. In this region, rather regularly spaced columns of vapor come off the vapor region surrounding the wire; and the liquid does not touch the wire.

We also see that as we reduce the heat flux from E′ to D, the film collapses, and nucleate boiling is reestablished on the curve between B and C at the heat flux equal to D.

I am going to discuss today a number of perhaps disconnected subjects in boiling, but I have a few messages that I would like to leave with you. We are going to talk about nucleation first and attempt to convince you of a few postulates regarding the nucleation phenomena. Then, I would like to review some of the old boiling correlations. We went back and revisited them, and I want to try to straighten out some of the myths in the literature on that subject. I also want to tell you a little about some of our recent research on boiling of liquid metals where we have gotten into some unusual instabilities which check out the same kind of instabilities observed here at ORNL and a few other places. Finally, I

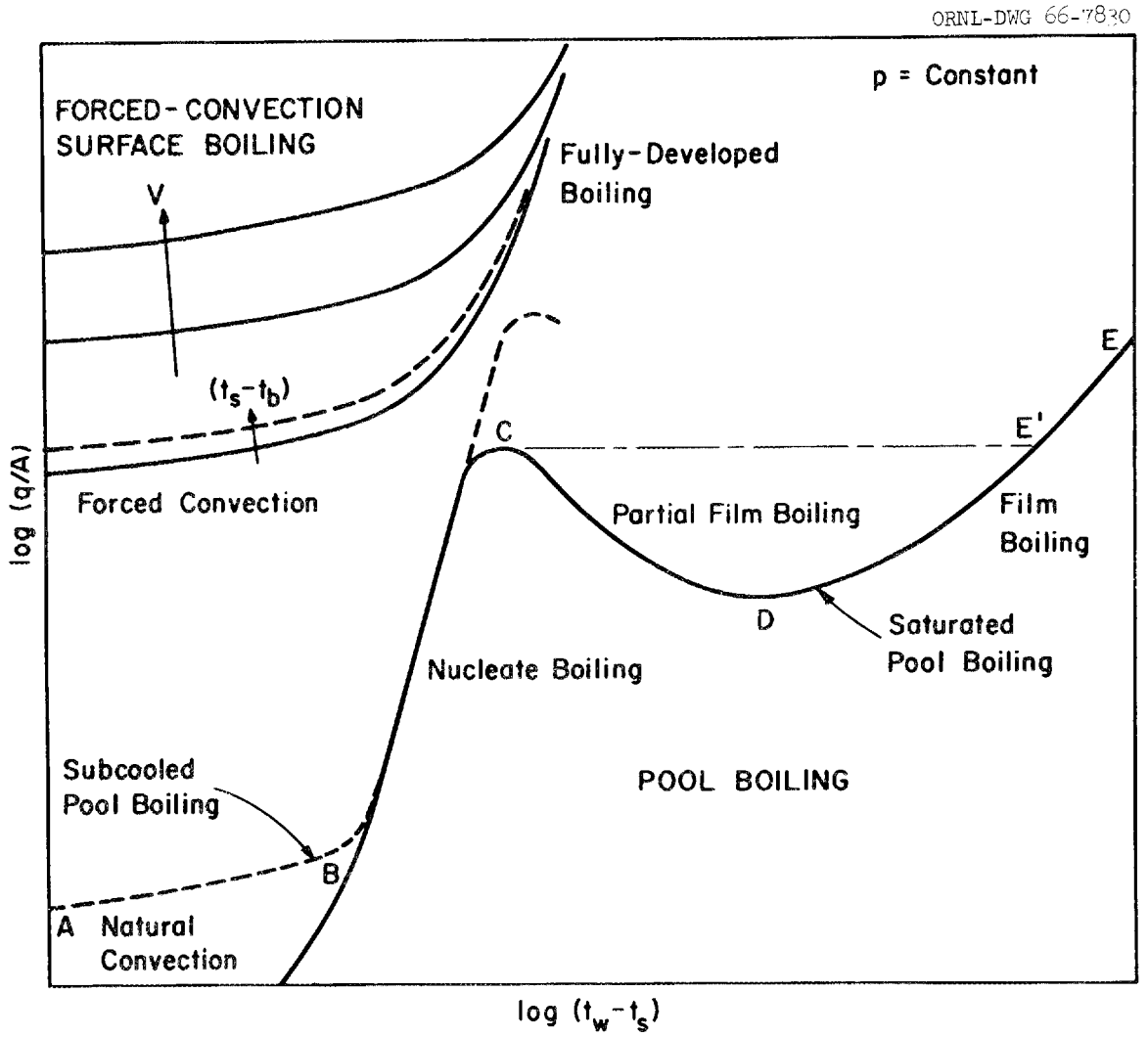


Fig. 1. Regimes in Boiling Heat Transfer.

would like to look at the effect of nonuniform heat flux on burnout which is a rather significant thing in nuclear reactors. Thus, we will go back and revisit some old stuff, and then add a little new stuff. There will be a few disjounted pieces.

As you know, in most of the work we do in heat transfer we like to write down the differential equations and boundary conditions and then solve them to determine the temperature and velocity fields. Then, if the experiment does not agree with what you have calculated, you go back and wonder what you did wrong in the experiment. You did not satisfy the boundary conditions with the mathematics. From what you saw a moment ago of the boiling process, can anyone define the fluid motion and write down the boundary conditions for the Navier-Stokes equations? We have had a little difficulty trying to do that. This means that we have to use a certain amount of ingenuity and bypass that step. This is what makes boiling facinating, and why we are still in business after all these years.

Figure 1 is the panorama of really all of pool boiling and, of course, convection boiling at very low qualities. The solid curve BCDE′E represents the characteristic curve for pool boiling of saturated liquid — nucleate boiling occurs in the region BC and film boiling in the region DE. The peak heat flux occurs at C, and the portion of the Curve CD is called transition boiling. This part of the curve cannot be reached with electric heating; wall temperature here must be the independent variable — by using a condensing liquid, for example.

If the liquid is subcooled, sufficient heat flux may be added to produce boiling. This is represented by the lower dashed curve of Fig. 1. The asymptote to the left, when $T_w = T_{sat}$, is at the heat flux for natural convection when the $\Delta T = T_{sat} - T_{liq}$.

For the BC portion of the curve, there are two sets of conflicting data. Bergles in our MIT laboratory found that the curve moves slightly to the right (higher ΔT) as the subcooling is increased for boiling on a horizontal tube. Shrock at the University of California found this portion of the curve to move to lower ΔT as subcooling is increased for boiling on vertical flat plates. Presumably, the difference is due to the geometries which produce differing connection effects.

The forced-convection effects at low quality or subcooled boiling are shown in the upper left of Fig. 1. Here again the left-hand asymptote represents the heat flux corresponding to the forced-convection velocity and the subcooling. This asymptotic q/A increases with both subcooling and velocity; hence, the curves arrange themselves in that order also, since at the right-hand end they all appear to converge into a single curved called the fully developed boiling region.

We will look at (1) conditions under which boiling begins, (2) the nucleation point under these various conditions, (3) some boiling correlations with the idea of correcting one or two of them, (4) the burnout heat fluxes for nonuniform heating, and (5) the conditions for Curve BC for liquid metals.

NUCLEATION

To gain some insight regarding the conditions for nucleation, consider the equilibrium condition in Fig. 2 of a free surface of vapor and liquid within a capillary tube providing a curved surface. The liquid will rise a certain distance. The pressure in the liquid, $P_{\ell r}$, is less than the pressure of the vapor, P_{vr}. Of course, the pressure in the vapor decreases with height, but the decrease is small for the case shown in Fig. 2. This is the first approximation. Further, the conditions of equilibrium for this interface are the same as conditions of equilibrium for a spherical vapor bubble in a liquid. Figure 3 shows these conditions. The conditions at equilibrium would be that the temperatures of the liquid and vapor phases would have to be the same, the chemical potentials, $\zeta = h - TS$, would have to be the same, and the pressures are related by $p_{vr} - p_{\ell r} = 2\sigma/r$. For these three conditions, there is one pair of liquid and vapor states ($a'a''$, $b'b''$, etc.) for each radius of curvature (r_a, r_v, etc.), where r_a represents an infinite radius or a flat surface as shown in Fig. 3. The vapor in the bubble would be at a higher pressure than in the liquid, of course, because of the surface tension. Now, Fig. 3 is a rather complex picture; and it does not lend itself very nicely to writing simple explicit equations. Observe in Fig. 3 that the vapor states are very nearly the same as the saturation states for a flat interface. With

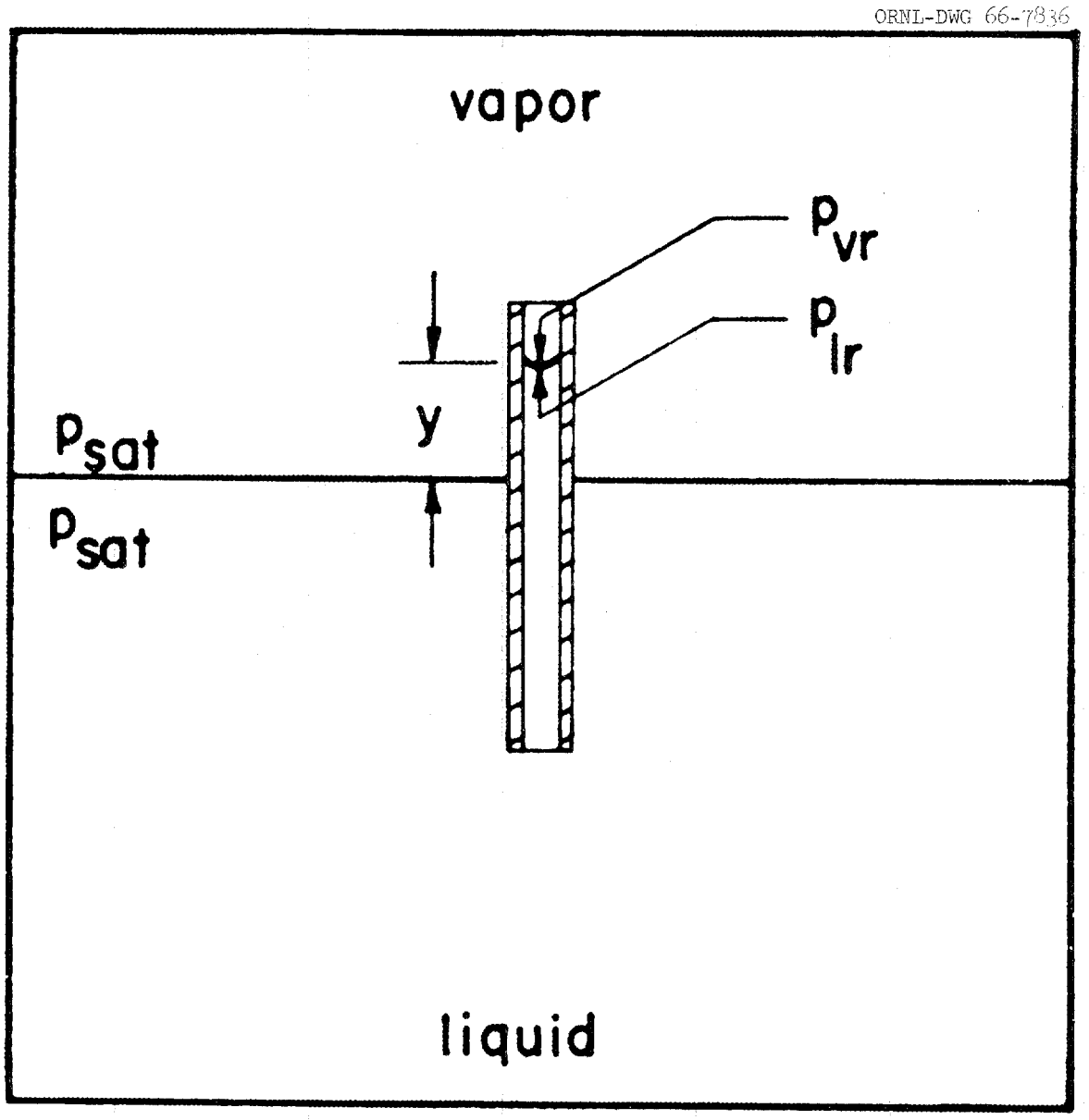


Fig. 2. Equilibrium Model for Liquid Rise in a Capillary.

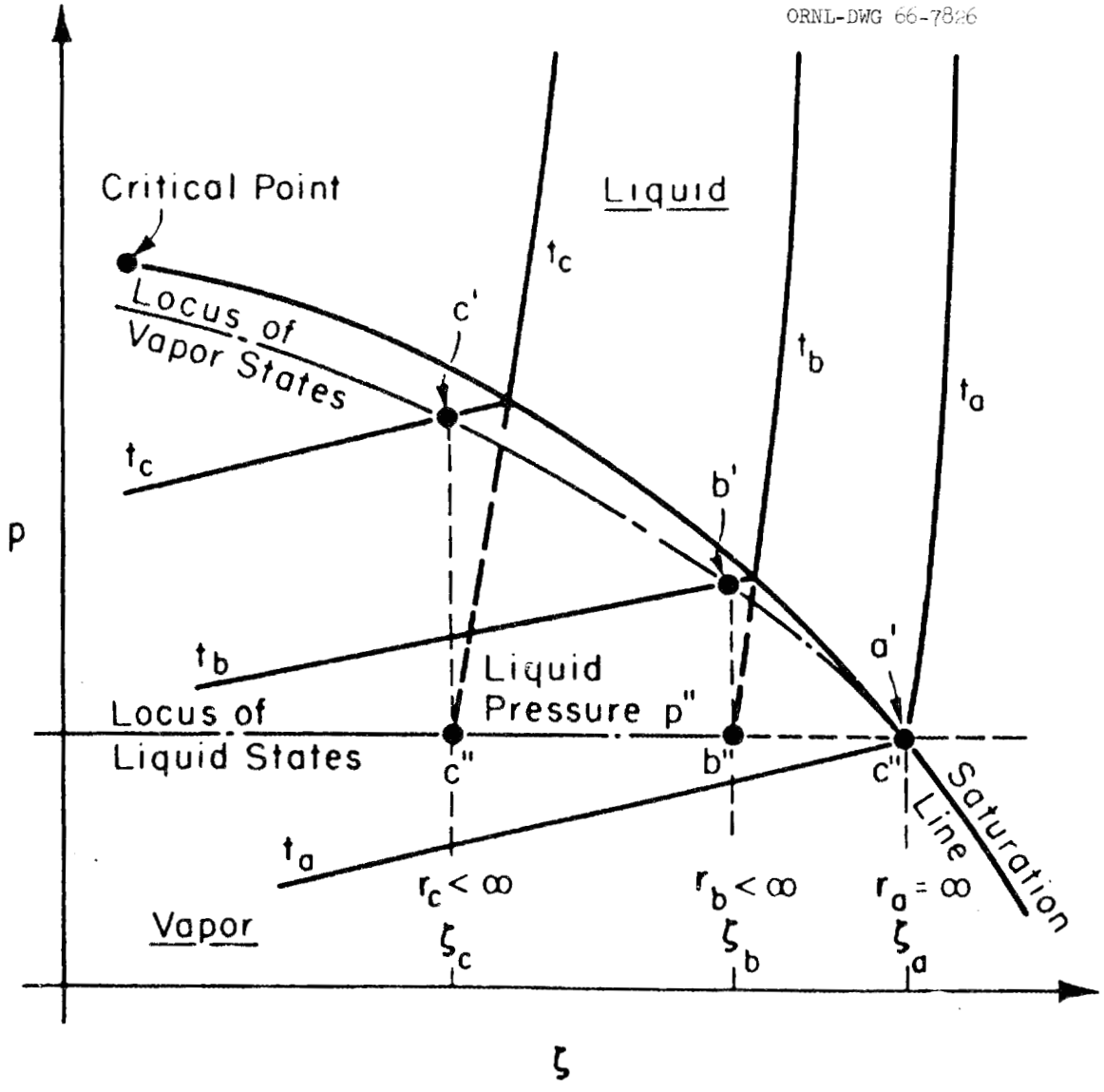


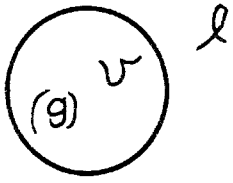
Fig. 3. Description of Equilibrium States for Liquid-Vapor Interface in a Capillary or for a Spherical Vapor Bubble in a Liquid.

this assumption, the force balance equations in Fig. 4 are readily written. Here, the bubble contains vapor at its own saturation condition. The equations include allowance for possible presence of a noncondensable gas (g). At equilibrium both liquid and vapor are at the same temperature. If we now assume that the vapor inside the bubble is at saturation (this is an approximation but a reasonably good one), then the condition of the vapor inside the bubble is shown by the dotted line denoted by v, where the curve is the saturation curve for flat interfaces. This gives the liquid pressure. The pressure of the liquid is less than the pressure of the vapor inside the bubble by $2\sigma/r$; and consequently, the saturation temperature of the liquid would be less. The temperature of the liquid cannot be less than the vapor temperature. Thus, the liquid temperature must be at least at equal to the temperature of the vapor in order for this bubble to grow. The Clausius-Clapeyron equation with the perfect gas approximation (third equation in Fig. 4) relates T and P along the saturation line. The ΔT in this equation is the minimum superheat required for equilibrium or, as a first approximation, the minimum superheat required for a bubble to grow. Combining the second and third equations of Fig. 4 results in the fourth equation giving the superheat required for a bubble of radius, r, to grow.

For nucleate boiling, we postulate (a) that for a surface there are a number of cavities of different radii, (b) that there is some radius of the bubble initiated in the cavity related to the size of the cavity, and (c) that knowing the cavity radius gives an indication of the superheat required to get a particular cavity to give off a bubble.

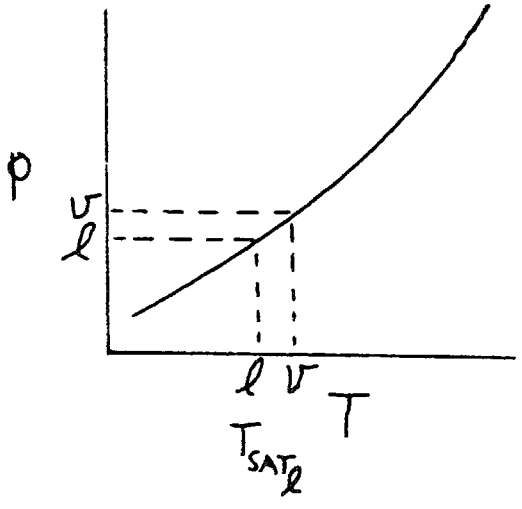
Let us now take a look at a number of experiments that tend to lend support to this kind of thesis. Figure 5 shows a conical-shaped cavity with vapor at the bottom. As heat is added, evaporation takes place at the interface which then grows. At a temperature corresponding to that given by the last equation of Fig. 4, a bubble grows forth from the cavity. When this bubble departs, liquid flows in behind it trapping vapor in the cavity for the next bubble. If we have radii of curvature as shown in Fig. 5, the pressure of vapor in b is less than the pressure

NUCLEATION



$$\pi r^2 (p_v + (p_g) - p_\ell) = 2\pi r \sigma$$

$$\Delta p = p_v - p_\ell = \frac{2\sigma}{r} - (p_g)$$



$$\frac{\Delta p}{\Delta T} \approx \frac{h_{fg}\, p}{R_v T^2}$$

$$T_v - T_{SAT} = \frac{R_v T^2}{h_{fg}\, p}\left(\frac{2\sigma}{r} - (p_g)\right)$$

Fig. 4. Derivation of Equation Describing Superheat for an Equilibrium Spherical Bubble in a Liquid Pool.

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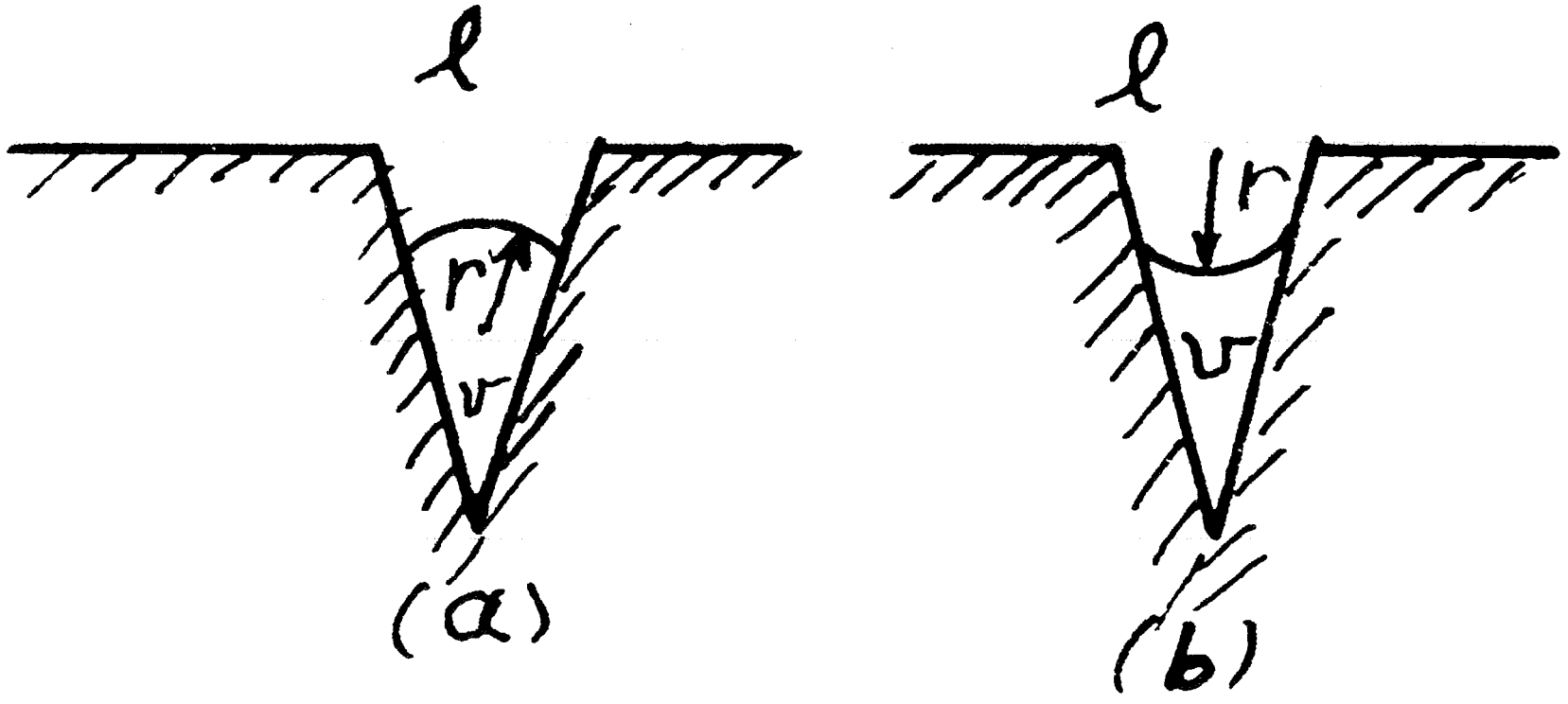

Fig. 5. Conical Cavities Depicting Effect of Bubble Radius of Curvature. In (a) the vapor bubble pressure is always greater than the liquid pressure; in (b) the vapor bubble pressure is less than the liquid pressure.

of the liquid and the pressure of the vapor in a is always greater than the pressure of the liquid because of the curvature. If this is a very sharp cavity, the radius of curvature in b gets smaller and smaller and the pressure difference gets greater and greater as the liquid moved down into the cavity. If we go to a small enough radius of curvature, even with a 1000 lb/in.^{2} pressure on the liquid, it is possible to get a small enough radius of curvature for the pressure under that radius of curvature to be fractions of a psi. This means that one may cool the system down to as low as even 32°F and still have vapor present, provided the point of the cavity were sharp enough. The next time the cavity is heated, this vapor grows; and a bubble is ready to depart as before. For a, cooling down even to 213°F at atmospheric pressure would snuff out this cavity by filling it with liquid. Then, to form vapor the liquid must reach fracture pressures which would require a 100 or more degrees of superheat to break the liquid. If enough cavities of this kind get snuffed out, we get what we have labeled the hysteresis effect and must overshoot to get to some high superheats before vapor forms. Then, vapor forms rapidly renucleating the cavities; and the process returns to the stable operating curve again. This is one explanation for hysteresis in boiling.

Figure 6 is one frame of a high-speed motion picture, taken in the laboratory of Professor J. W. Westwater, of a surface on which boiling takes place. We see black spots in this photomicrograph. It would not be inconsistent to call these black spots cavities. Figure 7 is later in the sequence of pictures; under this bubble is one of those black spots. One conclusion that you may draw from this observation is that this bubble came from that cavity.

Figure 8 is a series of tests that Peter Griffith and John Wallis made in our laboratory some years ago. They used a sharpened gramaphone needle to make a conical cavity by pushing it into the copper, then smoothing up the surface. The surface is all at a relatively uniform temperature. They observed the temperature at which bubbles started to come off from that cavity and compared that temperature with the superheat calculated from the simplified $2\sigma/r$ model of Fig. 4.

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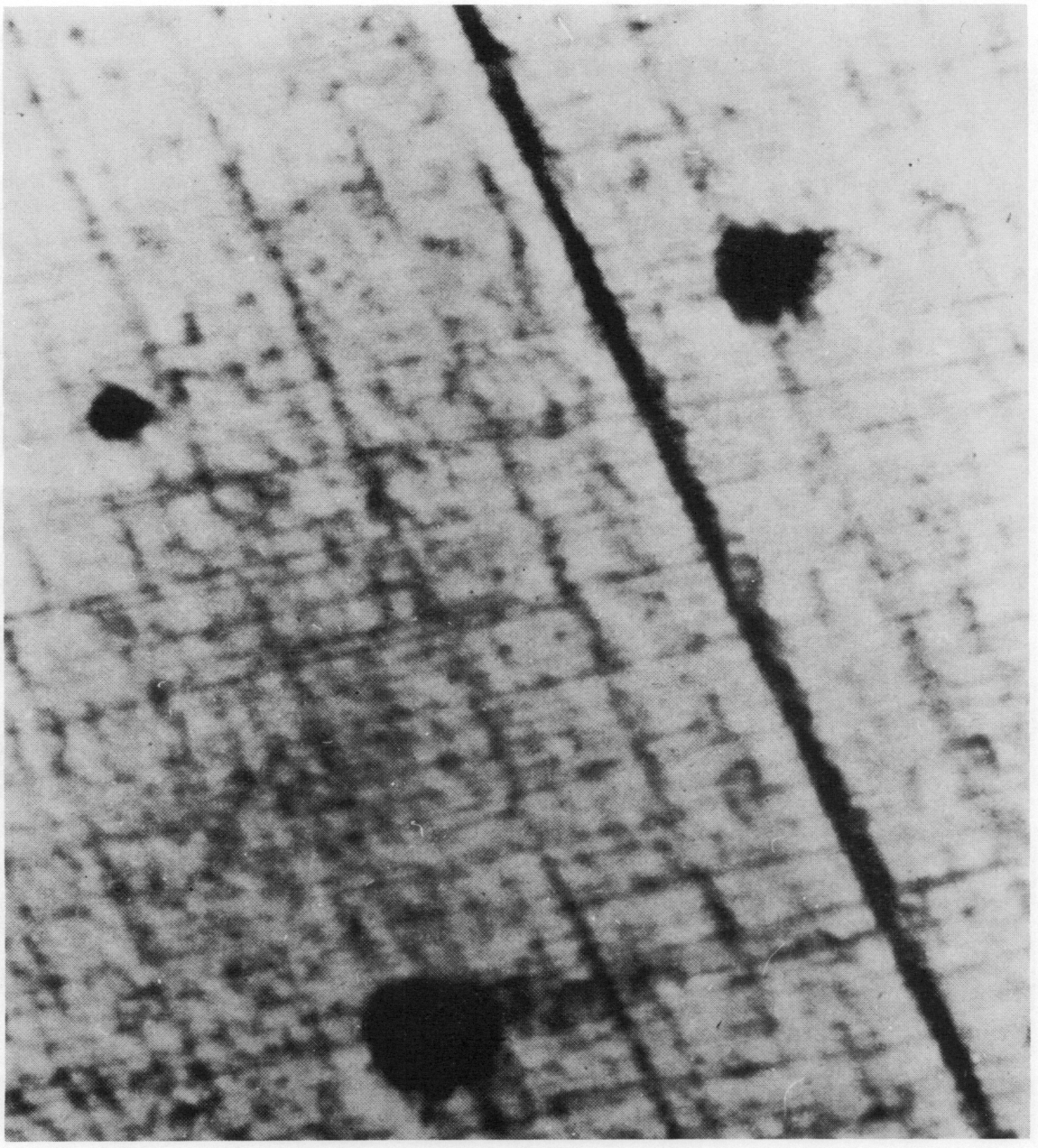

Fig. 6. Frame from High-Speed Motion Picture by Westwater of a Surface on which Boiling is Occurring. Black spots are probably surface cavities.

