THE UNIVERSITY OF MICHIGAN
INDUSTRY PROGRAM OF THE COLLEGE OF ENGINEERING

FIVE REPORTS ON CURRENT RESEARCH at THE UNIVERSITY OF MICHIGAN

Transcript of the Sixth Ann Arbor Industry-Education Symposium

May, 1961
IP-555

TABLE OF CONTENTS

Page

BRIEF REPORTS ON CURRENT RESEARCH PROJECTS.................... 1
J. Louis York

HUMAN FACTORS IN ENGINEERING AT THE UNIVERSITY OF MICHIGAN.... 27
Paul M. Fitts

A NEW PROCESS FOR GRINDING WHEEL ANALYSIS..................... 45
Lester V. Colwell

SOME RECENT RESEARCH ON EFFECTS OF SURFACES ON THE MECHANICAL PROPERTIES OF SOLID METALS......................... 73
Edward E. Hucke

THE RUBY LASER.. 101
George Makhov

APPENDIX - LIST OF CURRENT ENGINEERING RESEARCH PROJECTS AT THE UNIVERSITY OF MICHIGAN...................... 117

BRIEF REPORTS ON CURRENT RESEARCH PROJECTS

J. Louis York

Department of Chemical
and Metallurgical Engineering
The University of Michigan

BRIEF REPORTS ON CURRENT RESEARCH PROJECTS

J. Louis York

I do not believe that any university, much less the University of Michigan with all of its faculty, has a genius who can possibly understand all of the things that are going on here. We started just trying to list them with an average of four lines per item, and you see the 30-page document that is in front of you (see Appendix). I might say a few words about that document before we get too far away here -- it is marked "Draft", for the obvious reason that we cannot possibly keep that thing up-to-date. It came off the press yesterday, and it is obsolete this morning, and it will continue to be, but it can give you some approximation of the things we are doing.

There are evident in here several items about the way we do business. Some of these groups are listed entirely as departments. Others are listed with subsidiary laboratories. For instance, the Electrical Engineering Department has gone into a divisional concept to some extent, and they have several laboratories listed. We will talk a bit about one of those a little later. This shows partly some of our academic freedom, I suppose, partly some of our prima-donna action-that we do things in a different way all the time.

The magnitude of the job of just leafing through this could be emphasized, I think, by another kind of a measure, one perhaps that the industrial people like to use occasionally, and we do at the University occasionally, as to how large a research program is — dollars. Some of you see some of the various reports that come out of the University, some do not. I might summarize and add to them briefly this way. We have several ways in which research is financed. Among these are sponsored research contracts for individual companies, such as yours, who come to the University and make contract arrangements to have research conducted by faculty groups, either on a part-time basis or on a full-time basis. They pay the faculty a fee for their supervision of it, they pay student employees or full-time employees for their time, they pay an overhead charge, and out-of-pocket expenses. Last year the amount of this was $22,300,000 in contract research, which puts us among the largest of the contract-research agencies. And I think we should be marked that way. We actually are going, now, into our 40th year, I believe, as a sponsored research activity, and we have been growing steadily throughout that time.

In addition to that sponsored work, we have money which is given to the University with various restrictions upon it — grants of money, let us say, from a foundation to carry out research in specialized areas. This has a category in our bookkeeping called "restricted funds", not that the others are unrestricted, but these are funds which usually do not carry an overhead or anything more than nominal charge for services that are necessary, if any. These restricted funds are usually dispensed by faculty people to graduate students and they added up to $3,850,000 last year, which brings us up to a total now of $26,200,000 in round numbers.

And I, as a faculty member, am well aware of the fact that this does not include a lot more things that go into research expenses. For example, you have in this list a large number of doctoral thesis research projects. Some of these are partially paid for out of sponsored research activity. Many of them have out-of-pocket expenses, not counting the labor of the graduate student who is available to us, but the out-of-pocket expenses are borne out of current operating costs of the department, and out of our operating budget, which comes out of the legislative appropriations. I do not think anyone dares try to make a realistic estimate of that, because it is so hard to decide exactly how much of this -- when we go buy a hundred pipe fittings, or a hundred gallons of gunk of some kind -- we do not know how much of it goes into instructional purposes how much of it goes into maintenance of the physical plant, how much of it goes into research activities. It is very difficult to keep these things sub-divided. We are down to the cost-accounting headaches now. But if you include that, plus any reasonable estimate of faculty-men's time in directing student research, doctoral research, and under-graduate research, not counting their own personal research, I think you would add another chunk of money to this which would bring it into the neighborhood of $30,000,000 a year.

Now this $30,000,000 is a considerable chunk of money in any budget, and it corresponds to a lot of people. It is hard to measure exactly how many people are carrying on these things. On our sponsored-research activities, we had over 1700 people employed in various degrees last year. Some of these were purely part-time, some were full-time. That included just under 500 faculty members who were involved in this program, and 100 students who were working toward advanced degrees simultaneously with their work on these projects, perhaps using some of that work from the projects.

I might add that I have not seen in my years here, and that does not mean that it is not available, but I have not seen a list such as this compiled recently, so I was quite fasinated by the magnitude of it and the scope of it. I ran a little summary of some of the things that are involved here. I checked off this list 127 doctoral theses actively in progress now, and I know of one or two that are omitted, therefore there must be a few more than that. I also know that we have over 200 active projects in our sponsored-research program, so you can see that we have a fairly large operation in terms of size. The subject matter is equally broad. We range from the infinitesimal to the infinite, and from the purely human, very personal problems to some of the most abstract problems that the world knows, and some that we are just beginning to think about — some of the most "blue-sky" type operations. Obviously I cannot summarize these - as Dean Mouzon indicated, it is impossible. I can pick a few to give you just a little bit of a feel for some of them. We have four speakers on the program, who are going to tell you in more detail about specific aspects of some of the research they are doing so that we will start with this very general operation and then we will come to it a little better.

Let us take one aspect — we do some things for people, for example. There is a research project now active in the University which is sponsored by Federal money, which involves seven engineering faculty members from two different departments, and faculty members from the Medical School, from Anatomy, Physiology, and other places, who are concerned with understanding and developing better prosthetic devices. They call them orthetic devices, because they are at the moment particularly interested in the upper extremities. This is obviously a problem to many of the handicapped who need such devices and there is a continual program underway in an attempt to improve this. This is one that may well involve not only the decrease in human suffering, and an increase in comfort and usefulness for people who are so handicapped, but it will also involve devices that are going to be of interest to people who are manufacturing, developing and marketing such things.

Professor Edmonson, who is here, and Professor Westervelt who works with him, have a project which is concerned with optimization of a power-plant process and really an entire transmission system. They have done a great deal of work on it. In fact the Industry Program has made available to its subscribers one report on this problem. They are attempting to develop a computer system — perhaps I shouldn't use the word "attempt", because they might think I should have said it was successful — they have achieved considerable success. They are still working on improving it. The computer program will do part of the work of optimizing this system so that when an emergency arises, or when a sudden change in load on a power system occurs, the proper steps are taken to give maximum efficiency in the process, and at the same time maximum service to the customer. This has involved not only studying the process, but building a simulating device which will write its own program for the computer insofar as it is capable, and nobody knows for sure how that is going to be limited. This is because it will take more time for an engineer to try to write down all of the variables in the proper fashion to feed directly into this computer program that we could possibly afford, so we are trying to get it down to a point where it will operate so that the general factors can be given to the machine. It will then write its own program in some detail and then be able to solve the problem, if necessary. If that is not a fair statement, Professor Edmonson can help answer questions on it later.

I might interpolate a comment here — I just said "we" and I will probably continue to say "we". I use this in strictly editorial sense. By "we", I mean the community at the University — faculty and students. The second comment clarifies that further — I have deliberately avoided discussing any programs in which I have had an active part as a direct participant. Therefore, the "we" is clearly not credit to me, it is credit to other people. It also means that I am going to be transmitting information to you that I have had to learn from other people. I hope it is correct. Perhaps some of the people on the faculty will be able to help me answer questions as they may develop in the operation.

Another area that we might think about, and one which is common in the newspapers and the technical journals today — it is not significantly represented in the four papers that we have (that is good justification perhaps for me to bring it up) — is that the University is not abandoning the field of hottest interest today — that of space. We have been in it for years, and we are continuing in it. In some areas we are certainly the leaders in the operation. Much of this is information for information's sake, scientific investigation, and I mean this in both the pure science and the engineering science sense. Some of it is for defense purposes. Therefore some of it we can talk about, and some of it we cannot talk about. We might have a quick look at some of these things in a moment.

There is one bit of device which is related to some of the things that I have been talking about. In a sense it might bridge some of these. I would like to show you Figure 1 to describe this piece of equipment. This comes out of the Electrical Engineering Laboratories, and is named by them the "Crestatron", and it is fundamentally a high-power microwave amplifier, a vacuum-tube type. It has been developed here at the University, it is being developed still further on very active projects. Professor Dow is one of the people quite prominent in that; he is here and you might be interested in asking him about it in more detail, if you are interested in possible applications of it, or consideration of its characteristics. This figure shows some of the component parts for one model and Figure 2 shows another unit. These

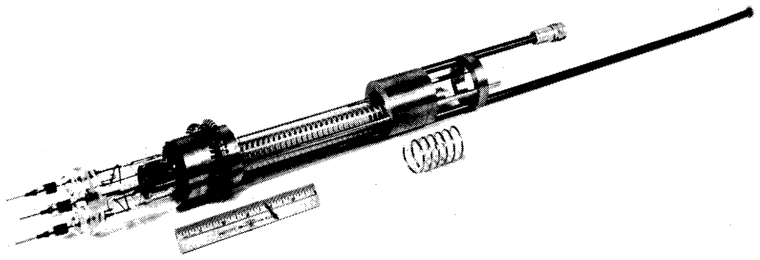

Figure 1. UHF Crestatron and Coupled-Helix Couplers.

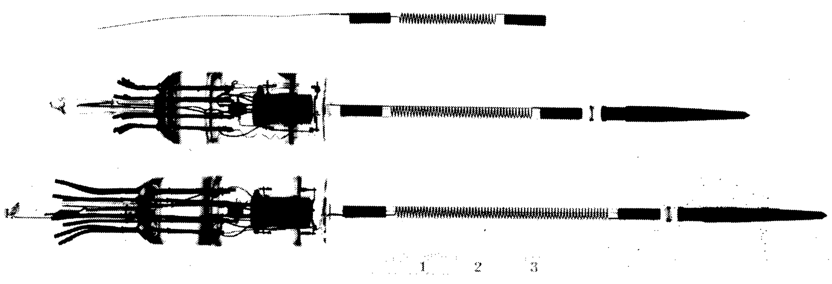

Figure 2. S-Band 100-Watt Crestatron. $G = 10\text{-}12$ db, $\eta > 20$ Percent.

operate, I believe, in the 100-300 megacycle range. Is that correct? Yes. Notice the scale -- that is six inches, so these are quite compact devices. The other devices which have been proposed to accomplish the same thing that are done by this "Crestatron" are much larger. In fact, Professor Dow's description is that some of them are as big as a house, so this means a major step forward in what you might call miniaturization. At any rate, this is a significant step in reducing the size and maintaining the desired characteristics of a piece of equipment.

Let's look at Figure 3. We are going to move now for a moment at least, from Electrical Engineering into Mechanical Engineering. The subject of interest here is related to rocketry — I mentioned we are going to go into discussion of our work in space. One of the prime problems in rocketry is fuel supply in a reliable manner to the rocket engine as the engine is ignited and the rocket takes off and goes through its complete flight to burnout. The liquid-propellant rockets, of which practically all of those you read about in the newspapers are fine examples, are usually controlled as to fuel supply, partially at least, by a pressurization technique. The tank has gas pressure applied to it, it may be helium, it may be nitrogen, it may be oxygen, it may be different kinds of gases, but at any rate it is pressurized in order to maintain the system at a proper suction pressure for any pumps that are included. In some cases there are no pumps and the complete supply is a combination of acceleration of the rocket and of pressurization. One of the projects which is going on here under the general direction of Professor John Clark in Mechanical Engineering is concerned with the things that happen when we pressurize cryogenic liquids. The one which has been most actively used is liquid oxygen, which boils at -320°F. The one which is coming up in future operations, liquid hydrogen, is somewhat colder, liquid helium may possibly be used as part of the pressurization system -- this is one area where we can get some discussion from people who are involved in rocketry. At any rate, the cryogenic fluid so called the cold liquids or condensed gases, are important in rocketry simply because they provide a concentrated form for supplying these propellants, for carrying them around, and getting them there. One of the things that is involved is that when you pressurize these you have some temperature problems, you have some heat transfer from the outside which is tending to cause them to boil away and the pressure goes up too rapidly, and you lose the liquid part of the propellant. It tends to convert into a vapor and escape through the safety valve which is built into the system. In an attempt to investigate this, Professor Clark's group have simulated in the inner container (Figure 3) a propellant tank which is carrying the fuel — this actually is a simulation of the Jupiter missile, and they are working at the moment on the Saturn missile, which you hear quite a bit about. The tank has heating elements added to the side of it. The liquid level would be at the location of the small

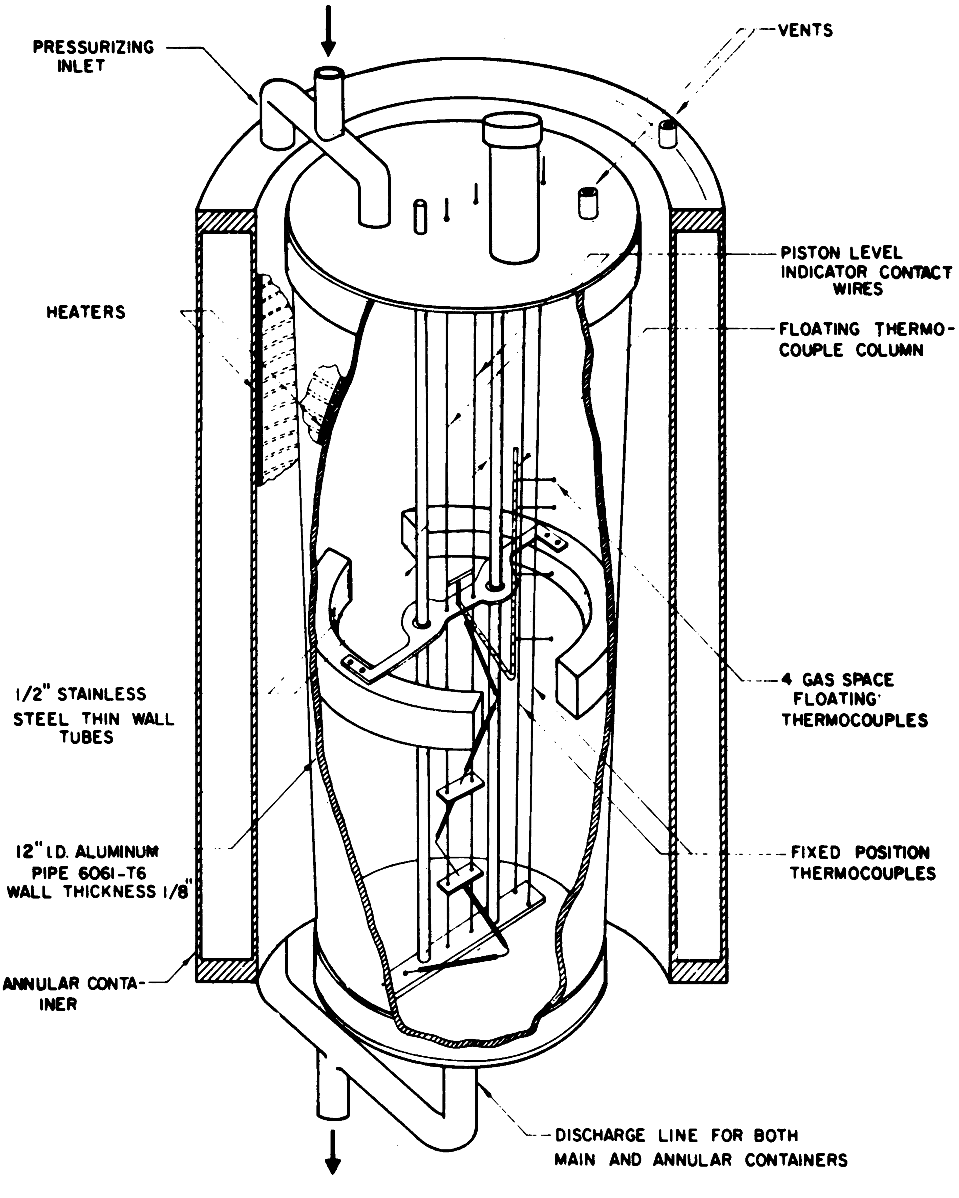

Figure 3. Test Container.

floats which are carrying thermocouples. There are thermocouples mounted at various locations in the system permanently and as moving thermocouples. The problem begins with the tank full of liquid, you bring gas in and force the liquid out through the bottom. You are determining what is happening as it goes out and as heat is added at the side. The outer annular tank is purely a guard-ring for control of the heat transfer, so it is not part of the missile operation, but it is part of the experimental equipment. The floating thermocouples which ride just at the level of the liquid in the tank should tell us something about the equilibrium temperature relationships there and we get a range of temperatures in the liquid phase and in the gas phase.

Figure 4 shows the temperature of the gas being fed to the system in degrees Rankine as the abscissa, and the ordinate is a mean density which is a reciprocal function of the final gas temperature, so that we are determining a heat transfer rate. Figure 4 is plotted so that the gas is hotter as you go down the ordinate. The experimental data points are the black dots. The lines are not an attempt to correlate the data; they are computed lines from heat-transfer theory as to what we would expect to happen. The only problem with such computation is that we do not know with certainty what one of the coefficients is, and that coefficient could be anywhere from 1 to 4 as a first guess on the theory. So a set of calculations were made and the lines were computed, and you will note that most of the data points are reasonably well represented by a line corresponding to a coefficient of 2. So this is one way that you can determine what these coefficients are — by a trial curve fitting technique. This is a natural-convection coefficient. The idea was to determine whether we truly had natural convection.

The objective here, though, is to recognize what happens under conditions other than simple atmospheric operations. This was done at atmospheric conditions. The tank was just sitting here. Now the rocket takes off. It is going to be subjected to acceleration forces, — we're going to have higher than normal acceleration. If you remember the many, many comments about Alan Shepard and his trip, he was subjected to seven or eight "g" anyway on the takeoff, and as high as eleven times the force of gravity on reentry. What happens to the heat transfer on takeoff when we are going through this process of acceleration? This would be with increased values of gravity. Let us look at the next figure (5) and see what is going on there..... This one is a centrifuge. The heating unit is established in the test vessel. We have a counterweight and the whole thing is spun in order to give us a centrifugal force to get conditions which are greater than the normal force of gravity.

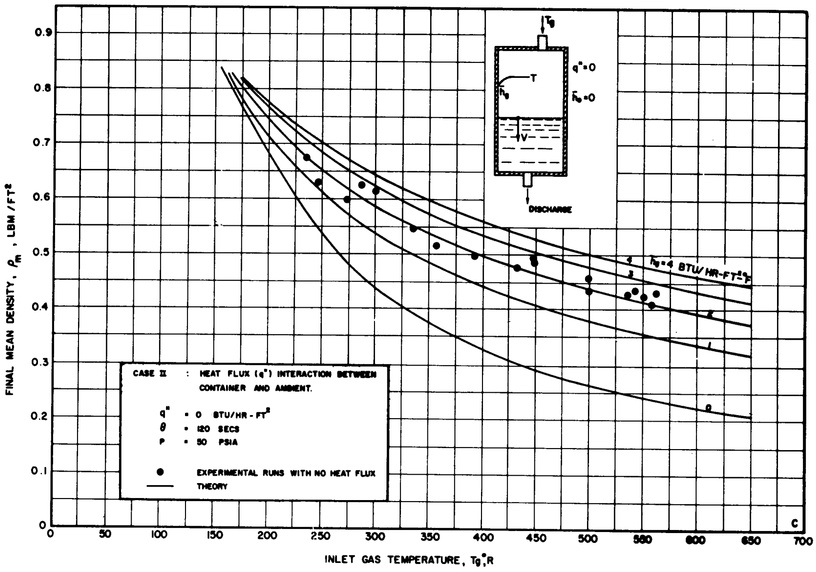

Figure 4. Final Spatial Mean Gas Density as a Function of Inlet Gas Temperature.

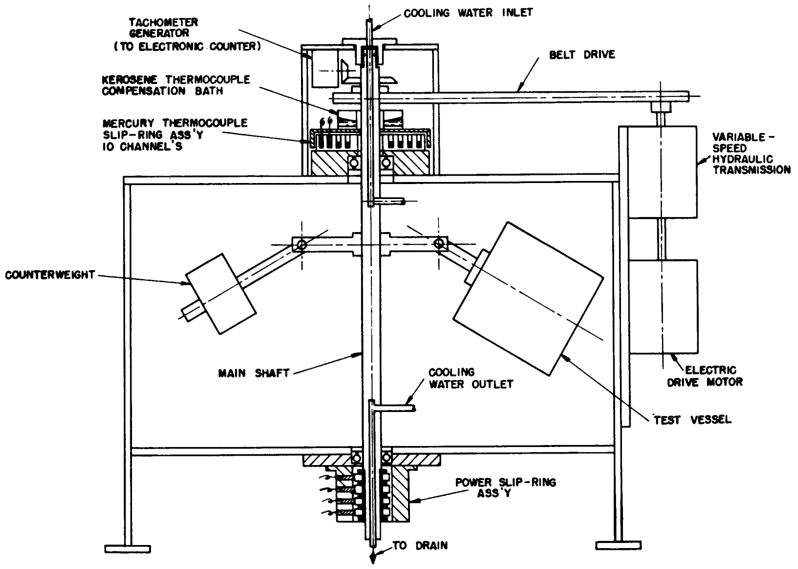

Figure 5. Centrifuge Assembly.

Figure 6 shows the test vessel. We still have a tank. The tank in this case is partially full of liquid nitrogen, as a simulant for all of the other propellants. It has a heating plate at the bottom which is not as large as the entire tank bottom. It has also built into the system a so-called "flow guide". This is just a tube which controls, really, the size of the system. We have only liquid inside the tube which is receiving the heat at a high rate of heat flux when we have the flow guide in place. If you remove the flow guide you have the whole tank receiving heat, but only from a spot which is smaller than the bottom of the tank, which gives you a different kind of circulation effect. Figure 7 shows some of the results from this. We are plotting on the ordinate on a logarithmic scale the amount of heat actually transferred. On the abscissa is the temperature difference, so that, as we would expect, the greater the temperature difference the greater the amount of heat transferred. One line is for heat transferred to the liquid, when it is not boiling, and under 1 g or the normal acceleration of gravity as we would find it without any spin on the centrifuge. As we increase that acceleration, you notice that the rate of heat transfer increases. It does not increase at a fantastic rate. It obviously is going to be stopped at some point, but we do get a higher rate of heat transfer at the same temperature difference as we increase acceleration. That is because the convection coefficients are somewhat stronger, too. As we go into the boiling range, you notice that the curves begin to cross, and at some of the higher temperature driving forces in the boiling range, as we increase the force of gravity we get a reversal and we do not get as good a heat transfer rate as we would have without acceleration. So these are things that must be taken into consideration in the design.

I mentioned that we had that flow guide present in Figure 6. The small drawing on Figure 8 shows the shape of the tank with and without the flow guide, the heater being smaller than the bottom. With the flow guide present, actually only the central portion of the liquid is affected by the process. Without the flow guide present, we get the lower correlation. This happens to be the one which fits the available information in the literature, so obviously this was designed with the concept of a smaller heating surface than we had in the main bulk. But when you put the flow guide in, it jumps up. The Nusselt number is a measure of the rate of heat transfer and is plotted versus a couple of dimensionless constants, which need not be explained for those who are not heat transfer experts, and need not be explained for those who are, because one group does not need to know at the moment and the other group will know. Figure 9 shows another item.

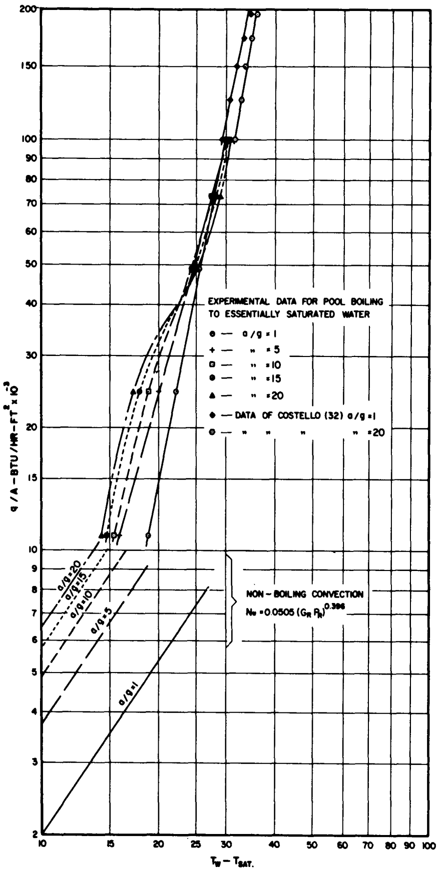

Figure 6. Influence of System Acceleration Normal to Heating Surface on Convection and Pool Boiling.

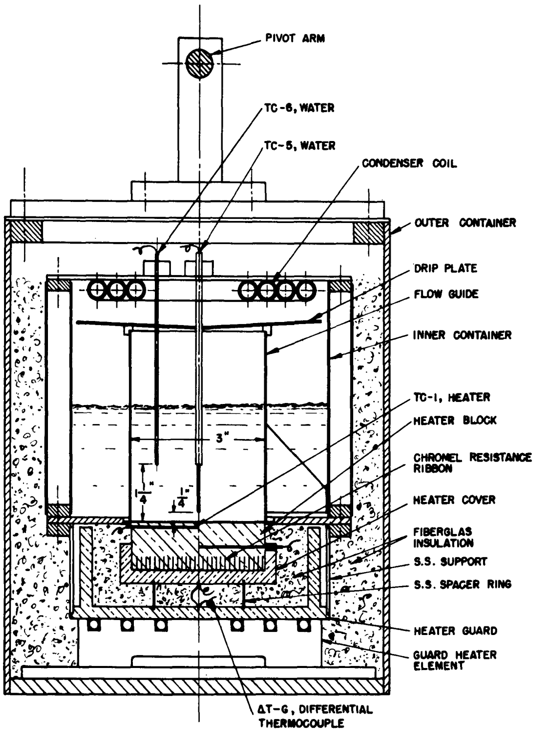

Figure 7. Test Vessel.