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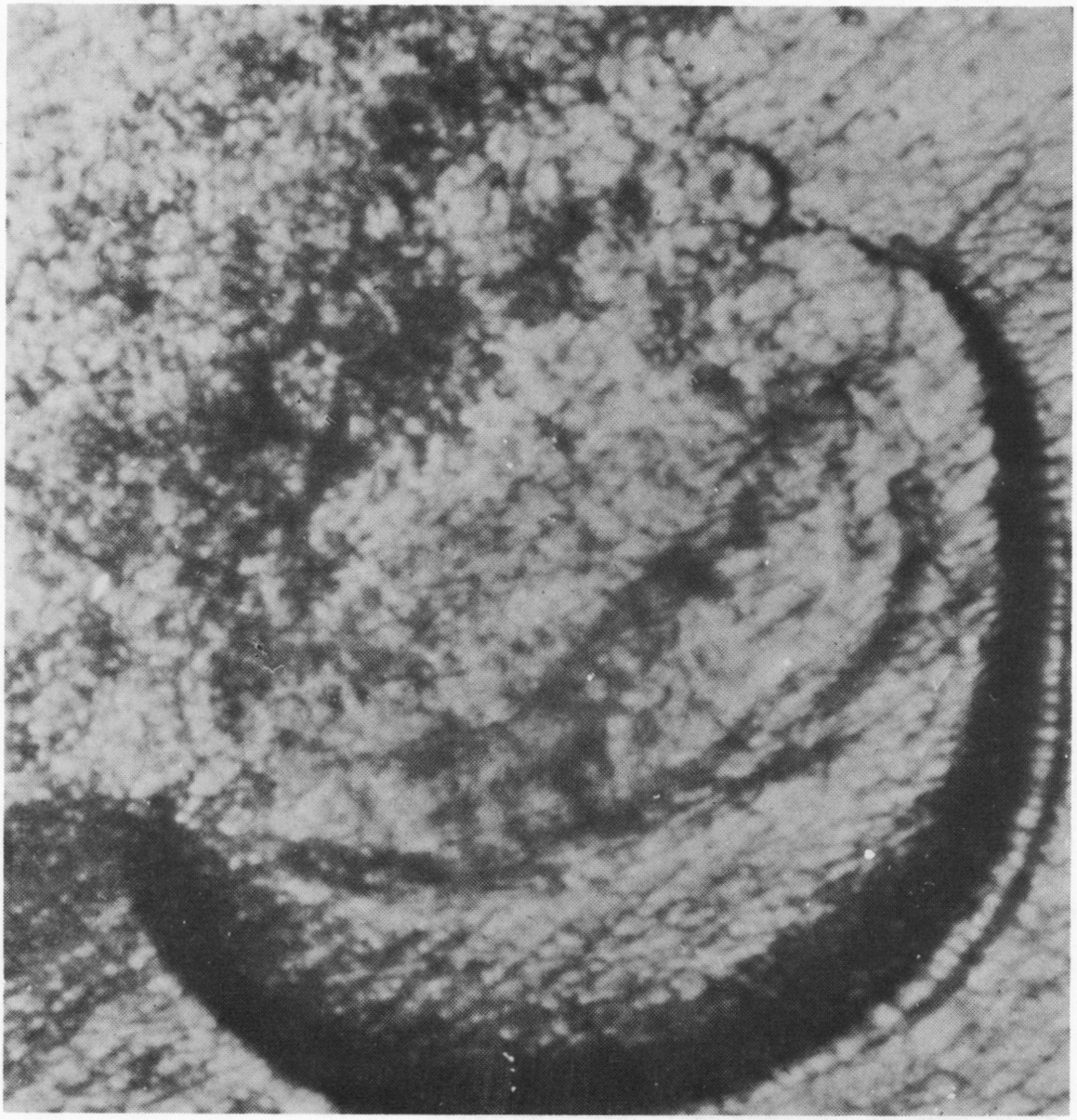

Fig. 7. A Later Photograph in Motion Picture Sequence by Westwater Showing Bubble Forming at Black Spot on Surface.

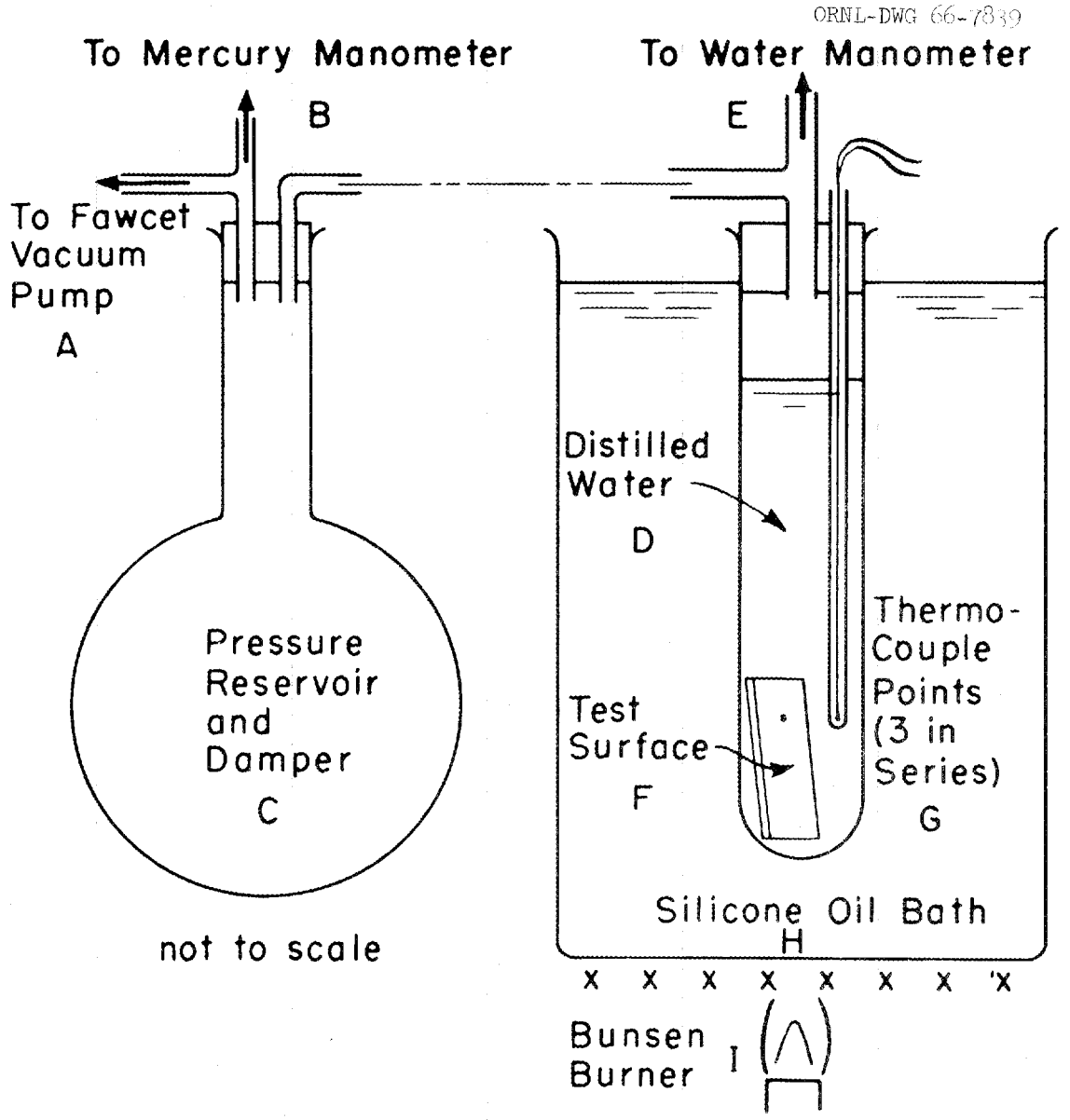

FIG. 9 – TEST APPARATUS

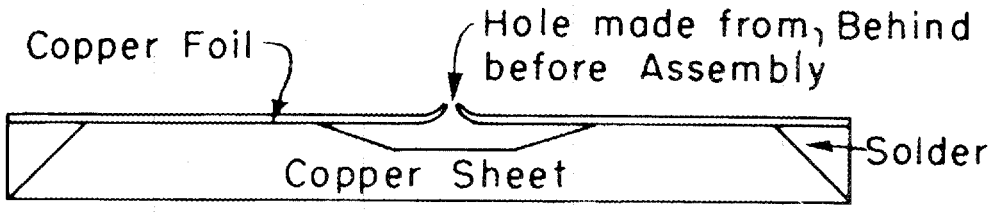

Fig. 8. Apparatus Used by Griffith and Wallis to Study Nucleation from a Conical Cavity in a Copper Surface.

Figure 9 shows the results of the experiments outlined in Fig. 8. They measured the diameter of the hole or cavity with a microscope micrometer to be 0.0018 in. The middle curve is the calculated result for D = 0.0018 in.; the other two lines are for 0.0017 and 0.0019 in. The data suggest that in a uniform temperature bath this $2\sigma/r$ idea does indeed agree with bubble formation at a cavity of this size.

Figure 10 shows another test that Griffith and Han performed in our laboratory. The same surface, scrupulously cleaned, was run with methanol, ethanol, and water; and the density of active cavity population counted and plotted against ΔT. For this ΔT, the radius is calculated from the equation at the bottom of Fig. 4; and thus for each of these data points, you can calculate the radius, r^*, of the last cavity which was nucleated. All of these points fall on a single curve suggesting that there is a cavity size distribution (inset on right-hand graph of Fig. 10) on this surface that is constant and independent of whether you are boiling water, ethanol, or methanol. This is a kind of an integrated curve representing the total number of cavities greater than a particular radius, r^*. The derivative of this curve would be the distribution function or the number of cavities lying between D and D + ΔD (inlet). We were not quite bold enough to take the derivative of this curve and draw the distribution function; but if we were, wouldn't you expect that the distribution function would look something like the insert of Fig. 10 showing distribution in number against diameter or radius. There is some shape of this curve that is representative of the surface. Whatever that shape is, it determines the position of the q/A versus ΔT plot.

This suggested, then, that if we were to construct a surface with cavities of the same size, the temperature required to nucleate all of those cavities would be the same. If q/A increases with the number of activated cavities and if all cavities require the same ΔT to be nucleated, then the q/A versus ΔT curve ought to be vertical. When Dr. Han tried this experiment, he found nucleation took place between the cavities on the parent surface. Figure 11 shows us perhaps why this happened. This is particularly applicable to a forced-convection system; but it is also applicable to pool boiling.

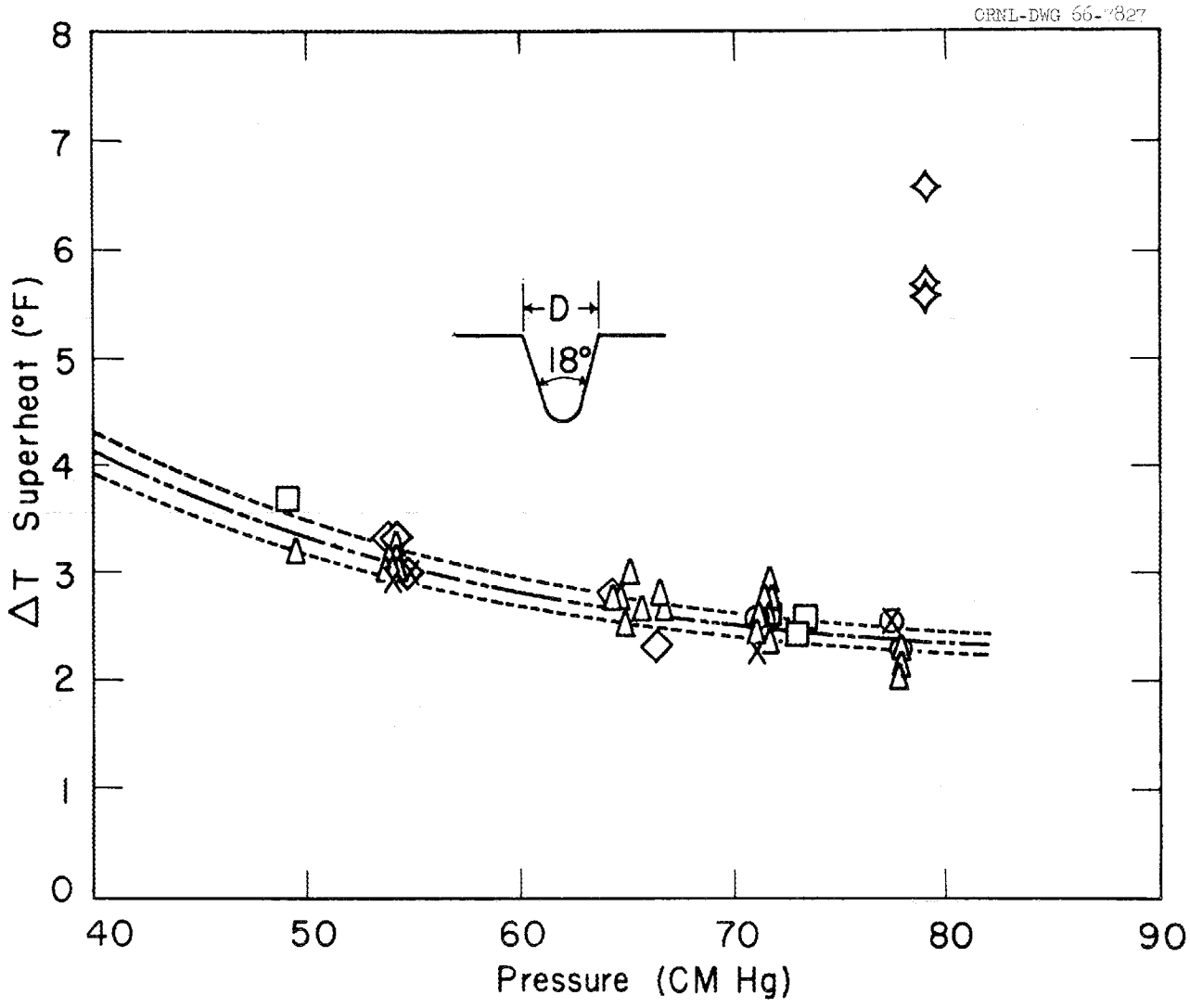

Fig. 9. Results of Griffith and Wallis for Boiling from a 0.008-in.-diam Conical Cavity on a Copper Surface. Calculated curves are for D = 0.0017 in. (upper), 0.0018 in. (center), and 0.0019 in. (lower).

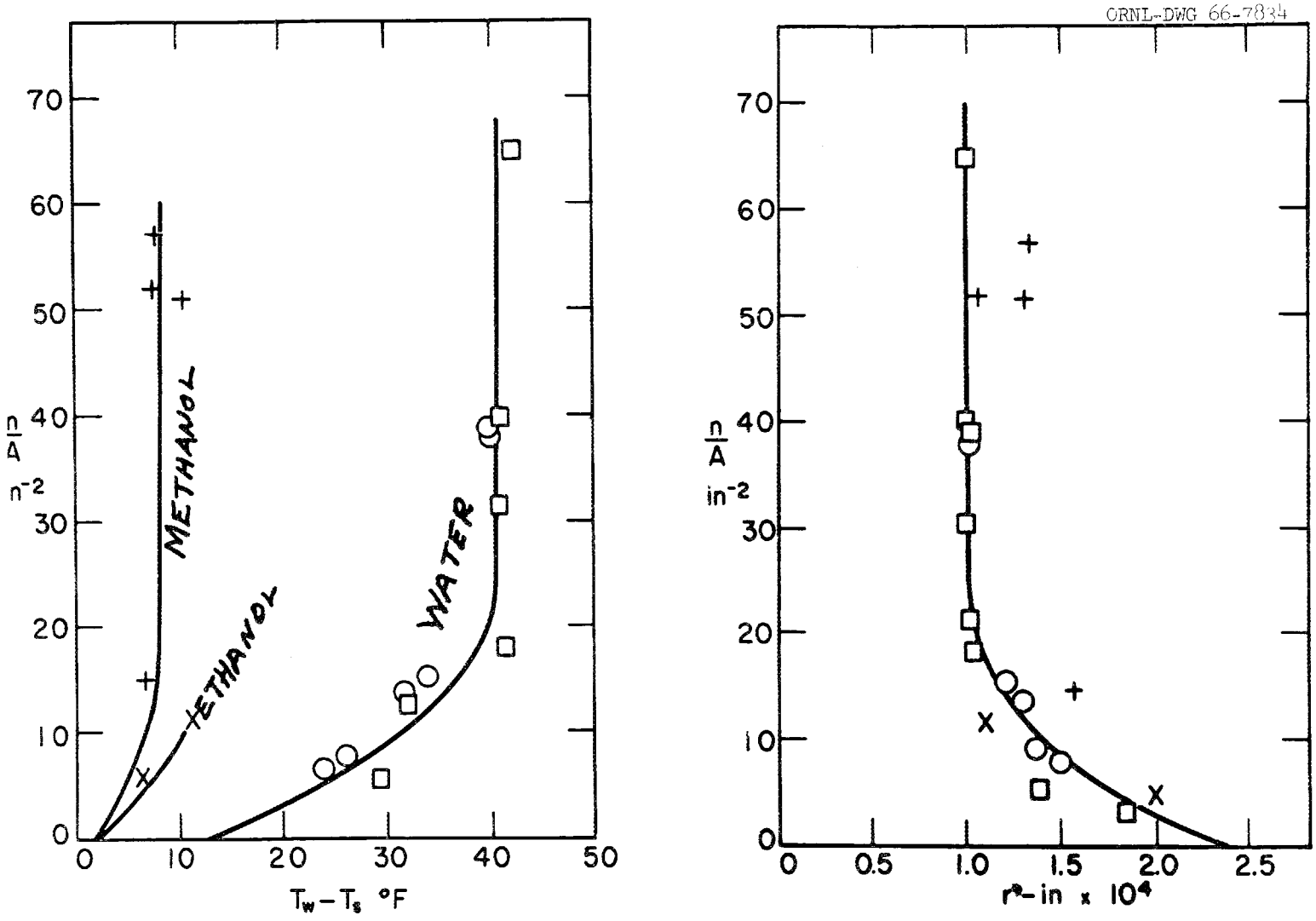

Fig. 10. Active Cavity Population as a Function of Superheat and Cavity Radius as Obtained by Griffith and Han from Studies with Methanol, Ethanol, and Water.

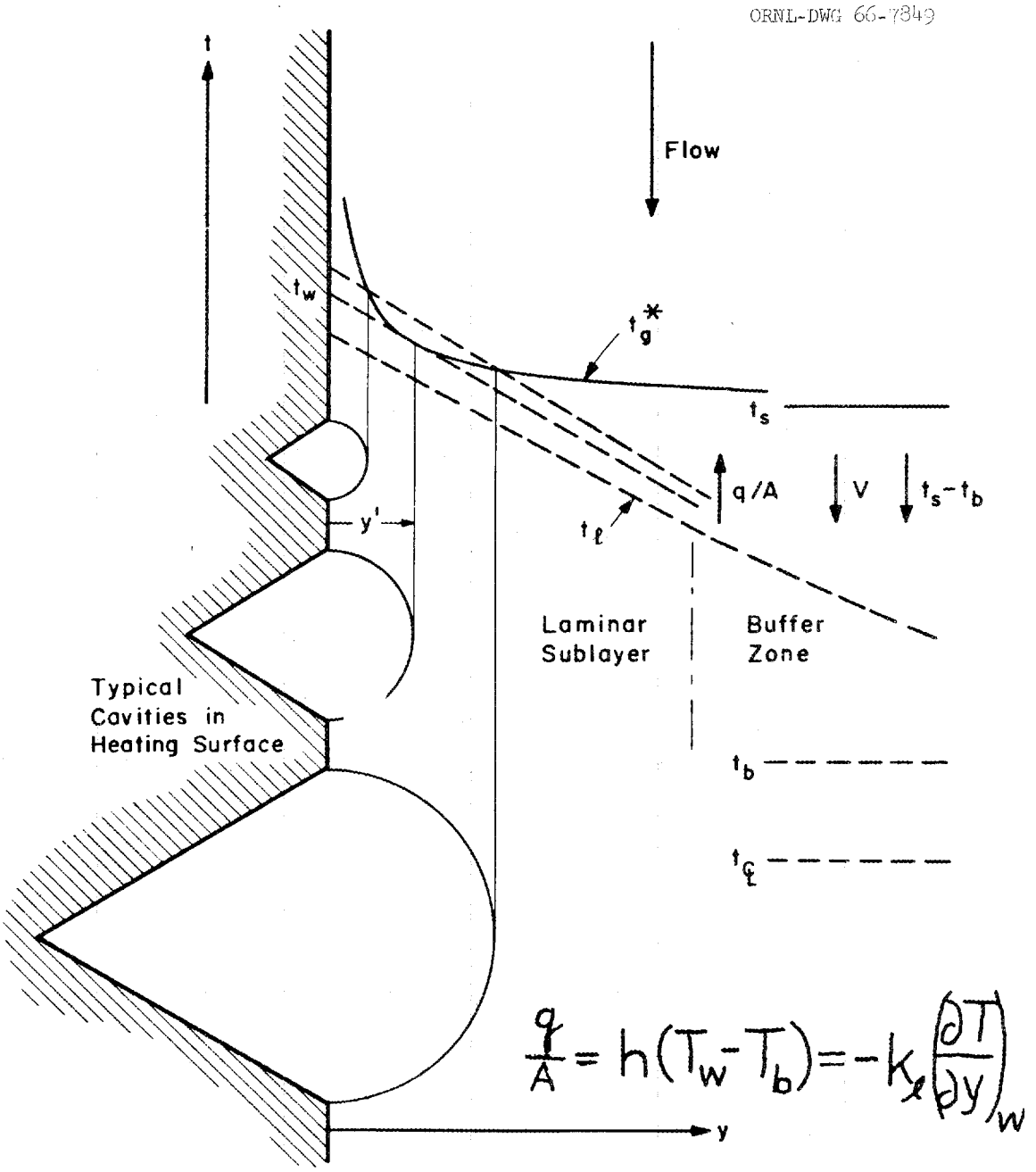

Fig. 11. Initiation of Bubble Growth in Forced-Convection Surface Boiling.

In Fig. 11, we assume the surface has cavities with some size distribution like Fig. 10. The curve labeled t_g^* is calculated from the equation at the bottom of Fig. 4. The equation at the bottom of Fig. 11 represents the condition at the solid surface up to the point where nucleation begins. The dashed lines represent the temperature distribution in the liquid at various heat flux values. Based on work by Hsu and Graham, Han and Bergles proposed a simple operational scheme for predicting the wall superheat required for boiling to begin. They suggested boiling would begin when the dashed curve became tangent to the t_g^* (or $2\sigma/r$) curve. This suggests that the size of cavity that first nucleates is the size y' corresponding to the point of tangency. Raising the heat flux slightly, the dashed curve cuts across the t_g^* curve suggesting the size range of cavities nucleated at this higher heat flux. If the man-made cavities were to lie outside this range, then nucleation would begin on the parent surface between the man-made cavities. This is what Dr. Han observed.

The procedure outlined in Fig. 11 for predicting incipient boiling agrees reasonably well with much test data. In some cases, particularly for subcooled boiling, higher superheat than those predicted in Fig. 11 are required. Also, this procedure does not predict incipient boiling in natural-convection situations where the slope of the dashed curves are so low that this point of tangency prediction requires larger cavities than those which are actually present.

Figure 12 shows some results for three tests that Bergles ran on certain test sections. These predictions agreed with the observation. Figure 13 shows how this looks on a q/A versus ΔT plot. The arrows are the predicted points for boiling initiation.

Figure 14 shows the results of this prediction scheme for water at various pressures. Each line presents the locus of points where boiling should begin at a particular pressure. The curves are independent of velocity and subcooling. Any combination of velocity and subcooling that yields a q/A and a ΔT on the line (or to the right of it) should produce nucleate boiling. Note also that this point-of-tangency procedure suggests that the incipient boiling condition is independent of the cavity size distribution as long as there exist cavities at least as large as those predicted.

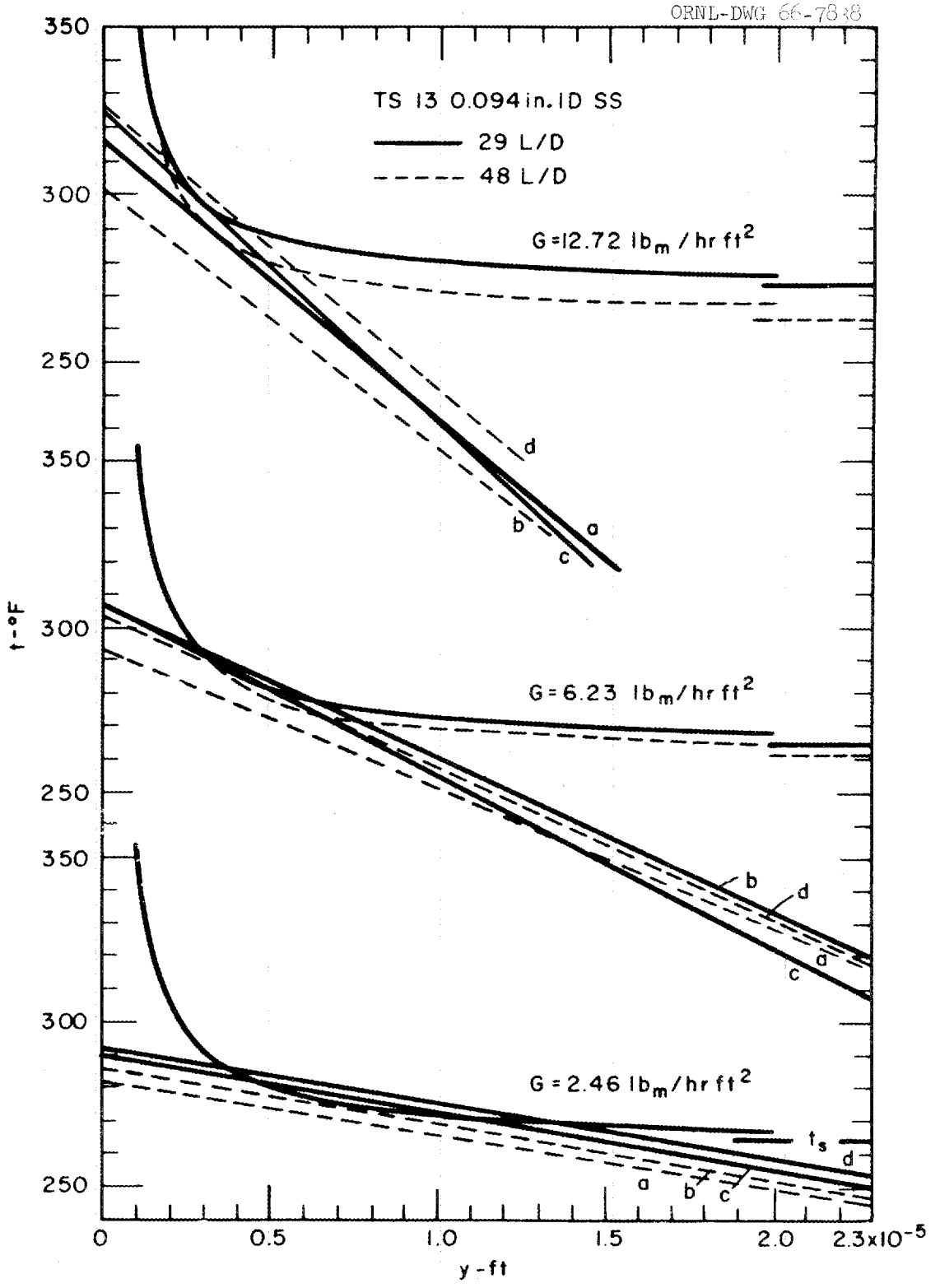

Fig. 12. Analytical Determination of Incipient Boiling with Forced Convection.

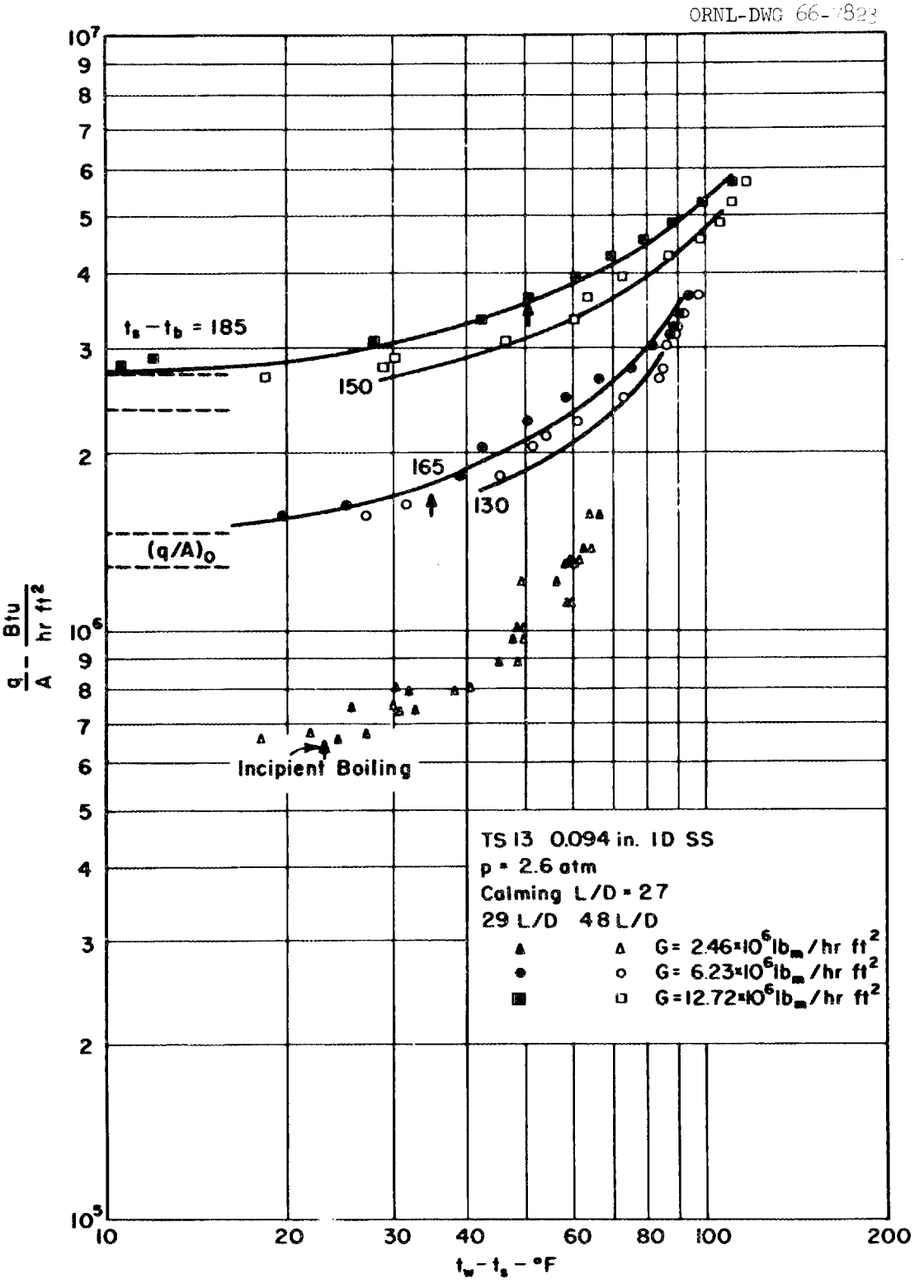

Fig. 13. Forced-Convection Surface Boiling of Water in a Tube.

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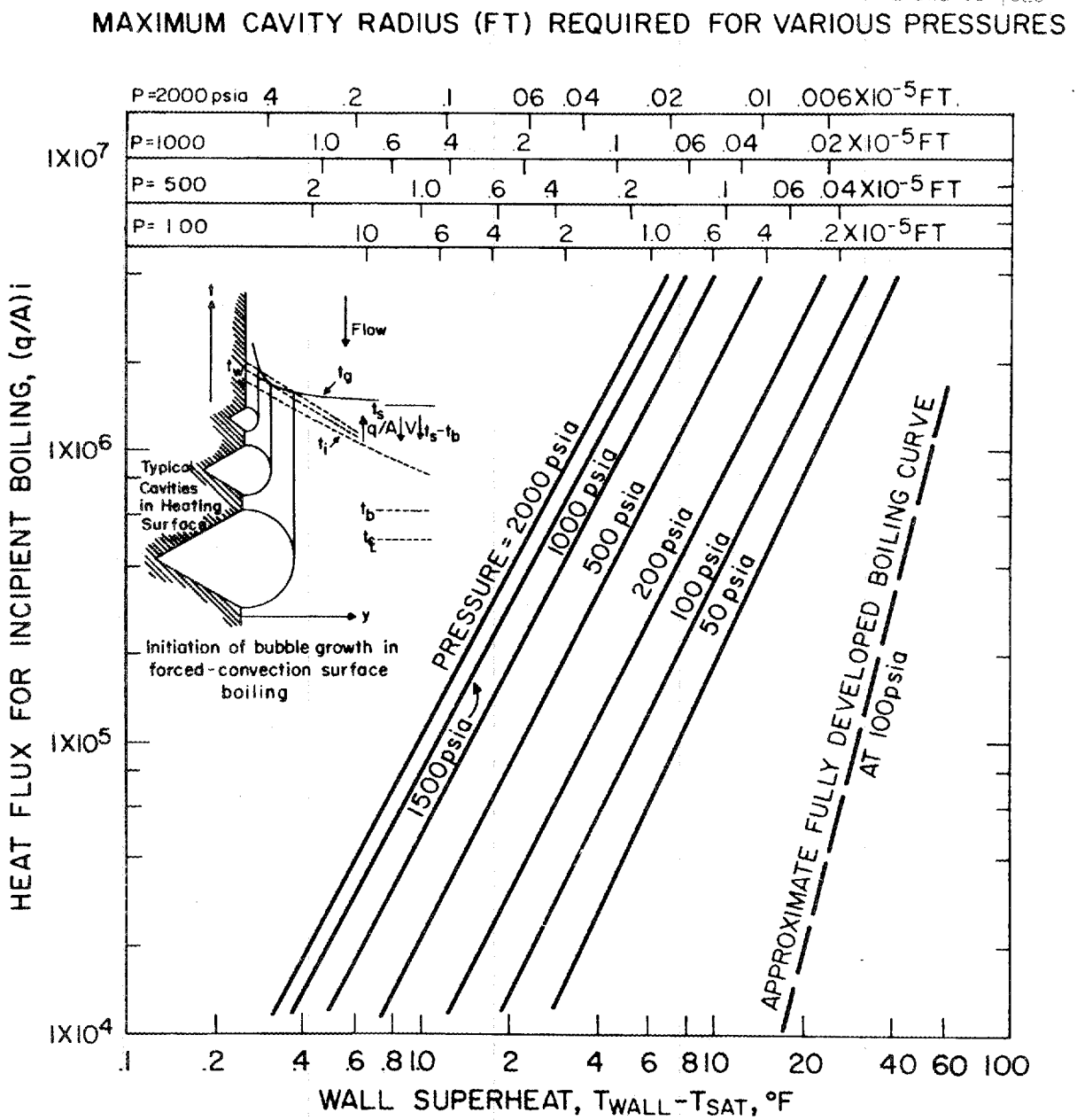

Fig. 14. Bergles-Rohsenow Criteria for Incipient Boiling of Water:

$$(q/A)_i = 15.60\ P^{1.156}\ (T_{wall} - T_{sat})^{2.30/P^{0.0234}}$$

Figure 15 shows the prediction graph for sodium in comparison with that for water. Because the thermal conductivity of liquid metals is so large, the temperature gradients in the liquid are very small; hence, the point-of-tangency procedure would require that there be present extremely large cavities. For liquid metals, the first cavity to nucleate is the largest one which has gas or vapor present.

Figure 16 shows some data that emphasizes the fact that these different cavity size distributions can shift the boiling curve. Different surface finishes, with the same fluid, mean different cavity size distribution curves. The liquid is the same, the pressure is the same, the only thing that changed is the surface finish. Now, think of a boiling correlation equation, q/A versus ΔT, with only fluid properties in the equation. It is ever possible for any boiling correlation equation that doesn't involve some statement about the surface to correlate the data? Any boiling correlation equation that involves only fluid properties cannot account for the displacement of the curves in Fig. 16, because the fluid properties are the same for all of these cases. On the other hand, the coefficient would be a function at least of the surface and perhaps of the surface-fluid combination. It is impossible to have a constant coefficient in a boiling correlation equation which contains only fluid properties; Fig. 16 proves that point. Figure 17 for a liquid metal further emphasizes the surface effect.

POOL BOILING CORRELATIONS

Let's look now at the old boiling correlations briefly, because I want to make a change in one. Many years ago in going through a set of logic which we will omit here, we arrived at the correlation shown at the bottom of Fig. 18 where C_{sf} was to be a function of the surface-fluid combination. This equation is q/A versus ΔT modified by fluid properties. The form of the dimensionless equation is the first term of the power series.

We managed to find, for a whole host of fluids, raw data for q/A versus ΔT and evaluated C_{sf}, m, and n. We managed to get the same exponents on the Prandtl number and on the quantity given by the ordinate of

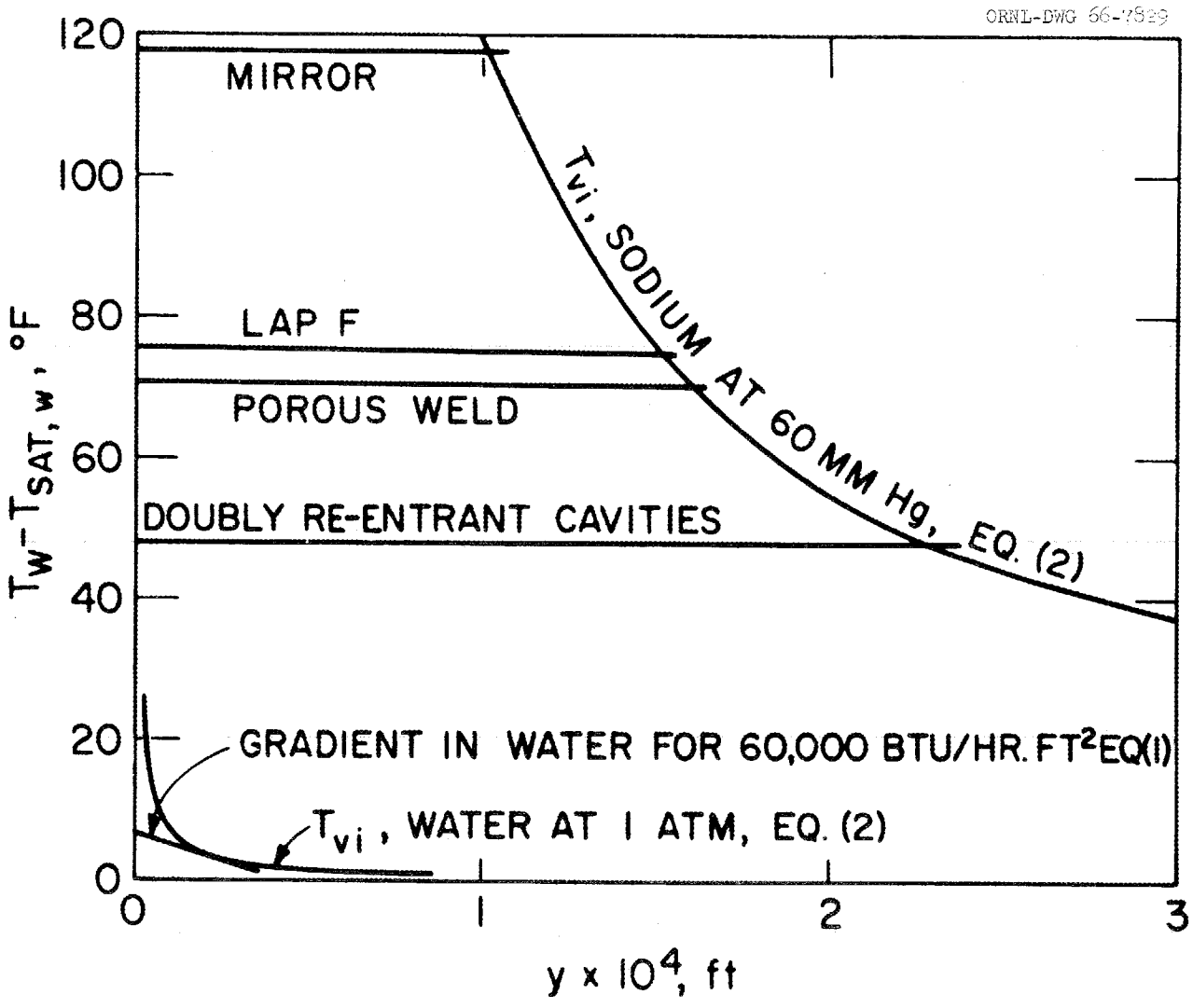

Fig. 15. Prediction of Cavity Size Required for Nucleation with Sodium on Several Surfaces. Lower curve shows prediction for water.

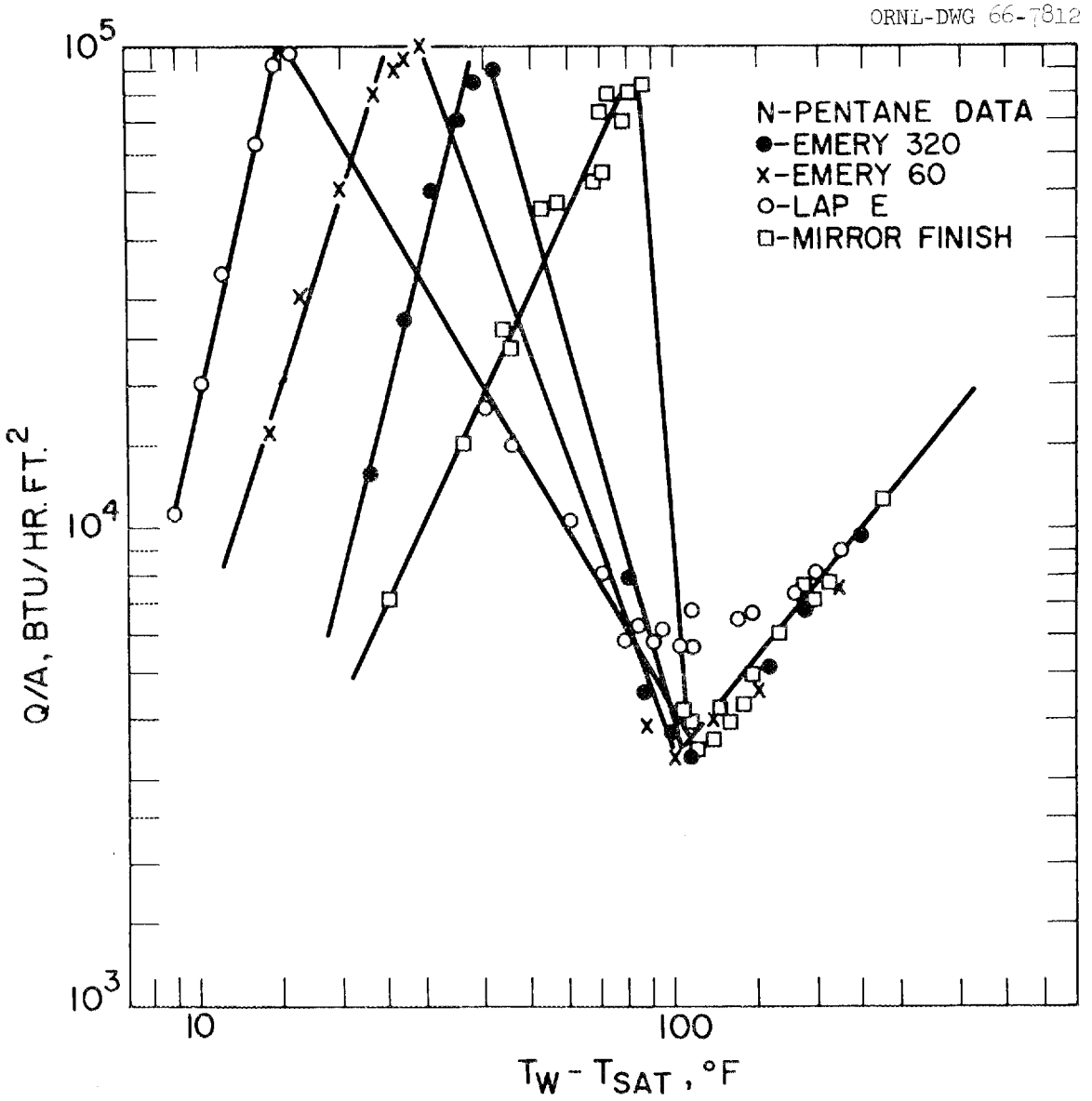

Fig. 16. Effect of Surface Roughness on Pool Boiling from a Horizontal Flat Plate.

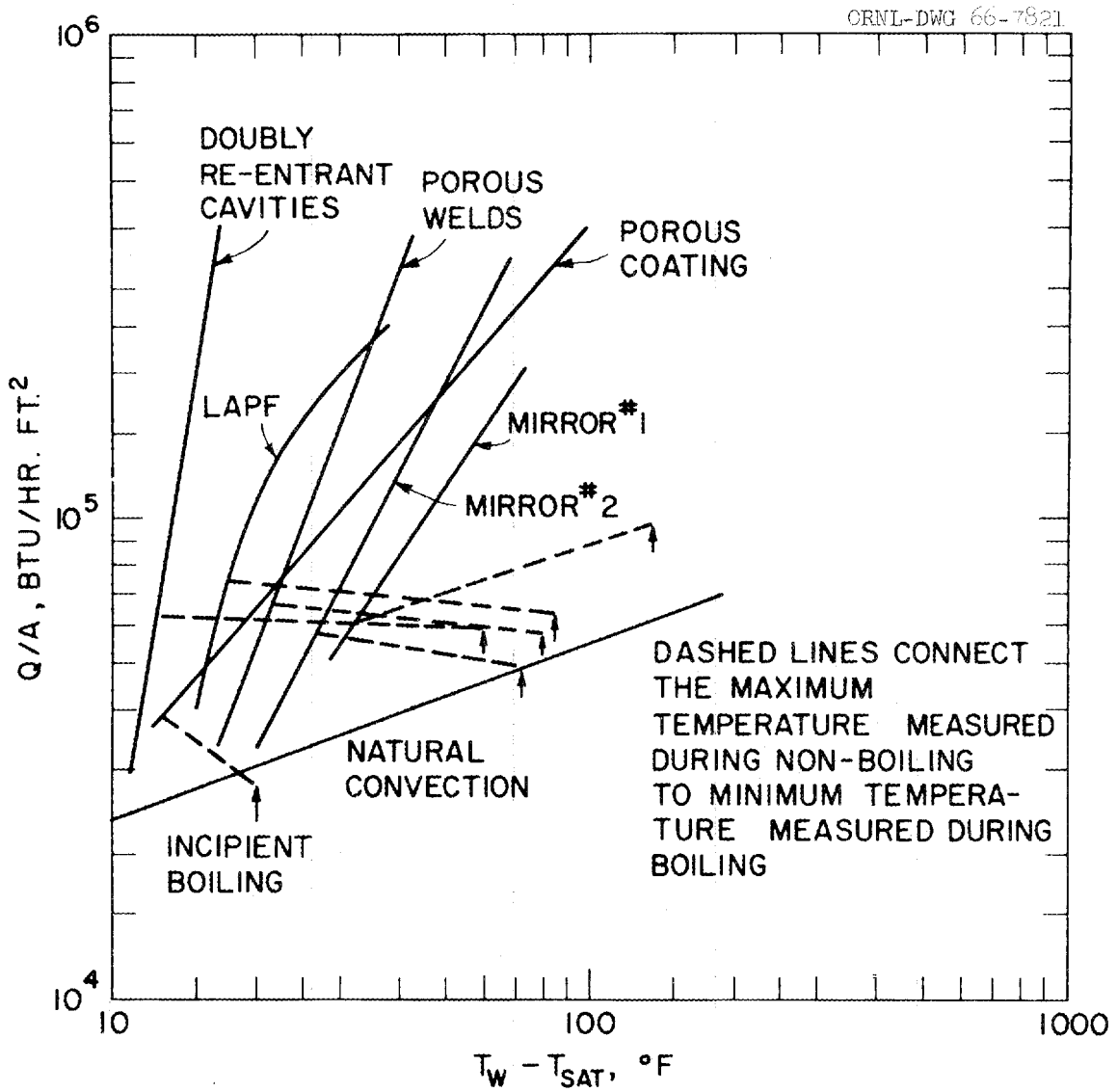

Fig. 17. Comprehensive Plot of Sodium Heat-Transfer Data at an Average Pressure of 65 mm Hg.

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NON-BOILING FORCE CONVECTION

$$N_{Nu} = \phi\,(N_{RE}\,,\,N_{PR})$$

BOILING

$$N_{Nu,b} = \phi\,(N_{RE,b}\,,\,N_{PR})$$

$$\frac{N_{RE,b}\,N_{PR}}{N_{Nu,b}} = \frac{c_\ell\,T_x}{h_{fg}}$$

$$\frac{c_\ell\,T_x}{h_{fg}} = \phi\,(N_{RE,b}\,,\,N_{PR})$$

$$\frac{c_\ell\,T_x}{h_{fg}} = \underbrace{C\,\beta^m}_{C_{sf}}\left(\frac{q/A}{\mu_\ell\,h_{fg}}\sqrt{\frac{g_o\,\sigma}{g(\rho_\ell-\rho_v)}}\right)^m\left(\frac{c_\ell\,\mu_\ell}{k_\ell}\right)^n$$

Fig. 18. Boiling Correlation Including Effect of Surface-Fluid Combination.

Fig. 19; however, the coefficient changed considerably. The value $m = 0.33$ shown in Fig. 19 says that the slope of these curves is 3 to 1. The slope of the q/A versus ΔT curve is $1/m$ and depends upon the shape of that curve. If by doing something to the surface, like polishing, the cavity size distribution changes, it is reasonable to expect that both the position of this curve and the slope will change. However, there seems to be a rule of nature that if you don't do anything to the surface by hand, just let a machine do it, then test results suggest the m is very close to 3. On the other hand, when hand finishing, such as with emery cloth or polishing, is done, then m can indeed be very much greater than 3. Thus, this 3 to 1 exponent is merely a guess from experience that for commercial surfaces – cleaned, however – the 3 to 1 slope is perhaps about right. When this data was processed originally, we found $n = 1.7$ looked pretty good; but we were using the wrong Prandtl number data. Later (since 1951), we obtained better Prandtl number data for water at high pressures and had to go back and reevaluate this boiling data; it now seems that 1.7 should be 1.0 for water. With all other fluids that we have tested, it still should be 1.7.

The Forster-Zuber correlation was based on a Reynolds number which was calculated from the growth of a spherical bubble in an infinite atmosphere; the dimensionless groups involved are shown in Fig. 20. Forster-Zuber obtained the third equation (Fig. 20) using only a limited amount of data for peak heat flux. We reevaluated their suggested equation using data all along the q/A versus ΔT curve and obtained the bottom equation of Fig. 20; this is shown in more detail in Figs. 21 and 22. The agreement of this modified equation with data, as shown in Fig. 22, is rather good; however, the K_{Sf} is far from being a constant as shown on Fig. 21.

Two other suggested correlation are shown in Fig. 23; again, the implication is that the coefficients are constant. This cannot be so in light of the data of Figs. 16 and 17, because the equations involve only properties of the fluid.

In Fig. 24 data for water at three pressures (14.7, 770, and 2465 psia) is replotted and compared with two correlation equations. The curves labeled F-G represent the second Forster-Greif equation, Fig. 23;

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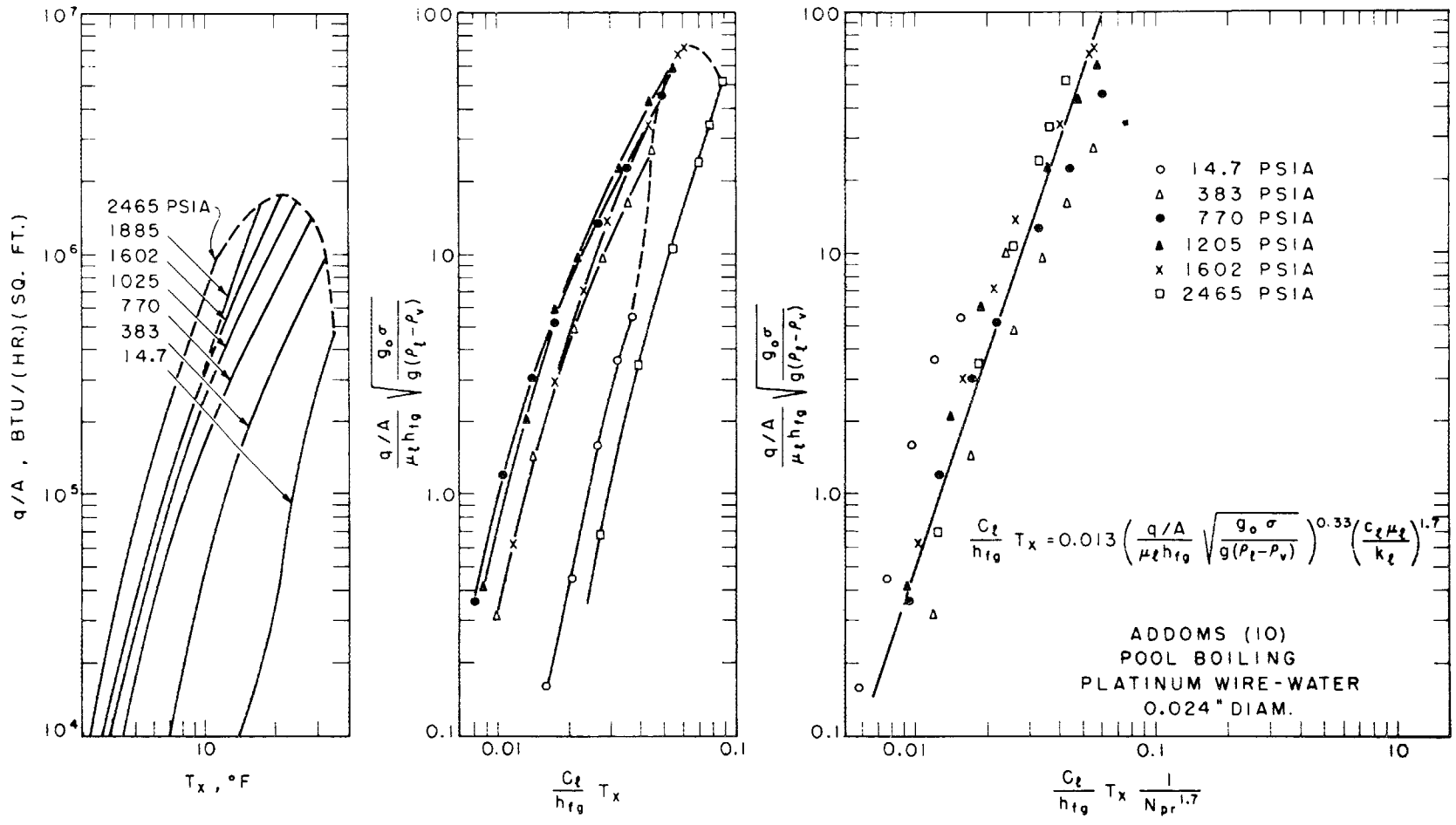

Fig. 19. Graphical Summary of Development of Correlation for Pool Boiling of Water.