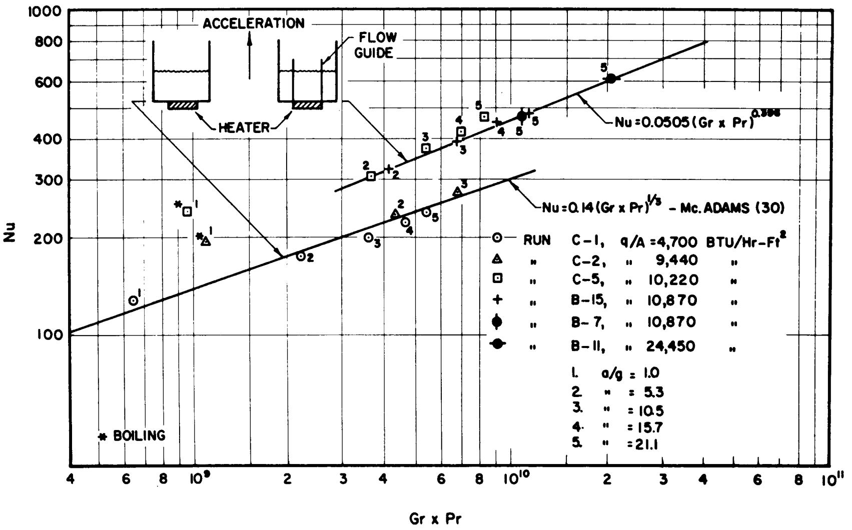

Figure 8. Correlation of Natural Convection Data with Acceleration Normal to Heating Surface.

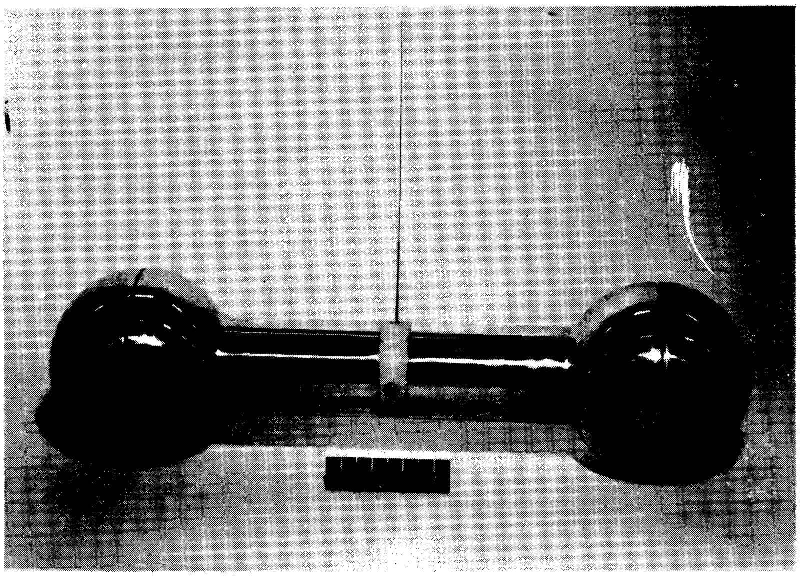

Figure 9.

We are moving back now to the Department of Electrical Engineering, although they do not have a monopoly, necessarily, on some of these things. The dumbbell object is one form of a Langmuir probe which is fired by sounding rockets into the upper atmosphere in an attempt to determine some of the things that can be measured by electrical equipment in the high altitudes -- just to tell us something more about what is actually up there. If you look at Figure 10 you will see the amount of material which has been packed into that probe. The sphere diameter is small -- the heads are off and some of the electronic gear is pulled out of it. This ionization probe actually goes through a process of feeding a super-imposed cycling voltage on the system, and as the ion content of the atmosphere surrounding it changes, the current will be changed according to the difference in flow between the two ends of the dumbbell. That is the simplest way that I can understand it -- there are probably much more sophisticated explanations.

Figure 11 shows the trace which is obtained. The shape of this curve, as compared to the shape of the curve you would expect from a definite atmospheric concentration of ions will give you a measure of the actual ion concentration there. Several thousand such cycles are obtained in one flight which lasts only a few minutes. The result of all of this is reduced and computed and comes out in curves like the ones you see in Figure 12. The ordinate is the altitude in kilometers which you can convert fairly simply-300 kilometers is about 250 miles.

The procedure is that the rocket is fired and at between 50 and 80 miles up, while the rocket is coasting (it has reached burnout), the nose of the rocket is opened up and a spring ejects the dumbbell and it then proceeds slowly ahead of the rest of the rocket, separating slowly from it. The rocket carries some instruments and the dumbbell carries some instruments. The results are all radioed back to the ground continuously and the traces are all made on the ground. The rocket of Figure 12 was fired in June at Fort Churchill on the shores of Hudson's Bay in Canada, up as close to the northern magnetic pole as is reasonably accessible for people from this area. The T_E curve is electron temperature, which is a measure of the vibrational velocities of the electrons which are detected by this probe technique. This is not necessarily the gas temperature because a large number of particles which are present in the atmosphere up there may be neutral and they may contribute more to the gas temperature than to the electrons, but it is at least one of the components of the atmosphere. The solid trace is the record as the rocket goes up, and the dash-dot trace as the rocket comes down. The rocket is not fired straight up, it is fired at about 4 or 5°

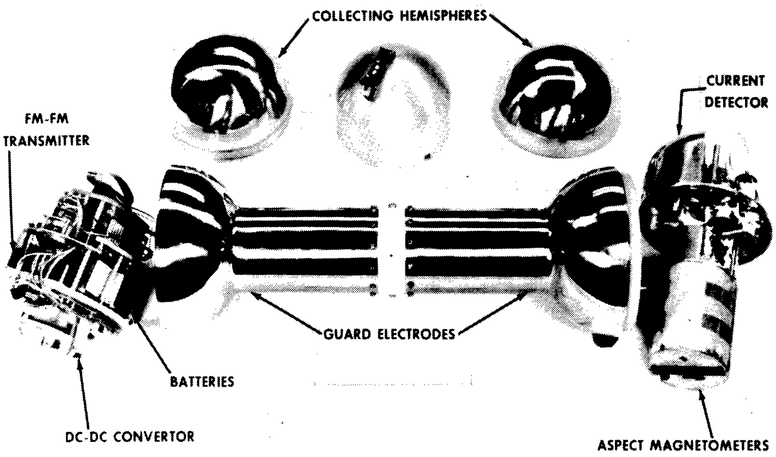

Figure 10. Disassembled Bi-Polar Probe.

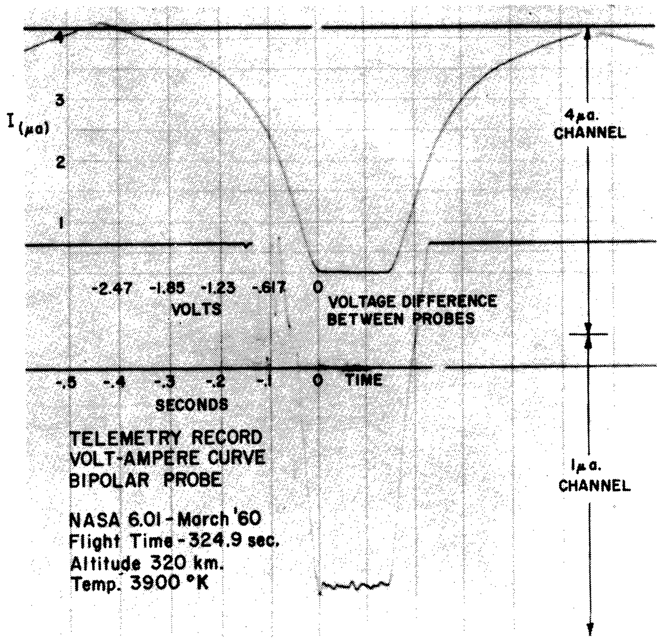

Figure 11. Telemetry Record Volt-Ampere Curve Bipolar Probe.

off vertical so it actually does travel some 100 to 150 miles horizontally during its path. The earth is rotating underneath it, and it is moving in a trajectory. The fact that we get a fine structure in the curve is of some interest, and the validity of that structure now seems substantiated. This is not an experimental error; this is actually a structure which exists as a function of altitude. You get it again on the way down, but this is not necessarily a perfect match, because you may be 100 miles away, at least at the bottom part of the curve. The curve showing the density of positive ions (N_P) is from an instrument also in the dumbell probe. The dash line, an ionogram, is a measure of the number of electrons — the density of electrons present up there — and is measured by an instrument carried in the rocket itself, so that these two are going on simultaneously. They are a short distance apart — according to the size of the atmosphere, at least, it is a very short distance. An interesting point is not only the fine structure, but the fact that these vary with the time of the year somewhat, and they vary distinctly with the latitude.

They got a different set of curves when they fired these rockets from Wallops Island, down in Virginia. Figure 13 shows the Wallops Island curve and you see that this one went up higher — it was a different rocket, and you get a somewhat different curve. If you match these two, you will see distinct differences between them. Again we have the same combination: the electron temperature, the positive ion, and the electron densities. So one of the things that this group in the Space-Physics Laboratory is interested in finding out is not only what's up there, but why, and they are carrying on quite a program to understand this more thoroughly. There is a somewhat similar complementary program, which is also conducted in the University of Michigan by the Aeronautical Engineering Department using different techniques. They measure some of the same things, and also different things. They are using aerodynamic effects: falling spheres and the pressure gradients that are received in the rocket as it traverses through its entire trajectory. So that these two groups are not fighting one another, they are actually operating as complements to one another. They use the same bases, both of them operate out of Fort Churchill and occasionally out of Wallops Island. The rockets used are the various types of small sounding rockets. There is no attempt to put these things in orbit at all.

Now we have one more type of problem which is of interest in the space field, and I will mention this one a little bit in order to bring out one more aspect of the things we do. I said some of these are very practical things — such things as the effect of pressurization

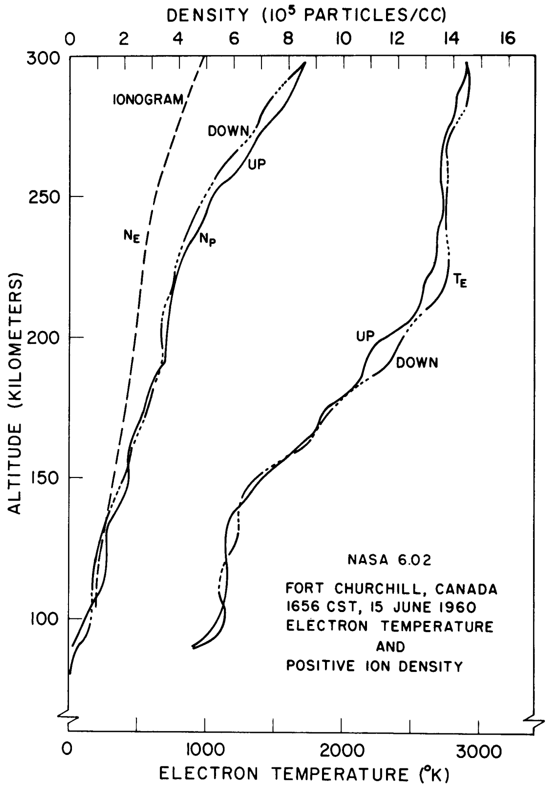

Figure 12. Electron Temperature and Positive Ion Density.

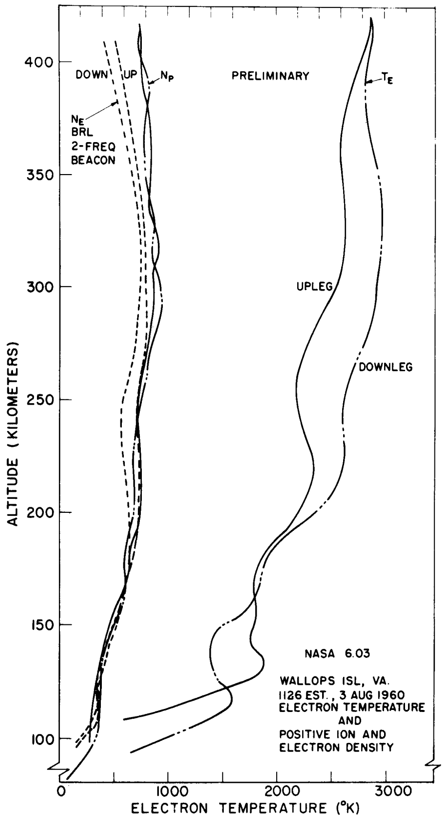

Figure 13. Electron Temperature and Positive Ion and Electron Density.

on propellants. This is important not only in rocketry, but is also becoming important on the ground in other fields than space. As you would expect, some of these technologies are slowly being absorbed into other areas. The things that are being learned to get charts such as these are significant to us in many other ways. They affect — or at least we hope we can understand and they can explain some of the factors that are influential in radio transmission, radar work, and communication systems of all kinds. At the moment this is just an extension of the tremendous program that we are mounting and must continue to mount to learn more about the earth. In fact it might be of interest to you to note that this program and the corresponding program in Aeronautical Engineering are classified in our sponsored research reports under the field of Earth Sciences, and not under the field of Engineering. We also classify Meteorology there, although it is carried on in the field of Engineering, too.

The next item, though, is in getting these things up there and some work has been done in those areas. We have carried on various aspects of the details of rocketry, engines, components of engines, and so forth. I thought it might be a good idea to end this with a brief explanation for some of you who may read the newspapers and have seen a lot of things, some of which are correct and some of which are not correct, about the rotating-detonation-wave rocket engine which has been publicized somewhat. So Figure 14 shows you a sketch of that. You are looking at a side view of the engine at the left. The front view shows a part of a circle. The exhaust slot goes all the way around the center. The one rocket engine which has been built and operated here and has about a seven-inch diameter, a little over seven inches across the circle. The combustion chamber is an annular slot, rectangular in cross section, using hydrogen and oxygen as fuels. It is started by firing a spark in a hydrogen-oxygen feed to set up a single detonation wave which rotates around the chamber. As it rotates around, it feeds on the fuel which is present in the chamber and as it comes back to any particular spot, more fuel has been added at that point, as fuel is introduced through a series of holes all the way around the back of the chamber. The rotating detonation wave, which is traveling at several thousand feet per second around the ring, gives you high pressures and it gives you a thrust as the gases are discharged out through this ring. The unit that has been run here is a single model just to see whether or not it can be made to work and it did work. It ran for a fraction of a second. The detonation wave traveled around the engine something over 100 complete circles and it was started and stopped on schedule, according to proper program, which I suppose is one measure of success. It has not been run for long periods of time, nor is it at the moment large enough to carry a big rocket, but at least it is

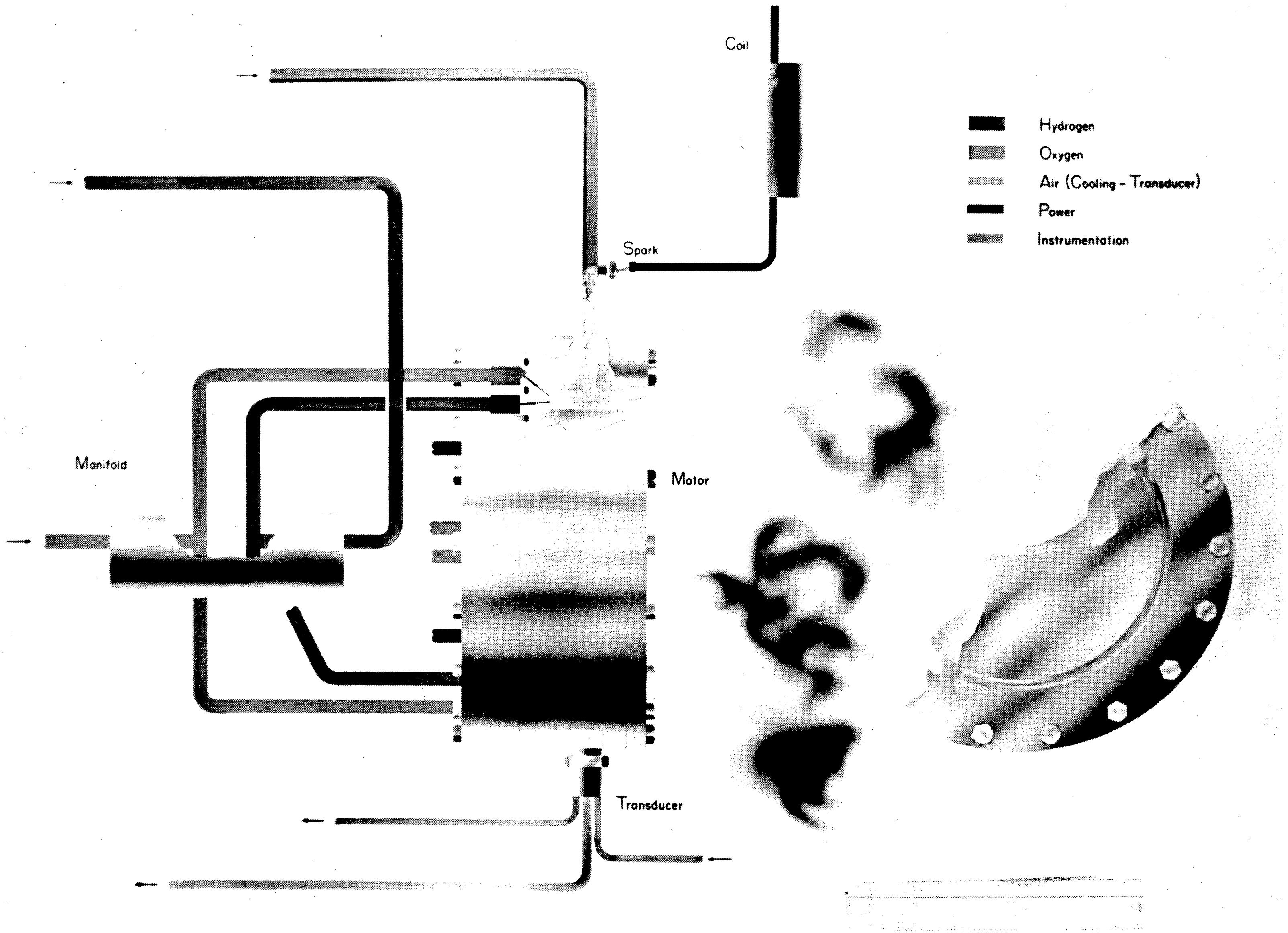

Figure 14. Detonation Rocket Motor.

a measure of something. The program is probably going to be continued. As usual, they always need more money, but we all do that, of course.

I have here (it looks like an alarm clock at first) a half-size model of the engine which is operating out on the North Campus, or can operate. The back plate has two grooves, one of which is for the introduction of the fuel, the other for the introduction of the oxidizer through a series of small holes drilled in the plate into the combustion chamber itself, which is just milled out as a slot. It is formed into a rectangle by that particular mill and a series of covers are placed on it so that the edges give you a rocket nozzle discharge. So the mechanical part of it is quite simple. The instrumentation and problems of making it work are not quite so simple, but it has been done. So I thought if you were interested in seeing a little more of it you possibly could have a look at this model and we can answer questions about it if possible.

Now I have talked longer than I intended. I will be glad to attempt to answer questions, and I tried to recruit people who were working on some of these things and other things to come here to answer question.

QUESTIONS AND ANSWERS

QUESTION: I was wondering what sort of thrust you would expect from something of that size, or the one they actually had operating.

ANSWER: The thrust was not measured on this one -- there was no attempt to set that up. I have got some data on the specifications for an engine which is designed but not yet built. They are hoping to build about a 200,000 pound-thrust engine which will be, according to their plans, no larger, and perhaps smaller, than existing engines of comparable thrust.

QUESTION: This charge then is through this annular ring -- that is where this comes out?

ANSWER: Right. The detonation wave moves around this ring, around the combustion chamber. The detonation wave is essentially a shock wave with combustion occurring behind it, and as it moves around this chamber, then the combustion gases discharge out through the slot. High-speed movies of it show a flame issuing, depending on the contrast you can see the temperature gradient all the way around, and the flame can issue for half the path. You can see in a high-speed movie of the operation where the flame front is, where the detonation wave is, and decreasing temperature as you go back around the path. Now this has been run. I guess you might call it a single-cycle operation. There is only one detonation wave in it. However, we understand the Russians have stabilized as many as seven detonation waves in a similar chamber, chasing them around after one another.

QUESTION: It is not clear to me exactly what the advantage of this is over, let us say, a straight-through system, which the detonation wave would travel down a stream, with discharge out the back at a detonation rate and be fed continuously from the front end. Why this circular pattern? What is the advantage to that?

ANSWER: Two things. I do not know whether Dick Morrison or some of his people happen to be here — I'll answer for them. I may not be perfectly correct, but I will do the best I can. There are two things that I know of that are possible good reasons for doing it this way. One is that by taking advantage of the detonation wave, you can operate with higher pressures in the engine and with higher pressures you then get a smaller combustion chamber. And the second one is that you are burning only a small volume of the fuel at any particular instant instead of having to have the entire combustion chamber as a flame zone, so it is a measure of increasing the energy release per cubic volume of the rocket chamber — the combustion chamber. That is its objective.

QUESTION: If you took the circumference of this and spread it out lengthwise in the direction of travel of the rocket and make a long narrow one, you could have the rate of burning the same and you would put the fuel in ahead of this detonation wave and keep the fuel moving down there fast enough so that the flame, although it is traveling at detonation velocity, cannot catch up with the supply, it seems to me you get the same effect. I am still not clear on this.

ANSWER: You would then get a pulse out of the discharge at the nozzle as the detonation wave came to the end. You would have to start another one, you see. You would be running a series of detonation waves one after another as it traveled the length of the rocket chamber. You would get your high-pressure discharge as a pulsing action. This gives you a continuous action, although not necessarily from all of the chamber simultaneously. As you fire the detonation wave lengthwise down the unit, your pressure at the waves will remain high, but the pressure behind it will begin to deteriorate so that you will have a lower average pressure at the same volume.

QUESTION: What you are saying is that you cannot push this fuel into a pipe fast enough, let's say, so that you can get the velocity equivalent to this detonation wave.

ANSWER: One of the problems even on this engine is trying to get the fuel in there by the time the detonation wave returns. You have to really pump it in fast and it has a high energy discharge per unit volume.

QUESTION: What causes the detonation wave to travel clock-wise or counter clock-wise?

ANSWER: You mean how do they control the direction?

QUESTION: Yes.

ANSWER: The inlet tube, which has a spark to make it start, just has a simple elbow on it which starts it in that direction and it goes from there.
Any other questions on any other of the subjects here. I can always say I don't know.

QUESTION: With this running continuously — you said something about one detonation wave....

ANSWER: One wave is cycling through the unit and it has been run long enough to have one wave travel something over 100 cycles.

QUESTION: So it runs continuously?

ANSWER: Yes.

QUESTION: And then does the apparatus heat up?

ANSWER: Not in the time that they run it now. They deliberately made it massive in order to have a large enough heat capacity in the system to avoid having to put all of the coolants on it. In a full-scale engine, I'm sure you would have to supply a cooling device, but that cooling would probably be the same principle that is now used in rocket engines. You would just feed the fuel through cooling tubes and they would get a preheating in the line prior to being ejected. So you would have to remove the heat from the walls in order to keep the entire chamber from disintegrating.

QUESTION: So far you have been using it just to get test data.

ANSWER: Right. Its been tested for a fraction of a second. In the same way that most rockets are experimentally tested for fractions of a second, or at the most a couple of seconds in their initial stages of development. In fact, some of the standard tests that are now conducted on rocket engines just to make sure they work before they release them, are run for at the most, a couple of seconds. Sometimes they run them longer than that, but more often about two seconds. They rely upon instrumentation to tell them what has happened, and it reaches steady state very quickly. It takes less than two-tenths of a millisecond to make one revolution in the engine design they now have, which is just twice this size, dimensionally. Another question?

QUESTION: Would you illustrate a little more how that electron temperature is measured in that upper atmosphere experiment?

ANSWER: Since Professor Dow is here, I would be happy to have him answer that question.

W.G. DOW: Why yes! You saw the shape of that part, did you not? I can draw it here -- the curve was shaped like that -- right?

QUESTION: The one I saw was the other way.

W.G. DOW: Alright, I will draw it the other way, if you like. Like that. If you extrapolate the upper straight part, that describes the ion current that comes from the ionosphere into the dumbbell. Then as the voltage gets low, some of the electrons are able to penetrate through it and as the sheath around the probe gets a lower voltage, more of them penetrate through. Then you measure the difference between the current that is measured and extrapolated ion current, that is the electrons. You plot the logarithm of that against the voltage and the slope of this curve is the electron temperature in electron volts.

J.L. YORK: We have a model of the nose cone and two of the big charts, including one that you saw on the screen, in the assembly room which is right behind the auditorium and we will be going to coffee very shortly so you will have an opportunity to see it.

QUESTION: You essentially get the velocity from its ability

W.G. DOW: We measure the electron temperature by the relative ability of the electrons to penetrate through the barrier according to their velocity and energy distribution. That spike that you saw there enables us to go all the way down the energy distribution curve. The point is that to measure deeply into the distribution curve, you need a ratio of areas of one object to another, comparable to the mass ratio of the parts. Does that help you some?

HUMAN FACTORS IN ENGINEERING AT THE UNIVERSITY OF MICHIGAN

Paul M. Fitts

Department of Psychology
The University of Michigan

HUMAN FACTORS IN ENGINEERING AT THE UNIVERSITY OF MICHIGAN

Paul M. Fitts

It is a real pleasure to be here. Although my position in the University is in the Psychology Department, for the last fifteen years I have been associated with engineers about as much as I have with psychologists. I am sure that I have talked more often to engineering groups than I have to my psychological colleagues, so I do feel it is a pleasure to be here this morning.

In the time that I have, I would like to try to paint a fairly broad picture. I have therefore given you some notes in order to help you keep track as I turn from one topic to another. During the first half of my time I shall try to say what human factor work in engineering is about. During the last half of the talk, rather than concentrate on any one activity within the University, I shall pick projects to illustrate the variety of problems that you get into when you begin to consider the interface between the sciences that deal with man and the engineering sciences. So to launch right in, what is human factor engineering? I think of it as having two facets -- a scientific side and an applied side. I have indicated in the outline that it aims, on the one hand, to undertake a scientific study of man with emphasis on determining man's capacities and limitations as you would any other physical system, and, on the other hand, it aims to apply these data having to do with man's characteristics to the design of man-machine systems and all the various products used by man.

The only thing that is in any way new about human factor engineering is that we are trying to handle a very old problem in a somewhat more scientific way than we have done in the past. I have been interested, for example, in looking at the definition of engineering from this point of view. The oldest definition of engineering that I have been able to find is one which was adopted by the Institution of Civil Engineers in London about a century and a half ago. That definition ran something like this: "Engineering is the art of applying the great sources of power in nature to the use and convenience of man". Now there are several things about this definition that might interest us here. One is that engineering has grown from an art in the direction of becoming a science. Engineering has certainly taken the scientific approach as far as studying the sources of power in nature and developing its interface with the physical sciences. However, when you look at the last part of the definition which says "for the use and convenience of Man", it seems to me that until recently we have still been in the state of an art. We have relied largely on intuition in the slow process of inventing something and then trying it out to see whether it works, i.e., to see whether the products of engineering are really adapted to the people who must either use them as consumer goods, as tools, or as machines. This is shown very clearly if you look at the history of the development of technology and its products. For example, if you visit the museum at Dearborn and look at the collection of old bicycles, you will find that sometimes fifty years went by before there was a major change in the bicycle in terms of adapting it to the rider -- in terms of locating the center of gravity, in getting the right gear ratio, in improving steering, and so forth. If you look at printing, at the telegraph, at the telephone, or if you look at the assembly line, you will see repeated many times this rather slow improvement. Very often an improvement is a result of the ingenuity of a particular man or inventor, who has worked for many years in trying things out before he found one that worked. Today we often cannot afford such slow progress. Military technology, particularly since World War II, has had a tremendous impact in accelerating work in human factors. I think of aviation as being the major catalyst in forcing human factors work into the laboratory and stimulating a scientific approach to human factors problems. We do not have the time in developing the modern airplane that we did in developing the bicycle. We want to be sure that an airplane or a space vehicle works and works the way that we want it to before we ever build it, and in the race that we are now in on the military side we want to solve as many human factor problems as we can in advance. This forces us to a scientific laboratory kind of approach to the solution of human factor problems.