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FORSTER-ZUBER

$$Re_{FZ} \equiv \frac{R\dot{R}\rho_\ell}{\mu_\ell} = \frac{\pi}{2}\frac{\rho_\ell}{\mu_\ell}\left[\frac{(T_w - T_s)\sqrt{k_\ell \rho_\ell c_\ell}}{\rho_v h_{fg}}\right]^2$$

$$Nu_{FZ} \equiv \frac{(q/A)R}{(T_w - T_s)k_\ell} = \frac{q/A}{(T_w - T_s)k_\ell}\left[\frac{(T_w - T_s)\sqrt{\pi k_\ell \rho_\ell c_\ell}}{\rho_v h_{fg}}\left(\frac{2\sigma}{\Delta p}\right)^{1/2}\left(\frac{\rho_\ell}{\Delta p}\right)^{1/4}\right]$$

$$Nu_{FZ} = K_{FZ}\, Re_{FZ}^{0.62}\, Pr_\ell^{0.33}$$

$$Nu_{FZ} = K_{sf}\, Re_{FZ}^{1.05}\, Pr_\ell^{-7}$$

Fig. 20. Forster-Zuber and Rohsenow Correlations for Nucleate Pool Boiling.

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$$N_{Nu,b} = C\,(N_{Re,b})^{m}\,(N_{Pr,1})^{n}$$

$$\frac{C_L(T_W - T_{SAT})}{h_{fg}} = C_{sf}\left(\frac{q/A}{\mu_L h_{fg}}\sqrt{\frac{g_0\sigma}{g(\rho_L-\rho_V)}}\right)^{.33}\left(\frac{C_L\mu_L}{k_L}\right)^{1.7}$$

$$\frac{q/A}{(T_W - T_{SAT})k_L}\left[\frac{(T_W - T_{SAT})C_L\rho_L\sqrt{\pi\alpha}}{h_{fg}\rho_V}\sqrt{\frac{2\sigma}{\Delta P}}\sqrt[4]{\frac{\rho_L}{\Delta P}}\right] = K_{sf}\left[\frac{\rho_L}{\mu_L}\left(\frac{(T_W - T_{SAT})C_L\rho_L\sqrt{\pi\alpha}}{h_{fg}\rho_V}\right)^{1/2}\right]^{1.05}\left[\frac{C_L\mu_L}{k_L}\right]^{-7}$$

FLUID - HEATING SURFACE	C_{sf}	K_{sf}
WATER - PLATINUM	.013	---
BENZENE - CHROMIUM	.010	1
ETHANOL - CHROMIUM	.0027	300
n - PENTANE - CHROMIUM	.015	.30
WATER - BRASS	.0060	---

Fig. 21. Rohsenow Correlation for Nucleate Pool Boiling Showing Variation in Fluid-Surface Factor.

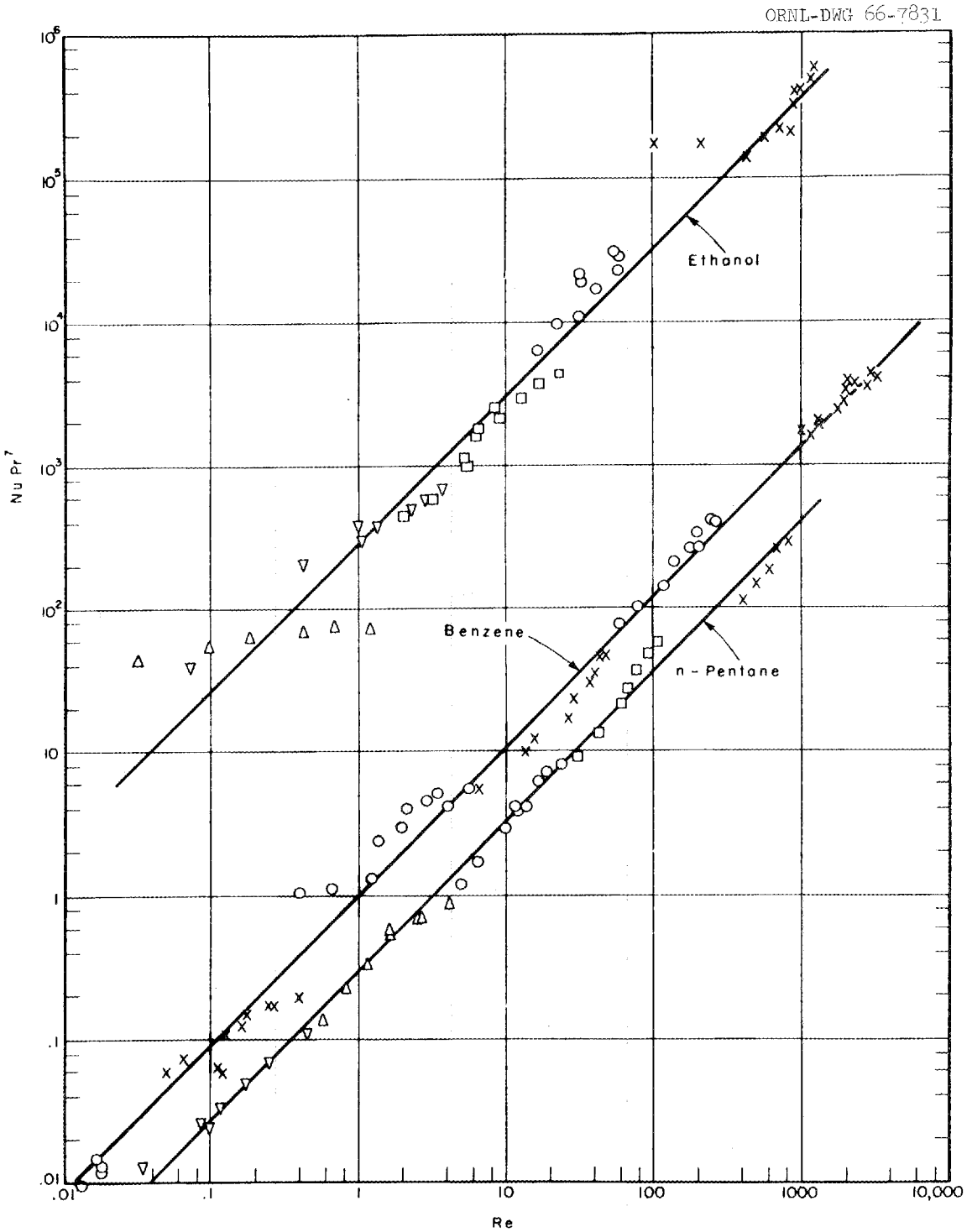

Fig. 22. Comparison of Rohsenow Nucleate Pool Boiling Correlation with Experimental Data for Ethanol, Benzene, and n-Pentane.

POOL BOILING

FORSTER-GREIF

$$\frac{\frac{q}{A}\cdot\frac{2\sigma}{\Delta p}}{k\ \Delta T} = C_1\left[\frac{\rho_L}{\mu_L}\left(\frac{c_L\sqrt{\rho_L k_L c_L \pi}\ T_{sat}\ \Delta p}{(h_{fg}\rho_v)^2}\right)^2\right]^{1/5} Pr_L^{1/3}$$

$$\frac{(q/A)}{\rho_v h_{fg}}\left(\frac{2\sigma}{a_t \Delta p}\right)^{1/2}\left(\frac{\rho_L}{\Delta p}\right)^{1/4} = C_2\left[\frac{\rho_L}{\mu_L}\left(\frac{c_L\sqrt{\rho_L k_L c_L \pi}\ T_{sat}\ \Delta p}{(h_{fg}\rho_v)^2}\right)^2\right]^{5/8} Pr_L^{1/3}$$

GILMOUR

$$\frac{(q/A)/\Delta T}{c_L G} = 0.001\left(\frac{p^2}{\rho_L\sigma}\right)^{0.425}\left(\frac{DG}{\mu_L}\right)^{-0.3} Pr_L^{-0.6}\ ;\quad G \equiv \frac{(q/A)}{h_{fg}\rho_v}\rho_L$$

OR

$$\frac{(q/A)D}{\mu_L h_{fg}\rho_v}\rho_L = 0.1\left(\frac{p^2}{\rho_L\sigma}\right)^{1.42}\left(\frac{\rho_L c_L \Delta T}{h_{fg}\rho_v}\right)^{3.33} Pr_L^{-2}$$

OR

$$\frac{c_L \Delta T}{h_{fg}}\cdot\frac{\rho_L}{\rho_v} = 1000\left(\frac{\rho\sigma}{p^2}\right)^{0.425}\left(\frac{q/A}{h_{fg}\rho_v}\ \frac{\rho_L D}{\mu_L}\right)^{0.3} Pr_L^{0.6}$$

Fig. 23. Forster-Greif and Gilmour Correlations for Nucleate Pool Boiling.

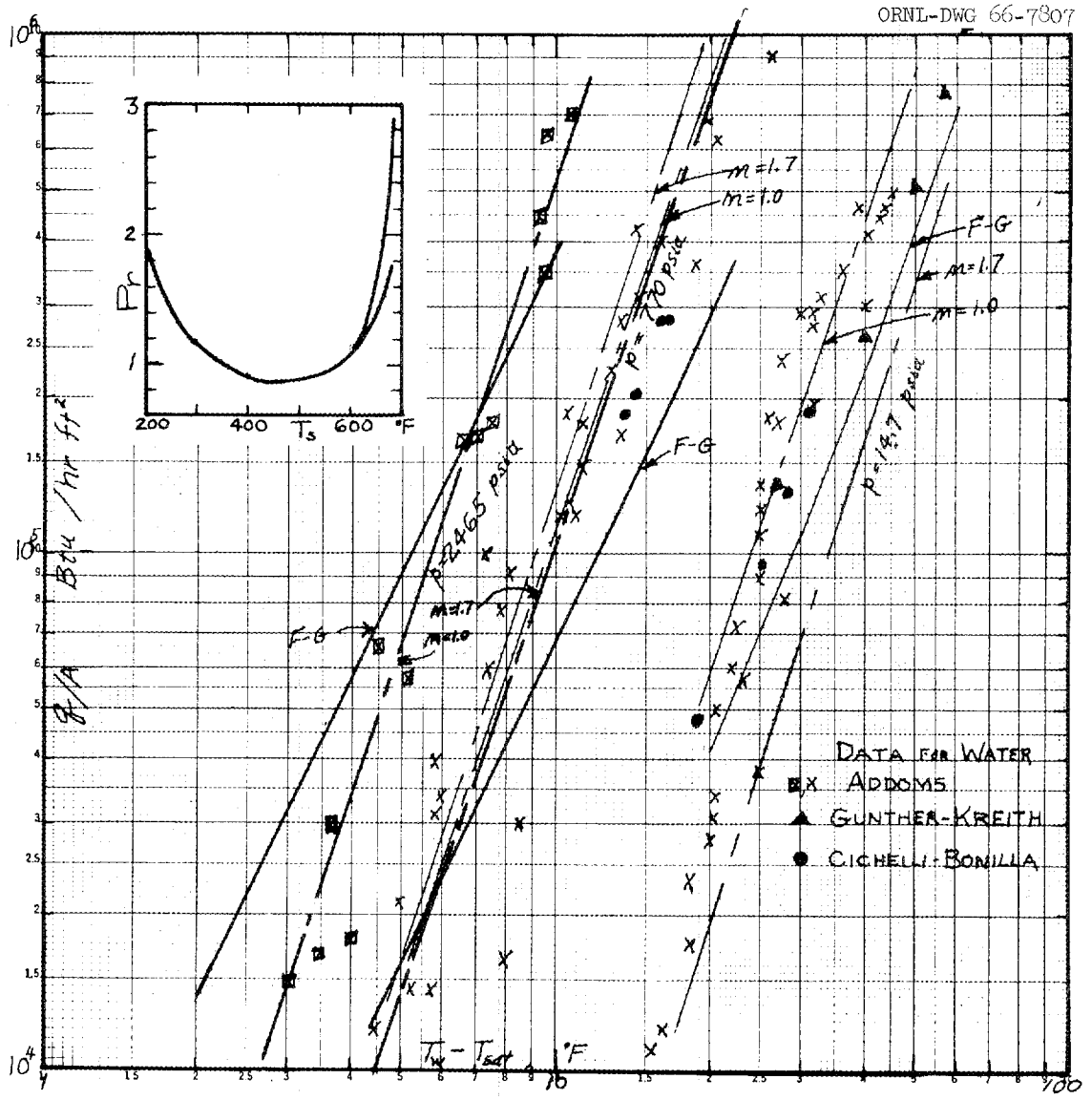

Fig. 24. Comparison of Water Nucleate Pool Boiling Data with Forster-Greif and Rohsenow Correlations.

the curves labeled n = 1.0 and 1.7 are for the equation at the bottom of Fig. 18, where n is the exponent of the Prandtl number. The inset in Fig. 24 is the curve of Prandtl number versus temperature; the lower curve was used originally, while the upper curve is the correct one. The equation of Fig. 18 does a good job of correlating the data over this wide pressure range, with n = 1.0 and C_{Sf} = 0.013. Note that for all fluids considered, other than water, n = 1.7 correlates the data better. The F-G curves are displaced and have a slope different from that of the data.

To summarize, for pool boiling correlations we do not know how to take into account the effect of the solid surface. Hence, the magnitude of a coefficient such as C_{Sf} (Fig. 18) must be determined experimentally for each case. Then, so long as this surface effect doesn't change, the equation predicts rather well the effect of pressure.

POOL BOILING, LIQUID METALS

Sodium was boiled on the horizontal surface shown in Fig. 25. Various surface finishes – welds, man-made holes, etc. – were studied with results as shown in Fig. 17. This work was reported in ASME Journal of Heat Transfer, May 1966.

For some surfaces, an unstable kind of boiling was observed. Figure 26 shows traces of a surface temperature reading and of noise level as indicated by a microphone pickup on the apparatus. The hash in the noise level is associated with nucleate boiling. Boiling appears to take place when the wall temperature is low. We suggest that the rise in wall temperature is associated with the "snuffing" out or deactivation of the nucleation sites. When the wall temperature gets high enough, a smaller cavity nucleates; and boiling is reestablished across the surface.

Figure 27 shows how q/A influences the frequency of this rather random instability. There appeared to be an upper limit heat flux above which these instabilities do not occur. [Later work (Fall 1966) suggests that these random instabilities occur only when noncondensable gas is present in the cavities. After long periods of boiling when the gases are removed from the cavities, these instabilities are not observed.]

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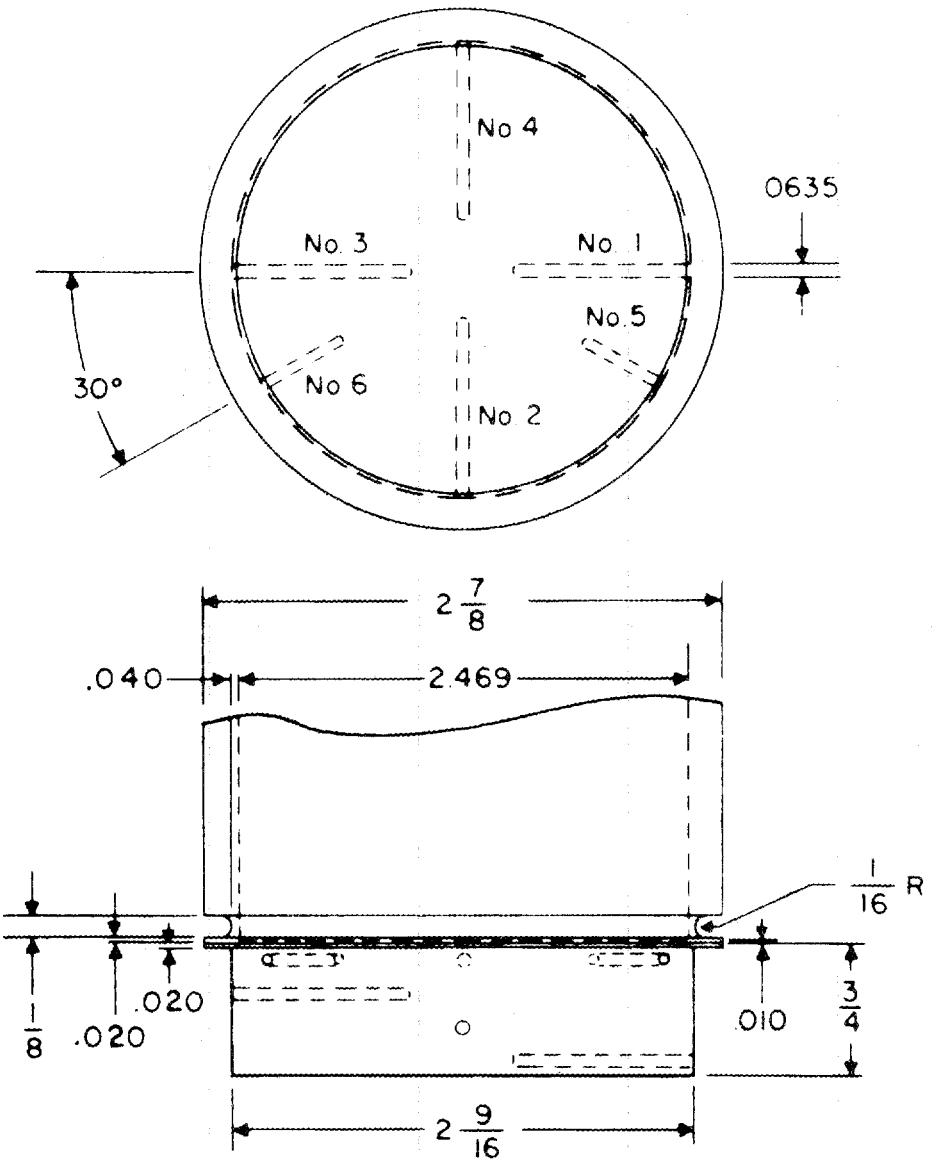

T.C. NO.	IMMERSION LENGTH	℄ DISTANCE TO BOILER SURFACE
1	1.0 IN.	0.660 IN.
2	1.0 IN.	0.470 IN.
3	1.0 IN.	0.280 IN.
4	1.0 IN	0.090 IN.
5	0.5 IN.	0.090 IN.
6	0.5 IN.	0.090 IN

Fig. 25. Surface Used in Sodium Pool Boiling Studies.

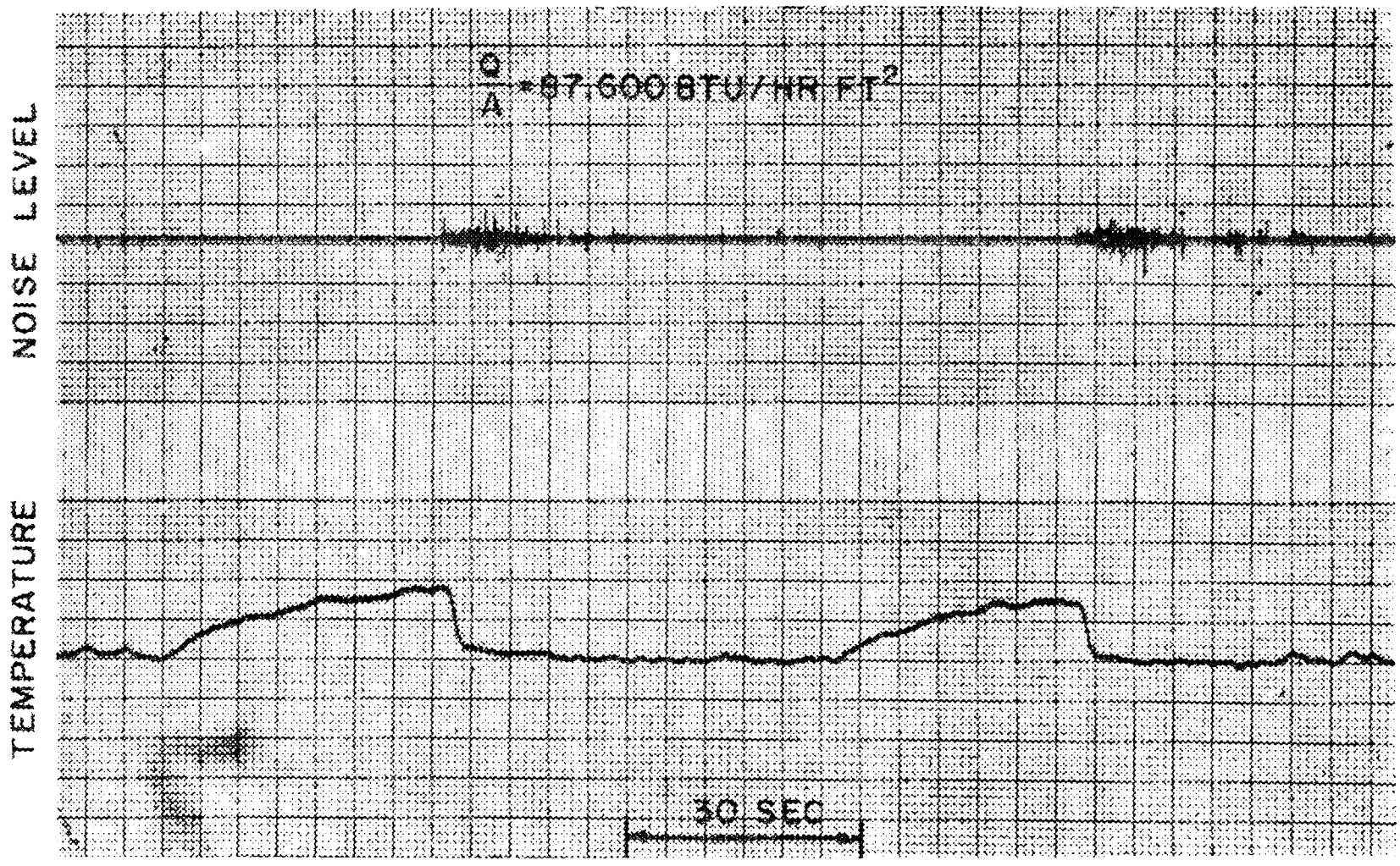

Fig. 26. Noise Level and Surface Temperature Traces Obtained with Pool Boiling Sodium. Hash in noise level curve corresponds to periods of nucleate boiling.

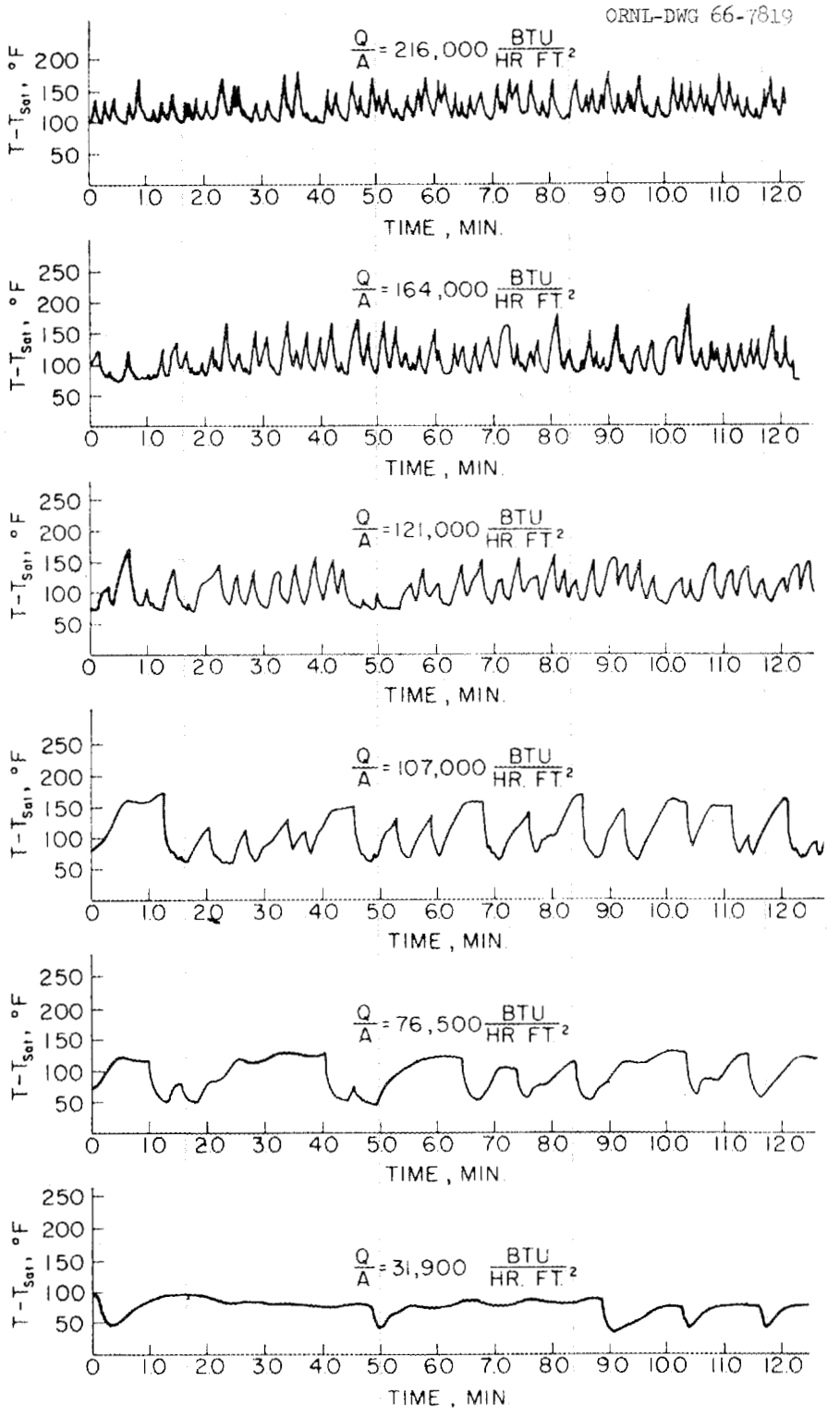

Fig. 26. Effect of Heat-Flux Level on Cavity-Snuffing Frequency with Pool Boiling Sodium.