Human factors work has received the greatest support in the last ten or fifteen years from the aviation industries. The electronic industries are a close second, and then following along, sometimes a few years behind, have been the business machine industry, automotive industry, home appliance industry. And we are just beginning, I believe, to see the application of this approach to the manufacturing process itself as contrasted with the systems or products which are manufactured. Time does not permit me to say much more about the background and the historical development of this scientific approach to this problem. Let me go back now and say a few things that will clarify, I hope, the nature of the human-factor problems I am talking about.

One way to identify human-factor problems is to look at any modern technological system and ask at what points in the life cycle of one of these systems can the data from the biological and sociological sciences and psychology be used by the engineer. The first opportunity, of course, is in the initial conception of a new system. If you look at new space systems such as Dinosaur, or Project Mercury, you will find that human factors scientists are working as members of preliminary design and feasibility study groups to determine what kind of system is feasible in terms of the human operators who may be a part of the operating system, and the people who will have to maintain it, provide logistic support, provide the communication links, and the like. Similarly, groups who are taking forward looks in fields such as business or home appliances, want to know what the customer is going to want in the future. For example, what sort of automation is a housewife going to want in ten years? So, there is a need to estimate the feasibility or demand for new systems or products from a human factor standpoint.

Next, there are a great many points in the detailed engineering design of a new system where human factor data are required. In particular, think of the interface between a man and a machine in terms of the instruments or displays by means of which a machine talks to a man and the controls -- devices -- by means of which the man provides the input to the machine. If you are going to have the man and the machine interacting as an integrated system, you must have some way that they can communicate with each other. You have to be able to know what the machine is doing and tell it what to do, or it has to tell you what it wants you to do. Thus, the design of displays and controls is one point at which a great many detailed technical problems of a human factors nature arise.

The third stage in the life cycle of a system at which human factor problems arise is in the testing of equipment. Now the earlier testing can be done better. One way to test a product is to build it, sell it, and wait to see whether or not the customer complains about it. Another way is to try to test it in the laboratory in advance of offering it for sale. For example, if you are selling a commercial product, it may be very important that the company which buys it finds it relatively easy to train people to use it. If two manufacturers are selling computers, presumably the one that can sell a computer that is easier for a girl to learn to operate or learn to program is going to have a competitive advantage. We would like to know in regard to all such systems that they are going to be useful and efficient when they actually come into use; to insure this and do it efficiently we need human factor testing methods.

Finally, after systems are in use, there is often a great deal which can be learned both in terms of improving them and in designing next year's model, or the next system in the series.

Now the nature of the human factor contribution is different at these four stages: preliminary design, product engineering, testing, and operational use, and the nature of the human factors effort where responsibility for it is assigned, and the kind of people who should mount the effort varies accordingly.

Next I would like to consider the question, what are some of the pay-offs from this type of work? We think first of efficiency. This has been a watchword of American technology. The efficiency of workers, of communication systems, of transportation systems, of the home appliance system, and the like are of general interest to many people. Much of human factors work is aimed simply at making the worker more productive, permitting the operator of a machine to do a job more efficiently, permitting the entire system, consisting of men and machines, to do whatever it is supposed to do more rapidly, more accurately, to simplify logistics, to improve maintenance, and to increase the efficiency of the manufacturing process itself. In addition to efficiency, however, there are other objectives or payoffs which often are overlooked, or if not overlooked, are at least not accepted by the typical design or test engineer as part of his responsibility. For example, consider the matter of training. It is very easy to say, if you are designing a new product, that somebody else is going to have to worry about training people to operate it after it is produced. But a great deal can be done in the design process itself to make subsequent training simpler, or in choosing a design which is easy for the consumer, or user to learn to employ. Thus, you can reduce training costs a great deal by designing products with this objective in mind. Likewise, consider manpower costs. By this I mean the costs which you incur if you have to find a very rare type of person to operate a new device or to perform a certain function in the system, as against being able to use people who have skills, knowledge, or abilities, which are already possessed by large numbers of individuals. Another objective, which has long been accepted, is that of building safety into new systems, and of reducing the hazards associated with the use and operation of new systems -- hazards such as noise and vibration and more recently, radiation hazards and things of this sort. Finally, and I think least often considered by the designer is the objective of increasing the satisfactions that an operator secures while using a tool or machine or performing his function in a new system. People who design and sell consumer products, of course, are very concerned with this objective. They want to design and manufacture products which people want to own. However, very often the person designing an automated industrial plant, or a military weapon system, gives little thought as to whether the persons who will use this system will find jobs which they will like and will be willing to stick with for years. I shall come back to this last objective in a moment because we have, I think, one or two striking examples of current research here on the Michigan Campus which shows what you can do on building in job-satisfaction into a system.

Now I am ready to turn to some examples of work around the campus which is either aimed directly at human factor problems, or basic research which is producing background information of importance for you people in engineering. I would like to apologize now for my selection of topics. I have been quite selective -- I am not trying to tell you everything that is going on around the campus. I have also avoided certain examples because I do not want to claim any credit for them or to imply that the people responsible for them necessarily think of them as being in the area of human factor engineering. I shall talk mostly about some examples of things going on in the Psychology Department, or involving collaboration between Psychologists and Engineers. I have not tried to cover work, for example, in the field of Medicine which was mentioned briefly this morning in connection with prosetic devices. I should also apologize for several references to my own work, but I am, of course, more familiar with it than some of the other things going on around the University.

First let me describe some work having to do with the broad question "What information do you give people in order to permit them to do a job better, and how do you present the information?" The largest effort along this line on the campus is in the general field of military intelligence information. Those of you who are following military systems are well aware of the importance of knowing what the enemy is doing. This is important in a cold war, and becomes increasingly important in a shooting war, since the opponent usually is doing everything he can to keep you from finding out what he is doing, and you need to know in order to plan your own strategy and tactics. This brings us, then, particularly to sensing devices such as radar, infrared, sonar, or optical systems, systems for looking where the other person does not want you to look. Now this is peculiarly, I believe, an area which crys out for collaboration between people who know the physics and engineering of how you acquire information from the electromagnetic spectrum, and who know mathematical theories of information and signal detection, on the one hand, and people, on the other hand, who are specialists in studying what information people can use and how to display it to people in such a way that they can make sense out of it. We are going to face a tremendous problem, for example, when we succeed in having working reconnaissance satellites. The amount of information which can be picked up by such a satellite circling the globe every couple of hours, and photographing a big portion of the earth is very great indeed. When you consider the task of digesting all this information, of filtering out the very small portion of it which is significant, you realize the tremendous job of processing ahead. Business men face exactly the same kind of problem, but on a smaller scale. The vice-president of an industry can not possibly know everything that is going on in his organization - if he spends all of his time trying to find out what is going on, he still can not know and he would not have any time left to make decisions and do other important things. So the business man also has an information handling and display problem. He must ask for the kind of morning reports, the kind of displays, and the information channels he needs to keep him informed about what is going on in the organization. Here on campus we have several projects dealing with these broad problems. At the Cooley Electronics Laboratory work is underway on signal detection theory. The Engineering Psychology Group which is part of the Institute of Science and Technology, is working on a variety of display problems. For example, studies have been made, in relation to photo interpretation, of the effect of how high you fly, the film you use, and how much definition is obtained in the photograph in relation to the kind of data extraction required of photo interpreters. You can not just say "I want larger photographs and more definition" because one always reaches a limit in terms of the cost involved. So the question becomes, what is the minimum requirement in terms of resolution of aerial photographs in order to extract certain kinds of pertinent information from it (when a man is doing the extracting). Much of the work of the Engineering Psychology group is concerned with basic studies of human decision-making. Why is this relevant? One of the first human factor conferences I ever went to, at Wright Air Development Center was attended largely by electronics people who spent a whole week on the topic of how we should design a war room for the top military commander and his staff. This is the room where the top commander sits, receives information from all over the world, and plans the battle strategy. Everybody had ideas as to the things they could do in the way of bringing in more information and displaying it, but the conclusion of the conference was this: you cannot design a war room until you know how the commanding officer makes up his mind. So some fairly basic work on human decision processes is going on, including the study of tasks where information is unreliable and the exact risks and payoffs are uncertain, as is usually true in real life decisions.

In the outline I have mentioned some other studies in the area of information displays, but I think I shall skip over them. I might just mention the last one: Information Sharing for Teamwork. One Ph.D. student in the Psychology Department Human Performance Laboratory is working on two-man teams, studying the extent to which two men can work together effectively in getting a job done as a function of whether each one of them is given different information -- just the information he needs to do his part of the job -- or whether both men are given all of the available information. You can probably think of many situations where this question arises. In an airplane, for example, if the pilot and co-pilot are sitting side-by-side, they can talk to each other, they can see each other's instruments, and each can see the controls which the other man is operating. But if you put the pilot and co-pilot in tandem -- one behind the other -- you have a very different situation. Now neither man can see the other's instruments and the only way they can exchange information is over the inter-com even though they may be seated only two or three feet apart. Similarly when two men work in different offices, you find a different dynamic relationship than when they work in the same office. This is an example of a fundamental kind of question and one in which engineering design can do a lot to change if we know how to achieve an optimum. I recall, for example, a new communications console used in air traffic control which was a very fine improvement from several points of view, except that the assistant who had previously been sitting by the side of the chief controller, and put him opposite the controller. This was a much more efficient arrangement in many respects, except that the chief air traffic controller found that his assistant still had not yet learned the controllers' job after a year on the job. He could no longer do on-the-job training of his assistant because his assistant was facing him and could not see what the controller was doing. An essential design characteristic for training had been overlooked.

Let me go on, next, to another large area of human factors work--that of designing human output devices, in manual control systems. We are seeing, in many areas of engineering, the rapid replacement of the human operator by automatic devices such as feedback control systems and servomechanisms. In many areas the problem of the manual operation of the machine is not nearly as important a problem today as it was during World War II. However, if we look at the total range of human activities including activities of musicians, surgeons, dentists, golfers, automobile drivers, and various jobs in industry that are still done manually -- typing is one of the most common -- it sums up to tremendous amounts of human labor going into the operation of output devices of one sort and another. Basic to an understanding of the design of such devices is research on human skill -- human capacity for learning perceptual motor skills of one sort or another (or studies of the rate of processing information and the rate of generating information if you like to think in information terms), as a function of the kind of coding system that people use. Here again history has recorded many gradual changes in the direction of modifying machines so as to adapt them more closely to human capacities. The telegraph is an extremely slow output device for a man to operate. He operates one key and the rate at which he can do this is fairly low. The step from a telegraph key to a typewriter or teletype keyboard gives a big increase in information rate for a human operator. A man can send messages much faster using all his fingers. However, a stenotype machine, where a man can use his fingers in many combinations, permits another big increase in the output rate of an individual. Associated with each of these new devices, however, are related training problems. I might mention here that the design of the control system in the Mercury Capsule is based on a great deal of current information about the capacities of the human operator. We did not do much on this problem at Michigan; however, I did have a chance to do some consulting for Minneapolis-Honeywell and I was pleased to note the publicity during the past week in which it was emphasized that our Astronout manually operated the Mercury Capsule where the Russian Astronaut did not.

The next area of research, I have called "environmental engineering". This is one of the areas most familiar to you, I am sure, because for many years we have seen work in illuminating engineering, acoustic engineering, and in communication engineering. Minimizing the effects of noise and vibration which are ever-present environmental disturbance in many systems, is still a problem, both in the automobile, in the home and factory, and in the design of military systems, such as tanks and space systems. Just to mention one example, which is involved in the collaboration of quite a few different groups, Professor Howe in our Astronautical Engineering Department has had quite a bit to do with the development of a ride simulator which is now in use at the General Motors Research Laboratories, and some of the human factors people at General Motors have been using this simulator to study the effects of different ride characteristics on the driver. Another current project being done for the Detroit Arsenal has involved again the development of simulation equipment for producing certain vibration patterns, and a team working on this problem in our Institute of Science and Technology has included engineers and representatives from the Psychology Department. Work is also underway on the effect of noise in the home, and I should also like to mention a program being conducted by Dr. Heffner in Psychology on color preferences. This last study completes the cycle of objectives of human factors work from efficiency, which I have been talking about in many of my examples up until now, to the area of consumer preference.

One thing that interests me in the study of color preferences is that it is being supported on an industry-wide basis by the Paint Research Institute of the Federation of Societies of Paint Technology. We, at the University, are always very pleased to have a chance to work on an industry-wide basis with this kind of support. The particular research is theoretical in nature; it involves testing alternative theories of how one represents color preferences in some kind of geometric space so that he can understand the number of dimensions involved in color preferences.

The next example I have listed is also Environmental Engineering. This is, to me, a very interesting project, centering in the School of Architecture, and having to do with the design of school buildings. You all know that the amount of money going into the design and construction of school buildings is very large. Several people in our School of Architecture have very forward-looking ideas about school environments. They reasoned that the architect and the builder, in creating the physical facilities for a school, do a great deal to influence the nature of the educational processes that can go on in this facility. I could, of course, take that item and move it down under my last category, which is Systems Engineering, because the architectural is primarily a systems approach. For example, one of the starting points in this project was a report published a couple of years ago by one of our Educational Associations entitled "Images of the Future". This report attempted to envision the nature of the entire educational system of the future. One of the main conclusions was that kids would spend close to half their time listening in very large groups to lectures such as I am attempting to give today, in rooms probably this size; they would spend part of the remainder of their time in groups of eight or ten, working intimately with the teacher; and they would spend a substantial part of their time in a very small area working alone. One of the main points of the report is that none of these activities can go on in the modern, school room which is designed to seat 30 to 40 students. Nor does the present school permit one to make as full use of modern technology such as television and teaching machines, as one might like. As a start of this research project it was decided to invite a number of leading architectural firms around the country to get together with their school people and talk about the schools of the future, and then to come here to the University to present their ideas. The result was one of the most interesting conferences I have attended in a long time. Some papers given during the conference, incidentally, are being published. A review of the literature is now being made on the effects of different environmental factors such as humidity, temperature, noise, lighting, color, and so forth on learning and related activities.

In view of the time, I think I will just pick out one other research activity to discuss. This is automation and the worker. I am sure we are all aware of the tremendous impact that automation is having on our way of living; especially on the life of the worker and the executive. One of the broader aids of human factor engineering, as I see it, is to look broadly at the sociological impact of technology. Now it is very hard to say who should be responsible for this. We are all concerned, as citizens, with problems like delinquency and we think that deliquency may somehow be related to the changing pattern of living and to technological happenings in our cities and rural areas. But none of us has any great personal responsibility here. Well, Dr. Floyd Mann, and several people in the Institute of Social Research here at Michigan, have had an opportunity for a number of years now to study morale and many of the human factor problems at Detroit Edison. Fortunately for the research, just about the time they were completing this study some years back, the company decided to push automation -- to introduce machines to do much of the work that had been done previously by people. Permission was granted to continue this study, during the time when the workers first heard about what was going to happen, the time decisions were being made as to how to manage the technological change, while it was happening, and then for a couple of years afterwards. I do not have time to give you the results -- I will just mention one result which has been of most interest to me. The total labor force was reduced in this process, but contrary to what many people had predicted, that the people who were left after automation took place would find their life now one long monotonous interaction with a machine, exactly the contrary effect resulted, apparently. Dr. Mann has referred to this as "job enhancement". If you now talk to the people who are left, if you measure their morale and what not, most say that it is a much better job than it used to be. They feel that they have more responsibility, that their job has been upgraded. This proves, I think, a real challenge for those of you who are concerned with the field of automation. How,in addition to the many other things we have been thinking about accomplishing through automation, can we manage technological change so as to minimize undesirable sociological and personal effects. In the past century we have eliminated many jobs that require physical work and most people are glad not to have to do backbreaking work. Now if we can accomplish the same thing in the field of skills and intellectual activities, we will have solved a very important problem.

I would like to close by making a brief reference to the training program in human factors here at the University. Within the Psychology Department we are concentrating primarily on Ph.D. programs for students who want to go out as human factor scientists, primarily into research programs. We have one of the best training program in the country for an individual with such interests. Many of the students who go into this program have backgrounds in engineering physics, mathematics, electronics, and the like, and they want to gain a general theoretical understanding of human capabilities and performance characteristics so that they can predict human capacities in exactly the same way that one specifies the capacities of computers or electronic systems. We give them very extensive research training but we do not attempt to give them a great deal of applied training. There are several other programs that emphasize human factors training within the University. Industrial Engineering has an emphasis in this direction, both for undergraduates and graduate students. There is a rapidly developing new program called "Communications Science" which offers training in a specialized area which seeks an understanding of both artificial and biological systems in terms of how systems process information and how communication process are accomplished. And I might also say that each summer, we offer a two-week short course on "Human Engineering Concepts and Theory" which has been quite successful if you judge from the number of engineers who have attended it.

I have tried to paint a rather broad picture of what human factors engineering is all about, and to give a few examples of specific topics in this area. I would be glad to answer any questions you might have.

DISCUSSION

QUESTION: What kind of measure would you judge would be best to use in rating a human in his environment?

FITTS: I would say that in the great majority of all of this work, we try to measure something about the output of man which is related to one of the criteria listed under item 4 in the outline. We might use a measure of efficiency such as the rate of handling information. We have made considerable use of information theory and information measures, the rate at which man can process information. Now you might ask, "How does a variation in the environment, -- temperature, vibration, or noise, for example -- affect the rate at which man can process information?" The effect becomes very difficult to measure if you ask, "How is the level of the intellectual functioning influenced by the environment -- how much better decisions will a man make if you put him in an air-conditioned office, for example?" I would say that we are struggling to obtain answers to this sort of question in the work on decision-making and the like. Now if you ask, "How does vibration in an automobile affect the driver's ability to steer?," the method used is fairly simple. It is not necessarily easy, but it is straightforward relative to the question as to how it affects his intellectual functioning, his decision making. Does that answer your question?

QUESTION: No, not really. What do you actually measure? -- in other words, we are sitting in an auditorium here. It is a nice place to sit. How would you go about measuring that?

Well, if you are asking me how I would measure the comfort of the seat you are sitting in, -- a lot of people are concerned with this -- this is another one of the difficult ones. One way is just to ask people if they feel comfortable or not, but this is not very satisfactory. Some people want to take a blood sample and do some chemical analysis, hoping to find something in the blood that is correlated with seat comfort. Eventually, we will have this sort of measure but we do not have one now.

QUESTION: Well, suppose you use adjectives to ask a person a question, will these adjectives not have different connotations to different people? Now, how do you filter all this noise out of this data?

FITTS Well, one of the better techniques that might be used here is a psychophysical one. It has been used in Psychology for over a hundred years now -- Psychology got started really in the field of Psychophysics. Basically, you present a person with two things (objects, stimuli, descriptive adjectives, etc.) and ask him to say whether they are the same or different. This is the way you measure the capacity of the ear in detecting differences in pitch, the way musicians tune their instruments. The first experiment I ever participated in as a graduate student, I think, might give an answer to your problem. A manufacturer of shaving cream wanted to know how his shaving cream compared in shaving comfort to several competitor's. This question is similar to that of seat comfort. So I and eleven other graduate students got free shaves every morning. We went down to the barber and he had a double shaving mug and he lathered one side of the face with one shaving cream and put a different one on the other side and he would take a stroke or two here and a stroke or two on the other, and at the end of being shaved, we made judgements as to the relative comfort on the two sides. Now you can apply very precise statistical analyses to these kinds of data -- in fact you can pick up even very small differences if you use sufficiently large groups of subjects. I have a friend who worked for the distillery industry, doing taste testing as a means of quality control. Very often he would pick up things long before the chemists could detect changes in the water supply. So there are measurement techniques, I am saying, which permit you to get at a few things like comfort and consumer preference. The better techniques are adaptations of the old psychophysical methods, of comparing two things and finding how much they can differ along some physical dimension before you detect a difference. There has been some very fine work done on color television from this approach. How can you get a satisfactory TV picture in terms of quality with a minimum allocation of band width to transmission of the television picture? At Bell Telephone

Labs they have been able essentially to cut in half the band width of the television signal by applying some good psychophysical data on factors making for clear reception of colored pictures. And this has been done again by the comparative technique. If you had asked me how I would measure the intelligibility of this public address system, this is fairly easy and has been done for many such systems. I can give information over the system and ask you to write it down and repeat the test over various different public address systems and provide you with very precise measures of intelligibility in human speech as a function of the nature of the public address system. If you asked a question having to do with the time it takes a man to do something, this is also fairly easy to measure.

MOUZON: Have we got one or two very quick questions?

QUESTION: You got into human assimilation of information. We need a buffer of some sort now days to buffer input so a man can read 10% or more instead of 1% of what he might like to read. Is there any hope in high-speed reading courses such as have been given recently?

FITTS: Such courses have some value if you happen to be a very slow reader. However, the real limit of human information handling is the rate at which the brain can handle information. You can do a lot to improve reading, but you do not do it merely by getting the eyes to move along more rapidly on the page.

QUESTION: They do not achieve a gain in assimilation rate, then, proportional to reading speed?

FITTS: It depends on many things including what you want to read, whether a novel, a physics book, or a market analysis. There are a dozen techniques for improving reading, but the dramatic increases in speed show up chiefly when the text is very easy, or highly redundant.

QUESTION: Well then, will the use of a more logical language do it?

FITTS: I think that if you could ever get the English people to look at language as an engineer would look at it — to try to create a language that is more efficient for communication of information. We could do a tremendous amount to increase speed of reading and level of communication.

QUESTION: Such has been proposed in logical language, I believe, in Scientific American....

FITTS: But just try getting anybody in the English Department to go along with it.

MOUZON: I am sure Professor Fitts would be pleased to talk to you privately if you would like to ask further questions, and I hope there will be time during lunch for this, perhaps.

A NEW PROCESS FOR GRINDING WHEEL ANALYSIS

Lester V. Colwell

Department of Mechanical Engineering
The University of Michigan

A NEW PROCESS FOR GRINDING WHEEL ANALYSIS

Lester V. Colwell

I have been warned to quit on time. When you lecture for one-hour classes for twenty-five years, you have a sort of inborn or inbuilt time constant that is difficult to get away from. To the extent that some of you can remember your students days and react accordingly, we will get through on time. The students will automatically begin to squirm and otherwise indicate that they have had enough. I have been so intrigued in listening to the other speakers that I have almost forgotten what I was going to talk about myself, and I would like to disgress for a moment and comment on a couple of things that were said.

Professor Fitts rather early referred to a definition of engineering as being an art, and implied that there might have been some change in the intervening 100 years or so — we certainly hope so. But we must confess also to this being a transition period. That is particulary applicable and valid in the area of manufacturing processes which has been my specialty. Many of you people are from manufacturing and recognize that it has a very broad base. If we look back over the history of manufacturing in this country and in particular say up through World War II, it has been an art, and the world position that was developed is a credit to what you might say some damned good artists, but we have not done anything about a reproduction process to continue that particular breed and we certainly should. We need to inject more science into manufacturing. But because of our earlier successes with the art technique there has been a more or less unconscious acceptance of the notion that this know-how is here, it is automatic, it is inborn — all these problems have been solved. But we are beginning to realize that there may be something wrong and we are perhaps almost to the stage where we should re-read the epic on the rise and fall of the Roman Empire.

I had a rather interesting experience — a couple of experiences in the last two weeks. The week before last, we were visited by the former Director of the Technische Hochschule in Aachen, Germany. That is one of the largest engineering schools in the world and certainly one of the best. They are very active in their research on manufacturing processes and manufacturing equipment. Professor Opitz, to whom I referred, visited this country for the first time at the World Metal Congress which was held in Detroit in 1951. Ten years later, he visited again and his last visit was preceded last year by a similar visit to

Russia. He is a very good friend of this country and I feel has was quite sincere when he said he was truly saddened by what he had seen here in the way of change in atmosphere and rate of progress from 1951 - 1961. He was honestly convinced that not only was far greater progress being made in this area in Russia, but that it was also of higher quality than what is being done here. He offered a few statistics to illustrate the magnitude of the problem. There is a central laboratory in Moscow staffed by 1,000 in round numbers, engineers and scientists working solely on the study — basic studies, really — of manufacturing processes and equipment — a corresponding group of 800 in Tiflis, Georgia, another group of roughly 500 in Prague. These groups are backed up by corresponding numbers of technicians, as in Moscow, for example, some 1500 technicians. Professor Opitz regrets the fact that he has only 200 in Aachen. Note that number 200 and the fact that it eclipses all of the corresponding activities going on in the country.

I have the impression myself, and Professor Shaw of MIT who has also witnessed these things first hand has a similar impression as does Professor Opitz, that the American machine-tool market in Europe has disappeared, not because of the common misconception that we are priced out of the market, but because we have been eclipsed on quality. The Germans, for example, do not hesitate to slap strain gauges on every last cap screw in a machine tool. They really study the process. Many laboratories that I visited literally had five oscilloscopes and oscillographic equipment, corresponding pickups and so on for every machine tool in their laboratory. They would have electron microscopes right on the floor along with very high speed and sophisticated radioactive tracer equipment and techniques.

A group of us in Toronto last week where we were getting together for the Productivity Meeting of the ASME and the Engineering Institute of Canada, Institute of Mechanical Engineers in England, and an association of German engineers - VDI, came to the conclusion that there are two important lines of demarcation. One is the Iron Curtain as we commonly refer to it. East of there, there is some very good work going on in tremendous quantity. West of there, there is very good work going on in substantial quantity up to the English Channel. West of the English Channel there is a marked change. So those of us who are doing research in manufacturing processes in this country feel rather lonely and rather discouraged.

The topic that I intend to talk about today is quite mundane. Professor York characterized much of the research that is going on here on the campus with various adjectives which could mean that the range of projects varies from the sublime to the ridiculous. I would not attempt to characterize my own efforts in this area — you will have to judge for yourself. I am going to depart from what is accepted practice in these things and read the introduction to my progress report to the ASME. I have to admit to having done a bang-up job on it, so it would be better for me to read it, at least the first part.

"The problem of measuring the hardness of grinding wheels has been the object of numerous investigations for more than 40 years. Several techniques have arisen from these studies but the fact that new investigations have repeatedly been started is mute testimony to the effect that the problem has not been solved sufficiently to meet the need.

Considered generally, hardness as a property of grinding wheels has substantially the same significance as hardness of metals as measured by the well-known Brinell and Rockwell procedures. The functional hardness of metals involves strength and load-carrying ability, ability to absorb shock, and ability to resist wear. To a very limited extent this is true also for grinding wheels and other bonded abrasives. But the similarity ends here for two very important reasons. One reason concerns significant differences in the definition of functional hardness as between metals on the one hand and bonded abrasives on the other. The other reason although not entirely independent of the first involves the essentially coarse-grained, heterogeneous nature of bonded abrasives as compared to metals.