In the May 1966 issue of ASME Journal of Heat Transfer, an approximate theory is proposed for predicting the conditions under which a cylindrical cavity (Fig. 28) may fill with liquid and be snuffed out between bubbles. The results predict the unstable heat flux reasonably well but give large errors in predicted frequency. This problem has been reanalyzed with results that predict rather well the measured bubble frequencies and describe the temperature variations within the heating surface (I. Shai, ScD thesis, MIT, January 1967).

Figure 29 shows the range of ΔT (dashed line) for a particular operating level. Here, 316 stainless steel was unstable (gas must have been present), and nickel was stable.

CRITICAL HEAT FLUX WITH NONUNIFORM FLUX DISTRIBUTION

Figure 30 shows the various flow regimes which may be encountered in a tube. We will limit consideration to conditions where the critical condition always occurs in the annular-mist-flow regime, where liquid is on the wall, and where there is a vapor core.

In the vapor core, the velocity increases down the tube because of the evaporation of the liquid. The liquid layer on the wall protects the tube. If that liquid layer were to disappear, then the tube wall temperature would rise rapidly. Now consider a given fixed flow rate, a fixed pressure, and a fixed diameter of tube, and ask how nonuniform heat flux influences the location of the critical heat flux. At a low heat flux, but very high quality, it is quite conceivable that no nucleation takes place; and pure conduction across the thin liquid layer produces the evaporation. There is no nucleation! In this case, the critical heat flux may be associated with a tearing off of the liquid layer. Let's postulate that, as this vapor velocity increases, it gets up to a tearing velocity and rips the film right off the wall. Then, regardless of the shape of the heat flux distribution, we will expect that the same vapor velocity (or if we are holding the same flow rate, the same quality) should give us a burnout condition provided there is no nucleation. This ought to be independent of the heat flux or heat flux distribution. When nucleation upends that thin film, it should tear off at a lower vapor

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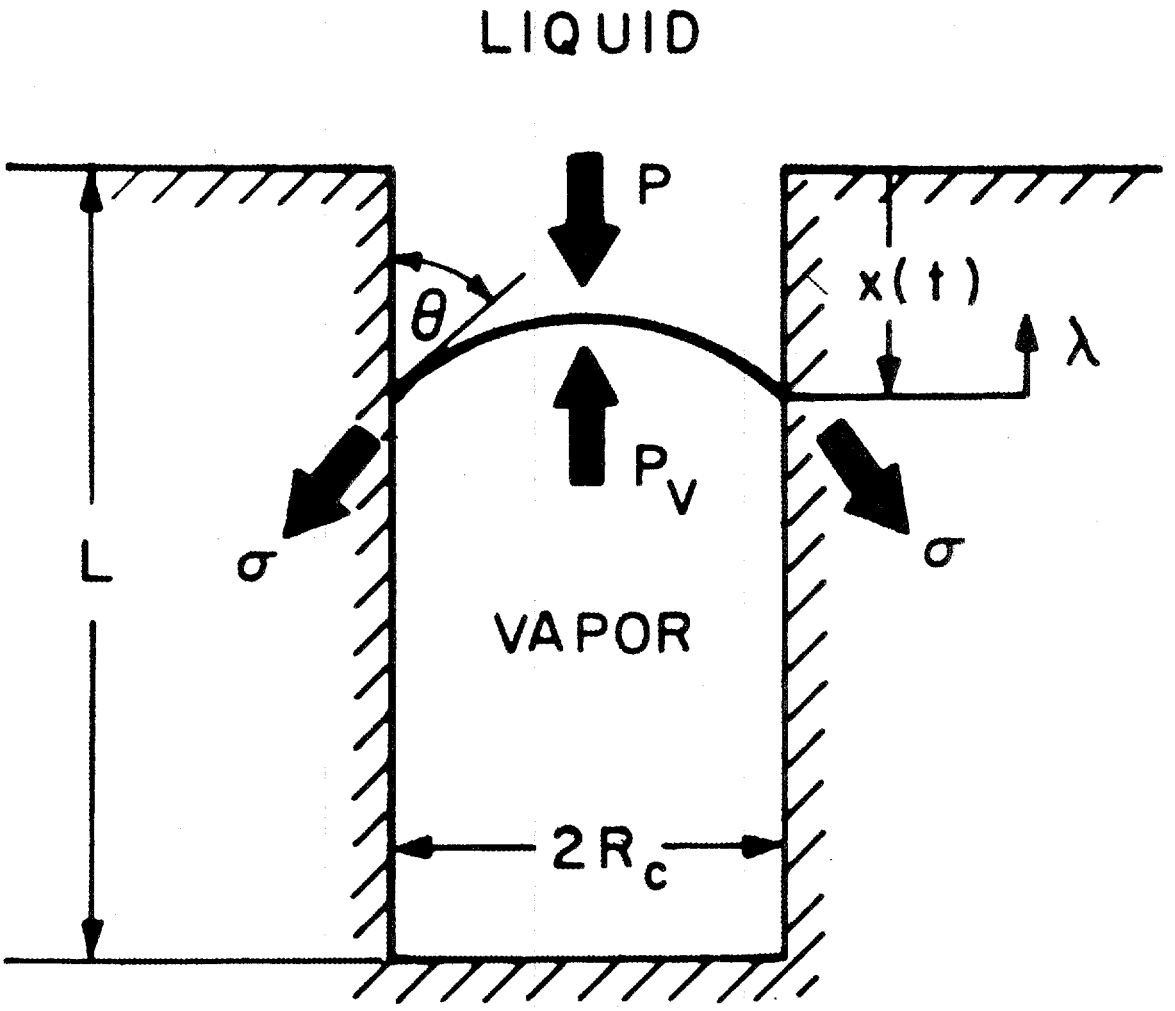

Fig. 28. Model Used in Prediction of Conditions for Deactivation of a Cylindrical Cavity.

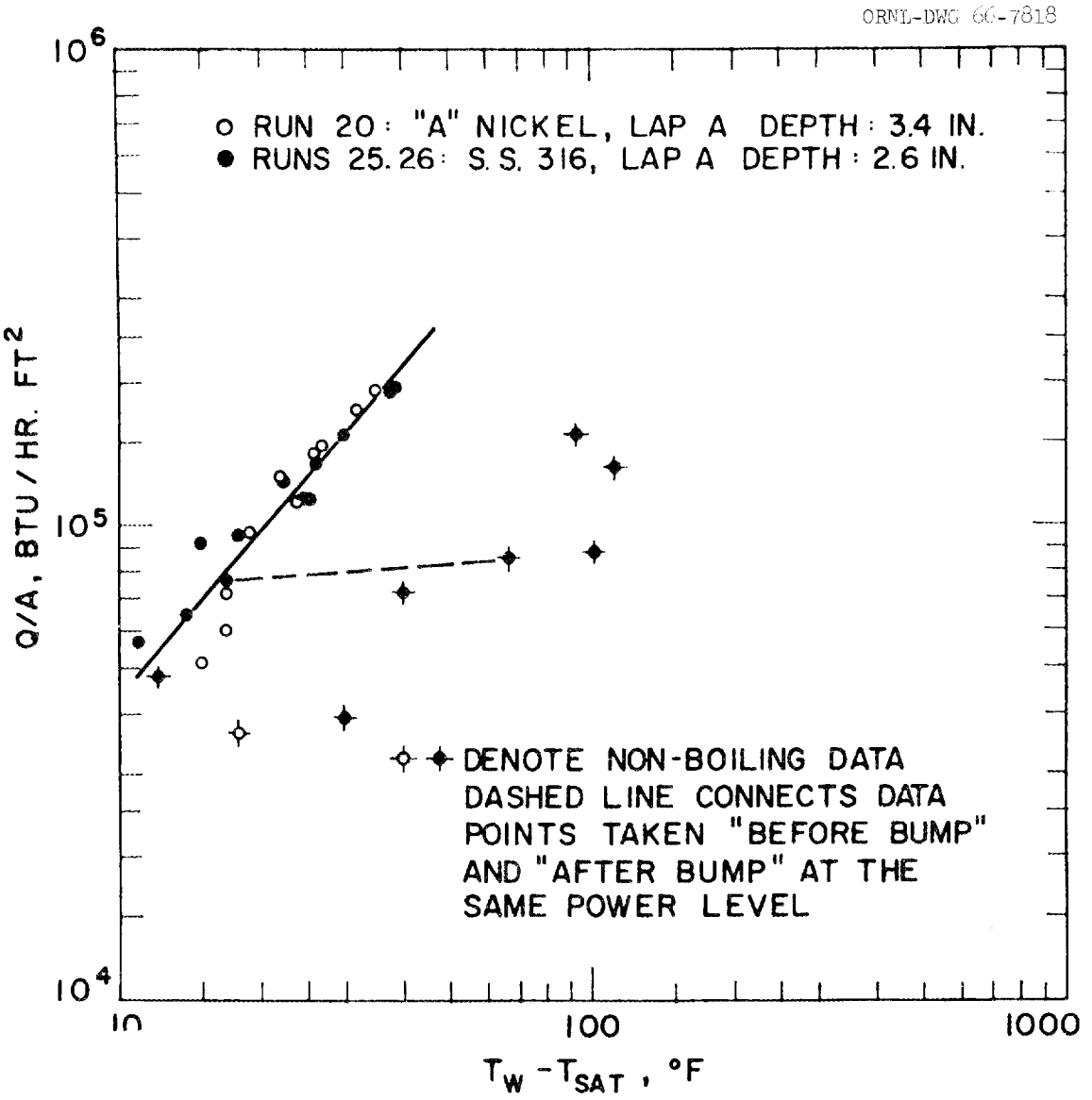

Fig. 29. Sodium Boiling on A-Nickel and Type 316 Stainless Steel Surfaces Showing Stable and Unstable Results.

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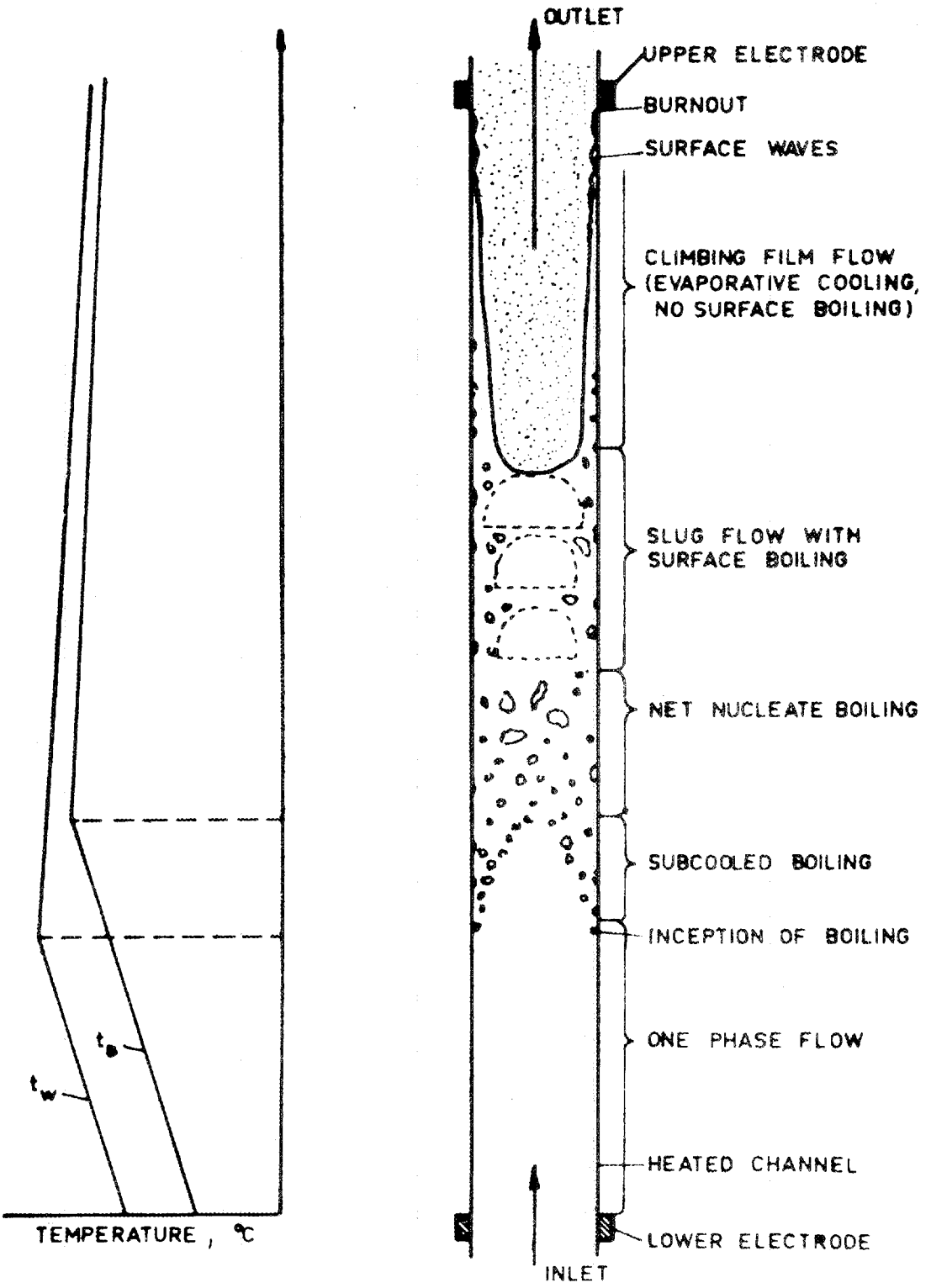

Fig. 30. Flow Model for Burnout in a Vertical Channel.

velocity or a lower quality. The more intense the nucleation, the less velocity of vapor it would take to tear off that film. This is perhaps an oversimplification of a complex process, but let us pursue this line of reasoning in comparing some data.

Figure 31 shows what happens to wall temperature in going through the critical condition (dry wall). At low heat flux and high quality, the wall temperature merely goes from a low value when the wall is wet to a higher value when the wall is dry.

Figure 32 shows the various heat flux distributions we studied (Todreas-Rohsenow, International Heat Transfer Conference, Chicago, Illinois, August 1966). Aluminum tubes (0.214-in. inside diameter and 30 and 48 in. long) were machined on the outside to provide a thickness variation with length such that the various indicated heat flux (I^2R) distributions were obtained. Circumferential notches provided the "spikes."

Figure 33 shows the flux distributions at which a burnout occurred at a particular flow rate and tube size. There appears to be very little agreement among the various conditions.

Figure 34 shows conditions and locations of burnout points for various flux distributions. Sometimes burnout occurs at the end of the tube and at other times upstream from the end.

One proposed postulate for comparing various heat flux distribution cases suggests that the integrated heat flux should be the same at burnout. Figure 35 compares such calculations for many of the cases tested here. There does not appear to be agreement.

Figure 36 is a graph of the heat flux at burnout divided by the heat flux for incipient boiling. When this ratio is below 1, there is no nucleate boiling; above 1, there is nucleate boiling. We suggested earlier in this discussion that when no nucleate boiling exists, the burnout data (critical heat flux data) ought to be independent of the heat flux distribution. This ought to occur at a given quality represented by a vertical line on this graph. There appears to be this nonnucleating region in Fig. 36. Above 1.0 on the ordinate, nucleation takes place. The ratio plotted as the ordinate is a measure of the intensity of nucleation. As

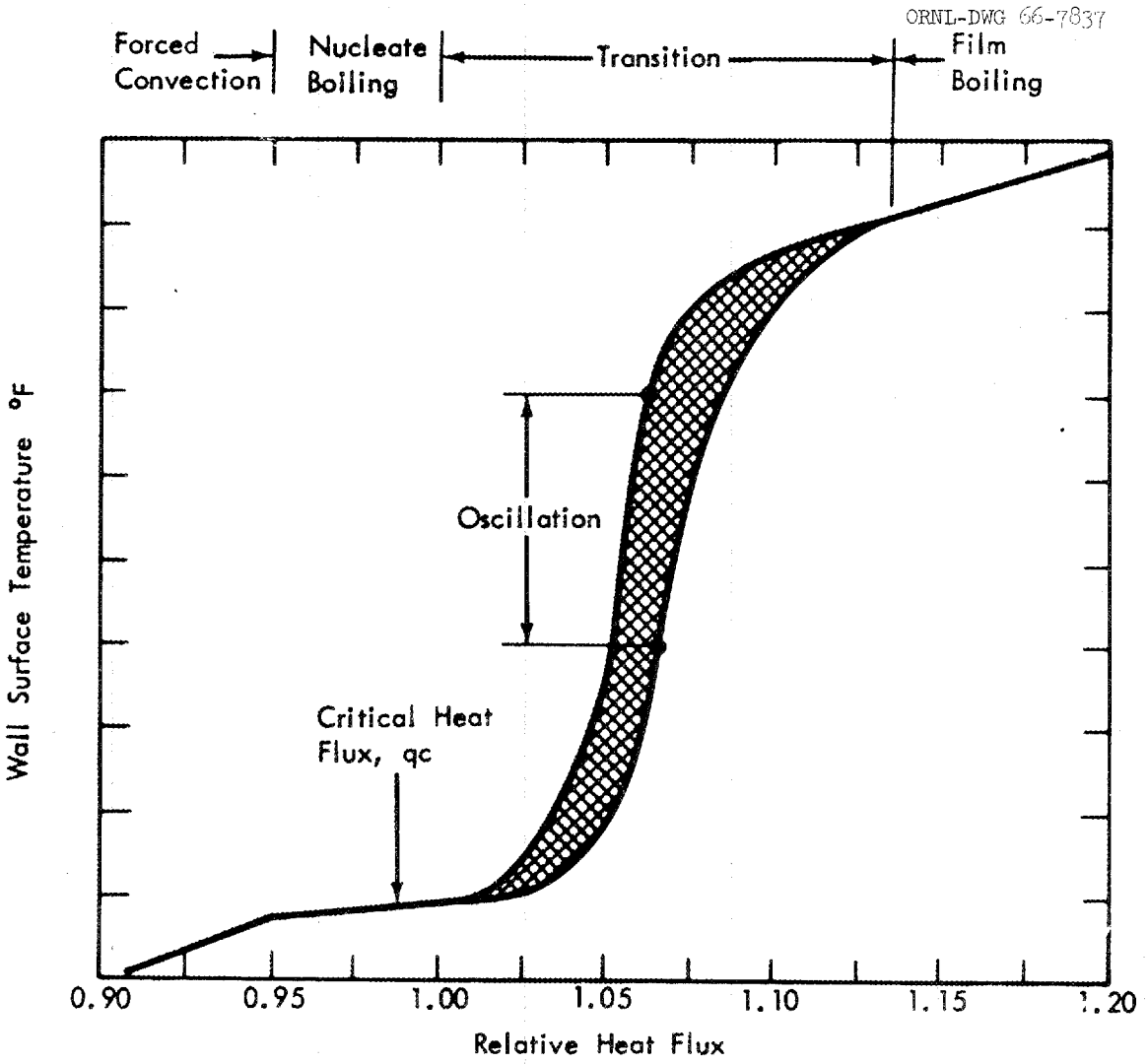

Fig. 31. Temperature History During "Burnout".

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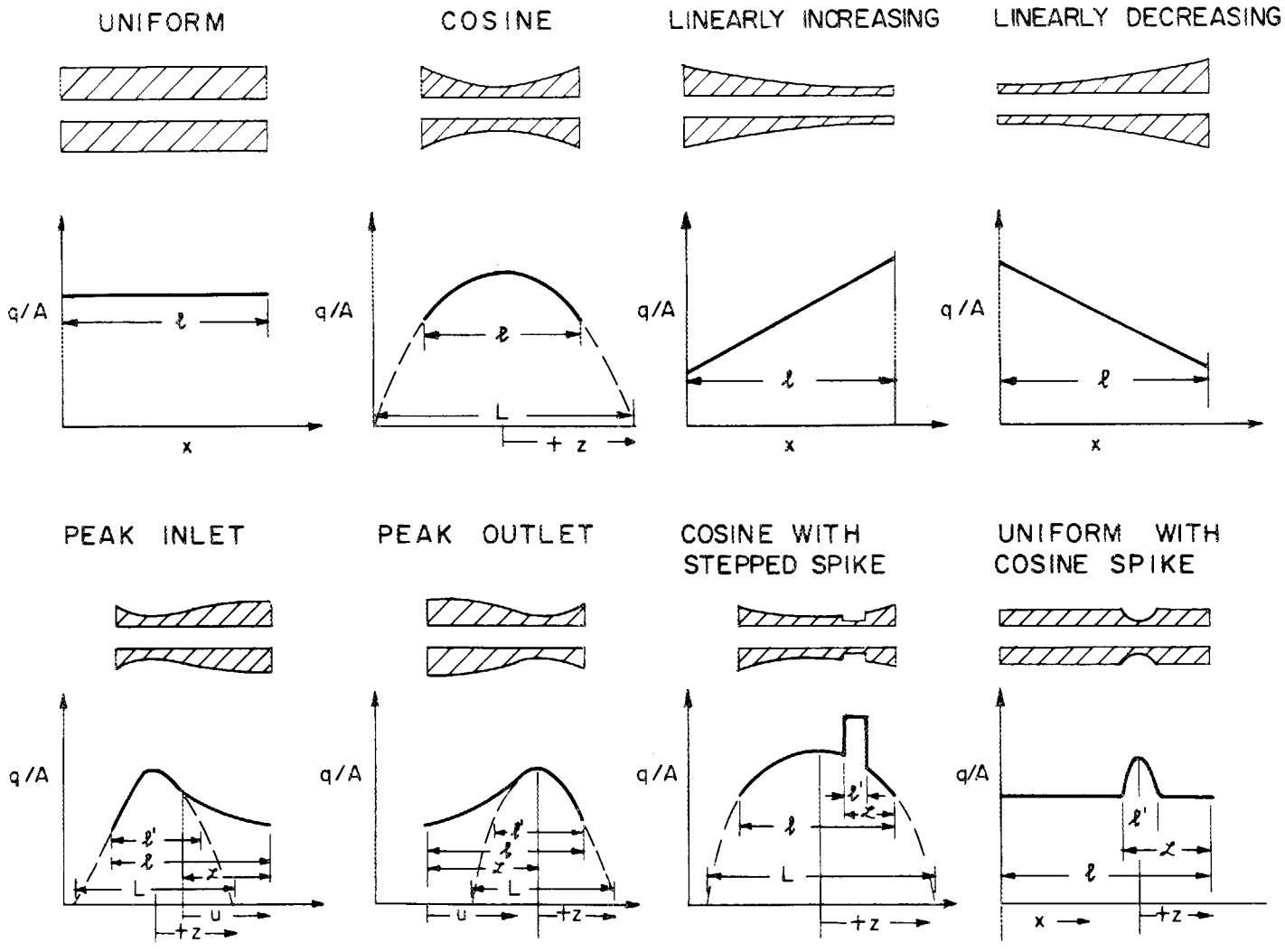

Fig. 32. Axial Flux Distributions Tested.

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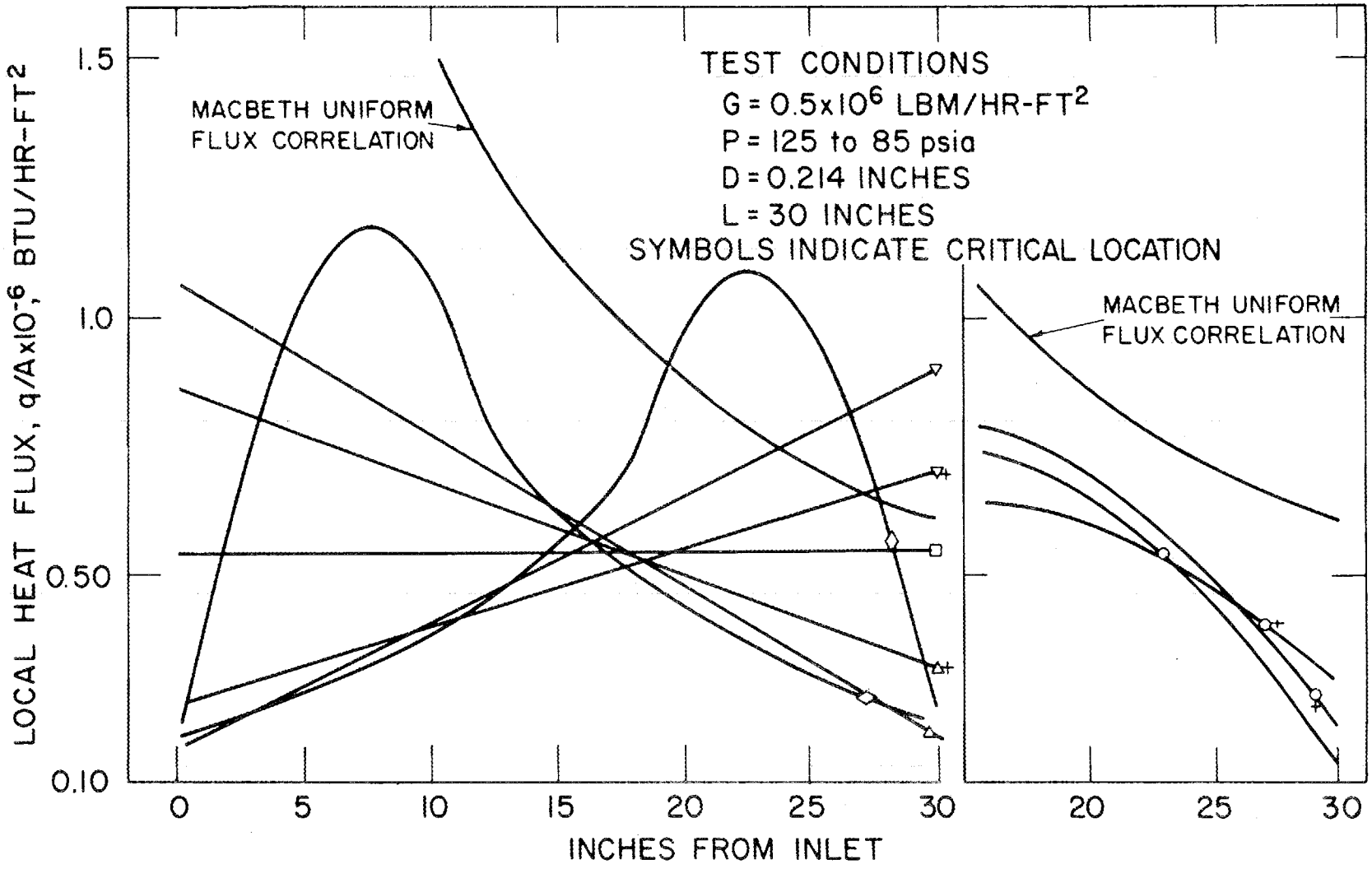

Fig. 33. Comparison of Test Results with the Local Condition Hypothesis.

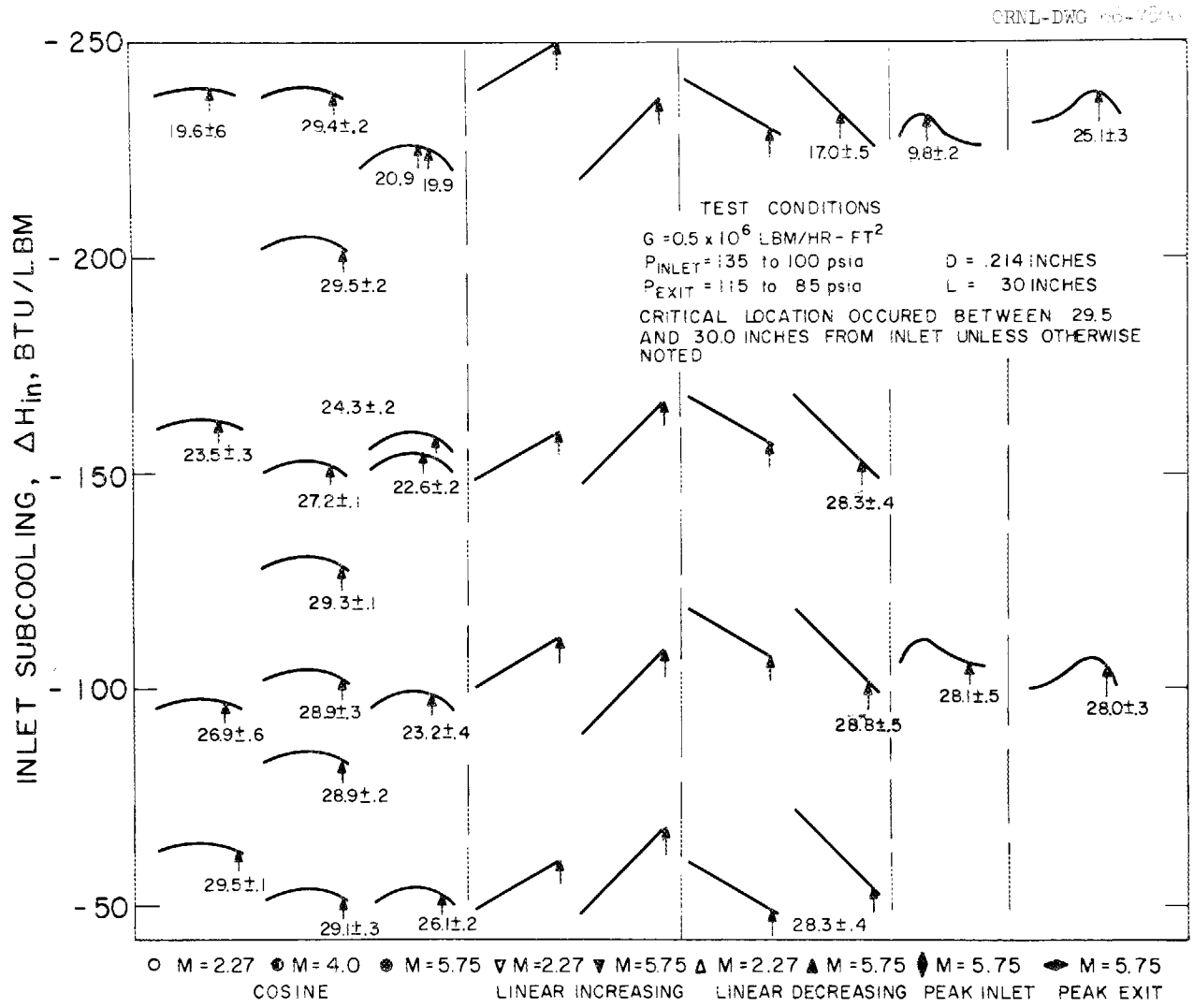

Fig. 34. Critical Locations (inches from Inlet) for the Nonuniform Axial Flux Distributions Investigated at $G = 0.5 \times 10^6$ $lb_m/hr \cdot ft^2$.

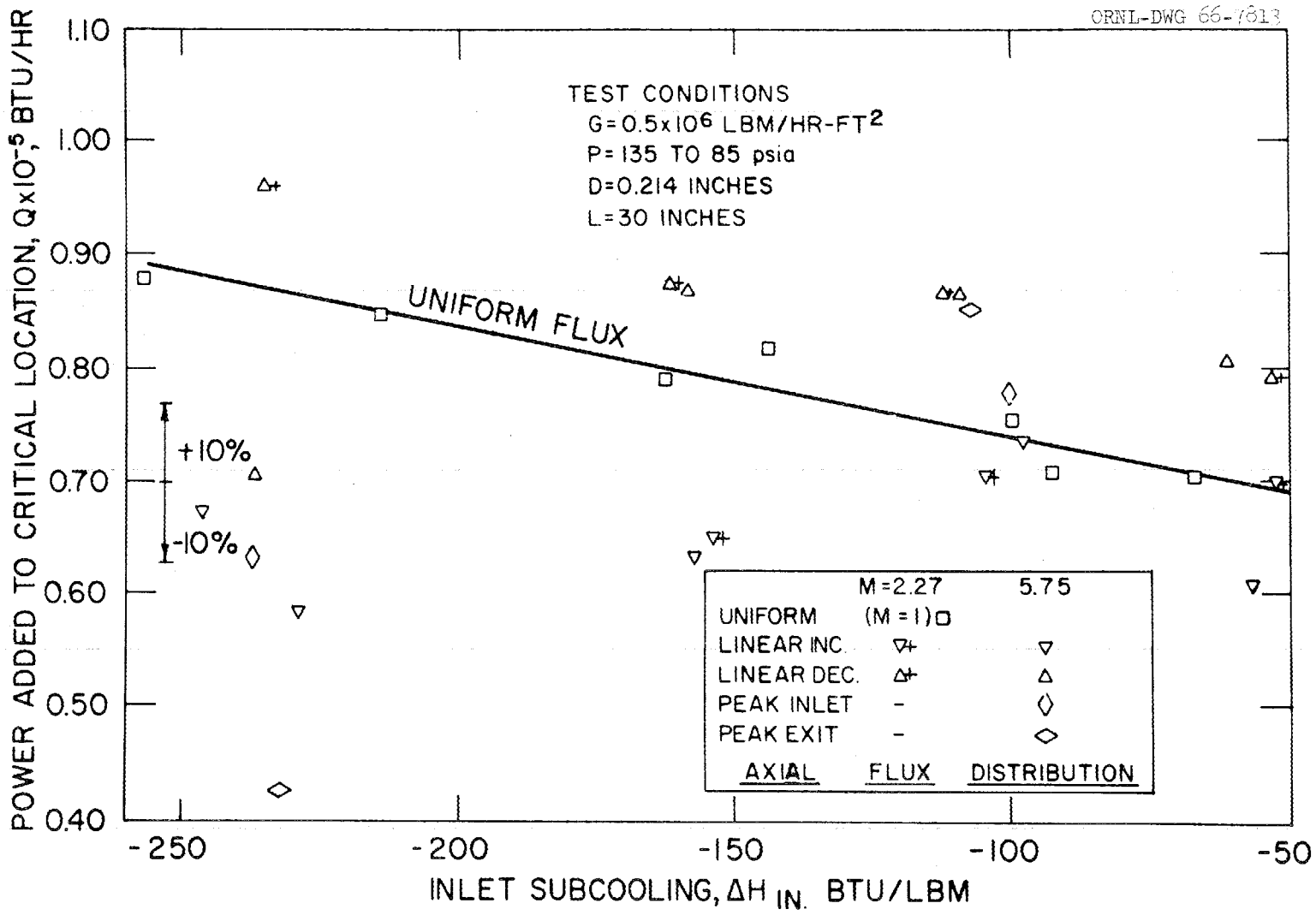

Fig. 35. Comparison of Test Results with the Integrated Condition Hypothesis.

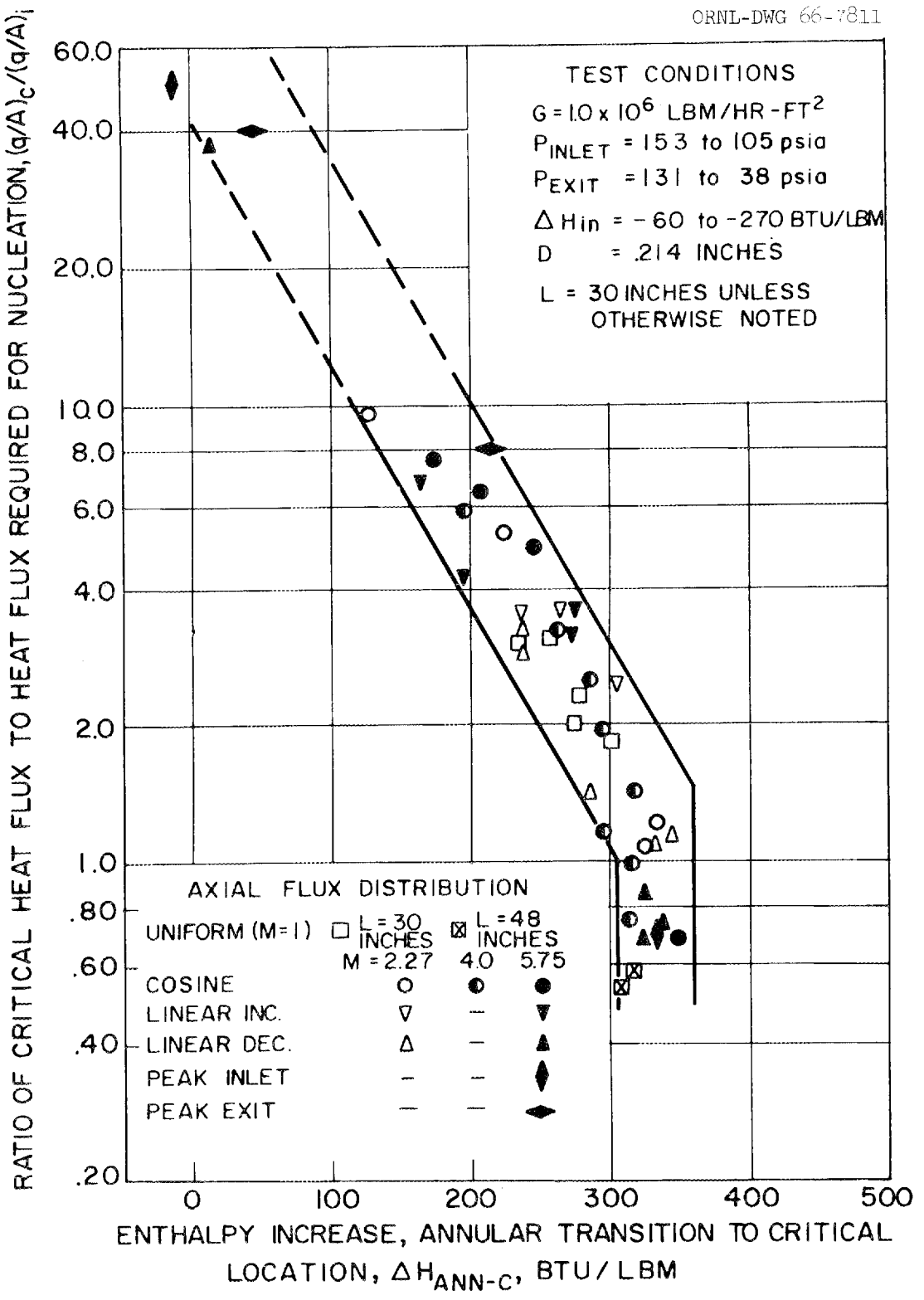

Fig. 36. Critical Flux Results at $G = 1.0 \times 10^6$ $lb_m/hr \cdot ft^2$.

nucleation intensity increases, the burnout occurs at lower local enthalpy, which is lower quality or lower vapor velocity.

The data for various heat flux distributions appear to come together on a single plot as shown in Fig. 36. Other plots show data for spikes plotted in the same way.

Figure 37 is a composite of data for three different flow rates. The spread of the data represents approximately a range of ±10% on the total power added to the tube at the burnout condition.

This has been a survey of some tidbits on boiling. We have gone back and revisited some old material; we have considered some recent studies. This last work I talked about (nonuniform heating) is to be presented next month in Chicago; and, naturally, I am most interested in your reaction to it. Thank you very much.

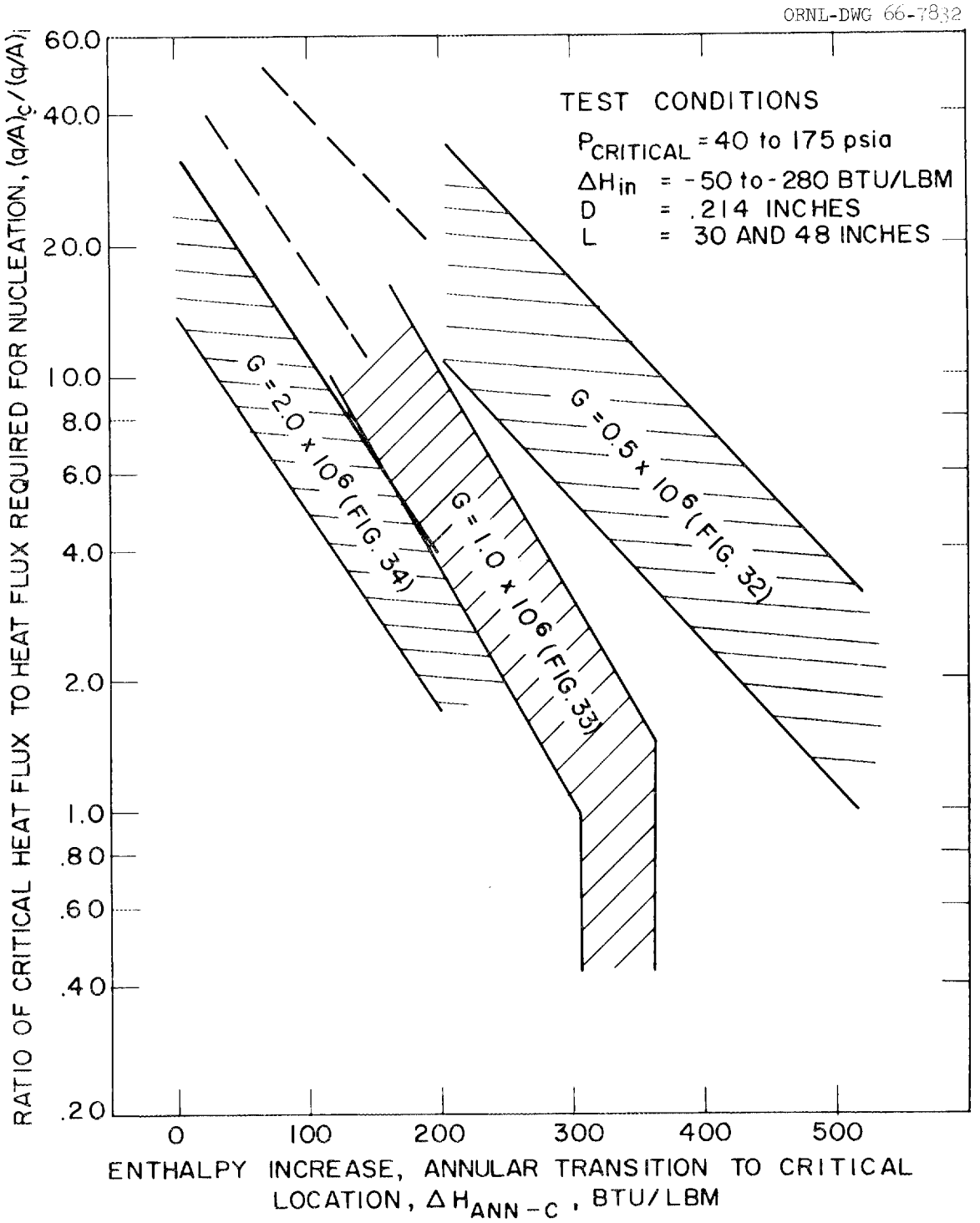

Fig. 37. Comparison of Critical Flux Results for the Range of Mass Velocities Investigated.

DISCUSSION

Chairman (H. W. Hoffman): Thank you. I think this has been more than a survey and that Professor Rohsenow has hit on the areas in boiling heat transfer which are really at the boundaries of our knowledge. Professor Rohsenow has indicated that he will continue this discussion in response to your questions.

R. N. Lyon (ORNL): Warren, I have really, I guess, three questions and one comment – in addition to the general comment that I think this was a very fine presentation of a very broad area; it is interesting to see what you are doing on a number of frontiers in boiling. In connection with subcooling, you mentioned initially that it appeared that the superheat when you had subcooled boiling was somewhat higher than in saturated boiling. I wondered whether you had looked at that from the same standpoint as you did with the cavity quenching or the nucleation-site quenching for the liquid metals. The subcooling might be quenching some of these nucleation sites, thus requiring additional superheat to reactivate them. This would mean on the average that a higher wall temperature would be required with reference to saturation.

W. M. Rohsenow: Yes, the temperature for nucleating a site for incipient boiling – the wall temperature for incipient boiling – with subcooled boiling would be higher because you get a higher heat flux for the same wall temperature. If we refer to Fig. 11, as subcooling in the liquid increases, the heat flux just prior to nucleation is higher; thus, the slope of the dashed curves is greater. Therefore, at the point of tangency, the wall superheat is greater. Therefore, what we would say is that for subcooled liquid, the point of incipient boiling would be at a greater wall temperature.

R. N. Lyon: I was thinking more of a dynamic effect in which, when the bubble jumps off, you have perhaps a sudden temporary quenching of the particular site due to the introduction of subcooled liquid. This, in turn, would require a higher temperature to reactivate the site.

W. M. Rohsenow: Well, it might be this type of thing that is occurring – particularly, at high subcooling.

R. N. Lyon: I have a related question with reference to the particular size of cavity which becomes activated in this temperature gradient near the wall. Would you not expect that pressure would change the location and the size of the nucleus which would first start boiling?

W. M. Rohsenow: Yes, the size and the required wall superheat would change, probably decrease, because the t_g^* curve of Fig. 11 is lower at higher pressures.

R. N. Lyon: You would find that different sites become activated at different pressures.

W. M. Rohsenow: That is correct.

R. N. Lyon: This means that in your little wire demonstration, if you could change the pressure, you would then not have the same spots.

W. M. Rohsenow: Absolutely correct.

R. N. Lyon: Finally, I have one other question, which I want to discuss at length with you later. I would point out, however, that when you use the Clapeyron equation in the bubble model, you are, of course, not quite accurate, since the Clapeyron equation requires that the pressures of the two media be the same.

W. M. Rohsenow: Yes, that is right.

R. N. Lyon: You didn't bring out – but I am sure you are aware – that actually you can use just the vapor volume rather than the differential volume and be correct.

J. F. Thorpe (University of Kentucky): I would like to know how you calculated the heat flux in the nonuniform test sections. They were electrically heated, were they not?

W. M. Rohsenow: They were electrically heated. From the wall thickness distributions, you can calculate the electrical resistance locally and thus determine I^2R locally. This then can be converted to q/A.

J. F. Thorpe: The reason I asked this question is that I have done some calculations of the axial distribution of heat flow in nonuniform electrically heated test sections. I find, for example, that if you include axial conduction and calculate the spike, the magnitude will be reduced by 50% – or 100% in some cases – over that calculated if axial conduction is neglected.

W. M. Rohsenow: I can't recall whether or not we took the axial conduction into account there.

J. F. Thorpe: The reason I asked is that, if you are using the local value of heat flux and it is off by 50%, it can make a big difference in your vertical scale where the curves intersect.

W. M. Rohsenow: Yes, it turns out on the spikes that the burnout always occurred slightly higher than these curves. They were the data contributing to the spread to the high side. This spread looks big on the q/A plot but is only 5% in total power to the tube. If Todreas had not taken this into account, the actual heat flux at the spike would be lower, thus reducing the spread in the data. This would help and definitely should be considered.

J. A. Edwards (North Carolina State University and ORNL): We have been doing some work in Oak Ridge with superheat and boiling of liquid metals, and we have found that the $2\sigma/r$ correlation is very good. We have made some small cavities 3 to 6 thousandths of an inch in diameter; and we were able to correlate our superheat by using the $2\sigma/r$ relationship. However, we would like to caution that at low saturation temperatures, the simplified equation that you had for predicting the superheat is far off. It is much better to go back and use the saturation pressure and temperature data and plug that into $2\sigma/r$ if you want to predict the superheat. A couple of other things that we have noticed – Dick Lyon suggested that maybe the fluctuations of the wall temperature that you had observed resulted from cool liquid flushing in around the cavity after it has nucleated and the vapor has gone on off. I would tend to support this idea, especially at low heat fluxes.

W. M. Rohsenow: You are correct. At low pressures, the more correct $2\sigma/r$ relation must be used. Recent work (ScD Thesis, Shai, January 1966) agrees that the colder liquid rushing to the cavity does quench the surface in boiling liquid metals. Shai's results predict this temperature variation and the bubble frequency rather well.

J. A. Edwards: Yes, but at a low heat flux we have observed that as soon as the cavity quenches, the temperature decreases in the neighborhood of the cavity and then immediately begins to rise again; it doesn't stay stable – it begins to rise again. In other words, the whole immediate surface area has to come up to this requirement so that the cavity can nucleate.

W. M. Rohsenow: At around incipient boiling then, you are expecting it to bounce?

J. A. Edwards: You notice fluctuations in pressure immediately. As soon as the temperature falls, you have an increase in pressure and then the pressure will drop off. However, if you continue to boil, this pressure would probably just stay stable. This is one of the things we have observed. Further, another thing we have seen – and I wonder if you have also observed this in your experiments with the sodium – is that a minimum superheat was approached as the saturation temperature increased. Since you had a fixed cavity size and can control this on your surface, your superheat should decrease as you increase the saturation temperature. In other words, we have noticed that, after a given saturated state is reached, we can no longer decrease the superheat required to maintain boiling. Have you observed this in any of your work?

W. M. Rohsenow: We only operated over a very limited pressure range; but in the range that we did operate, the superheat required did decrease with the pressure.

J. A. Edwards: We have observed this with potassium.

W. M. Rohsenow: Oh, you found that this reverses itself as you go up in pressure?

J. A. Edwards: No, it just reached a minimum value, in other words, given a controlled surface with controlled cavity sizes, after this reaches a certain saturation temperature, the equation would predict that the superheat would continue to drop. However, we found — or we think we found — that it reaches a minimum value. This suggests the mechanism of one cavity taking over from another cavity.

R. N. Lyon: This is just another comment on this business about the superheat increasing as the saturation temperature goes down. It must, in part, be due to the fact that at a given temperature there is a minimum bubble size which can be nucleated even at liquid pressures approaching zero. Thus, at low liquid pressure, the nucleating temperature may be almost independent of liquid pressure and, hence, of saturation temperature; and we would expect that the superheat, or difference between nucleating and saturation temperature, would decrease as you go up in pressure.

W. M. Rohsenow: Yes, this is what happens.

H. W. Hoffman: I would again thank Professor Rohsenow for his stimulating presentation this morning.

SPECIAL LECTURE IV

ATMOSPHERIC TURBULENCE AND DIFFUSION

Dr. F. A. Gifford, Director
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INTRODUCTION OF SPEAKER

H. W. Hoffman (ORNL), Chairman: We have an unusual opportunity this morning. It is not very often that one gets a chance to – I won't use the word attack – but at least to question a weatherman. Dr. Frank Gifford – at least to my knowledge – hasn't worked actively in weather prediction; but we still should be able to needle him a bit. Frank has a BS from New York University and an MS and PhD from Penn State in Meteorology. He is associated here in Oak Ridge with what was then the Weather Bureau and is now the Environmental Science Services Administration since 1949. This office has provided assistance to the AEC in the area of local micrometeorology. During the last few years, he has been Director of the ESSA Laboratory on Atmospheric Turbulence and Diffusion located here in Oak Ridge. He is also a member of the AEC Advisory Committee on Reactor Safety and Chairman of the Committee on Atomspheric Turbulence and Diffusion of the American Meteorological Society. An interesting point, which some of you who have interests in this area may wish to follow up, is that Frank has published a number of papers on Martian atmospheres; you probably can persuade him to give you the references. I tried to fit this topic into our meeting, but it just didn't work out. With this, I will turn the meeting over to Dr. Frank Gifford, who will describe for us the current status of work in atmospheric turbulence and diffusion.

ATMOSPHERIC TURBULENCE AND DIFFUSION

F. A. Gifford

Thank you very much. I should say, apropos of that introduction, that I once earned an honest living as a forecaster in the past; but I refuse to answer any questions on the subject today.

I take as my mandate today to describe to you the general nature of atmospheric turbulence, assuming that you are familiar with turbulent flows of different kinds (which seems evident from the speeches that I have heard yesterday and today) but that most of you probably don't know very much about the turbulence problem in the atmosphere. First of all, I think it is not too necessary to dwell on the importance of atmospheric turbulence; there are many obvious practical applications, such as air pollution, aviation, agriculture, the effect of winds on structures, and so on. To the meteorologist, in addition to these practical applications of atmospheric turbulence, there is the important fact that turbulence friction plays a very significant role in the general circulation of the atmosphere and, consequently, is of great interest to weather forecasters. To give you a little feeling for this aspect of turbulence, the winds in the atmosphere have at any one time a total kinetic energy which has been calculated to be the quivalent to 7×10^6 nominal atomic bombs. To put it another way, this is an amount of energy greater than the 100-year power output of the whole United States. If you turn this energy off, the atmosphere would run down in a little more than a week due to the retarding action of turbulent friction. The energy of the atmosphere which is consumed by turbulent friction has to be resupplied something like every nine to twelve days.

The sun's radiation, which is the source of energy that drives the atmosphere, is partially – about 35% – reflected by clouds, ice and snow, water, and the surface of the earth; the rest is absorbed, about 15% by clouds and moist air, a few percent by ozone, and about 4% by the surface. The incoming solar radiation, therefore, mostly gets through the atmosphere; the spectrum of solar radiation is strongly peaked in the visual region around 4500 angstroms. The solar energy gets to the earth mainly

over tropical oceans, because of course most of the energy is received in tropical latitudes; and if you will remember the map of the globe, there is not very much land between latitudes ±15 degrees. So the solar energy is used mainly to evaporate sea water. If you want to use the analogy of an atmospheric "heat engine," you could look at the tropical oceans as a fuel tank and the release of energy (combustion) into the atmosphere as taking place by condensation of the moisture which is given off by the tropical oceans. This gets done in pretty complicated ways which (fortunately for me) we don't have to discuss in detail this morning.

Water vapor collected by the atmosphere in the tropical oceans is first carried equatorward by the Trade Winds. The latent heat is released in equatorial latitudes by the formation of towering, cumulonimbus shower clouds. There then occur many complicated steps in conversion of this energy into the pressure gradient patterns that ultimately drive the atmosphere's wind systems. The tropical oceans are fairly uniform and evaporation into the air is fairly regular; and so the tropical oceans act like a high-inertia, thermal flywheel on the atmosphere. The tropical rains, from shower clouds, are quite irregular. Tropical cumulus clouds are relatively short lived and quite variable in number. It has been computed that 1500 to 2000 at a time, spread around the equatorial latitudes, would use up all the water vapor that is imported into the equatorial regions by the Trades. In cumulonimbus clouds the water vapor goes up very high into the atmosphere; this balances the local heat budget. In other words, it gives the surface of the earth a mechanism for getting rid of the excess heat that is received; and then it makes this available through condensation for atmospheric energy transfer away from the lower latitudes.

The tropics receive more solar energy at the surface than the poles do, but in the upper elevations of the atmosphere the temperature for one reason or another doesn't vary very much with latitude; and so, to maintain an equilibrium state, there has to be a net transport of energy at high levels toward the higher latitudes. I think that most of you probably are familiar with this general picture. About 10 to 20% of the total heat due to incoming solar radiation is transported poleward, across the 30-deg latitude parallels, by the oceans and by the atmosphere. Mechanical work is done against friction by carrying the working fluids from the

equatorial regions, which we could look at as the firebox (I am not quite sure what kind of engine this is), to the polar regions, which can be looked at as the condenser. The circulation of the working fluids results in the atmospheric winds, amounting to fluctuations of all sizes – high and low pressure centers in the middle latitudes, hurricanes, tornadoes, dust devils, and finally the small-scale turbulent fluctuations which essentially form the subject of this talk.