FUNCTIONAL HARDNESS OF GRINDING WHEELS

In order to define functional hardness competently one must begin in the production shop where grinding wheels are used. Here one observes that a grinding wheel may be moved from one grinding machine to another and be judged to act harder despite the fact the wheel as a structure and its real hardness remain unchanged. The increase in apparent or functional hardness is nonetheless real since it was the result of a decrease in overall system rigidity or stiffness of which the hardness of the grinding wheel is only one part.

System Rigidity as a Factor. The system, consisting of the grinding wheel, the machine, work holding devices, and the workpiece constitute a group of elastic springs loaded in series. The grinding wheel can be thought of as a simple compression spring in series with several other compression springs. Under load all will deflect; each by an amount inversely proportional to its stiffness. If any one of the springs is replaced by a softer or more flexible spring, all others in the series act harder or stiffer relative to the over-all stiffness of the system.

Thus it is only natural that the functional hardness of a grinding wheel will change as other elements of the system change. A stiffer machine makes a particular wheel act softer; a machine of lower stiffness makes the same wheel appear to be harder. Similarly, relatively rigid workpieces and work holding fixtures make a wheel act soft whereas the same wheel behaves as though it were harder when the elastic stiffness of either the workpiece or the fixture is reduced.

Up to this point in the qualitative analysis of functional hardness of grinding wheels it appears that elastic properties play a dominant role in contrast to penetration hardness of metals where plastic flow is a major factor.

Elastic Modulus of the Wheel as a Factor. It is also well known in production shops that other factors being equal, a resinoid bonded grind-wheel will act much harder than a wheel with a vitrified bond. This appears superficially to be something of a paradox since a resin bonded wheel is indeed much softer structurally than a vitrified wheel. The elastic modulus of a vitrified wheel may be from three to six times as large as that of a resinoid wheel.

However, this property operates in precisely the same manner as the other elastic elements of the system. After all, it is the individual abrasive grain which is the active cutting tool and so if a softer, springier grinding machine will make it act harder then so also will a lower elastic modulus of the grinding wheel itself.

Elastic Support of Individual Grains as a Factor. On the basis of present knowledge, one can only speculate as to the influence of this factor except to say that a softer, springier support certainly will make the grain act harder. It must be true also that the elastic properties of individual grain supports vary directly with the elastic modulus of the wheel but behavior locally in the immediate vicinity of each grain may depend strongly upon grain size, porosity, ratio of volume of bond to volume of abrasive, as well as some other factors as yet unidentified. Consequently, the elastic properties of individual grain supports may be substantially unique compared to those of the wheel as a whole in which case they must also be considered as another set of independent variables in the composite property called effective hardness.

Wheel Wear as a Factor. We come back to the simplest of the properties involved in over-all effective hardness. It is also the property which has caused the word hardness to be used rather paradoxically in describing influences that amount fundamentally to increased shoftness rather than increased hardness.

As abrasive grains wear, they become dull. The duller they become the higher forces build up before they will cut. Also, greater frictional heating will result. Eventually, the forces on individual grains may build up until the grain itself fractures or the whole grain is broken out of the structure thus presenting new and sharp cutting edges.

Fundamentally then, other factors being equal, a truly harder grinding wheel is one which hangs on to a dull abrasive grain to a higher degree of dullness thus creating greater forces which in turn can make it more difficult to control size. The greater frictional heat also aggravates quality control of the grinding process through both size control and sensitivity to grinding cracks and other manifestiations of thermal damage.

It should be noted in summary that this property is not singularly unique but that it involves the strength and friability of the abrasive material itself as well as the combined strengths of the bond posts. Either one factor or the other may dominate in specific instances. Doubtlessly both factors operate concurrently in others.

EFFECTIVE HARDNESS DEFINED

In summary, a qualitative definition of funcational or effective hardness of bonded abrasives and grinding wheels must include at least the following variable factors:

No. (1) Terminal dullness of individual abrasive grains.
No. (2) Elastic properties of individual grain supports.
No. (3) Elastic properties of the bonded abrasive as a body or structure.
No. (4) Combined elastic properties of the entire system.

The first of these is essentially a hardness or strength type of factor. The undesirable consequences of greater dullness or the corresponding hardness is augmented and aggravated by the presence of the three elastic factors.

The dullness factor is a property of individual grains. However, the total consequence of dulling is determined by how many dull grains are in contact with the workpiece at the same time. This is influenced by factors 2 and 3 above as well as by the geometry and size of the machining operation being performed by bonded abrasives. The other elastic components that go to make up system rigidity influence "spark-out" characteristics in grinding, dwell behavior, and other phenomena which affect size control, surface finish, and tendency toward chatter."

I have a number of figures giving the results of our work to date on this project. I should point out at this time that we think it is pure, classical, basic, academic research, which is just another way of saying that we had absolutely no sponsorship whatsoever. Nobody gets paid for anything. It is a progress report in that not all the questions have been answered. In Figure 1, I would like first to indicate just briefly the work that has gone before. This is a page from a German publication summarizing some of the earlier efforts at evaluating grinding wheel hardness. Number 1 up in the upper left hand corner is the old screw-driver technique which is still in use in this country today at least in some plants. The idea is to get some qualitative comparison in an attempt to penetrate. There are variations on this - some will apply a dead weight load, getting some penetration, and then measure the torque required to twist. A very crude indication, but bad from two viewpoints: 1) totally unreliable, and 2) a very poor sample of the product you are trying to control.

The second method (upper right) is a variation of the first wherein a spring supplies the limiting load and simply replaces the dead weight feature. The third method (lower left) is somewhat more progressive where a chisel of some sort is used to literally machine-off a controlled depth and then measure the hydraulic pressure through some sort of indicating gage. Another interesting variation (lower right) is essentially a pair of pliers which was used to pinch off portions of corners. I think that is all that need be said about those. They certainly are crude, as you can judge for yourself.

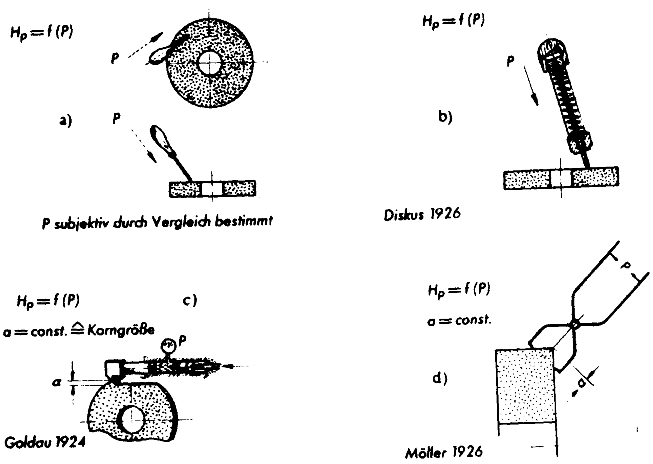

Figure 1. A schematic illustration of four of many different methods that have been suggested for determining the hardness of grinding wheels.

a. A subjective comparison determined by sensing differences in the force P required to break out grains with a hard screwdriver-shaped tool.
b. A variation of (a) wherein P is determined from compression of the spring
c. Force P is indicated by hydraulic pressure required for a depth of cut approximately equal to the grain size.
d. A pincers arrangement for snipping off a fixed amount from the corner. Operator senses the force required.

Now within the past year, work that has been going on in Germany has finally crystalized in the piece of equipment you see in Figure 2. This happens to be laboratory equipment that has been reproduced and is in use in some of the plants in Germany. It consists of a carbide or diamond chisel, mounted at this point in a dynamometer which can measure the forces required. The electrical apparatus is for force sensing. The force information is fed into it and, incidentally, reduced with statistical procedures.

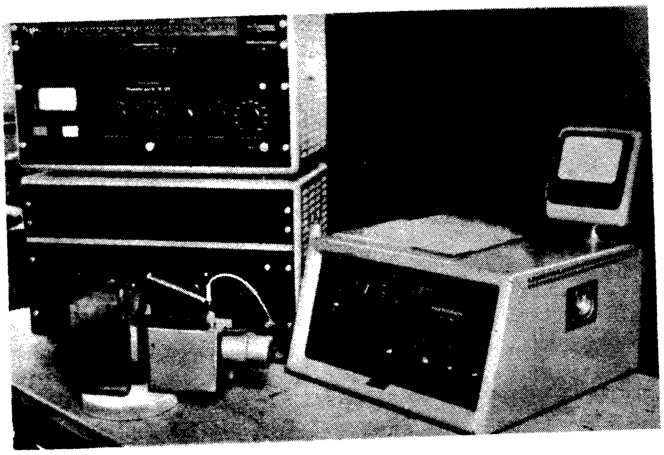

Figure 2. A modern mechanical-electronic wheel hardness tester designed for plant operations. It is now in use in Western Germany.

Figure 3 shows a typical recording of the sort of information produced by the dynamometer which is shown schematically at this point, with the carbide or diamond chisel up here, set to depth, and with the width of the chisel such that it was approximately the average grain size, which, incidentally, has a skewed statistical distribution. A single trace may be made longitudinally to the grinding wheel as shown here. In making the trace, you get blips of the sort you see here. They studied such information for some two years before they were able to characterize the origin or basic reasons for the different shapes that were obtained here. The process was thorough and has led to a tentative standard in Germany; I think that is shown in the next figure.

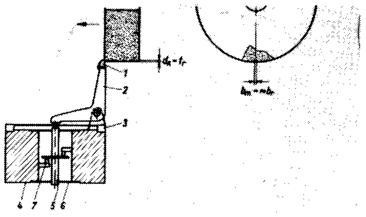

Bild 2 Versuchsvorrichtung für die Messung von Bindungskräften an den einzelnen Körnern

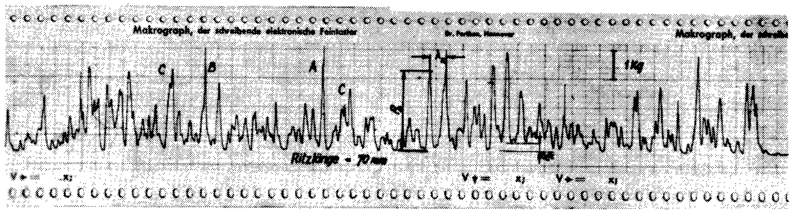

Figure 3. Schematic of the scratch test performed by the apparatus illustrated in Figure 2. A typical chart record of the forces produced as grains are broken out. (From the work of Dr. J. Peklenik at the Technische Aochschule in Aachen, Germany.)

Well, no, these (Figure 4) are typical distributions for the different grinding wheel hardnesses. For those of you who may not be familiar with it, quite some years ago, the Norton Company in this country proposed a classification of hardness designated by letters ranging from E up through Z, Z being the greater hardness. Here is shown the distribution for H, I, J, K, L, M and Q. You will notice that the softer H and J give quite symmetrical and sharp distributions whereas the greater hardnesses are substantially skewed toward the softer side; these (lower half of figure) are the frequency accumulation curves on those. That sort of study information led to the standards, then, which are shown in Figure 5.

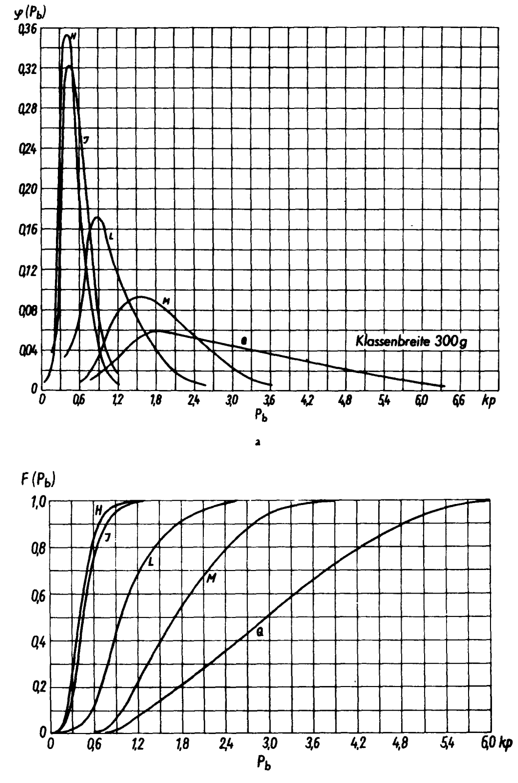

Figure 4. Frequency distribution of bond forces for a range of wheel hardnesses as determined by the Peklenik method. (60 grit, vitrified aluminum oxide wheels.)

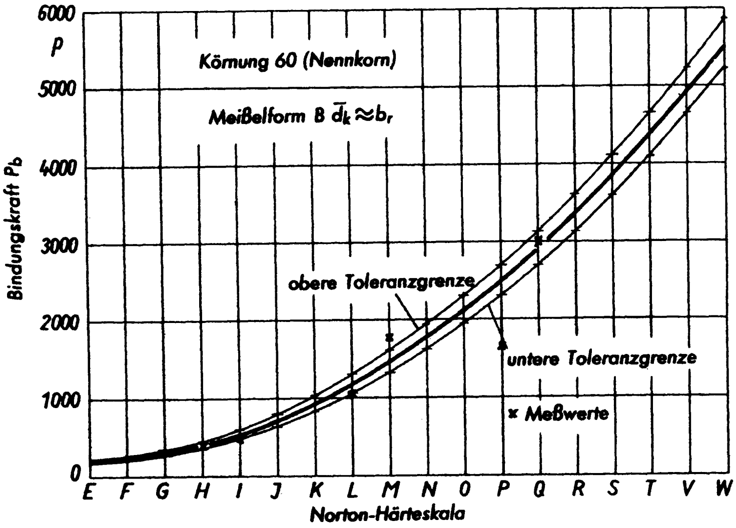

Figure 5. Suggested German standard for upper and lower limits of average bond strength (P_b) for 60 grit wheels.

Statistical capability of the process and optimization with cost led to upper and lower tolerance boundaries for the statistical mean, or statistical average. Here is one commercial wheel, for example, that is judged then to be completely outside of the standard. Here is another one which is just outside. This has now been proposed as an industrial standard for use in Germany. We have no such standards as yet in this country, but the work we have been doing here in our own laboratories, we feel, could very well lead to that sort of thing.

In Figure 6 we have a schematic of an experimental apparatus that has been set up in our own laboratory wherein we mounted a simple grinding wheel on the spindle of a twelve-inch swing engine lathe and then from the cross-slide which is generally over in this direction, built a necessary projection so that we could mount a two-component dynamometer which would measure force in the vertical direction, perpendicular to the screen, and one in the horizontal direction across the axis you see here. Now this houses a small spindle involving taper roller bearings so that the so-called cutter here,which is really a conical shaped cup with a 90° included angle,is free to rotate. That, in general, is brought in contact with the rotating grinding wheel at a controlled depth of cut or radial infeed setting. It is traversed by engaging the carriage of the lathe at a constant, predetermined feed rate. I will use the term depth and feed in subsequent figures, so you might note particulary the connotation here. Now the rolling action actually crushes material away from the wheel, both bond and abrasive grains.

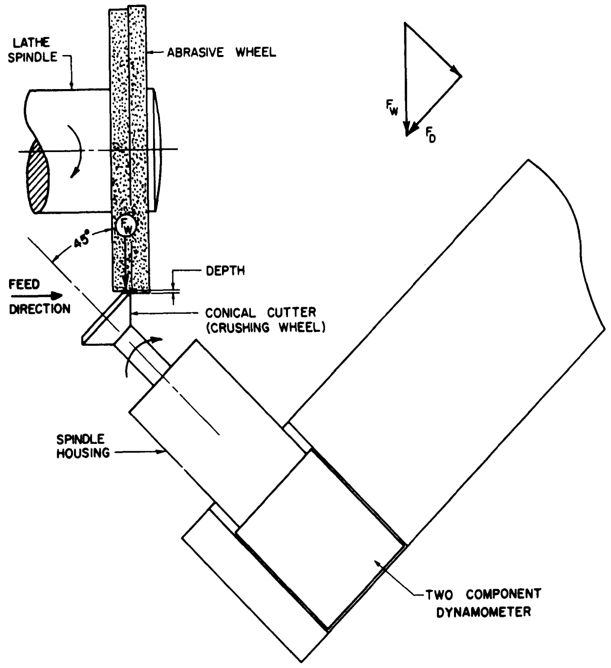

Figure 6. A schematic of the laboratory test set-up.

Figure 7 is a - I should not say typical - record since it was preselected. This record shows force or hardness vertically. As we move from the left hand side of the wheel, over to the right hand side, we note that there is quite a variation in the level. This happened to be a wheel that was returned by a customer and claimed to be defective. The inspection process had not been in use when the wheel went out, but this was mute testimony to the fact that it was really defective. It was a relatively large wheel, seven inches wide, 24 inches in diameter, and a 12-inch bore. We noted that the sides were substantially harder than the center by a ratio of almost two to one and by stretching the record out in this region, we noted also that these particular cycles were substantial variations in hardness around the wheel. This test was made in 23 seconds, which indicates that we can very quickly get a repeatable record of the relative quality of the wheel. We go into some detail in the ASME paper to indicate that it is closely related to the effective hardness which I talked about in introducing the subject.

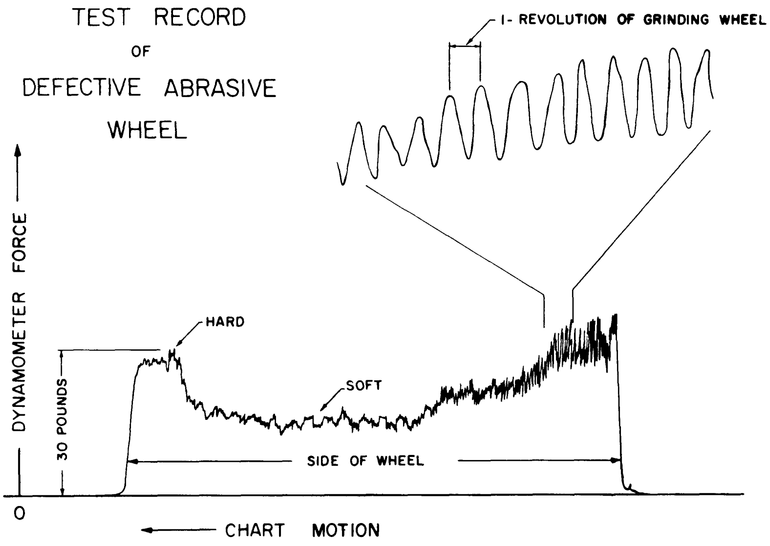

Figure 7. A reproduction of actual test record for the circumference of a defective grinding wheel.

Most of our experimental work was done with wheels which were substantially better than the one indicated before. We read average steady state values, maximum, minimum, and so on and plotted them for the purposes of trying to deduce what was going on — what the basic parameters are. We do not have all the answers yet, but we are making very substantial progress. In Figure 8, for example, we see the average forces indicated by the dynamometer — incidentally, the vertical force is negligible. The ratio feed force, reaction force, if you will, is the most important one. That is plotted here vertically on double logarithmic coordinants. This series of curves is for a constant feed of 0.052 inches per revolution, and several different depths.

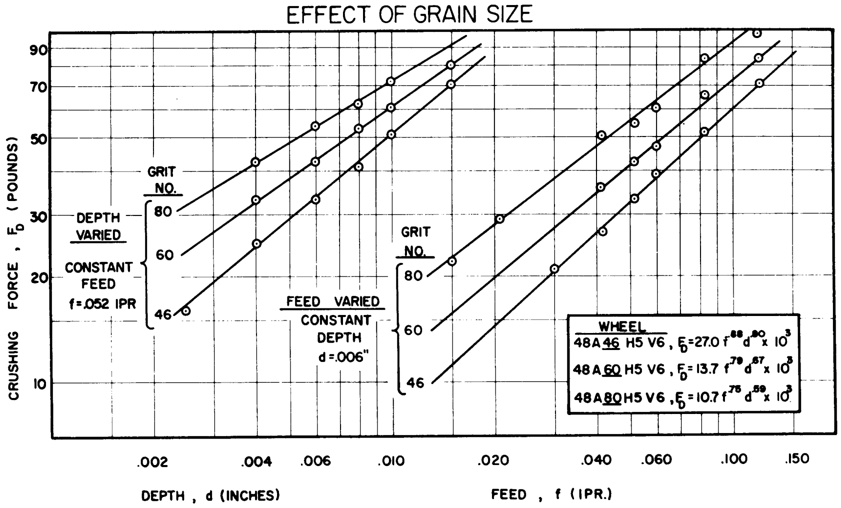

Figure 8. Test results of 12 x 1 x 5 vitrified, aluminum oxide grinding wheels.

Similarly this set is for a depth of 0.006 inches and several different feeds. Three different wheels are represented, differing only in grain size — so called 46 grit, 60 grit and 80 grit; we notice we have a converging series.

In Figure 9 we find a similar series wherein the structure of the wheel changes. This is for a 60 grit, so-called H hardness in three different structures. Structure refers basically to the ratio of bond to abrasive. An "H" hardness wheel on the simplest basis would have 45% porosity and 55% by volume abrasive and bond. Now for the number five structure — my pictures may be quite inaccurate — the percentage of bond may be 8% in which case 47% then is abrasive. If we go to number eight structure, the percentage of bond increases and the percentage of abrasive decreases correspondingly. We reveal here something that has been sensed for a number of years, that as we go from five to eight structure, the wheel acts softer by virtue of the lower percentage of abrasives. But as we go to twelve structure with still more bond, the hardness goes back up again. Our process has already revealed that this is due to agglomeration resulting from too much bond in relation to surface tension which is operative in the firing process.

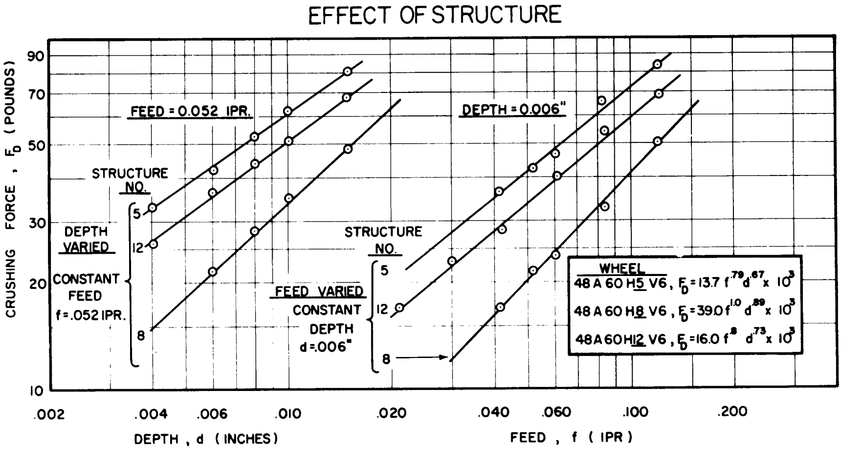

Figure 9. Results for wheels differing only in structure.

Figure 10 shows a similar series where the alleged properties changed only in the hardness level itself. We noted, interestingly that they turned out to be substantially parallel. A qualitative analysis indicates that there is very good reason why this should take place, but we have not yet nailed that down quantitatively.

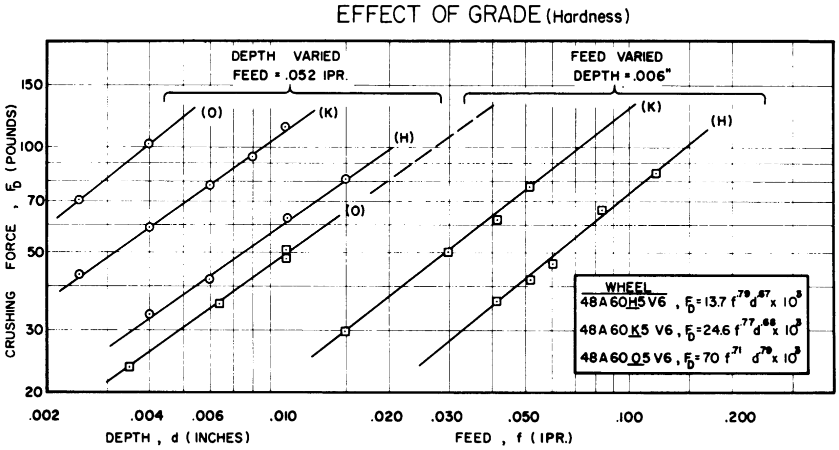

Figure 10. Test conditions same as in Figure 8.

Figure 11 is a similar set for just two different wheels, likewise differing only in hardness, but at a 46 grain size whereas the previous figures were for 60. I mentioned earlier that readings were made of maximum, minimum and average, again as a means for pointing toward the ultimate determinants.

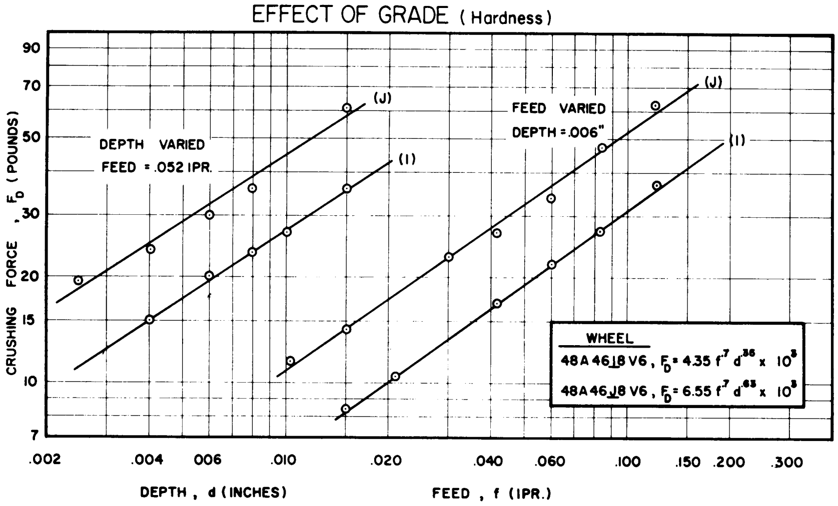

Figure 11. Similar to wheels in Figure 10 except that structure is softer and grain size is larger.

Here in Figure 12 I would like to point out that we have sets of converging curves. All of this data is for one grinding wheel; four different feed levels over four different depth levels over ranges of variable feed and variable depth respectively. Here are regression lines of the slopes indicating an orderliness in this property. Subsequent work has indicated that we can obtain positive slopes and still others where the correlation is positive to a certain maximum value and then becomes linear in a negative trend.