The atmosphere, let me say, is normally turbulent. This is true of most geophysical flows. I haven't studied the flow of galciers, but I presume they are not turbulent; this would be the only exception I can think of just offhand. Any region of the atmosphere can be a source of turbulent motion, through the shear of the mean winds that can arise in connection with these various wind circulations that I have described. Conversely, any region can act as a sink for turbulent motion because gravity, acting through thermal buoyancy, can consume energy. But most of the production and dissipation of turbulence in the atmosphere takes place in the lowest mile or so, a region known as the <u>planetary boundary layer</u>. This region is characterized by large, variable wind shears and buoyancy effects. Most of the study of turbulence I am going to describe to you takes place in this layer, both because it is the easiest to get to and it is of great importance in the energy budget of the atmosphere. I will talk, consequently, almost entirely about this layer and just note right now that there are some other places in the atmosphere where one commonly finds turbulence as I am sure all of you who have been in jet planes know. One occasionally runs into what is called clear air turbulence, quite high in the troposphere and in the lower stratosphere. This is again connected with the intense wind shear that is found in these regions. Also, there are regions in the lee of mountain chains such as the Rocky Mountains, at considerable elevations, which are characterized by lots of wind shear and turbulence.

The planetary boundary layer has some points in common with the boundary layers of technical flows that many of you are familiar with. It also has some very interesting differences which are peculiar to the atmosphere.

The Level of Atmospheric Turbulence

One of the most characteristic properties of atmospheric turbulence, and this is something that you will see if you went outside and measured atmospheric turbulence today – a nice, bright sunny day – over the ground surface, is that it has a very high-turbulence level. The turbulence level of the atmosphere is often of the order of 30 to 50%, sometimes higher, whereas in wind tunnels it is pretty difficult to achieve a turbulence level of more than a few percent. We can say that the fluctuations in atmospheric turbulence are commonly of the same order as the mean winds, particularly in the lower atmosphere (this is really the only place in which detailed measurements have been made).

The Problem of Averaging

Now I have spoken, already, of fluctuations and of a mean wind, and this implies that I know how to form some sort of average value of the wind in order to define the turbulence fluctuations and mean wind both. You might wonder, since the fluctuations can be about as big as the mean wind, how you decide how to go about doing this. In the atmosphere we behave as if we really could define the mean wind over some suitable space or time domain. However, it really isn't terribly obvious how to accomplish this.

Figure 1 shows an observed energy spectrum analysis of atmospheric turbulence, in which the longitudinal component of the turbulence is analyzed relative to the mean wind taken over a very long period. The energy spectral density is plotted against frequency (cycles per hour) with 1 cycle per hour being about the middle of the figure. This plot has been made on a logarithmic frequency scale because of the tremendous frequency range that is involved; and so, in order to preserve the useful property that area under the spectrum curve is proportional to energy, the log of frequency, n, is plotted against n F(n). Meteorologists call this the logarithmic spectral density. I am not exactly sure whether you use this convention in your own applications, but it is highly convenient for the meteorologist.

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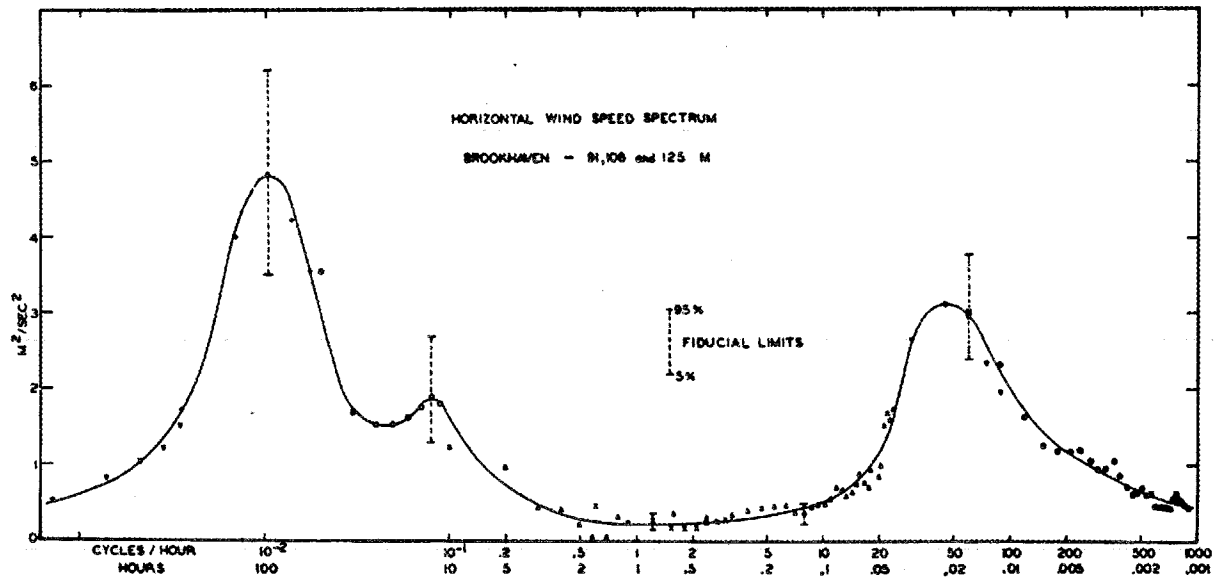

Fig. 1. Horizontal Wind-Speed Spectrum at Brookhaven National Laboratory at About 100-m Height.

There is a region of low-energy content around one cycle per hour, a peak on the high-frequency side of it and then several peaks on the low-frequency side. This "gap," or low-energy region, is characteristic of the atmosphere. It has been observed a number of times. It provides a reasonable basis for separating these fluctuations that we ordinarily regard as "weather," on the lower frequency side, from those that you could conveniently consider to be turbulent fluctuations, on the higher frequency side, the division being at a frequency of around one cycle per hour. You can easily see that those fluctuations which occur with a frequency of one per several days would correspond to the sort of the highs and lows that you see on the daily weather map. On the other hand, for this particular case, there is a peak frequency at about something like one cycle per minute or two, corresponding to the turbulent fluctuations. Of course steady conditions are required, with no frontal passage or any other such complications. If conditions are reasonably steady, then an averaging period of the order of an hour appears to separate the turbulence from the mean flow.

The Laminar Sublayer

To describe the nature of atmospheric turbulence briefly and concisely is a considerable problem, and it may simplify matters if I can refer to a few terms that will be quite familiar to you from other turbulence flows. One of these is the term "laminar sublayer"; is there a laminar sublayer in the atmosphere?

We can write a Reynolds number in the form of the product of a velocity, u_*, the familiar "friction velocity" of boundary-layer theory (i.e., the slope of the logarithmic velocity profile) times a length, z_o, the roughness length, which is a number that characterizes the geometric height of the ground roughness objects divided by the kinematic viscosity, ν, of air;

$$Re = u_* \, z_o / \nu \quad .$$

The question is, how big can this be and still permit a laminar layer to exist right next to the ground? Following results from the theory of boundary layers over flat plates, if Reynolds number defined in this way

equals or exceeds approximately 100, then the flow is aerodynamically rough; and we can ignore effects of molecular viscosity, such as a laminar sublayer. Since ν is on the order of 10^{-1} cm^2/sec in the atmosphere and u_* is about 100 cm/sec, the roughness corresponding to the upper limit above which a laminar sublayer does not exist in the atmosphere ($Re \gtrsim 100$) should be of the order of 1 mm. We are all living examples that roughness elements in the atmosphere can be bigger than 1 mm. Even if you don't take people to be the roughness elements, blades of grass, sand, dirt particles, and so on will be generally speaking no smaller than 1 mm and so, except for very special cases of flow in the atmosphere, we can start off by neglecting the laminar sublayer. The exception might be, for example, flow over smooth ice.

Equation of Mean Motion in the Lower Atmosphere

If we average the motion of the winds in the lower layers of the atmosphere, using Reynolds' scheme and dividing the mean and turbulent components up in the way that was just discussed, we will find the following equation of the horizontal mean motion in the planetary boundary layer:

$$\frac{d\vec{V}}{dt} = f\,\vec{V} \times \vec{k} - \frac{1}{\rho}\nabla p + \frac{1}{\rho}\frac{\partial\vec{\tau}}{\partial z} \quad .$$

The acceleration (per unit mass), $d\vec{V}/dt$, is balanced by: a Coriolis force, $f\,\vec{V} \times \vec{k}$, by the force arising from the pressure gradient, $(-1/\rho)\nabla p$, and by the frictional term, $(1/\rho)\partial\vec{\tau}/\partial z$; f is the usual meteorological symbol for the Coriolis parameter, the number that specifies the apparent force arising from the earth's rotation times the sine of latitude; $f = 2\,\Omega \sin\phi$. The mean air motion is essentially horizontal, so the mean vertical component of motion is negligible.

Now for a balanced flow with friction – which might apply just above the top of the planetary boundary layer, let us say – the frictional force and the acceleration term will equal zero. Then the Coriolis force and the pressure-gradient force are in balance and the wind blows parallel to the isobars with low pressure to the left. Within the planetary boundary layer, the addition of a frictional force has the effect of deflecting the wind to the left, toward low pressure. I am sure that you all have noticed this

on weather maps and perhaps wondered about the reason for it.

The Surface Layer

As a matter of observation, the pressure gradient and Coriolis forces in the free atmosphere are of the order of 10^{-1} dynes per gram, so the frictional forces must be of the same order for the forces to be in or close to balance. (Accelerations are usually small by comparison.) Since ρ is of the order 10^{-3} g/cm^3, we can write

$$\Delta\tau = 10^{-4}\ \Delta z,\ \text{dynes/cm}^2 \quad .$$

If we now define (having eliminated the possibility of a laminar sublayer) a layer through which the eddy frictional stress, τ, varies by no more than say 10%, then $10^{-1}\ \tau = 10^{-4}\ \Delta z$, or $\Delta z = 10^3\ \tau$. Since the magnitude of τ is typically of the order of 1 dyne/cm, we find that a layer Δz, which I can call h, has been defined which is roughly 10 to 100 m in depth, and through which the frictional stress is essentially constant. This layer is called by meteorologists the <u>surface layer</u>. Many of its properties are very similar to those of a wind tunnel boundary layer such as that over an infinite flat plate or at a wall. The flow is horizontally uniform but is strongly sheared in the vertical; and in the absence of heat flux through the surface layer, the velocity profile that results in this layer of approximately constant stress is logarithmic.

Those of you who have worked with logarithmic boundary layers in other applications may be interested in some of the numerical values that characterize such a layer in the atmosphere. The two quantities that characterize the logarithmic velocity profile are the friction velocity, which gives the slope, and the roughness length, which gives the zero intercept of the curve. Table 1 gives some typical values. For flow over ice or a very smooth mud flat, the roughness length has been observed to be around 10^{-3} cm and the friction velocity about 0.16 cm^2/sec. Going up the scale from smooth snow, and smooth sea, to lawns 5 cm high, or (like my lawn) 60 cm high, you go up a number of orders of magnitude to a roughness length of order 10 cm, and the friction velocity is approximately 10 times the values for very smooth surfaces.

Table 1. Typical Values of Parameters of the Wind Profile in the Surface Layer

[For $\overline{u}(2\ m) = 5\ m/sec$]

Type of Surface	z_o (cm)	u_* (m/sec)
Smooth mud flats, ice	0.001	0.16
Smooth snow	0.005	0.17
Smooth sea	0.02	0.21
Level desert	0.03	0.22
Snow surface, lawn to 1 cm high	0.1	0.27
Lawn, grass to 5 cm	1 – 2	0.43
Lawn, grass to 60 cm	4 – 9	0.60
Fully grown root crops	14	1.75

Effect of Heat Flux

Of course, the thing that makes the atmosphere, even in the surface layer, a complicated place from the standpoint of turbulence is the same thing that complicates life for most of you — the effect of heat flux. If we were to write out the complete equations of mean motion and heat flux for the surface layer, for the case of fully developed turbulence in the atmosphere, or if we wrote out the unaveraged equations of motion and heat flux for the case of free convection, the following parameters would turn out to be involved: u_*, the friction velocity; $g/\overline{T}$, where g is the gravitational acceleration and $\overline{T}$ the mean temperature; and $H/c_p\rho$, the heat flux divided by the product of the specific heat at constant pressure and the density. Molecular diffusivity and heat-conduction coefficients do not appear because these are essentially negligible in the flows that we are considering.

It turns out that from these parameters you can form a unique length, $L = -u_*^3\, c_p\rho\overline{T}/kgH$; k is von Karman's constant. L, this length, is essentially independent of height in the surface layer; you can look on it as a stability parameter. It is related to the better known Richardson's number, Ri, by $Ri = (u_*/k)(1/L\ dV/dz)$, where dV/dz is the velocity gradient. Richardson's number varies with height, but L is usually constant with height. L approaches infinity for the case of an adiabatic vertical temperature gradient in the lower atmosphere. It normally varies in the range $1\ m \lesssim |L| \lesssim 100 - 200\ m$, being positive for stable and negative for unstable vertical temperature gradients. These values can be calculated from wind profiles.

If then you use the friction velocity, u^*, and the length which involves the heat flux, L, as scaling parameters, an expression for the velocity profile can be formed by dimensional analysis. Consequently, for the general case of the surface layer with heat flux, the velocity profile has the following form:

$$\frac{d(V/u_*)}{d(z/L)} = \psi(z/L) \quad ,$$

which means that

$$V/u_* = \psi_1(z/L) - \psi_1(z_o/L) \quad .$$

Naturally, one of the things meteorologists are very active in is in making observations of this function, ψ_1, in the lower atmosphere. The profiles for stable and unstable conditions (i.e., with heat flux) are curved, becoming asymptotic to the logarithmic profile both for adiabatic conditions and at very low heights.

The Planetary Boundary Layer

The problem becomes a little more complicated when you have to work your way upward into the upper portion of the planetary boundary layer. Here the meteorologist makes the same assumption that everybody else does — namely, that the flux is proportional to the gradient; i.e.,

$$\frac{\tau}{\rho} = K_m \frac{\partial V}{\partial z} \quad ,$$

where K_m is a coefficient of eddy diffusivity of momentum.

In the surface layer, and in the adiabatic case, this means that

$$\frac{\tau}{\rho} = K_m \frac{v_*}{kz} = v_*^2 \quad ,$$

or

$$K_m = kz\, v_* \quad .$$

The coefficient of eddy momentum diffusivity, K_m, increases linearly with z in the surface layer. Now K_m is a number which in a sense characterizes the degree of turbulence that is present. You know from experience that, fortunately, the turbulence does not continue to increase as you go indefinitely higher above the ground, so this sort of behavior of K_m has to stop at some height. It, in fact, stops approximately where the variation of stress with height is such that you can no longer make the assumption that constant stress exists in the lower atmosphere; and the game in the study of the planetary boundary layer has been to make various assumptions

about the vertical variations of K_m and from that to derive the velocity profile.

The simplest assumption is that K_m is constant throughout the planetary boundary layer. A number of years ago (around the turn of the century) Ekman made a study based on this assumption, originally not in connection with his study of wind distribution in the lower atmosphere, but rather with the study of current distribution near the surface of the ocean which turns out to follow the same theory. If you assume that the K_m distribution is linear with height, as must necessarily be the case in the surface layer, then there have been various other ways that meteorologists have used to try to close this problem. One is by assuming that above the top of a surface layer the distribution of K_m is uniform. Another is to assume that the distribution from the top of the surface layer decreases to negligible values at the top of the planetary-boundary layer. Another set of assumptions is to assume a curve that is continuous in K_m in order to avoid the kink at the top of the surface layer. These are the sort of things that have been done to get at this problem.

Corresponding to, let's say, some of the more realistic assumptions about the vertical distribution of eddy momentum diffusivity, K_m, we find the horizontal mean wind vector distribution in the planetary boundary layer to be such that there are various angles, α, of the wind near the surface with the wind at the top of the planetary boundary layer. Some of the observed values for these parameters are shown in Table 2. Over

Table 2. Observed Values of Parameters of Planetary Boundary Layer

Parameter	Over Land	Over Oceans
α, deg	26	14
τ, dynes/cm^2	5	1
h (= height of surface layer), m	235	100
K_m (at h), cm^2/sec	144,700	28,000
H (= height of planetary boundary layer), m	1,000	500

land Ekman's assumption of a constant eddy diffusivity would have given an angle, α, of 45 deg, whereas the observations of this angle, and corresponding to this the more recent theories, give values about 20 to 30 deg over land, and about half or two-thirds of that over the ocean. The stress in the constant stress layer at the bottom of the planetary boundary layer runs about 5 dynes/cm^2 over the land, and over sea about 1. The height of the surface layer (the constant stress layer) is a couple of hundred meters – well, that is an extreme case representing a rather considerably developed depth of the surface layer. The value of the eddy diffusivity at the top of the surface layer runs in the order of 10^5 cm^2/sec over land and a couple of times 10^4 cm^2/sec over sea; and the total height of the planetary boundary layer is over land of the order 1000 to 2000 meters, over the ocean somewhat less.

These are the observed facts about the average structure of the planetary boundary layer, the region of most of the turbulence in the atmosphere. But, of course, we are also interested in the details of the turbulence structure and that I will have you consider next.

Structure of Turbulence in the Planetary Boundary Layer

L. F. Richardson, just prior to World War I, sort of took the equations of dynamic meteorology out of mothballs, dusted them off, and attempted to make with them a direct calculation of the pressure field in the atmosphere for the purpose of weather prediction. In doing this, he was forced to consider a number of highly interesting side issues. For example, the problem (and this struck me quite forcibly yesterday during Professor Churchill's talk) of numerical approximation of the dynamical equations was considered by Richardson; and to this day workers in this field still speak of the Richardson-Liebman process, a finite differencing scheme that is still in use. Another one of the questions more or less subsidiary to his main problem that Richardson had to attack in connection with his enterprise was, how do you treat the turbulence? Richardson went ahead in a characteristic, direct way; he assembled all of the observations of K_m that he could find or infer from various sources in the literature – these appear in Table 3. They include: a value of the order of 10^{-1} cm^2/sec for molecular diffusivity, a value of the order

Table 3. Values of Horizontal Eddy Diffusivities, K_m, at Various Scales, ℓ, after Richardson (1926)

Scale, ℓ (cm)	K (cm^2/sec)	Source of Data
5×10^{-2}	1.7×10^{-1}	Molecular diffusion
1.5×10^{3}	3.2×10^{3}	Low-level wind shear
1.4×10^{4}	1.2×10^{5}	Low-level wind shear
5×10^{4}	6×10^{4}	Pilot ballons, 100 to 800 m
2×10^{6}	10^{8}	Manned and unmanned ballons
5×10^{6}	5×10^{8}	Volcanic ash
10^{8}	10^{11}	Cyclonic storms

of 10^3 and 10^4 inferred from winds in the surface layer (this particular observation as I remember came from the Eiffel Tower which had wind profile observations from several anemometer levels), and the values computed from pilot balloons, balloons that weather observers send up to measure the wind at high elevations; and going up still higher on the scale, values of K_m computed from manned and unmanned balloons. He was able then to infer from all these observations, and from inferences of K_m from volcanic ash deposits and ultimately from weather cyclones – that is to say low pressure areas on the weather map – a range of length scales from 10^{-2} to 10^8 cm. For these, he assembled eddy diffusivity observations or inferences ranging from 10^{-1} to 10^{11} cm^2/sec. It seemed, when he plotted them, that there was a considerable degree of organization to these data; and Richardson deduced the equation

$$K_m = (\text{constant})\ \ell^{4/3} \quad .$$

Richardson concluded that what is going on in the atmosphere is an energy transfer process. You see, the eddy diffusivity seems to depend on the scale of the process that is involved. Perhaps you are all familiar with his rhyme. It is, as far as I know, the only time a fundamental law of nature has ever been put into doggerel verse.

> "Great whirls form smaller whirls, that feed on their velocity,
> Small whirls form smaller whirls and so on, to viscosity."

In 1941 there was a more formal explanation for this given by Obukhov. If in fact the energy for atmospheric turbulent motions is introduced at a very large scale and is abstracted by molecular dissipation processes at a very small scale, then in between there will be a scale that depends only for its properties on the rate of eddy energy dissipation, ϵ, cm^2/sec^3. Obukhov suggested that this was the case and therefore that on dimensional grounds, $K_m \propto \epsilon^{1/3}\ \ell^{4/3}$.

Now, of course, there is a good deal more to this question than I am stating here; for example, the inertial range in the atmosphere is really not thought to extend from the very small dissipation scale to the scale of large weather cyclones. One of the reasons is that atmospheric motions are restricted in the vertical, whereas horizontally they are comparatively

unrestricted; and one thinks of the inertial range of eddy sizes as being a range in which the properties of the eddies don't reflect any particular such geometrical restriction. It has been suggested that an upper limit for the size of inertial eddies in the atmosphere is about 100 meters. This, even so, means that the inertial subrange in the atmosphere is really quite large, compared with what can be achieved in any technical flow that I know about; and therefore, there is a lot of interest in the theoretical predictions that can be made as a result. If the structure of turbulence in the atmosphere (or anywhere) is controlled only by eddy energy dissipation, and not by the particular way energy is introduced into the atmosphere, or removed from the atmosphere — introduced by the large-scale circulations in the atmosphere and removed from the atmosphere by molecular scale dissipation — then a number of particularly simple predictions can be made. Namely, (1) the wave number energy spectrum is proportional to $k^{-5/3}$; (2) an eddy structural function [i.e., essentially the correlation function between relative turbulence velocities, at points separated in time or distance] is proportional to $k^{2/3}$; and (3) the constants of proportionality in these expressions include $\epsilon^{2/3}$.

These are quite remarkable predictions, and they probably couldn't be verified any place else except in a flow that has a large inertial subrange. In general, the predictions of this theory have been verified in the atmosphere; in particular, the $k^{-5/3}$ law for the high frequency range of the eddy energy spectrum has been found to hold.

It is, I think, interesting about atmospheric turbulence that it has the property that typical measuring probes — say cup anemometers, bivanes (which are vanes that follow the direction of the wind in two dimensions), acoustic anemometers, the various devices that have been tried to measure the fluctuations in wind in the atmosphere (I must say that hot wires are not used much) — all have the property that they are much smaller than the typical length scale of eddies, certainly of the large, buoyant eddies, in the lower atmosphere. It might not seem that a device as clumsy as a wind vane perhaps 1 or 2 ft long would give you a very precise indication of the turbulence; but the fact is that when these are made carefully, they are capable of giving very precise measurements of the detailed structure of eddies.

Also, there is the possibility in atmospheric turbulence to make measurements in a Lagrangian frame reference — namely, to float balloons or other tracers and to watch their motions, either by optical, radio, or radar measuring devices. The atmosphere is a comparatively simple place in which to measure fluctuations of the wind in the Lagrangian framework, which is very important because theories of diffusion are usually constructed in this framework. On the other hand, by far most measurements of the details of turbulence spectra have been made by fixed wind measuring devices of various kins. In Fig. 2 is an example of a simultaneous measurement of turbulence spectra (in this case vertical velocity spectra obtained by means of a fixed anemometer mounted on a tower at Brookhaven National Laboratory) and the simultaneous fluctuations of a floating balloon of zero buoyancy. I want to use this figure to illustrate several points. For one thing, as compared with the balloon (or Lagrangian spectrum), the tower or Eulerian spectrum is displaced toward higher frequencies. Based on this work, if you measure the Eulerian spectrum and divide the frequencies by something of the order of 2 to 10 (depending on the stability of the lower atmosphere), you will get some kind of a rough representation of what the Lagrangian spectrum probably looks like. This is important if you want to use fixed point, Eulerian turbulence fluctuation measurements to predict diffusion.

Another interesting property is this: this particular spectrum pair was taken from a series of measurements that started about 10:00 in the morning and ended about noontime. As the incoming solar radiation became greater and the ground heated up, in general, what happened was that the peak of this spectrum, which in this particular example was at perhaps 1 cycle per minute, decreased from an even higher frequency to about 10 cycles per hour. This apparently represents the increasing amount of buoyant, convective eddies that accompanies the increase of heat input to the ground.

Also, if you vary the height at which you measure these spectra, you will find a variation in the location of the peak toward lower frequency for greater heights. This is due to the fact that the higher you go the more room there is for large eddies to exist. They are less limited by proximity to the ground.

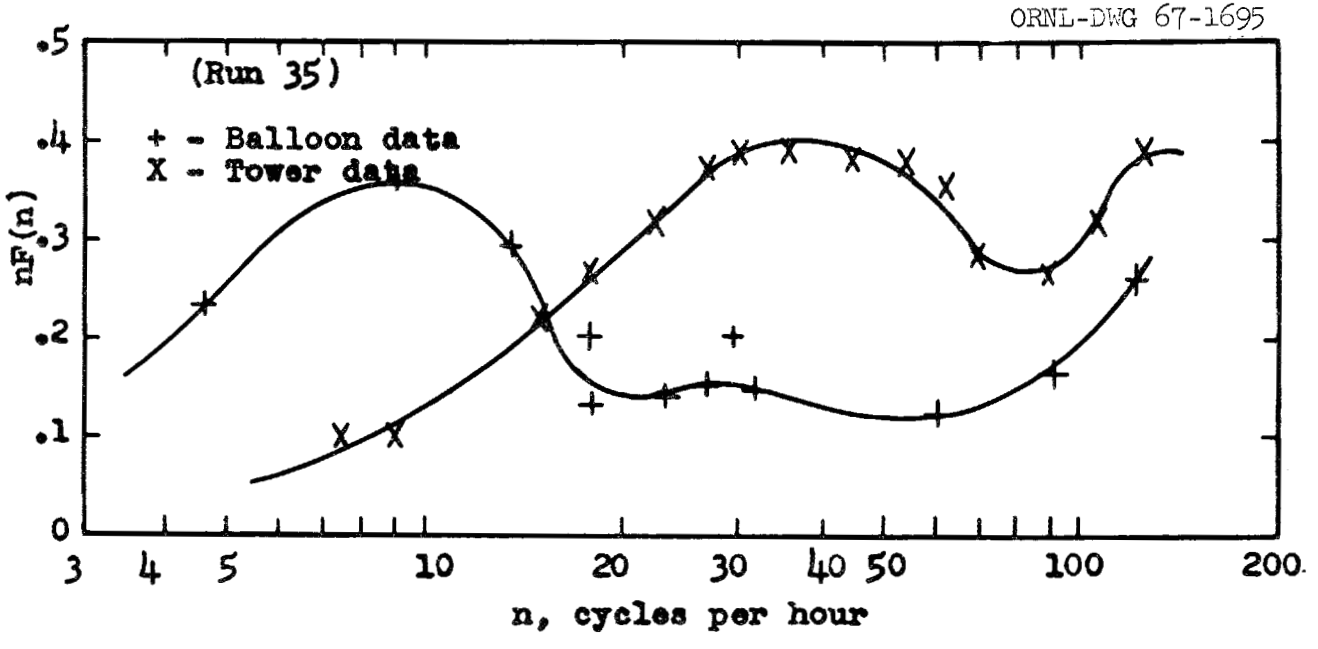

Fig. 2. Balloon (Lagrangian) and Tower (Eulerian) Vertical Velocity Energy Spectra.

Scales of Atmospheric Turbulence

I won't try to cover too much more. I think it is characteristic of meteorologists who have gone into the study of turbulence that they get sort of hooked on it; and we are somewhat, I think, like the previous speaker in that we could go on for a very long time discussing our favorite subject. But I would like to present some of the properties of atmospheric turbulence in terms that may be familiar particularly to those of you who work with turbulence, so that you can compare the turbulence in your own experience with that which occurs in the atmosphere; these are the various turbulence "scales." The Taylor microscale of turbulence – that is, the zero intercept of the vertex osculating parabola of the velocity correlation – which is sometimes thought of as measuring the size of "small" eddies, runs about 10 cm for the atmosphere. The Kolmogoroff microscale of turbulence, which probably represents the lower limit of the scale of eddy sizes below which molecular dissipation is all important and above which dissipation is of increasingly less importance, has been estimated to be around a millimeter in the atmosphere. The integral scale of turbulence, a length representing essentially the integral of the velocity correlation curve, equals about 10 meters under stable conditions and about 200 meters under unstable conditions, in the atmosphere, in terms of Eulerian-space type measurements. To get the corresponding Lagrangian integral scale, one can multiply these by 3 or 4 and convert to seconds by assuming distance is equal to the mean wind times time.

Summary

Now, let me summarize the character and properties of atmospheric atmosphere. Let me tell you what I would hope that you would get out of this talk and perhaps remember a little. The atmosphere is characterized by a very high-turbulence level; approximately 50% turbulence levels are not uncommon. These are much higher than are found in wind tunnels. The Reynolds number in the atmosphere is difficult to specify; but if we use, let's say, height above the ground as the length parameter in Reynolds number, the atmosphere is characterized by a very high Reynolds number. The assumption for theoretical purposes is that flows are horizontally homogeneous in their turbulence properties. Actually, of course, there are variations due to variable underlying surface conditions, and this is a problem that hasn't been studied very much. There is no laminar sub-layer that we need to worry about. The surface layer, or approximately constant stress layer, extends to 10 to 100 meters above the ground. It is characterized by values of the friction velocity, u_*, of roughly 15% or so of the mean wind at, let's say, 10 meters. The roughness length, z_o, characterizing the velocity profile can be highly variable, depending on the kind of underlying surface, being anywhere from 10^{-3} cm to perhaps 10 cm; over cities, maybe there are even higher values of several meters. Above the surface layer, the mean wind veers and increases, turning toward higher pressure with increasing height and attaining the free (i.e., non-turbulent) atmosphere value at elevations normally about several thousand feet in the atmosphere.

Atmospheric turbulence is characterized by a quite large inertial subrange of eddy sizes; the dissipation scale is on the order of millimeters; and the integral scale, that is the upper end of this inertial range, must be a least 100 meters under most conditions. The effect of heat flux is the following in the atmosphere. On the one hand, when the vertical gradient of temperature is quite stable — i.e., when temperature increases with height — the turbulence is strongly suppressed. Consequently, most of what I have said really applies primarily during the daytime. At night, you would find a highly different situation because of the stabilization of the lower air layers by the ground cooling; this

situation hasn't been studied as much as the daytime case. It is quite clear that the daytime kind of turbulence is strongly inhibited at night. On the other hand, the converse of this situation – heating by the ground and resulting instability during the daytime – leads to an augmentation of lower frequencies of the eddy spectrum, due to thermal buoyancy.

I mentioned that Reynolds number is very high and this simply says that the inertial forces swamp the viscous effects in the atmosphere. A similar remark can be made about the Peclet number; i.e., that bulk thermal convection strongly dominates the molecular temperature conduction. Richardson's number, which is the ratio of turbulent energy production by shear to consumption by buoyancy, is variable depending upon the nature of the stability in the lower atmosphere.

There are some attractive outstanding problems that we would like to study. We are very interested in the effect of heat flux on the mean structure of the planetary boundary layer. This is now getting to be fairly well understood for the surface layer. It is not at all well understood for the planetary boundary layer as a whole. We would also like to understand the effect on the mean structure of the planetary boundary layer of the atmosphere of accelerated flows of various kinds; for example, those due to variable roughness lengths, to gravity flows, to sea and land breezes, and so on. We would like next to start studying in some detail some of these complicating effects that we haven't really done too much with so far. I think there also is a very attractive field in atmospheric turbulence of making detailed measurements of turbulent eddy structures. The only turbulent flow I know of where it is at all convenient to get right inside you might say, to speak loosely, the turbulent eddies and to measure their detailed properties – in which, in other words, you are not forced to make the assumption that all eddies are like sine waves, or something – is atmospheric turbulence.

And finally, the same tantalizing possibility is dancing in front of us as was suggested by Professor Churchill yesterday – that of numerical solutions of turbulent flows. In a sense this has already been done in the atmosphere. Namely, if you consider the whole atmosphere with all of its Highs and Lows and so on, to be an example of a turbulent flow, and solve

the equations of motion by means of electronic computers (but with very great simplifications owing to the finite number of grid points that are available on the current generation of computers), it is possible and is being done everyday to get numerical solutions to the equations of motion for the atmosphere — upon the basis of which weather predictions are actually made. Now this is similar to what we are talking about, except that the turbulence we are worrying about is operating on a much smaller scale. So, the numerical solution of this problem will probably have to wait for another generation of computers with very large capacity; but it is something that, when these become available, should certainly be of great interest.

Thank you very much; I will be glad to try to answer any questions except what the weather is going to be tomorrow.

DISCUSSION

Chairman (H. W. Hoffman): Thank you, Frank, for a most interesting lecture. Do we have any questions now?

R. N. Lyon (ORNL): Frank, I infer from what you say that there is a net pattern of wind; for example, trade winds near the equator, the north to south winds near the surface, and the reverse winds going back toward the North Pole in the upper atmosphere. These trade winds, I guess, go from east to west; i.e., the equatorial winds would be from east to west — does this mean then you are making calculations and trying to relate what is going on on the Earth to a sort of infinite flat plate where you are studying the boundary layer and that you can take a uniform velocity on the average in that region and then study the turbulence or something produced by that kind of wind?

F. A. Gifford: So far, yes. The way the turbulence for the most part has been studied is by assuming that the driving force is a constant wind and, in fact, an even more special kind of a constant wind which is called a geostrophic wind, which says in addition to not being accelereated the flow is also not affected by the curvature of the mean wind. Now the reason why this sort of simplification on the upper boundary condition is useful in addition to, at the present state of development, appearing to be necessary, is because the actual curvatures and the actual effects of other kinds of large-scale motions in general can be neglected and still leave some interesting results in terms of the turbulence. On the other hand, it is quite clear that there are problems that we normally think of as being controlled by such idealized solutions — for example, the diffusion of radioactive material away from a reactor which is having an accident — which in practice are affected by the sort of complications that you are implying here. One can give an example, the Windscale accident, during which a front passed, and the mean wind changed 90 degrees. We haven't erected theories for this kind of complication to any extent yet; and I am suggesting this is the next step that we should look at.

R. N. Lyon: I had another question. In connection with the change in wind direction which is presumably caused by the interaction of the friction near the earth and the Coriolis force, that kind of twist, does that affect all the rules on how turbulence would develop? You are having not only a change in velocity magnitude but also a change in the actual direction.

F. A. Gifford: Yes, I have to say again, unless you have a boundary layer where the constant stress assumption is valid, the amount of theoretical work is rather meager. But it appears that the Coriolis force acts actually as a stabilizing influence on the turbulence. This is the statement that can be made about that; but little work has been done on this point.

D. G. Thomas (ORNL): Frank, in the drawing you had with the tower experiment and ballon experiment you mentioned that the experiments were done between 10 and noon and that the thermal forces were causing the peak in the tower experiment to shift to smaller wave numbers. Were measurements made with ballons so that you know whether that peak stayed at the same position or did it decrease?

F. A. Gifford: Both measurements were made; and the character of these was that both the peaks shifted toward low frequencies, so that what you find in the fixed-point measurements were also reflected in the ballon measurements. There was an airplane that was flying past, to try to get the spatial pattern, but unfortunately we only have one example of that spectrum that was made simultaneously with the other two kinds.

H. W. Hoffman (ORNL): I was wondering what has been done in the way of looking at the turbulence structure over cities and things like that where you have buildings which are heated on one side and unheated on the other and, hence, have strong natural-convection effects.

F. A. Gifford: The answer is, very little. This is a subject that we are studying and obviously is a subject that has considerable degrees of practical application in terms of air pollution and also is quite relevant to the reactor field now in which the push is to get reactors closer and closer to cities. The answer is that very little has been

done. There are only one or two series of measurements, for example, that would give you the roughness length and the friction velocity over a city, compared with hundreds that exist for open country. This is something that a great deal more should be done about. The general character that we expect has two aspects — one is that the roughness elements are much greater; the other is that the city has what is called the "heat-island effect." The city, being composed largely of concrete and asphalt and so on, absorbs heat during the day and emits it during the night; and so the temperature variation over a city does not show the wide swing from day to night that exists over a countryside. Generally, I suspect that regarding the mean properties, flow over a city may even in some ways be simpler than flows over the country, but this has not yet been studied as much as it should.

L. W. Crawford (Tennessee Technological University): I don't quite understand what the significance of the ballon and tower experiment is. Just what does it mean that the frequencies are shifted to low values?

F. A. Gifford: This is only the significance that is attributed to the frame of reference following an individual fluid particle, which you suppose that the ballon does (of course, it doesn't but you do the best that you can); this is the Lagrangian frame. The Eulerian frame is what the tower measurement gives you. The reason why this is important is that it is very difficult to make Lagrangian observations — i.e., observations of motions of a "particle of air" — while it is comparatively simple to make the fixed-point tower observations. The statistical theory of diffusion, G. I. Taylor's theory and all the ramifications of it by other people, depends on the idea of particle-attached, or Lagrangian specification of turbulence; that is, following the motion of particles. So what you are trying to do is to estimate, by measurements made at a fixed point, what the statistics are of the distribution of particles at some specified distance or time from an initial position. The importance of this perhaps would have been clearer if I had condensed the first part of my remarks and then talked about diffusion a little more; but this is the significance as I understand it, anyway.

ABSTRACTS OF PROGRESS REPORTS

STUDIES IN RADIATION GAS DYNAMICS

T. S. Chang
Virginia Polytechnic Institute

This progress report describes some of the research efforts on "Radiation Gas Dynamics" which are currently undertaken at Virginia Polytechnic Institute.

Approximate models describing the flow of real gases under the influence of radiative heat transfer will be presented. The effects of nonequilibrium dissociation, collision, and thermal radiation on the structure of shock waves will also be considered.

FREE CONVECTION HEAT TRANSFER TO AIR BETWEEN NARROW CHANNELS

K. G. Soderstrom and C. E. Reoyo-Sánchez
University of Puerto Rico

Mathematical models have been analyzed of the heat transfer from the fuel elements of the PRNC reactor resulting from a loss of water. An experimental model has been built for the purpose of measuring the free convection heat transfer to air between the fuel plates. This model uses two stainless steel plates of the exact size and spacing of the plates in the PRNC reactor and power generation is simulated by electric heaters. The measured variables are used to calculate the heat transfer and convection coefficient from the plates to the air in the narrow channel as a function of fuel plate temperature.

EXPERIMENTS ON THE STABILIZATION OF ROTATING FLOWS BY INTENSE MAGNETIC FIELDS*

J. J. Keyes, Jr.
Oak Ridge National Laboratory

Confined, vortex-type flows generated by tangential impingement of gas on the interior surface of a tube are of current interest for certain advanced energy conversion applications including the gas-core (or gas-fueled) nuclear reactor. One gas-core reactor concept employs a balance between centrifugal force and diffusional force to affect continuous separation of the high molecular weight fuel from a low molecular weight coolant. The coolant absorbs heat primarily by direct molecular exchange and can, in principle, be heated to temperatures sufficient to sustain a high level of ionization. The vortex reactor might be integrally coupled with a vortex magnetohydrodynamic (MHD) generator which extracts electrical energy from the rotating plasma by interaction with an axial magnetic field. This concept is discussed briefly.

These applications of vortex flow may be feasible only if the level of turbulence is very low, particularly in the fuel zone where eddy mixing limits the separation process. Unfortunately, experiments have revealed that the flow is unstable at values of Reynolds modulus (based on the tangential velocity) much below the range of interest. On the other hand, since the gas is an electrical conductor (plasma) at least in the fuel zone, hydromagnetic stabilization may be possible.

This presentation describes results of exploratory flow visualization studies employing motion picture photography. These studies reveal the strong stabilizing influence of an intense axial magnetic field (up to 75 kilogauss) on vortex flow of an electrolytic conductor (concentrated aqueous ammonium chloride solution at 95°C) generated by two-dimensional, tangential wall jets. Using a 4-in.-diameter vortex tube having two feed jets and a single exit orifice at the center of one end of the tube, the tangential Reynolds modulus at transition to instability was increased from 500 with no magnetic field to 7600 with the 75 kilogauss field (corresponding Hartmann modulus based on tube radius of 172). Even when the tangential Reynolds modulus is above the critical value for complete stabilization, the magnetic field is nevertheless effective in stabilizing the interior of the vortex flow.

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*Research sponsored by the U. S. Atomic Energy Commission under contract with the Union Carbide Corporation.

TURBULENT TRANSPORT IN A WALL-JET FLOW

F. N. Peebles and J. O. Nelson
The University of Tennessee

Time-average temperature and velocity profiles were measured in a two-dimensional plane turbulent wall-jet. The jet was formed by expelling preheated air through a nozzle 0.75 in. thick onto a rectangular flat plate 18 in. wide by 84 in. long, bounded by sideboards. Temperature profiles were measured using a differential thermocouple system. An impact pressure tube and micromanometer were used to measure velocity profiles.

Transverse temperature and velocity traverses were made at 8 in. intervals along the longitudinal centerline of the jet at distances ranging from 8 in. to 64 in. downstream from the nozzle exit. The nozzle velocity and the temperature difference between the nozzle exit and room air were maintained constant during these traverses at 87.7 ft/sec and 32.1°F, respectively. An additional traverse was made at 32 in. downstream from the nozzle with the exit values maintained at 58.9 ft/sec and 24.1°F.

Temperature profiles at each downstream location were plotted as the difference between jet and room temperatures versus distance normal to the wall. Velocity profiles were obtained in an analogous manner. The self-preserving nature of the flow was demonstrated by the universal temperature function which resulted when temperature profiles were normalized by dividing local temperature differences by the maximum difference for each profile and by dividing transverse position by the distance from the wall at which the local temperature differences was half the maximum value. When treated in a similar manner, the velocity profiles produced a universal velocity function. Normalized temperature and velocity data for the profiles measured with lower nozzle exit values coincided with these universal functions confirming the similarity of both heat transport and momentum transport in the wall jet. All scaling factors were found to be simple functions of the downstream distance from the nozzle exit.

The distribution of the dimensionless eddy thermal diffusivity, E_t, was calculated from a similarity solution of the thermal energy equation for the observed universal temperature profile function. The dimensionless diffusivity was almost linear in the region near the wall and approached a value of 0.037 in the outer region of the jet. The values of the eddy thermal diffusivity agreed favorably with similar measurements of the eddy diffusivity of mass obtained from experimental studies of turbulent mixing of 0.1 per cent helium-air mixtures with ambient air in the same wall-jet apparatus.

A STUDY OF PRESSURE LOSSES IN TUBING AND FITTINGS

C. W. Bouchillon and C. T. Carley
Mississippi State University

The objectives of the research study are to develop means of predicting the performance of tubing and fitting systems for steady state and transient fluid flows. One type of fitting or hose which has not been included in the classical prediction techniques is the corrugated flexible metal hose.

One of the results which has been obtained to date is the measurement of the pressure losses in flexible metal corrugated hoses of various geometrical configuration and the development of a correlation equation which appears to predict the pressure losses reasonably well for air.

Another result is a computer program for prediction of the performance of a generalized system composed of various types of components in various combinations for steady state and slow transient, i.e., quasi-steady state, conditions which may be applied to systems of tubing and fittings.

Work is continuing in the development of analyses for the improvement of the prediction equation for the corrugated flexible metal hose as a function of the geometry of the hose convolutions. The prediction of transient phenomena in tubing and fitting systems remains as an area of investigation which requires further study in order to establish a technique for obtaining satisfactory approximate solutions to the resultant system of partial differential equations.

AN ARC HEATED WIND TUNNEL INVESTIGATION OF THE EFFECT OF HEAT-TRANSFER RATE, LOW DENSITY, AND ORIFICE SIZE ON MEASURED STATIC PRESSURES

R. W. Guy
NASA – Langley Research Center

An experimental investigation of the effects of orifice diameter and heat transfer rate on static pressure measurements has been made in the regime between continuum and free molecule flow. This investigation is part of a continuing program to accurately measure heat transfer and pressures in a low-density, hypervelocity stream. Preliminary pressure measurements on slender cones fell considerably below theory and a search for the cause of these low pressures indicated that orifice size, density level, and heat-transfer rate could cause the measured pressures to be low.

In order to investigate this orifice effect, heat-transfer and pressure measurements were obtained at a single axial location on a slightly blunted, slender cone for a variation in orifice diameter and test conditions. The experimental results for the orifice effect are general when presented in terms of parameters from the semi-emperical theory for the orifice effect as originally derived by Potter, Kinslow, and Boylan.

The tests were conducted with both air and nitrogen as the test media in the Langley 1-ft (0.305-meter) hypersonic arc tunnel at Mach numbers from 12 to 14 and enthalpy levels from 3×10^3 to 5×10^6 J/kg. The free stream unit Reynolds number varied from about 4×10^4 to 9×10^4 per meter. The results extend the work of Potter et al., to a wider range of flow conditions, since the measured heat-transfer rates were higher at the density levels involved, than previous work. The measured heat-transfer rates are higher than predicted by theory even with an approximate correction to the theory for the effects of transverse curvature and induced pressures. More work is required to determine the cause of the high heat-transfer rates. All pressure data obtained with heat transfer to the model showed a significant decrease in measured pressure with a decrease in orifice diameter.

It is shown that, in general, there is good agreement between Potter's theory for the orifice effect on pressure measurements and the present experimental data. The results of this experiment indicate that in many types of tests it may prove more practical to use a conveniently sized orifice and correct the data, rather than to try to eliminate the effect by choice of orifice size. The need to know heat-transfer rate in order to correct the pressure data must also be emphasized.

REFLECTANCE STUDIES FOR FILM-COVERED WATER SURFACES

J. T. Beard
University of Virginia

and

J. A. Wiebelt
Oklahoma State University

Absorbed solar energy acts to increase the driving potential for evaporation, as may be seen in the Dalton Equation. The application of a monolayer or thicker chemical film on a water surface can change the reflectance of solar energy.

An analytical investigation has shown that reflectance is a function of the optical properties, the film thickness, the energy wave length, and the angle of incidence. With the aid of a computer, the change of reflectance with variation of the above properties was determined. The results indicate that by selecting a film forming substance which has desirable optical properties, the energy reflectance can be significantly increased.

Experimental work was done to determine the change of reflectance due to the presence of a compressed cetyl alcohol monolayer on a water surface. Though some increase in reflectance was noted, the results showed that a cetyl alcohol monolayer does not significantly increase the reflectance water in the energy range between 0.40 and 2.20 microns. The results correspond to the predicted theory as the anticipated optical properties of a cetyl alcohol monolayer are not extreme enough for a significant reflectance change.

Further work on the reflectance of film-covered water surfaces will consider such chemical materials as dyes and polymers.

The completed work was done under the Bureau of Reclamation Contract No. 14-06-D-5232.

ON THE SOLUTION OF NONLINEAR EQUATIONS

D. R. Scholz
Louisiana State University

The final step in the solution of many of our problems is the task of finding, or perhaps simply of asserting the existence of a zero of $F(x)$. If F is a nonlinear operator Newton's method may be employed to do both. Either the differentiability properties of F must be known or, if solutions must be obtained, the first derivative of F must be computed. In practice this derivative is often replaced by a difference quotient which, as shown by the theorem below, can be chosen so that the existence of the zero is not in doubt and the substitute iteration converges with the same speed as Newton's iteration.

Let F be a nonlinear operator mapping a Banach space X into X and having divided difference on the sphere R,

$$\|x - x_0\| \leq R \quad ;$$

i.e., for every pair x', x'' there exists a bounded linear operator $F(x', x'')$ such that (1) $F(x', x'')(x' - x'') = F(x') - F(x'')$, and (2) $F(x', x'') = F(x'', x')$. If, for every triple x', x'', x''' in R,

$$\|F(x', x'') - F(x'', x''')\| \leq K\|x' - x'''\| \quad , \tag{a}$$

and if

$$\|F(x_0)\| = a \quad , \tag{b}$$

$$\|[F(x_0, \bar{x}_0)]^{-1} = b \quad , \tag{c}$$

$$h = 2Kab(1 + b) \leq 1/2, \quad a(1 + 2b) \leq R \quad , \tag{d}$$

where $x = x + F(x)$, then the sequence $\{x_n\}$ defined by

$$x_{k+1} = x_k - [F(x_k, \bar{x}_k)]^{-1} F(x_k)$$

converges to a solution x^* of the equation

$$F(x) = 0 \quad .$$

A QUASI-STEADY STATE SOLUTION FOR UNIDIRECTIONAL FREEZE-DRYING

D. F. Dyer, T. C. Hardin, and J. E. Sunderland
Georgia Institute of Technology

A closed form analytical solution to predict the rate of sublimation dehydration is presented. It is assumed that the time variation of properties are negligible in comparison to the space variation. The validity of this assumption is demonstrated by comparing results of the present solution with those of an "exact" solution. This comparison is made for the particular set of boundary conditions used in the exact solution. The present solution accounts for heat transfer to the moving frozen-dry interface from both the frozen and dry regions. The external boundary conditions involve constant surface temperatures at both exposed faces. The interface temperature is assumed constant in accordance with experimental and theoretical information. The analytical results are determined through the simultaneous solution of the conservation equations for mass and energy for the frozen and dry regions.

Typical results of the theory for freeze-drying bovine muscle are compared with experimental data and show an excellent correlation. In addition, it is shown that the drying time can be decreased by as much as 40% or more by transferring heat through the frozen layer. It is necessary to maintain a vapor seal at the frozen surface in order to achieve this improvement in drying rate. A method is suggested for constructing freeze-drying equipment utilizing this type of "unidirectional drying."

RESEARCH ON POOL BOILING RESPONSE TO PRESSURE DECAY

G. E. Tanger and R. I. Vachon
Auburn University

The research is the study of the history of liquid superheat in a pool boiling system subjected to a sudden pressure release. A 304 stainless steel heater surface in contact with distilled degassed water was monitored during system pressure decay through a 1-inch orifice to the atmosphere. The data show that liquid superheat reaches a maximum during the initial transient pressure phase and becomes negative during the final transient phase with release time of approximately 12 seconds. This phenomenon is quite probable in cryogenic systems.

RESEARCH ON POOL BOILING ON PREPARED STAINLESS STEEL SURFACES

R. I. Vachon and G. E. Tanger
Auburn University

The research is a collection of data for pool boiling water from variously prepared 304 stainless steel surfaces. The stainless steel surfaces have been mechanically polished, chemically etched and Teflon coated to produce variation in nucleation sites and surface wetting. The data are discussed and compared with what data is available for stainless steel and water surface-liquid combination. The Rohsenow pool boiling equation has been used to correlate the data.

ANALYTICAL AND EXPERIMENTAL STUDIES OF FILM AND TRANSPIRATION COOLING OF HYPERSONIC NOZZLE THROATS

H. C. Roland
University of Tennessee

Analytical studies have been made of liquid film, gas film, and transpiration cooling of hypersonic nozzle throats. The liquid film was treated in a stepwise fashion using an adaptation of an analysis by D. L. Emmons. Gas film cooling was treated in a straightforward finite difference manner by dividing the coolant and mainstream gases into layers and proceeding to take momentum and energy balances in incremental steps through the nozzle. Two different methods of evaluating the diffusivities were examined. Transpiration cooling was treated both by an adaptation of the Emmons solution and by providing for fluid injection at each increment in the gas film cooling analysis.

Experimental studies of gas film cooling have been made using an instrumented nozzle with 0.5 inch throat diameter. Stagnation pressures up to 500 psia and stagnation temperatures up to 7000°R were used. The experimental results have been compared with the analytical results and reasonably good correlation achieved.

Parametric studies of gas film cooling are continuing for a larger nozzle, and experimental studies are being made using a 7-inch-diameter throat, stagnation pressures up to 500 psia and stagnation temperatures up to 3000°R.

PROGRESS REPORT ON APPLICATION OF SIMILARITY TECHNIQUES IN SPACE VEHICLE TESTING

R. L. Young
The University of Tennessee Space Institute

The techniques of similarity analysis are applied to develop criteria for deducing from model experiments steady-state and transient temperature distributions for a typical space vehicle element exposed to space conditions. The specific purpose is to determine if useful information can be obtained from models in space environment chambers. Thermal similarity criteria are deduced from expressions for transient conduction with radiation boundary conditions. Based on these criteria, a prototype and nominally one-half scale model were designed and tested in a space chamber. To attain thermal similarity, some geometric distortion was necessary in the model. Both prototype and model were thermally cycled. Temperature data for prototype and model are compared at equivalent locations and times. Results show that temperatures agree within an average of approximately one percent with a maximum deviation of approximately five percent. The model was subsequently altered, thus destroying its thermal similarity. After alteration, the temperature data differed significantly from that of the thermally similar model and prototype. These experimental results demonstrate the correctness of the derived criteria and show that close adherence to similarity rules is required.

SOME AREAS OF THERMAL PHYSICS RESEARCH IN THE RESEARCH PROJECTS LABORATORY, NASA-MSFC

H. L. Atkins
NASA – Marshall Space Flight Center

Four of the areas of thermal research presently being investigated, both in-house and on contract, are discussed. These are listed below with a short description of the activities.

(1) Theory of the Thermal Emission of Materials – Past theoretical work has resulted in a revised expression for Kirchhoff's Law when applied to thermal non-equilibrium conditions. Present work on this project is to measure the energy transfer for a system in non-equilibrium conditions and compare this with the theory.

(2) Thermal Contact Conductance – Data is presented on thermal and electrical contact conductance. A brief description is given on a program to use one- and two-dimensional transient theory and a comparison of this with data. This theory of the transient conditions and the associated data is concerned with holding contact conductance constant and the thermal environment is changed and also where the thermal environment is constant and the contact conductance is changed.

(3) A discussion is given of the program of thermal conductivity measurements of non-metallic media. This will include the objectives, techniques, and a description of the experimental setup, with some preliminary results. Efforts to evaluate the contribution of each of the three modes of heat transfer separately are described.

(4) A discussion is presented on the thermal energy emitted by geological materials in the infrared which are of importance in space exploration. The objectives and experimental approaches for determining the temperatures by infrared radiometers, composition by infrared spectroscopy, and infrared images by thermal scanning are given.

TRANSIENT THERMAL STRESSES IN A CYLINDER WITH INTERNAL HEAT SOURCE

S. S. Chehl
Southern University

and

D. R. Scholz
Louisiana State University

This paper deals with the thermal stresses in an infinite hollow isotropic elastic cylinder. The cylinder is in thermal equilibrium due to constant heat generation axisymmetrically within the cylinder and to conditions imposed on the boundaries. The temperature is held constant on the inner surface while the outer surface is perfectly insulated against heat flux. The inner surface is suddenly cooled but kept constant creating a time dependent temperature distribution which in turn generates varying stresses within the cylinder.

The initial steady state condition is expressed analytically in terms of elementary functions. However the solution of the partial differential equation representing the transient temperature distribution is expressed as an infinite series of Bessel's functions, the latter not being amenable to numerical computation. Accordingly, finite difference techniques are used to obtain numerical estimates of the solution of this partial differential equation and, additionally, to compute the thermal stresses.

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