In Figure 13 we try to characterize the interactions of the elastic forces involved in the actual crushing of vitrified, bonded wheels. The vitrious material, of course, is glass — and all glasses fail in tension. They are completely elastic up to the point of fracture. This total width, here, represents the thickness of the wall of the cup that is rolling with the grinding wheel. The feed rate is represented by this particular width. Moving initially from top to bottom, the abrasive grains come in contact with the surface of the steel cup. The contact is elastic until sufficient force is built up for crushing. Then there is an accumulation of debris. There are still other abrasive grains which make belated contact and never do reach a fracture load. They go on through and eventually come out of contact. All of the loads accumulating from these

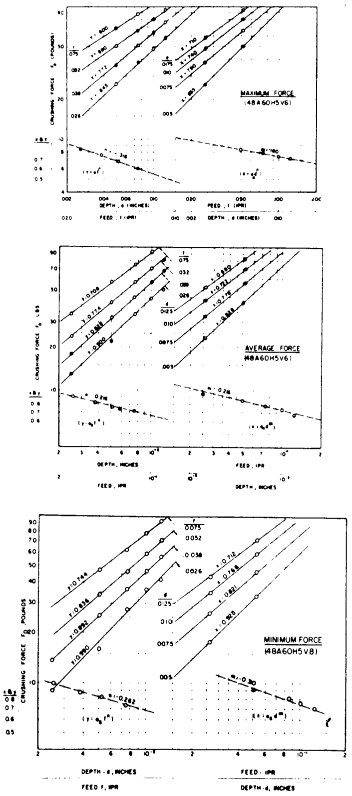

Figure 12. Crushing force for variable depth and feed at four different values of constant feed and four different values of constant depth for the same abrasive wheel.

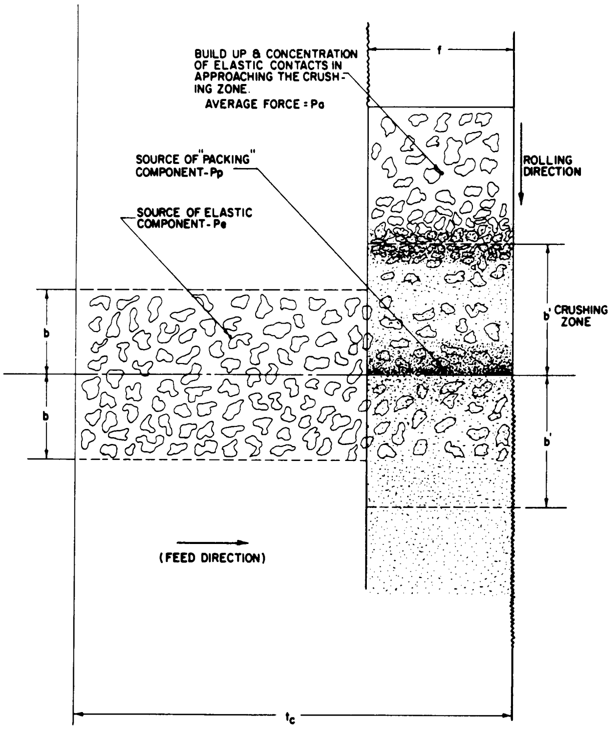

Figure 13. Front view of probable conditions in the contact zone between the crushing wheel and the grinding wheel.

individual abrasive grains contribute to the overall reading and represent the minimum that can be read off the records. After these abrasive grains go around in the next revolution, they show up again in this area and once more add to the total elastic force.

Now! So what? Work has been done in Germany which indicates that these elastic properties really dominate behavior in actual grinding. The Germans characterize the grinding wheel as consisting of abrasive grains supported in the radial direction with springs; with the spring constant varying of course, from grain to grain. Now as the wheel in Figure 14 rotates clockwise in contact with the work piece represented by the cross-hatched portion here, loads build up. Metal is not cut immediately -- there must be a certain amount of radial load before -- as they say in the shop -- before you can get a bite started on this thing. Now, suppose we were to take a look at the surface of a grinding wheel, and that we confine our search within two planes, no more than maybe one 10,000th of an inch thick and count all the abrasive grains at a particular radius, within a matter of a couple of millioniths of an inch. We would find surprisingly few grains within that particular radius specification. However, as we push down on an abrasive grain it comes in the vicinity of others at smaller radii, so that there is, in effect, an increase in the concentration of abrasive grains per unit area as with increases in the total load applied in grinding. The Germans investigated this property with an interesting thermocouple technique that is shown in Figure 15.

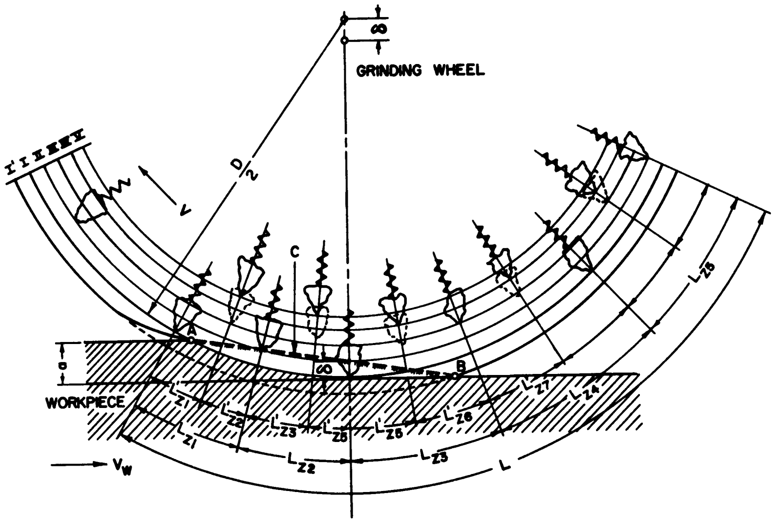

Figure 14. The influence of elastic deformation of bond posts on effective grain spacing.

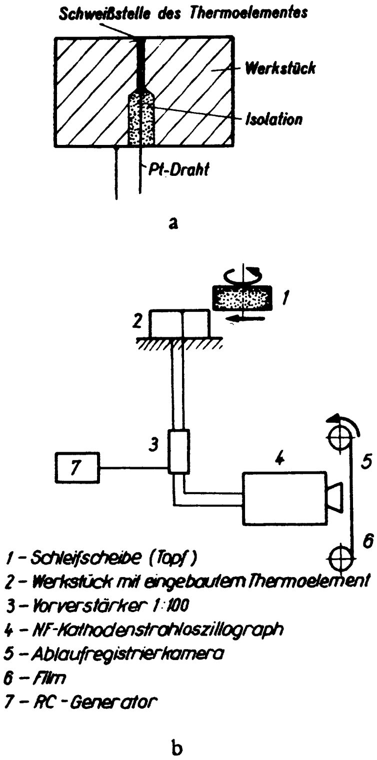

Figure 15. Schematic of the test set-up used by Peklenik for measuring temperature and frequency of grain contact in grinding. The sketch at (a) shows the thermocouple formed by a platinum wire insulated from the workpiece. A high speed moving film camera was used to photograph the temperature impulses as they were displayed on an oscilloscope as shown in (b).

They put a platinum wire up through a steel block. They used platinum wires thousandths of an inch in diameter with about one-thousandth thick insulation. Then as the grinding wheel passed over, it smeared the end of the platinum wire over the steel and created a thermocouple; it gave rise to temperature blips on the face of an oscilloscope.

Figure 16 is a typical oscilloscopic record which, like the hardness indications, had to be reduced statistically so as to get the necessary information. This shows the outline of the platinum wire and then here at higher magnification the actual junctions or welds that provided the thermocouple action.

Well, from that they found that there can be as many as three or four hot spots, as we call them, on individual abrasive grains (Figure 17) and through calculations recognizing the diameter of the wire and the time spacings, really, and the corresponding differences involved in the oscillograph record, they finally came up with the information on the actual grain spacings, which are shown in Figure 18.

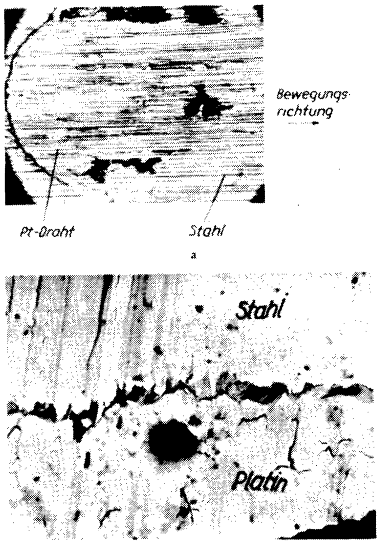

Bild 3 Verschweißung des Thermoelementes Stahl-Platin während des Schleifens
a) Lichtmikroskopaufnahme, b) Elektronenmikroskopaufnahme (× 1200)

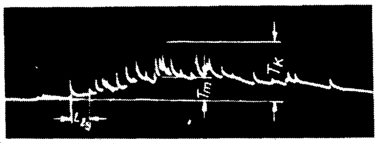

Bild 4 Verlauf der thermischen Beanspruchungen beim Schleifen

Figure 16. Magnified pictures of the end of the platinum wire showing welding with the workpiece on the downstream side. The oscillogram at the bottom is a typical record showing temperature increase vertically while time is increasing from left to right.

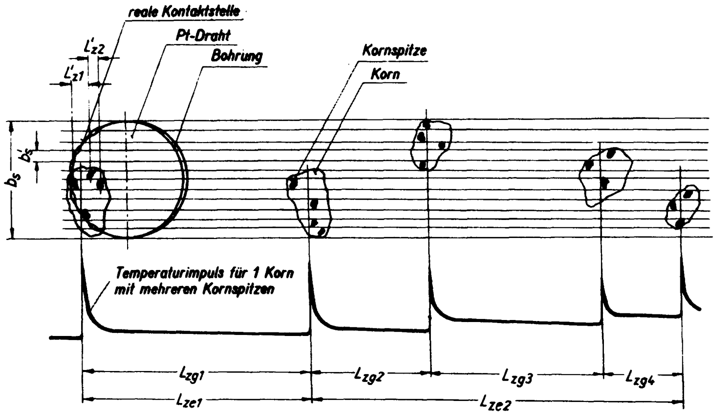

Figure 17. Abrasive grains approach the platinum wire from the right. Metal may be cut in several different places at the same time on the same grain thus leading to several hot spots which produce only one temperature impulse as suggested by the corresponding oscillogram reproduced below the grains.

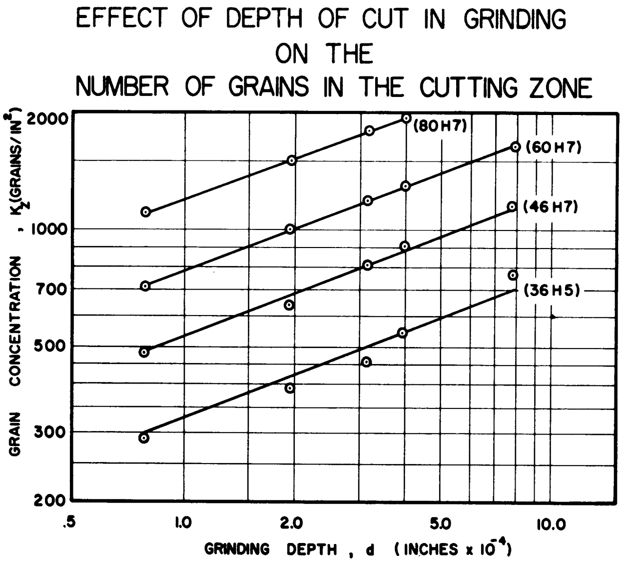

Figure 18. Shows the increase in the number of abrasive grains per unit area in the cutting zone as the depth of cut in actual grinding is increased.

Here are the results for 36, 46, 60 and 80 grit wheels, otherwise substantially the same where the grinding depth, (I have converted this to inches) varied from less than one ten thousandth of an inch up to about a thousandth of an inch — well, a thousandth of an inch would be here and we found out later as they have that these can be extended quite some distance before they actually level off. It is interesting that the slope here, these all being substantially parallel, are around about .37 and the area in between our crushing wheel and the grinding wheel varies linearly with the feed and with the square root of the depth cut. The depth is so small that it happens to fall in that category, the contact area being substantially parabolic. So that the number of grains that one might encounter in the crushing varies linearly with the feed and .5 exponent of the depth, plus any concentration arising from the greater loads, that increase in concentration is .37. We did have one test condition wherein we did get the slope of 1 on feed and a slope of .89 on the depth. There we had sufficient system rigidity so that with a single-pass across, all the abrasive grains were broken out that would break out. In other words, a retrace gave rise to no residual force whatsoever whereas at other conditions — such as at heavier loads with the same system rigidity — we would have some residue elastic force.

Figure 19 is a Table of Composition that was worked up by the Russians where we have the different hardness indications here, the letters E through V and there should be a W out here too, indicating that the volume of pores or porosity percent varied from 49.3 down to 25.5, and then over here with the structures they have the corresponding percentage of abrasive grain and all these figures then are bond. Now actually there is no grinding wheel company in the United States which uses a table like this. Norton, for example, would likely be skewed down here instead of having straight columns, and no two of them use the same formulations which up to the present time, have been company secrets. We are still in the process of smoking this out rather rapidly and as a matter of fact we have already had offers from some companies to publish the formulations which they use. Not all of these combinations are useable. As a matter of fact Figure 20 gives us a good indication of the areas within which we must live. The Germans believe that the useable portion looks much like this. Up in here where we have very small percentages of the bonds, we could not even get the wheels to stick together, so obviously those are not useable. Those down in here are quite akin to native Vermont granite, so they likewise are not particulary useable. Here is the Norton hardness scale. We have increasing hardness toward the right, but the really important coordinants are the changes in ratio of bond to abrasive as we move down. Up here at 0 we have very little bond and practically all abrasive and down here there is a substantially reduced volume of abrasive and considerable bond. It has been indicated, that there are only limited workable regions. For example, with a 46 grit wheel it is suggested that we limit ourselves to this region here; for 36 grit we could use this range down to this point, but only in hardness from 0 on down. I would not go into the details here, but we are beginning to pull some of these things together.

Norton Hardness Scale		E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	V
		Volume in Percent																
Struc-ture No.	Grain Volume in %	49.5	48	46.5	45	43.5	42	40.5	39	37.5	36	34.5	33	31.5	30	28.5	27	25.5
		Bond Volume in Percent																
0	62	--	--	--	--	--	--	--	--	0.5	2	3.5	5	6.5	8	9.5	11	12.5
1	60	--	--	--	--	--	--	--	1	2.5	4	5.5	7	8.5	10	11.5	13	14.5
2	58	--	--	--	--	--	--	1.5	3	4.5	6	7.5	9	10.5	12	13.5	15	16.5
3	56	--	--	--	--	0.5	2	3.5	5	6.5	8	9.5	11	12.5	14	15.5	17	18.5
4	54	--	--	--	1	2.5	4	5.5	7	8.5	10	11.5	13	14.5	16	17.5	19	20.5
5	52	--	--	1.5	3	4.5	6	7.5	9	10.5	12	13.5	15	16.5	18	19.5	21	22.5
6	50	0.5	2	3.5	5	6.5	8	9.5	11	12.5	14	15.5	17	18.5	20	21.5	23	24.5
7	48	2.5	4	5.5	7	8.5	10	11.5	13	14.5	16	17.5	19	20.5	22	23.5	25	26.5
8	46	4.5	6	7.5	9	10.5	12	13.5	15	16.5	18	19.5	21	22.5	24	25.5	27	28.5
9	44	6.5	8	9.5	11	12.5	14	15.5	17	18.5	20	21.5	23	24.5	26	27.5	29	30.5
10	42	8.5	10	11.5	13	14.5	16	17.5	19	20.5	22	23.5	25	26.5	28	29.5	31	32.5
11	40	10.5	12	13.5	15	16.5	18	19.5	21	22.5	24	25.5	27	28.5	30	31.5	33	34.5
12	38	12.5	14	15.5	17	18.5	20	21.5	23	24.5	26	27.5	29	30.5	32	33.5	35	36.5

*As reported by V.J. Ljubomudrov (Bibliography No. [illegible]).

Figure 19. Standard proportions of abrasive, bond and porosity.

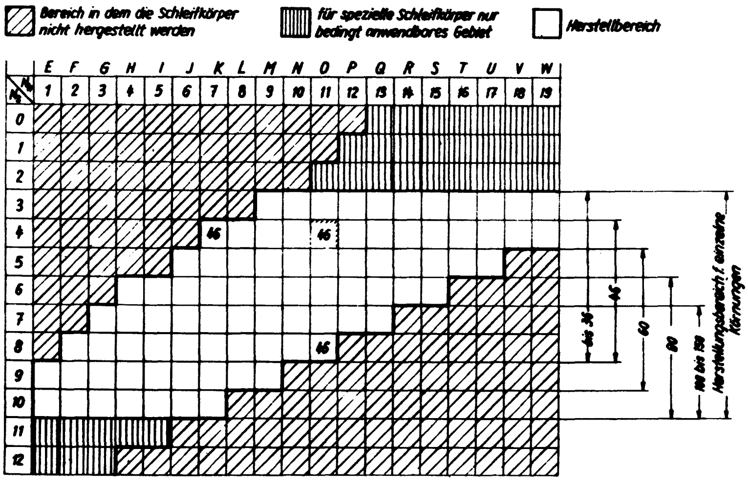

Figure 20. Workable regions of the hardness-structure chart. It is suggested that only the white region is universally useful while the region crosshatched with vertical bars has some limited utility.

A variation on our laboratory technique involved measuring the force as we had done before and on the second pass, not increasing the infeed, but just retracing back across and measuring any residue of forces involved. Figure 21 contains plots indicating that sort of information where this was the force when we made our initial pass and here was the force on the retrace. Tests were made over ranges of both depths and feed. We noticed that these two lines converge at a particular feed level. This has physical significance. It means that nothing has been crushed off in the first pass, and that contact conditions would be totally elastic. We are making very good use of this property in continuing investigations. We like to have dimensionless ratios or dimensionless numbers to express many qualities of properties. A number of such ratios can be evolved from data of this

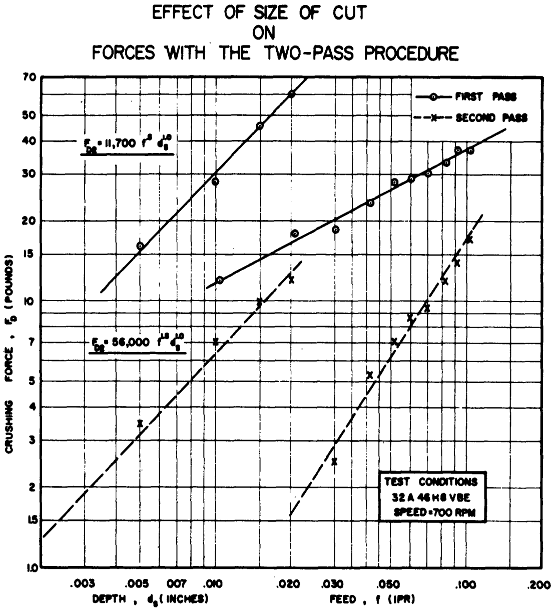

Figure 21. Plot of crushing forces on successive passes in a procedure wherein the cross slide was adjusted inward on every other pass.

sort. We could have this force divided by this one, or this one divided by this one, ratios of differences between them, and so on. We have tested different combinations in this way and I think I have some samples in the next figure. Yes... this you probably can not read either, but it represents quite a range of grinding wheels from four different manufacturers.

Wheel	F_{D1}-Lbs.	F_{D2}-Lbs.	$F_{D2}/F_{D1}(X10^3)$	F_{D1} - %
48A46H8V6	18	2	111	100
-I-	24	3	125	133
-J-	40	14	350	212
-K5-	62	32	515	344
48A46H5V6	41	15	366	228
-60-	50	22	440	278
-80-	90	50	555	500
48A60H8V6	40	10	250	212
(a) 48A60H8V6	41	12	293	228
-12-	54	20	372	300
(a) 48A60H8V6	56	25	446	311
48A60K5V6	87	50	575	483
48A46H8VMM	22	3	136	122
-L8-	40	12	300	212
C60K11B1268	108	92	850	600
-L11-	134	108	810	745
DA60F9V2D	22	2.5	113	122
-G9-	28	5	178	155
-J9-	42	14	343	233
39C60I7V	26	4	154	144
-J7-	34	8	245	189
-K5-	72	40	555	400
32A46H8VBE	28	7	250	155

Test Conditions: Depth Setting: 0.010 inches; Feed: 0.052 ipr;
Speed: 700 rpm.

(a) Test on another wheel of the same specification.

Figure 22. A table of experimental data and evaluations based on an alternate-pass procedure.

In Figure 22 one column lists the forces in pounds for the initial passes for a fixed size of cut, and these are for the retrace. Then we divided the smaller one, by the larger one and multiplied by 1000 to get rid of decimal points. This gave a hardness range, if you want to call that number hardness, from 111 up to 850. Two of them were 850 and 810, which incidentally, were resinoid bonds. All the rest were vitrified bonds. It is rather interesting to point out on this approach that the maximum hardness would be 1000. You would get that same hardness if the grinding wheel was a hardened steel wheel, and also if it were just pure rubber. In neither case would we be crushing anything off and we would get the same force on retrace that we had on the initial pass.

I just jotted down a few brief comments here in the way of summary and then we are going to show you about three minutes of movies of the actual process as we have it set up in our lab. The results of the study up to this writing have established several useful facts and show evidence that still more may be evolved. Among those things which are already reasonably established are that: (1) the crush-cutting process is orderly and susceptible to precise measurements, (2) the measured forces also are orderly and correlate in a logical manner with the elastic and grain concentration factors which Dr. Peklinik in Germany has found to be important in actual grinding, (3) the measured forces reflect all of the elastic properties of the bonded abrasives as well as those of the entire system, (4) effective hardness of bonded abrasives involves not only the strength property which determines the force at which a dull abrasive grain will be released, but also the soft or elastic factors which tend to aggravate and intensify the delerious effects of dullness, (5) grinding wheels can be graded in production competently, quickly and economically, and (6) a universally applicable hardness scale involving dimensionless numbers can be established and corresponding industrial standards can be developed. Further studies along these same lines can be reasonably expected to produce a basis for predicting chatter conditions in grinding. We have made substantial progress in that since the writing of this paper. In addition information on the effect of machine rigidity on grinding behavior is likewise capable of evaluation; there is much more to be learned there. In the area of machine tools, for example, there are no standards relating performance to the rigidity or stiffness characteristics of machine tools. We need to make some rapid progress along those lines because the Europeans have already taken significant steps. The process has industrial applications (1) for quality control within a grinding wheel manufacturing plant (2) for assessing or predicting the actual performance characteristics in the users plant. Perhaps more important is the capability of this process as a research tool in telling us things about the complex structure of the grinding wheel and its behavior in grinding. We were very excited about it and I would be only too happy to entertain any questions you might like to raise at this time.

Shall we have the movies here now. Perhaps that will generate some questions. I have not seen these yet myself, so I do not know what you are going to see. I might point out some of the things here that I recognize.

Movies. Here is the lathe. You see a white grinding wheel here. Here is the actual recorder and strip chart. This happens to be the carrier amplifier that takes the information from the dynomometer. I can tell by the lever settings there that the speed is 319 RPM. That is a 14-inch diameter wheel. Here is the cutter which will roll with the wheel. I presume we will have close-ups later which will tell you a little more about it. I see some of the debris spraying down here. This is an exhaust hose that we set up so that we would not abuse the lathe too much. (Is that in as sharp focus as it can be?) You see a force record accumulating here from this right hand side as a base. These are relatively slow traverse rates. We can traverse across there at least one-tenth of an inch per revolution and gather the same sort of information.

That is one of our graduate students who is carrying out a little project this semester. We have made some provision at this point to introduce flexibility into the system so as to get control over variable system rigidity. We found that the rigidity with this particular system in about 7000 lbs per-inch. It is quite flexible, really. I know now what is intended here — we "rigged" the show by making a grinding wheel which was supposed to be quite defective. So defective that you can see it with the naked eye, but whoever did it succeeded almost inadvertently in getting uniform hardness over the face. You can see the streak here — this was actually a different type of abrasive. You can see the pen climbing up on the reference. You can also see the abrasive streaming down. There seems to be a soft spot near the middle.

Production wheels of the thinner variety, say from an inch on down, may come out substantially harder on one side than on the other by a ratio of practically 2:1. We have found that it is due to friction on the outer rim of the mold. There is need for some research on this; perhaps with the use of ultrasonics or other means for overcoming the friction problem at the mold surface.

These are undoubtedly per-revolution variations in hardness in this general region. This particular recorder happens to be heavily filtered electrically, so we have both mechanical and electrical attenuation. If one feeds this information directly into an oscilloscope he will get a much broader range of variation. Now you see the debris being sprayed down in this region. We have just completed some exploratory work this week where we replaced the cone wheel here with a rather wide disc, swinging the spindle axis around from 45° to parallel with that of the lathe spindle. After applying very light radial loads subsequent to dressing the wheel with diamond we quickly developed a washboard condition, much like you find on the gravel roads that are so common around here. This appears to happen in actual grinding practice. We have known for years that the input power to a grinder spindle, for example, can increase and become asymptotic to a certain maximum level over a substantial period of time and then within a relatively short time interval it will drop off. Well, it appears now that the coarser structure of the wheel which is really coarser than the grains themselves actually breaks out at the time when that power drops off. Well, I think maybe we have had enough. Thank you.

DISCUSSION

MOUZON: Might there be a quick question or two?

QUESTION: I wonder if it would be too trivial a question to ask you why you introduce the word "hardness" into this discussion at all since it seems that in the beginning you sort of set up a strong man and then knock him down. It would be better, would it not, if you just got rid of the word hardness here and used something like quality index or figure of merit or something like this.

COLWELL: I quite agree with you. I think Professor Fitts would characterize this as a communications problem. The word "hardness" has been used incorrectly for so many years in industry that it has developed a particular connotation as an area of difficulty and it is proper, I think, to change it.

QUESTION: The reason you use it, then, is because it is already in the industry?

COLWELL: Yes. Well not only in the grinding wheel industry itself, but the users have their own notion of hardness. In the grinding wheel industry it has been E, F, G, and so on on up and the Germans have proposed even to drop the letter system and work with eighteen stages of increasing levels of force. Incidentally, the Germans have already duplicated the apparatus which we are now using and are off in a cloud of dust to accumulate information of the same sort. They have been sharing it with me and they tell me that the process does work, in their opinion.

QUESTION: From the physicist's point of view, it has always seemed to me that we would be better off it no one ever used the word "hardness", except to let the layman use it to talk about how hard a rock is, or how hard a job is, but it is a term that we can not define in terms of fundamental dimensions and if we would have called it "resistance to indentation" or "resistance to crushing" or "resistance to fracture", or something of this sort...

COLWELL: In that respect, it is something like coefficient of friction which I define as a monument to engineering ignorance. It is purely a ratio that is convenient until we know something better to talk about.

MOUZON: Is there another quick question?

QUESTION: Have you done similar work with belts?

COLWELL: No, we have not. All of this has developed just since Thanksgiving Day. We have been busy as beavers, but we welcome time and support to really get down to the meat of this whole thing.

SOME RECENT RESEARCH ON EFFECTS OF SURFACES ON THE MECHANICAL PROPERTIES OF SOLID METALS

Edward E. Hucke

Department of Metallurgical Engineering
The University of Michigan

SOME RECENT RESEARCH ON EFFECTS OF SURFACES ON THE MECHANICAL PROPERTIES OF SOLID METALS

Edward E. Hucke

Fracture strength of metals is often dependent on the nature of the environment which surrounds the metal, even though this environment may be non-corrosive. In particular, when a metal is surrounded by certain liquid metals, it may fail at a relatively low stress in a brittle manner. In many of these cases, no chemical or dissolution attack by the liquid metal is apparent.

Several workers interested in the theory of brittle fracture have suggested that the fracture stress for brittle fracture should be dependent upon the reversible energy, the so-called surface energy, required to form the fracture surfaces that result when the metal breaks. This energy is dependent upon the environment which surrounds the metal. The general form of the relation predicted between fracture stress, σ_f, and the surface energy γ of the crack surfaces in contact with the surrounding environment is:

$$\sigma_f = K\left[\frac{E\gamma}{d}\right]^{1/2} \qquad (1)$$

where

d is the average grain diameter,
E is the Young's Modulus, and
K is a proportionality factor on the order of 5.

The surface energy, a thermodynamic quantity, also determines the degree to which a liquid wets a solid and is revealed in the "dihedral angle." Figure 1 shows a small amount of liquid metal lying in a corner where three grain boundaries meet. The dihedral angle "Theta" in this Figure is determined by the relative magnitudes of the grain boundary surface energy and the energy of the interface between the solid and the liquid. Figure 2 shows the same angle "Theta" formed when a liquid metal droplet is on an external metal surface, covering an emerging grain boundary. Therefore, one can measure dihedral angles and calculate surface free energies for certain liquid metals and see if the fracture stress of the solid metal does indeed change as the solid-liquid surface energy changes.

Dr. D. A. Kraai performed some experiments to test these ideas using copper wires in environments of molten alloys of lead and bismuth. When the composition of the lead-bismuth alloy is varied, the surface

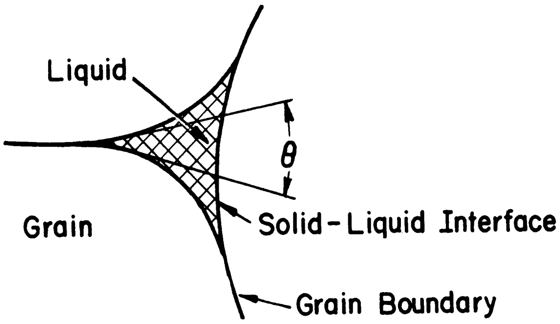

Figure 1. Internal Liquid Phase Dihedral Angle.

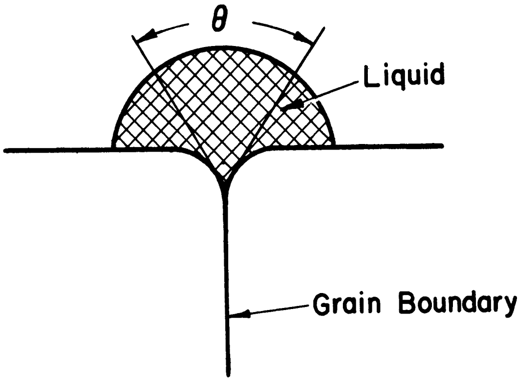

Figure 2. Liquid Drop on a Metal Grain Boundary, Representative of External Dihedral Angle.

energies, as revealed by the dihedral angle, also change. This is shown in Figure 3. Note that the surface energies also depend on the temperature.

Figure 4 shows a plot of the fracture strength and the ultimate strength as a function of the environment. You can see from the lower curve representing the ultimate strength, that going from a lead environment to a bismuth environment at constant temperature resulted in a decrease of strength from approximately 19,000 lbs/sq. inch down to about 9,000 lbs/sq. inch. The fracture strength, however, shows a much more dramatic change; the strength has dropped from about 45,000 psi down to 9,000 psi. It must be noted here that there is no ductility in the range from 0 to about 40% lead, hence the fracture strength and the ultimate strength are about the same, whereas in the high lead region there is substantial ductility and the fracture strength is very much higher than the ultimate strength. What this figure illustrates is that in a non-corrosive environment the fracture characteristics can be changed from high ductility to complete brittleness and the fracture strength can be lowered by a dramatic amount. In other words, the idea that a face centered cubic material like copper is not subject to brittle fracture has to be modified by specifying what environment is under consideration.

In Figure 5 a plot of the quantities in Equation (1) is presented. We have the fracture stress squared on the left hand side and the interfacial free energy plotted toward the right. If the equation were correct we could expect a straight line. As it turns out, it is a straight line over a large portion of the diagram. It is apparent where ductility starts to set in, on the lead side of the diagram. In fact if you were very brash, you would attempt to draw two straight lines with these data. I have refrained from doing that, but I think there is some justification. Certainly the fracture mechanism has changed (brittle fracture to ductile fracture on the right). But in any case, you can see that the interfacial free energy does bear a striking resemblance to the form predicted by the equation. It is a different matter when one checks the constant - it turns out that it does not fit any of the theoretically predicted constants as well as you might expect, but we have a reason for believing that the theoretically predicted constants should be modified.

Table 1 presents data, not only for lead-bismuth systems, but also for lead with tellurium added, cadmium added, zinc added, and antimony added. There are effects in all of these cases. Figure 6 summarizes these data, and shows that, as has been the experience of some other investigators, elements which tend to form intermetallic

TABLE 1

SUMMARY OF FRACTURE STRENGTH DATA

Average strengths of 14 gage copper in liquid metal environments at 650°F, $ZnCl_2$ used a wetting agent.

Liquid Alloy	Weight Ratio	Average Ultimate Strength psi	Average Deviation (%)	Average Fracture Strength psi
Pb/Pb+Bi	0.000	7,220	3.0	7,220
"	0.111	8,020	3.0	8,020
"	0.200	8,810	0.8	8,810
"	0.400	10,700	5.2	13,300
"	0.600	13,300	9.8	18,600
"	0.800	16,000	3.6	31,100
"	1.000	18,300	1.9	44,900
Tl/Tl+Bi	0.050	8,400	6.7	
"	0.100	8,330	5.4	
Cd/Cd+Bi	0.0025	8,180	5.5	
"	0.0075	7,870	2.9	
"	0.050	8,710	0.3	
"	0.100	9,550		
Zn/Zn+Bi	0.0025	8,240	5.6	
"	0.0075	7,700	6.5	
"	0.020	8,250	2.9	
"	0.045	10,800	4.9	
"	0.073	10,200	8.0	
Sb/Sb+Bi	0.0025	11,500	4.8	
"	0.0050	12,800	3.1	
"	0.0075	11,800	3.9	
"	0.050	12,100	1.9	
"	0.100	12,600	5.5	

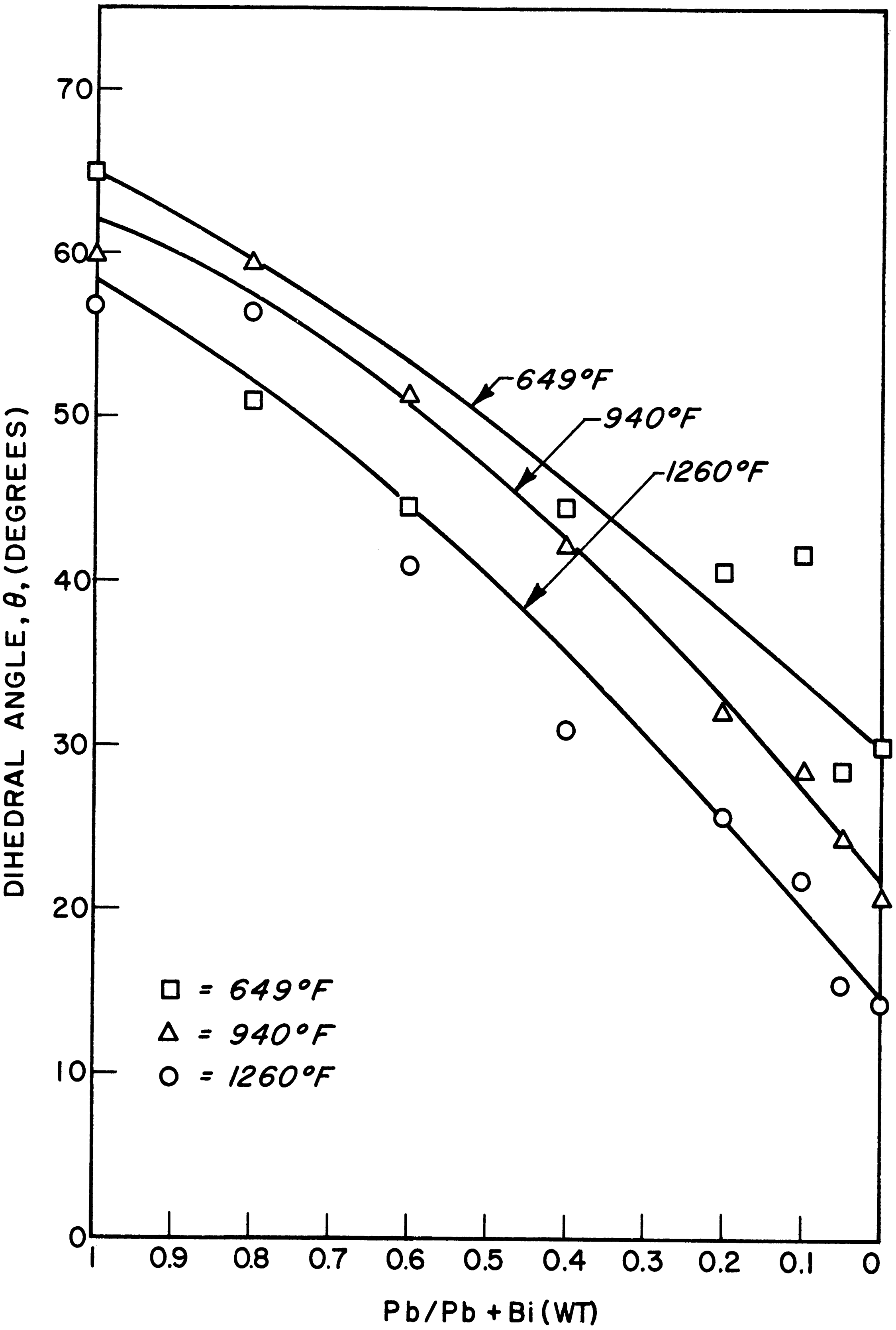

Figure 3. Equilibrium Grain Boundary Dihedral Angles vs. Ratio Pb/Pb + Bi.

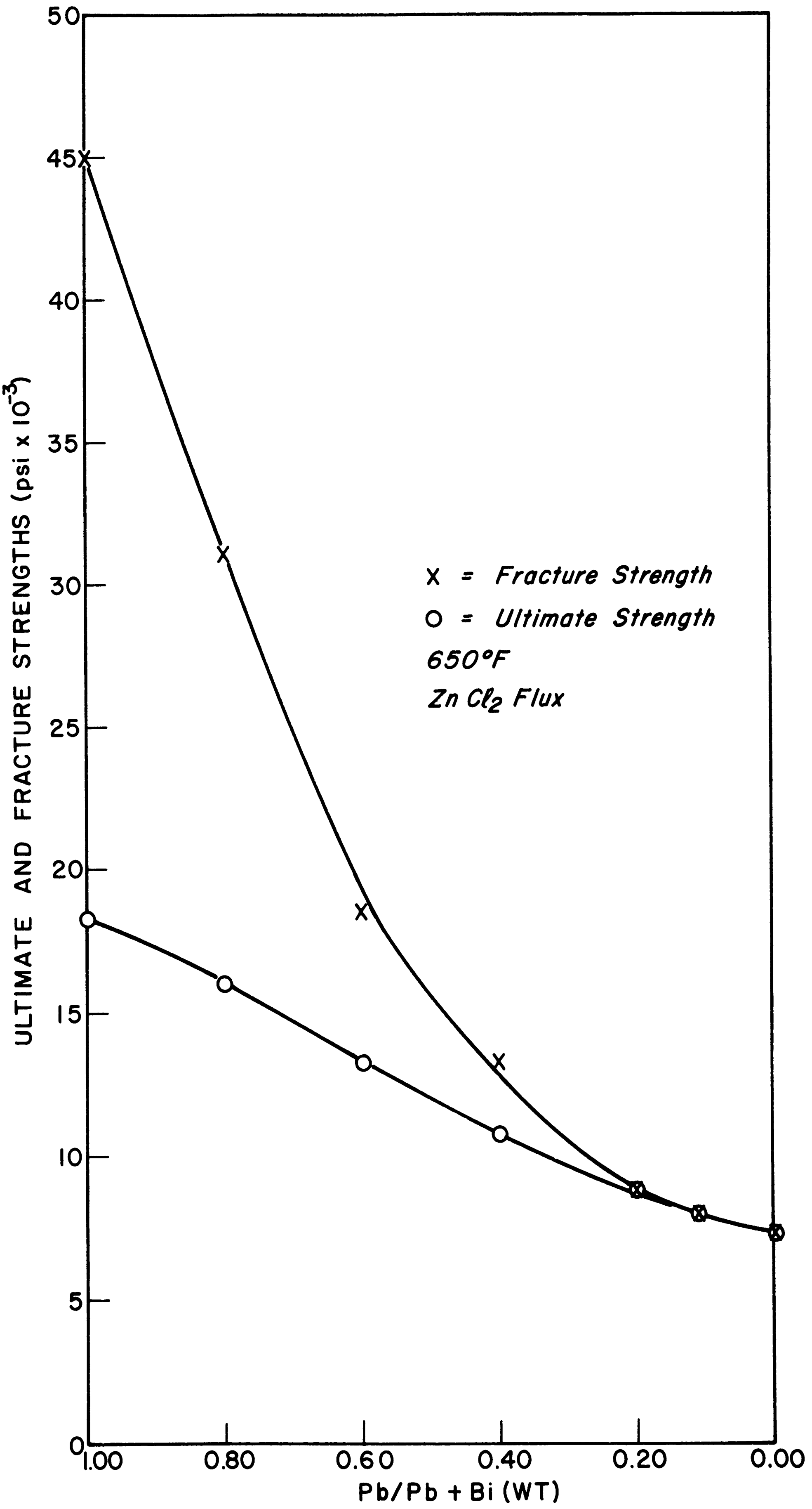

Figure 4. Average Strength of Copper vs. Ratio Pb/Pb + Bi.

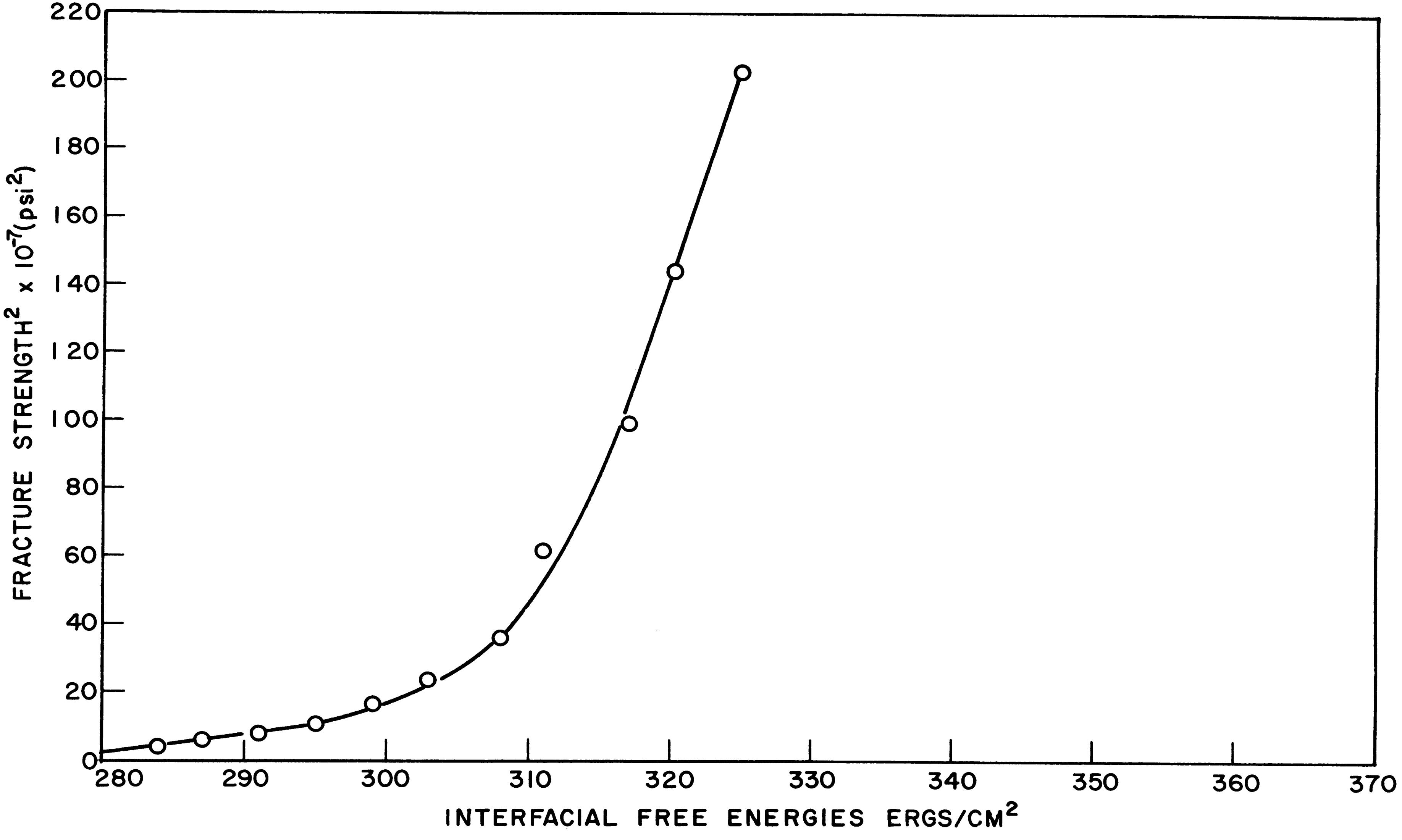

Figure 5. Fracture Strength of Copper vs. Interfacial Free Energies.

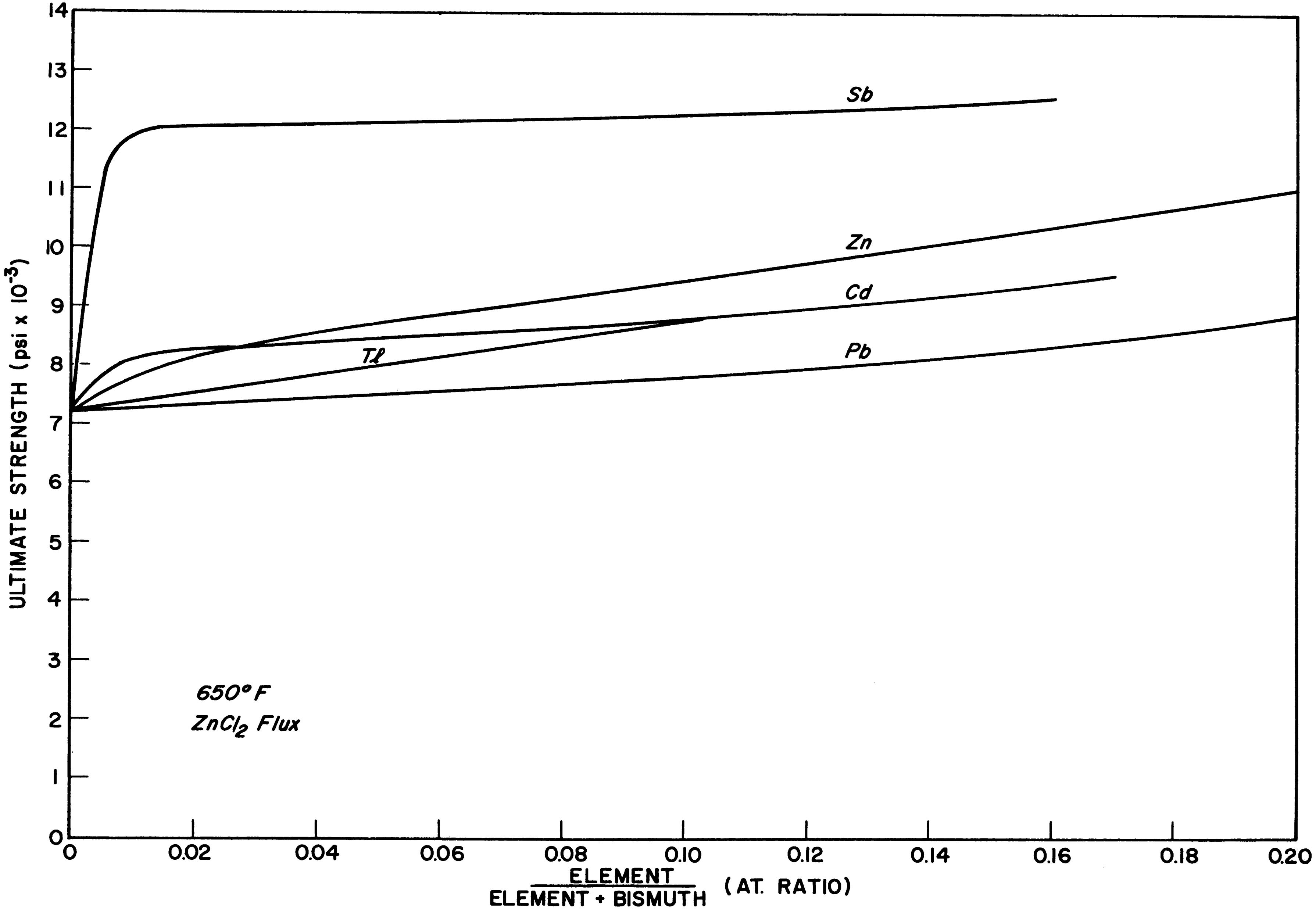

Figure 6. Ultimate Strength of Copper Fractured in Liquid Alloys of Bismuth and Small Amounts of Other Elements (atom %).

compounds are not as bad embrittlers as are those which have mutual insolubility; that is, tend to form miscibility gaps, even when the quantity added is below the concentration where an intermetallic compound can form. Now let us spend just a moment on this figure discussing what could be the practical significance of phenomena of environment embrittlement.

There are a number of practical applications which require liquid metals, such as heat transfer agents as coolants on rocket motors, and various other pieces of hardware where a substantial amount of stress is likely to exist on structural members in contact with liquid metal. Furthermore, we are in many cases in the habit of coating our materials with cadmium. Cadmium plated titanium bolts, for instance, show catastrophic loss of strength about the melting point of cadmium. And we should also realize, these are not the only places where we have liquid metals in contact with solid metals. We have them if we consider internal surfaces as well as external surfaces, in every casting that we make, and in every weld pool that we make. Every time we braze an alloy our object is to wet the solid with a liquid. The presence of a residual stress in brazing often causes a catastrophic crack to propagate from a liquid metal, i.e., brazing metal, which wetted the solid. Indeed there are other internal types of liquids which exist - the sulfides in steel at the hot rolling temperatures and copper as an excess phase in steel. Above the melting point of copper, with steel containing copper in excess phase form, there is roll cracking - severe roll cracking. A small amount of molybdenum, which raises the melting point of this internal liquid phase, will reduce and substantially elimiate the effect. Leaded steel and other forms of special steels above the melting point of the liquid phase show catastrophic loss in ductility in forging operations.

There is another particularly serious consequence of this type of embrittlement, it tends to be much worse in stronger, harder materials. These materials by their very nature store more elastic energy per unit elongation and are far more subject to embrittlement than are the soft ductile materials that you had in the original case. There is the effect of temperature, and there is an effect of oxide and other film formation. Oftentimes we have a basically troublesome environment lurking in the neighborhood, but our solid is, for one reason or another, kept out of intimate contact with this environment by a protective film, except that on some occasion it might be overstressed and cause the film to rupture locally. Or by way of abrasion or some other mechanical action the detrimental liquid or gaseous environment, for that matter, is brought into intimate contact with the solid, which means that a portion is now subject to very rapid embrittlement and catastrophic failure often results.

Another consideration which I would like to point out is the effect of size. In smaller pieces, and notably in whiskers, where the surface is a more dominant portion of the strength of the material, the environment that the surface faces is of proportionately greater importance. This has been reported in the technical literature and it is also true in other things besides metals. The effects that I am mentioning have been borne out in glass, in sodium chloride, in magnesia, and in other materials which are normally thought to be non-ductile. People have found that the non-ductile ceramic pieces can, with a very careful treatment of the surface, be made to undergo considerable elongation, and hence are not inherently brittle. But in many cases they become brittle because of an interaction between the solid and the environment. In general, there are applications in other fields, if you think about cutting metals, grinding metals and ores and so on. Here again we are interested in creating surfaces and any time we can lower the amount of energy required to create surface, we can bring about a change in the gross behavior of the phenomena. Now I would like to treat another related, but sufficiently separate piece of research, the effect of environment on the elastic properties, or the apparent elastic properties of solids. This work was performed by Dr. S. Floreen.

Here in Figure 7 is the apparent Young's modulus of a series of copper specimens of different sizes, A_o being the external area, and V being the volume. You can see with a very careful procedure, it is possible to detect changes in the apparent elastic modulus for a specimen of given size, depending on the environment. These are merely two examples of some of the environments that we have studied. Notice that besides the effect of environment, we have here, really, the effect of specimen size. The magnitude of the changes are not overwhelming - in fact you have to be pretty careful to get good measurements, but they are big enough to be measured. They are not so large as to make you try to utilize the increased or decreased Young's modulus, but they are measurable.

In Figure 8 we have some results in other environments. These specimens have been cleaned in a very special way to insure intimate contact with the environment; a lot of care has been taken to eliminate recrystallization textures, any perferred orientations, and so on. We have here triply-distilled water, and 20% saturated stearic acid solution in water and a saturated stearic acid solution in water.

In Figure 9 we have measurements at three different grain sizes. This is showing the effect of the extent of the internal surface on the apparent Young's modulus vs. specimen size, or area to volume ratio. In Figure 10 through 16 are similar data, taken in different environments. In general, I would like to point out that these Figures all show a linear behavior of apparent Young's modulus vs. area to volume ratio. We have the internal grain boundary area to volume ratio at given external area to volume ratios. This was obtained merely by varying the grain size of specimens which were cut a very special way, and one gets a similar set

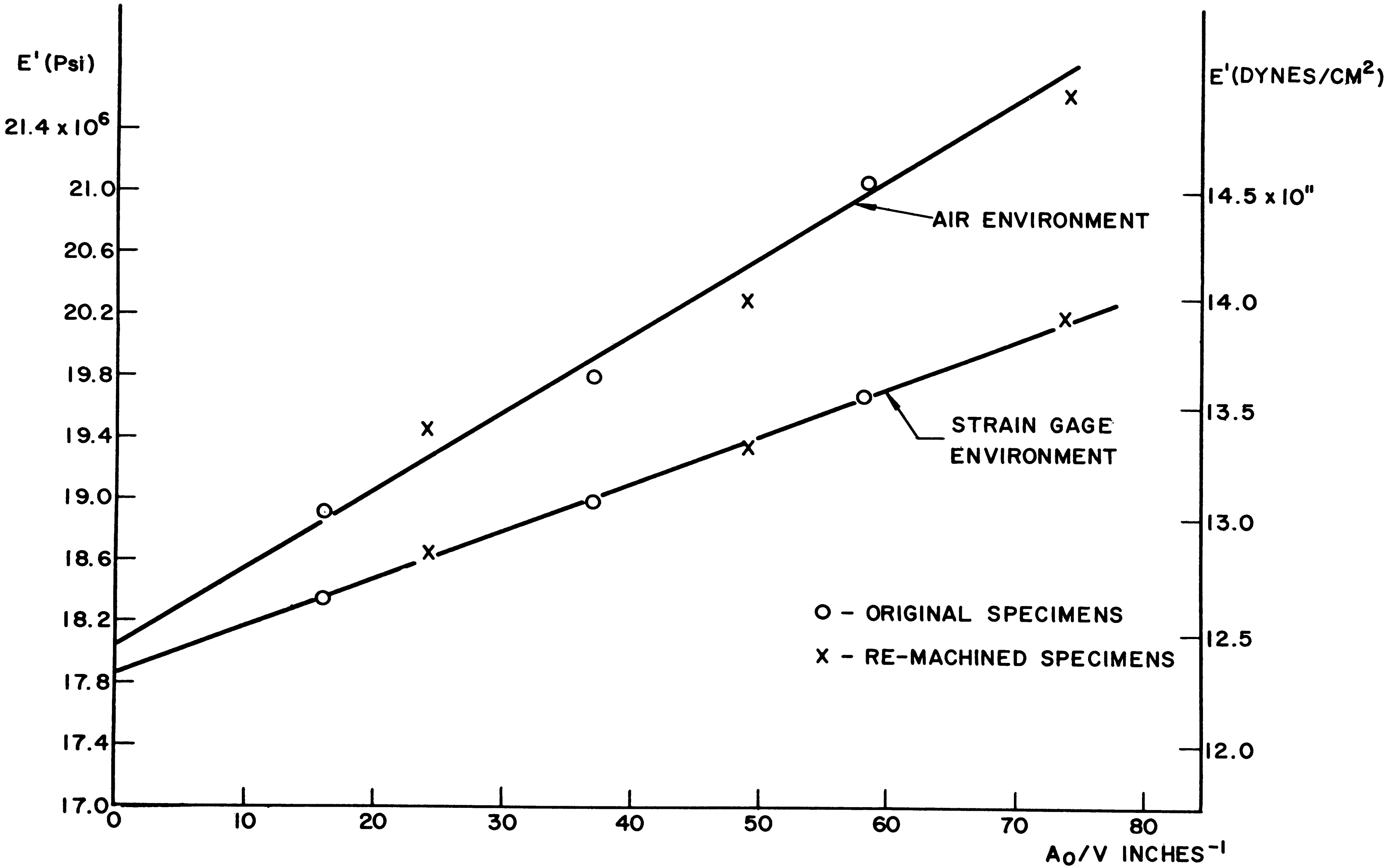

Figure 7. E' Versus A_o/V for Specimens Annealed at 600°F Showing Effect of Changing A_o/V.

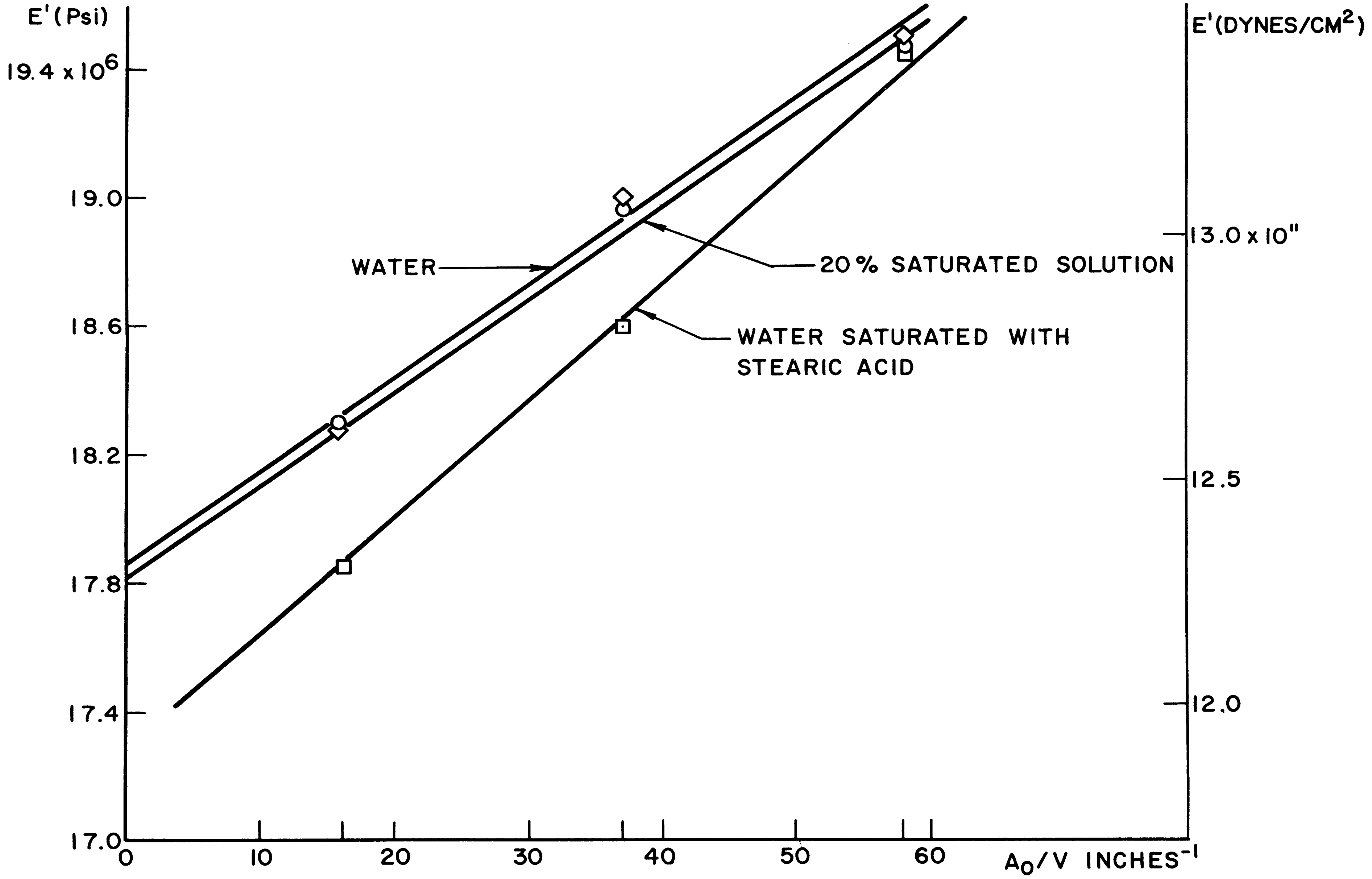

Figure 8. E' Versus A_o/V for Specimens Annealed at 1200°F in Three Liquid Environments.

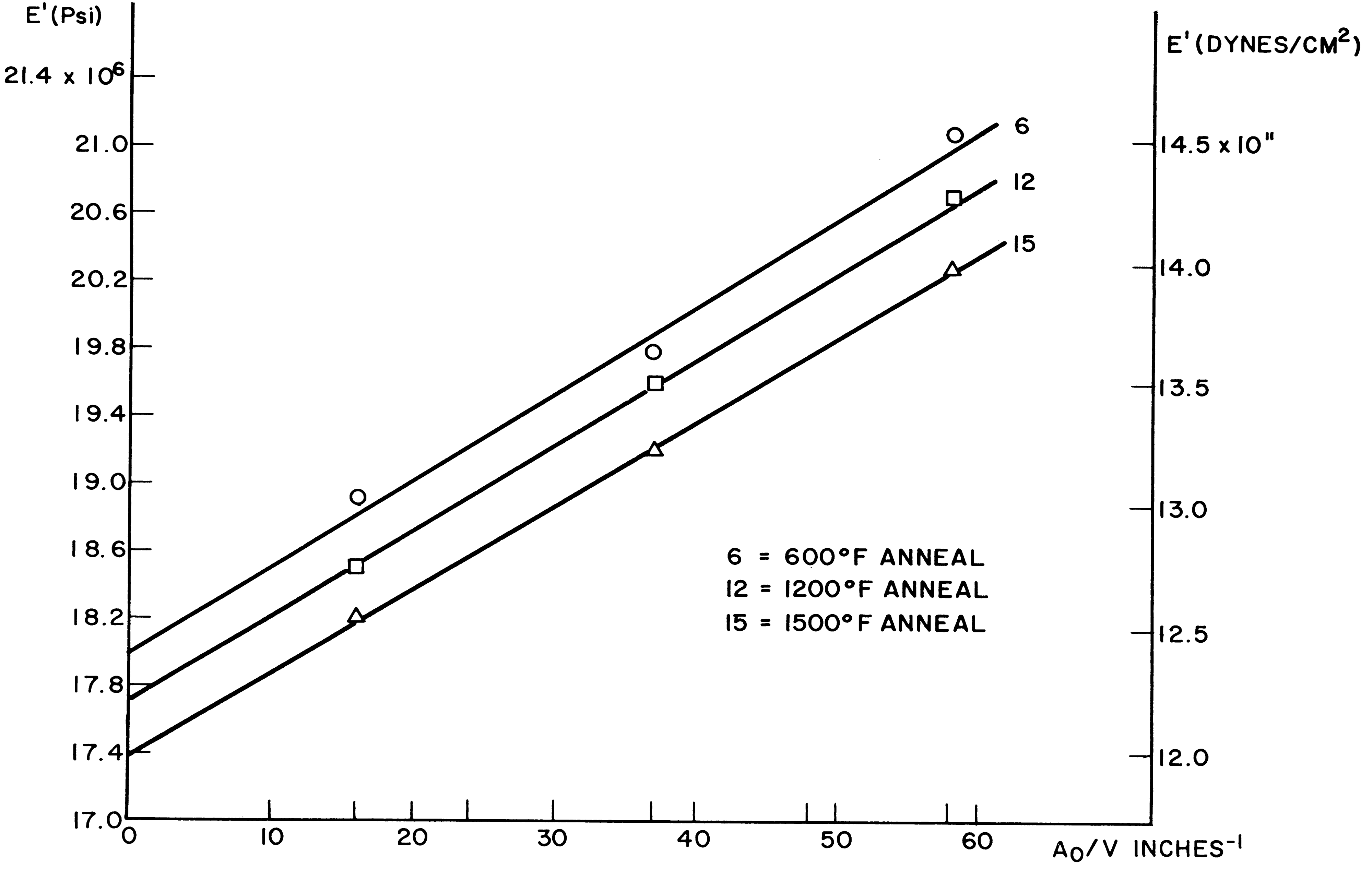

Figure 9. E' Versus A_O/V Air Environment.

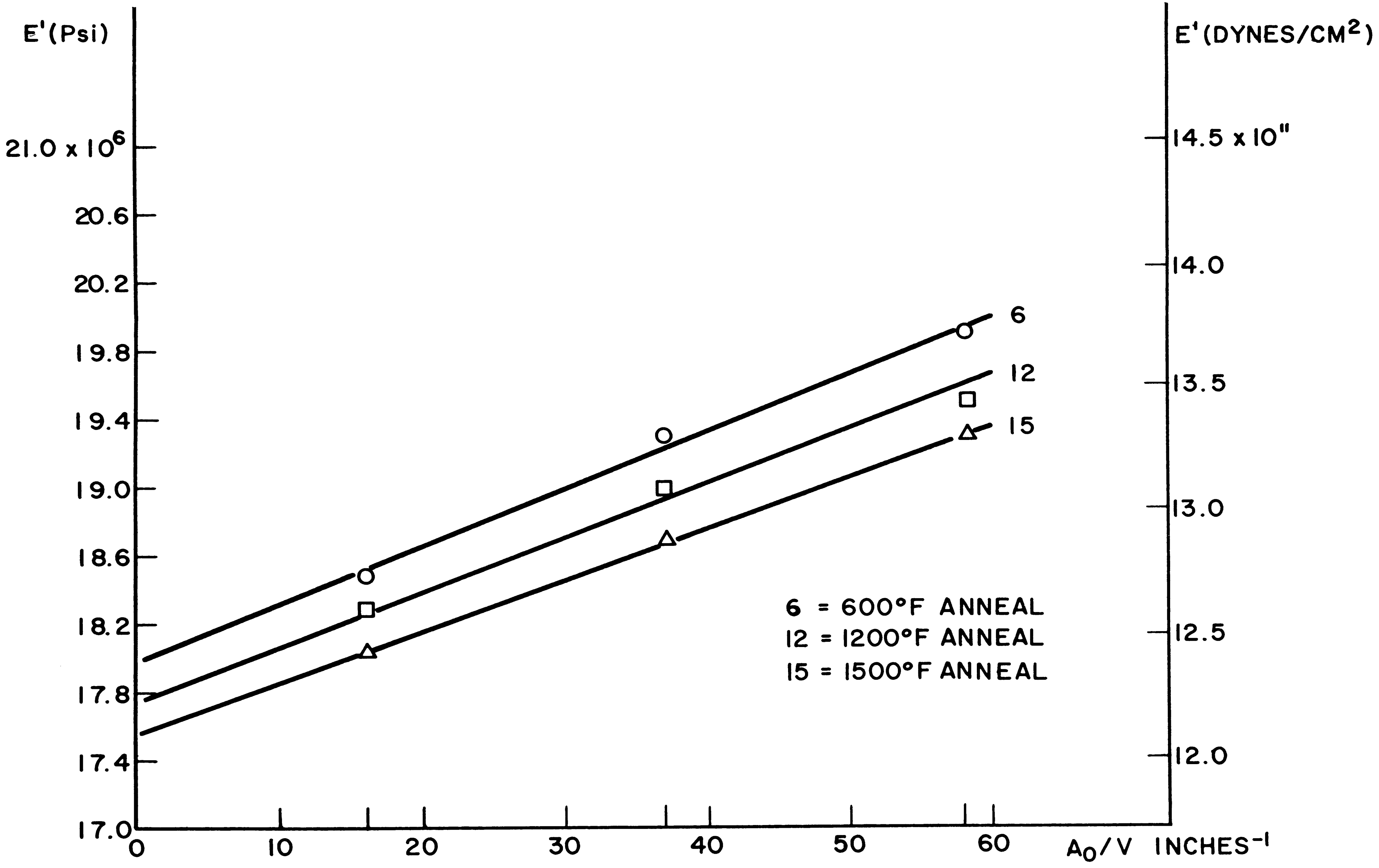

Figure 10. E' Versus A_O/V Water Environment.

of apparent modulus determinations. In summary, there is an apparent Young's modulus, that is the number that you will measure as the slope of a stress-strain curve, depending on what environment you do your measurements in, and depending on the size of the specimen; that is, its external area to volume ratio and its internal area to volume ratio (the grain boundary area to volume ratio).

Now I would like to point out that we did not do this to study Young's modulus. Our reasons for making these measurements were to try to establish the change of the Helmholtz surface free energy which occurs on elastic stressing. At this time we cannot go through the total mathematics involved, but again I will give you an intuitive argument. Essentially the Young's modulus of the piece that we might pull represents how much energy we have to put in to deform this material a given amount, and by measuring how much energy it takes to deform this material a given amount for a set of specimens with different amounts of surface area, we can find out what contributions we have been making towards stretching the surface. We can do the same thing for the grain boundary. In essence what we are trying to do is measure the difference in the amount of energy it takes to deform a big piece and a little piece and the difference in the amount of energy it takes to deform a piece with fine grains and small grains.

For a reason which is not particularly straightforward, one expects that apparent modulus that we measured to be a linear function of the external area to volume and the internal area to volume. From that data, one can derive an equation which is of the following form: that the average Helmholtz surface free energy of a surface at elastic strain ϵ, is the value at zero strain, plus some constant times ϵ^2, and this constant is in essence a surface modulus. It has an analogous meaning to the Young's modulus. Likewise one can derive for grain boundaries a similar equation, and the values that are obtained for these two numbers are on the order of, in the first case, 10^8 and in the second 10^9 dynes/sq.cm. - surprisingly large. What it amounts to is that the elastic strain in a specimen results in storing a surprisingly large amount of energy in the surfaces. This observation helps us to make more palatable some of the discrepancies we have found in fracture theory between the surface free energy that we back-calculate for a solid from fracture equations and the surface free energy which we measure for the same solid, but not undergoing a fracture process. These did not agree before, but one can find out here that with elastic strains at 10^{-3} one can easily get a contribution to the surface energy due to the straining on the order of 10^3 dynes/sq.cm. which is of the same size, or in many cases larger than the surface free energy at zero strain.

Now there is another important conclusion to be drawn here and that is that if the surface free energies change with strain, that environments which are non-wetting with zero strain can be made to wet if we strain them. Hence, if one would consider a copper grain boundary with a globule of lead in it, this globule might not spread or wet the grain boundaries under zero strain, but with the application of strain, we might cause it to wet, and indeed it is now my opinion that most of the trouble we run into in hot shortness, as a general category, indeed results from the changes of the dihedral angles which occur when the specimen is subjected to stress, either purposely or accidentally through constraints during cooling.

Figure 1 merely illustrates a lead globule inside a grain, and Figure 17 represents some preliminary work we did in trying to check out this idea - the median dihedral angle is measured here at three levels of stress at the surface of the specimen, half way in, and toward the center. The data at the surface looked contradictory, or there does not seem to be a change in the same way as at the mid-point. We feel that these experiments need repeating because they were done in air and it is possible that oxygen contaminated the surface layers by a diffusion process. But in the center we find the predicted result, that the application of stress indeed causes a wetting to occur, that is, a reduction in the dihedral angle. Further work along this line is progressing. I have talked too long now, and I think that I will, at this point, open the discussion, and if anyone has any questions, I will try to answer them.

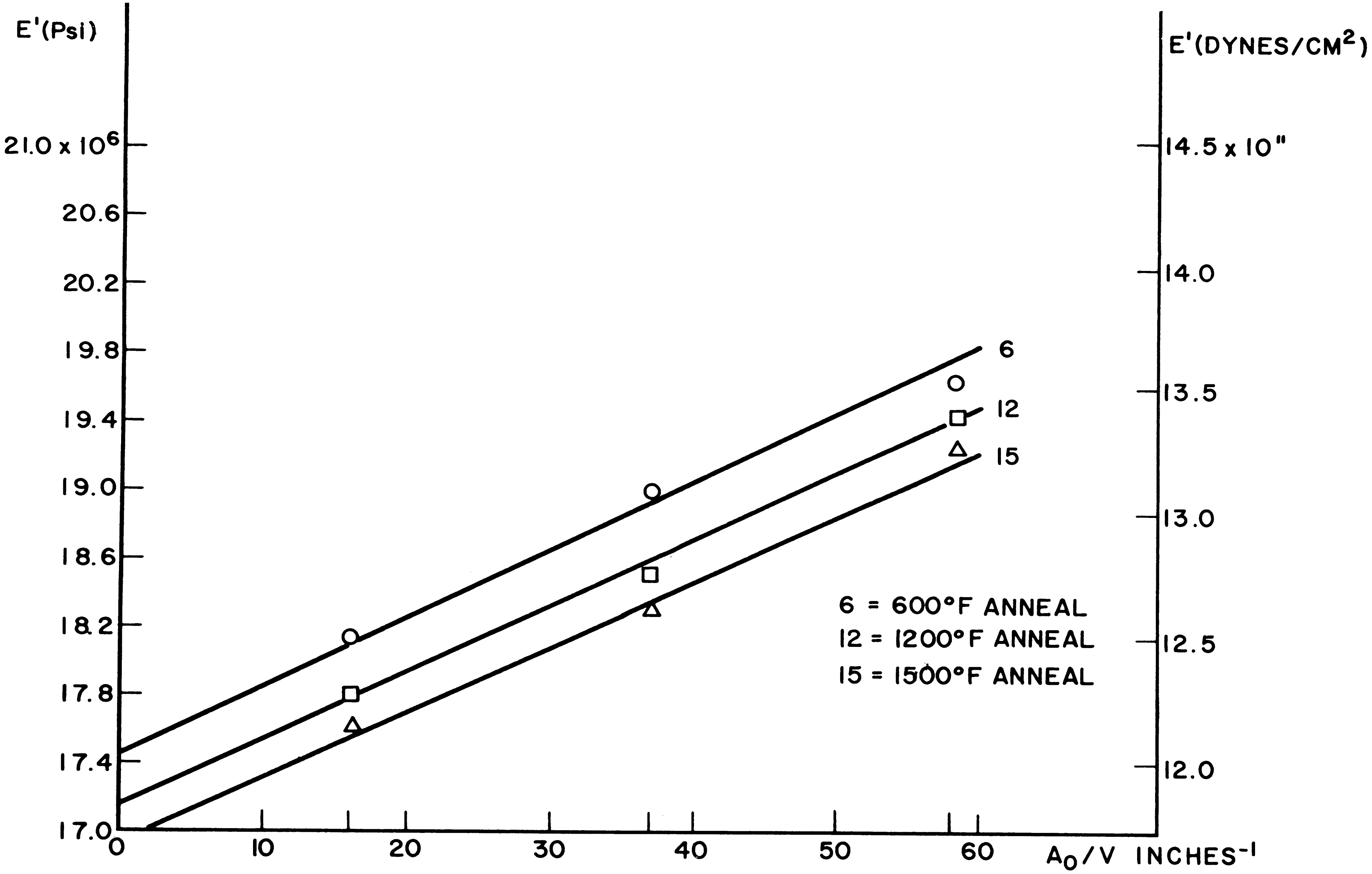

Figure 11. E' Versus A_O/V Water Saturated With Stearic Acid Environment.

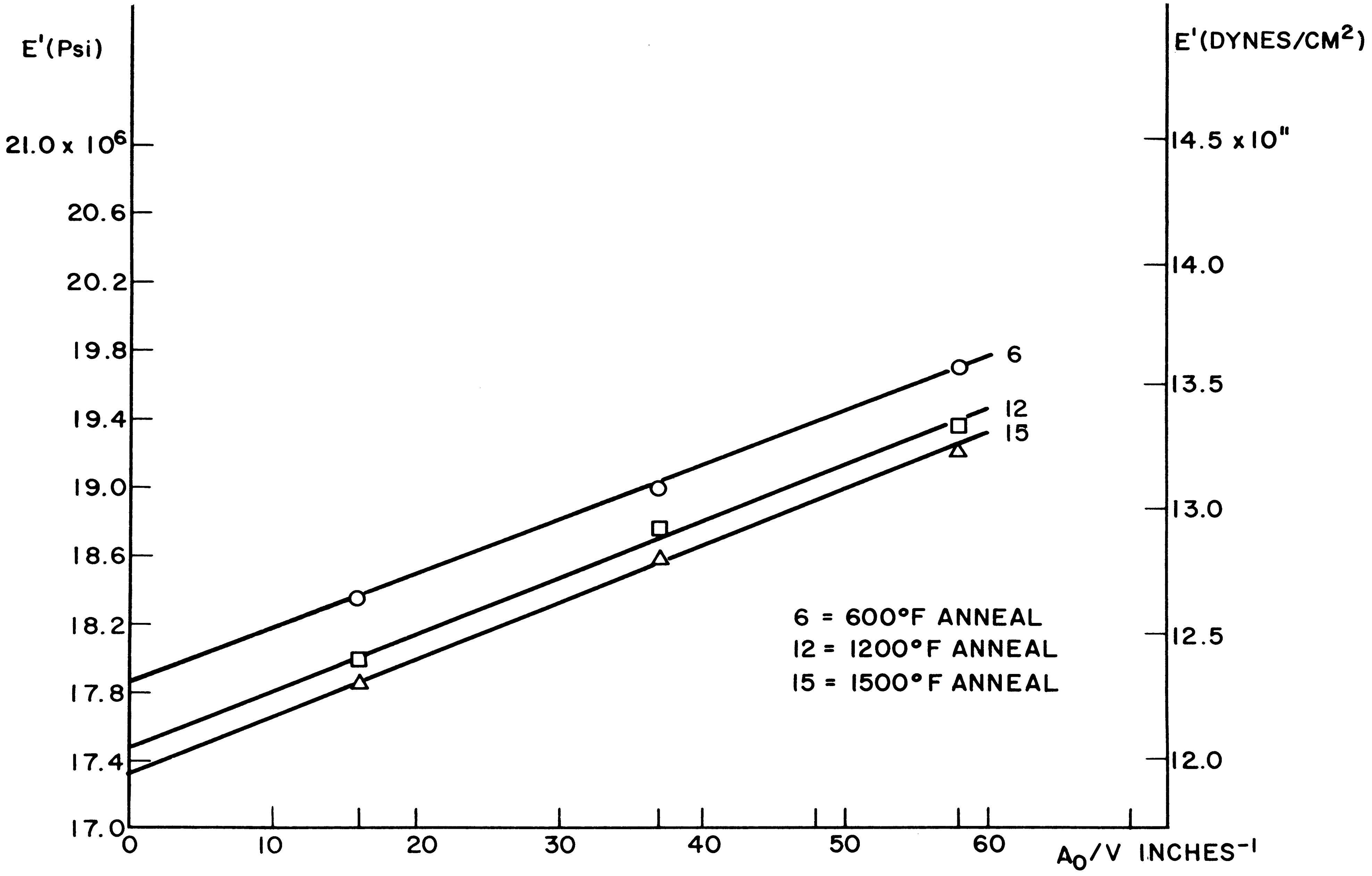

Figure 12. E' Versus A_o/V Water Environment.

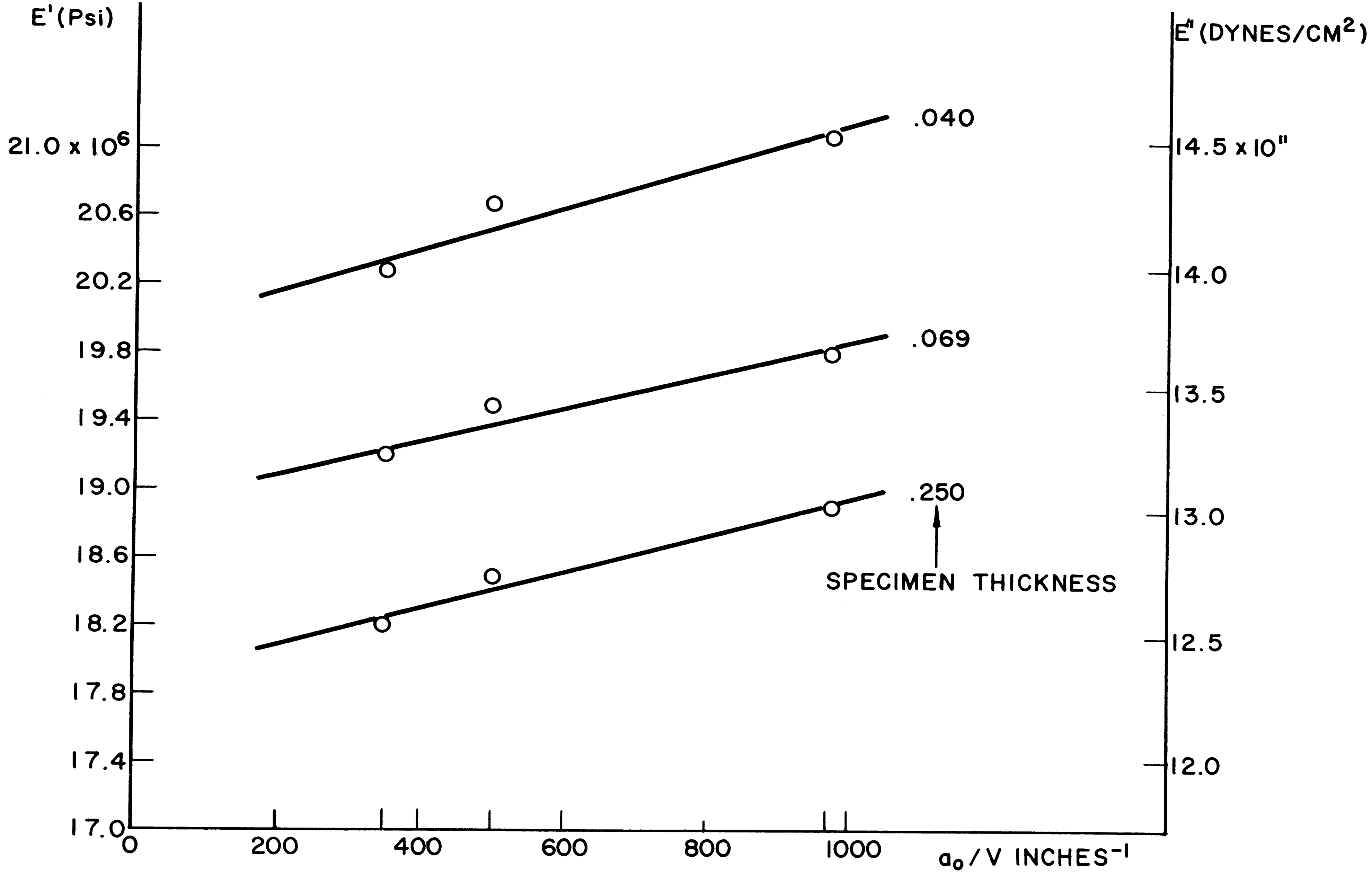

Figure 13. E' Versus a_o/V Air Environment.

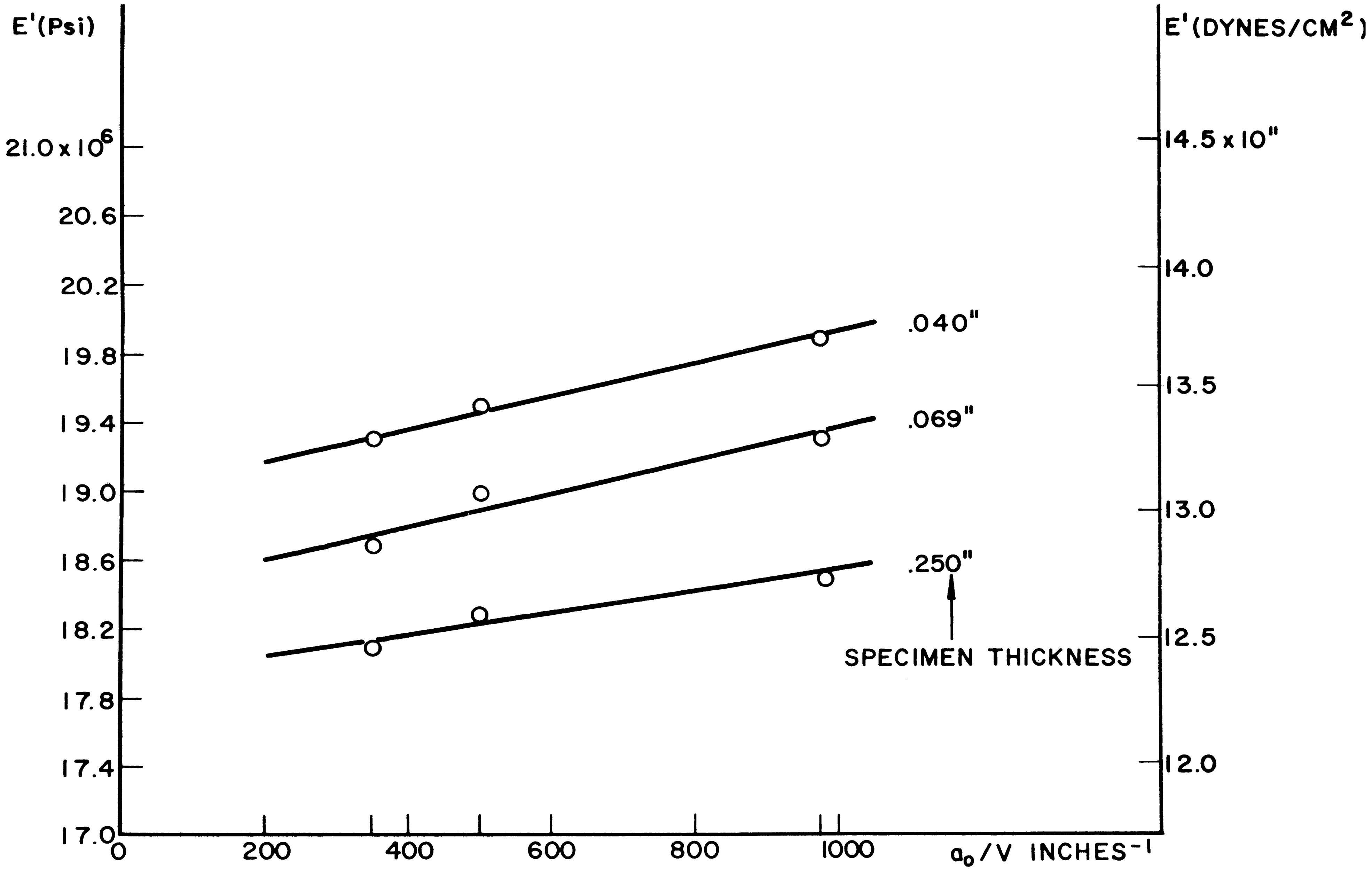

Figure 14. E' Versus a_o/V Water Environment.

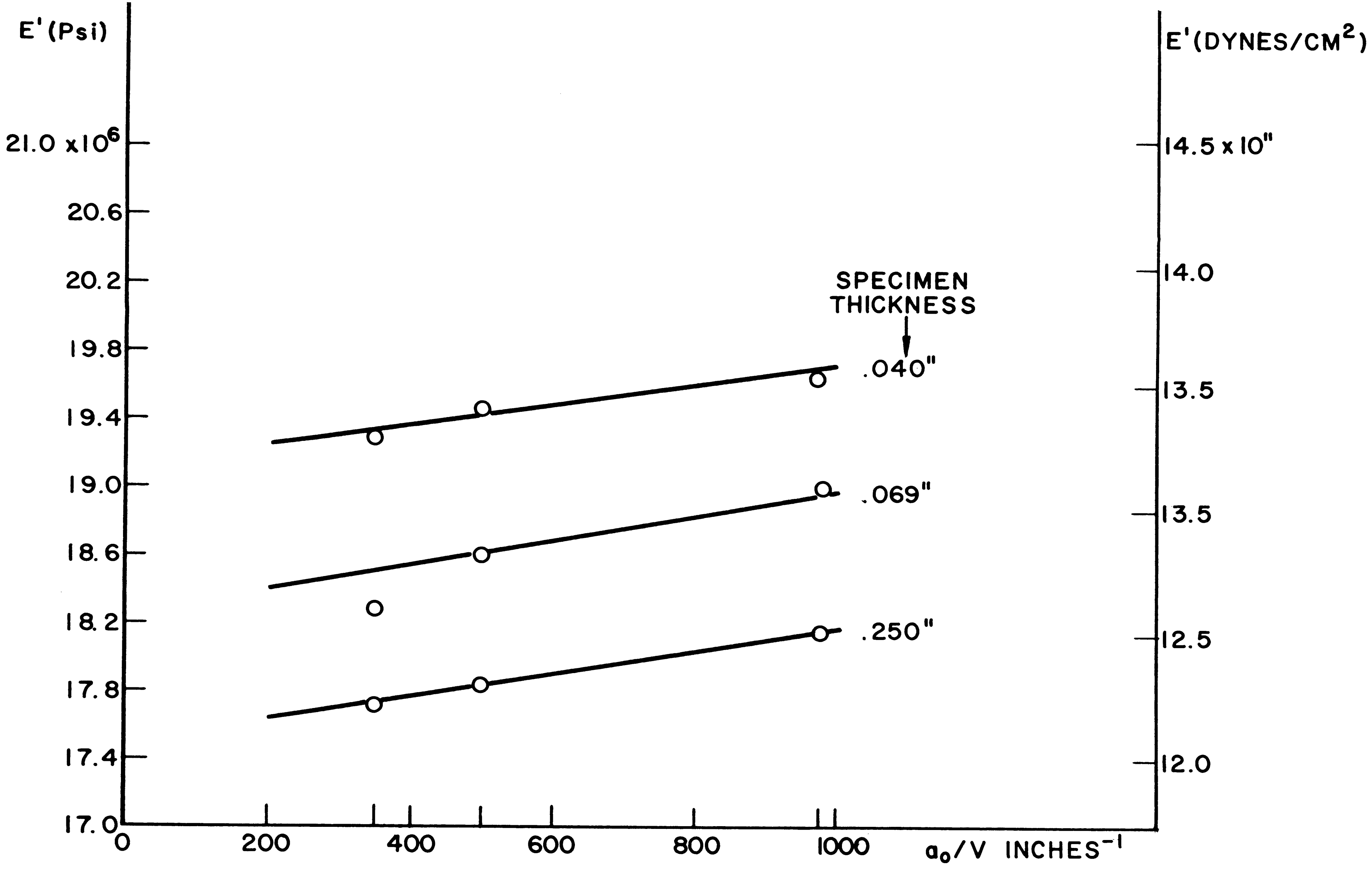

Figure 15. E' Versus a_o/V Water Saturated With Stearic Acid Environment.

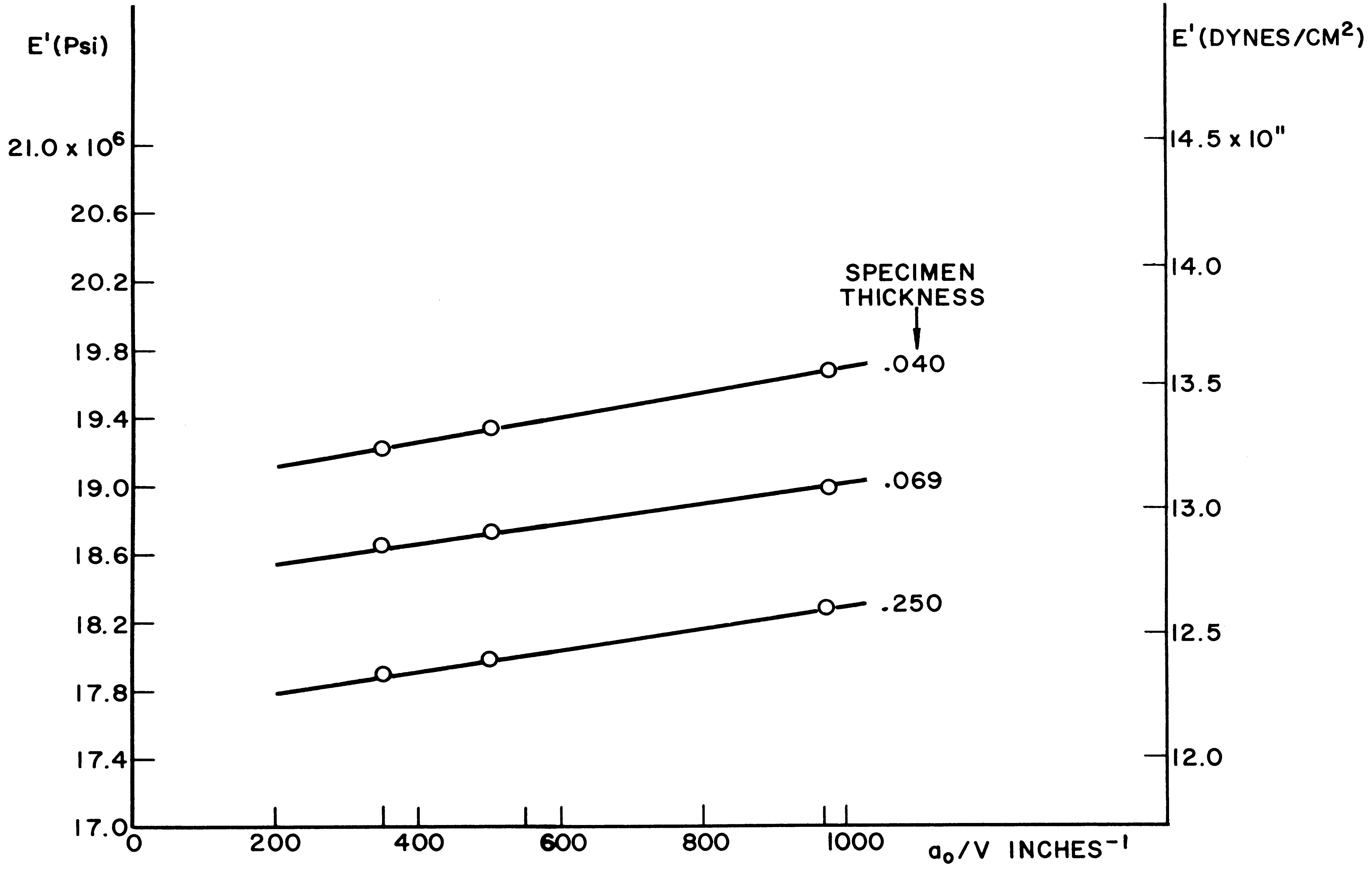

Figure 16. E' Versus a_o/V_o Strain Gage Environment.

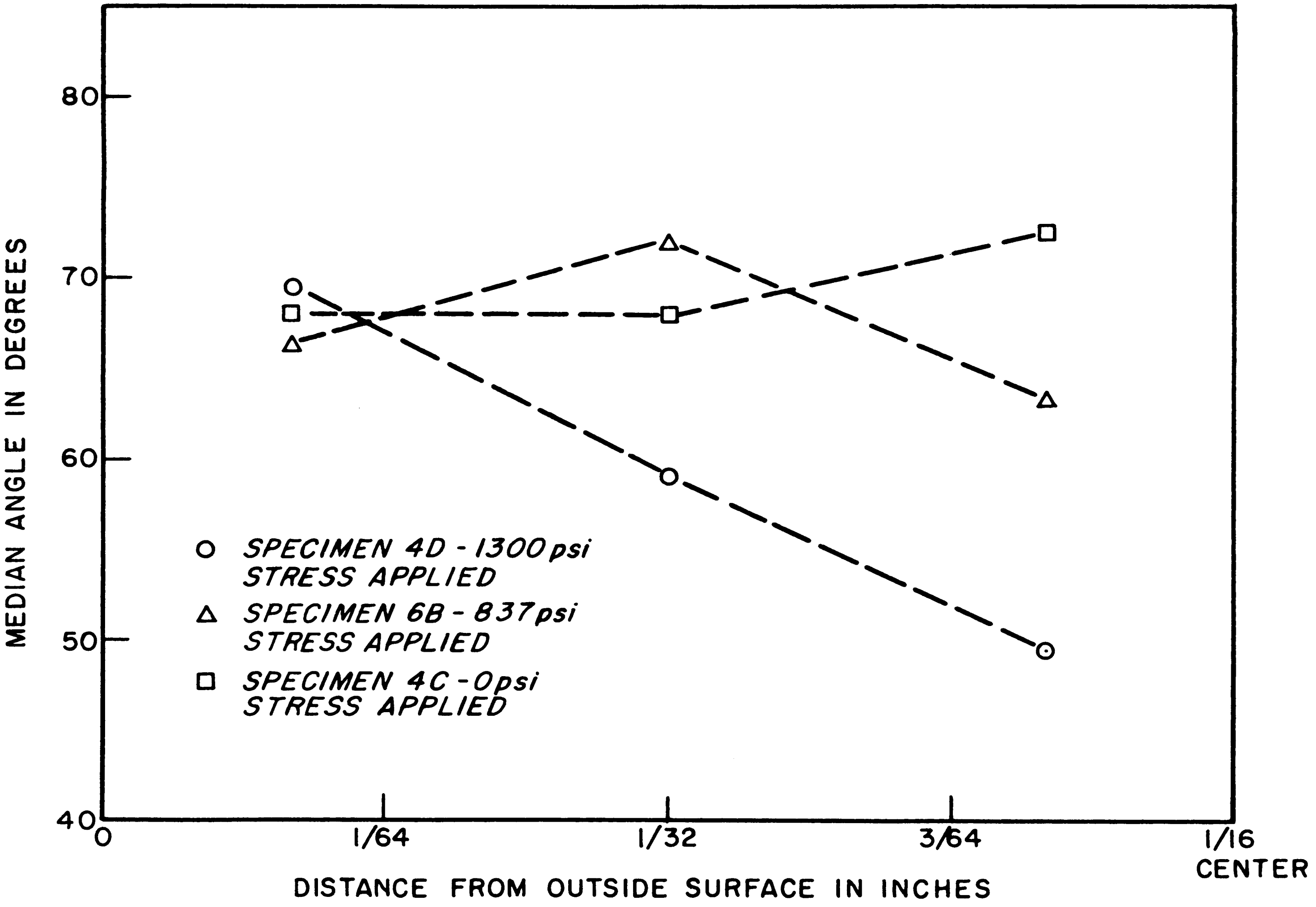

Figure 17. Dihedral Angle Versus Distance From Surface.

DISCUSSION

QUESTION: This is a very interesting presentation, and what you might call, I suppose, the thermodynamic approach. I am wondering whether you can interpret this in terms of a microscopic approach using consideration of dislocations, or by diffusion of intersititial materials introduced by the liquid metal environment.

HUCKE: Yes, one can interpret this. The macroscopic interpretation of thermodynamics is in no way in conflict with the micro-interpretation via dislocation theory. I will point out, however, that in the first set of results I was showing you there is no longer any diffusion because one can make this thing reversible. We can place the piece in the bismuth, let it soak for a hundred hours and since this is a small wire, take it out, clean the bismuth off of it, pull it and we get the same, or virtually the same strength we would get if it had never been in the bismuth. On the other hand if we put it back in the bismuth and pull it with the liquid present, it has a catastrophic failure so there are values, interfacial energies themselves that can be interpreted in terms of the dislocation density on a given interval. But that is the difficulty at present - there is no very good way of quantitatively starting from the atomistic and deriving a number for the interfacial energies. We have a certain qualitative trend, but we cannot really derive a good number. Hence, my approach is this. We will measure a number, via certain auxiliary experiments which may indeed be determined by how many dislocations are present, but the value of that number is what we are seeking.

QUESTION: Have experiments been performed along this line with specimens in fatigue in a liquid to see whether the fatigue is similarly affected?

HUCKE: Yes, they have, and it is similarly affected.

QUESTION: But if you put the coating of lead on there, and you dip this in the liquid and coat it with lead, pull it out and just leave a lead coating on there, you do not get this effect. Is this correct or incorrect?

HUCKE: First off, if you were talking about copper, the lead would run off. It does not ordinarily wet copper. If you could keep the lead on, say in a bath, you would find on stressing that it would start to crack and it would keep cracking; if sufficient lead were present it would at some time stop cracking and proceed to fail in a ductile manner. This is surprising to me because even a very small dab of liquid, I would think, would have lots and lots of atoms to satisfy the surface bonding necessary as the crack opens up. But we have done it many times, and some other people have done it. One puts a drop of lithium, for example, on a steel plate, a hard steel plate, and the plate will start to crack. If it is a small drop it then stops. If you put it in a bath and with the same stress, it will crack all the way through.

QUESTION: It is not simply that there are no atoms getting down the crack - there are plenty that go down the crack?

HUCKE: We do not understand this, quite frankly, but you cannot quarrel with so many results. I do not know how to interpret them.

QUESTION: Are there any environments which actually increase the strength of the material?

HUCKE: Yes, there are many that actually increase it, but you must specify increasing it over what. It is my opinion now that the best you can possibly do is to fracture in an absolute vacuum and that all changes from that level would be downward, but there are many cases where small additions decrease the fracture trend. Oftentimes you are fooled, and we have been fooled in the literature many times by the fact that the environment which is apparently present, is not actually present in terms of intimate contact with the surface.

QUESTION: Suppose one had a tin copper wire at fairly elevated temperature, but nowhere near the melting point of say a 60-40 tin lead mixture. Would you expect embrittlement, or must the coating actually be liquid?

HUCKE: It is much more pronounced when it is liquid. However, there is every evidence both in theory and from experiments, that it is really not necessary to have a liquid phase present. You would only note it at much slower strain rates, but it would still be present, in my opinion. I think there are data to support that conclusion.

THE RUBY LASER

George Makhov

Solid-State Physics Laboratory
The University of Michigan

THE RUBY LASER

George Makhov

I would like, first of all, to apologize for the failure of the equipment which we planned to demonstrate. Something happened in transit - there is a flash tube which is very essential to the operation of the device which got broken and we will not be able to actually demonstrate the device. However, you are welcome to come down after the session and inspect the construction. I have some figures which will show some of the details.

I think I will start following Dean Mouzon's prediction that I would explain the difference between the maser and the laser, and depending on how you look at it, it may be a very significant difference, and then it may not be. The word "laser", as you may know, stands for light amplification by stimulated emission of radiation. Now the word "maser", as originally constructed, stands for microwave amplification by stimulated emission of radiation. However, it has been suggested that it also might stand for something like "money acquisition scheme for expensive research", and this was indeed the case after the ruby maser had been originally operated in our laboratory. The money acquisition became considerably easier than it was prior to that event. The word "laser" may be somewhat misleading insofar as it has led us to believe that amplification is obtained. Up to the present time it has been found impossible to operate a laser as an amplifier. In its present form, it is merely an oscillator which produces light, extremely intense light - exceedingly narrow band light - an extremely monochromatic light and coherent light - and as you may know, it is not possible to produce this type of light by any other means. So actually the word should not be laser, but something like loser, but perhaps you cannot forget that this word may sound like looser and that is not very good either. Anyway, a laser is a device which produces, as I mentioned, extremely monochromatic light - extremely intense light, and it is thought that it produces coherent light.

I am not sure of the backgrounds of this audience, so I think I will start by going very briefly through the principles of operation of the laser or maser, which are very similar. The principle is that we employ transitions between quantum mechanical levels which yield emission of radiation. Let us say we have a system of three levels - three energy states. Just how these levels arise is of no consequence at the present time. We apply an electromagnetic field at an appropriate frequency to produce transitions between level one and level three.

At a certain intensity of the field, which is known as a "pumping field", we obtain the condition where the population at level one, which may consist of electrons, molecules, ions or whatever you have, is equal to population at level three. Now of course this is not an equilibrium condition, though it can be thought of as dynamic equilibrium. That is, as long as the field exists this condition will hold. However, if you remove the field, the distribution of ions on these levels will assume Boltzmann form.

Now when this is achieved, depending on the separations of the levels in energy, and also depending on the relaxation processes between individual levels, we can have the three following conditions: We can have N_1 equal to N_2 equal to N_3. If the relaxation processes are very fast, you can have a condition which is very close to that. Obviously this is of no use to us. We can have a condition where N_3 will be greater than N_2. That is, the population level of 3 will be greater than the population level of 2. In this case you can expect net emission of radiation due to transitions between these two levels. Finally, we can have a condition where N_2 is greater than N_1 in which case you might get net emission of energy between the latter two levels. The important thing is to realize that we have here what is known as inverted population, that normally is, of course, the distribution of ions would be governed by the Boltzmann relation which states that the higher the level of energy, the less the population. Here we have, essentially, inverted the Boltzmann relation, which brings up an interesting point. In order for this condition to exist, you must have what amounts to negative temperature. If you write out the exponential e^{-E}/kT, the only way that you can obtain these conditions, you would have to change the sign of this quantity. It is an effective ion temperature, it is not the actual temperature of the system. Well, anyway, when we have these two conditions, we can get emission of radiation, and this essentially is the principle upon which the operation of the laser and the maser is based.

The difference between the laser and the maser is in essence only in frequency: the maser operates in the microwave region, as the name suggests, 10,000 megacycles, 100,000 megacycles, and we hope it will go into the millimeter range. The laser operates at frequencies something like 10^{15} cycles, which of course is considerably higher, in the near infrared, or in visible regions.

Well, to begin construction - or to begin the research and development of such a device, first of all you have to have the materials. We also have to have a circuit. The emitted energy has to be coupled to some kind of circuit. In the case of microwaves, it is going to be a cavity; in the case of the laser it is going to be something that really no one fully understands. It is a ruby rod with the two ends silvered, which can be regarded as a traveling wave structure - it can be regarded as a cavity, or whatever we please. No one has really analyzed this as yet. Ruby turned out to be the first material in the case of laser and in the case of maser, the first efficient material. Now the name "ruby" may imply that you have to be really wealthy to do research on this device. This is not so at all. Linde Company as well as a number of European manufacturers offer for sale synthetic rubies which are also used for watch bearings and similar applications, at extremely low prices. In fact the price of a 300 or 400 carat ruby is less than $5.00.

The first figure will show some pieces of ruby. This (B) is what Linde calls the dark ruby and it contains roughly 1% chromium. This one does not work as a maser as the concentration of the impurities is too high. Ruby, I might mention, is aluminum oxide with chromium in it. This ruby (A) is the pink ruby which has something like one-tenth of a percent of chromium and this works very well as a maser, and as well as a laser. Now a ruby has a rather simple energy level structure and Figure 2 will show that. This will be applicable only to the laser because of the scale employed. If we were to represent the maser energy level diagram, it would be right down here unresolved. Basically, we have the following: we have two intense absorption bands, one lying in the green and one in the blue or violet, and also some in the ultraviolet and the light which is supplied by a flash tube - which got broken in our gadget - is absorbed primarily in the blue and in the green, and then there is another nonradiative transition from these bands to the R_1 and R_2 levels. These R_1 and R_2 levels are responsible for the very intense red lines of ruby.

Now the transition from these bands to the R_1 and R_2 levels does not radiate. The energy is dissipated in the lattice, that is the energy is given by the electrons off to the lattice. This is a very unfortunate thing as you see by the relative energies of various transitions. Almost half of the energy which is put into the ruby is dissipated as heat. In some experiments that we have done, when we placed a piece of ruby next to an intense mercury vapor lamp, the temperature of the ruby rose something like 500° in 30 seconds; consequently the amount of energy dissipated by this transition is quite high. Now the R_1 and R_2 are metastable states, that is the lifetime associated with these states is quite long, something like three milliseconds. By putting a sufficient number of electrons from the ground state into the absorption bands, we can obtain over-population of R_1 lines with respect to the ground state. In order to obtain the inverted population, we must move from the ground state to roughly two-thirds of the normal equilibrium population. Now the emission occurs at 6934 Å which is a red light. In fact it is right on the long-wavelength border of visible.

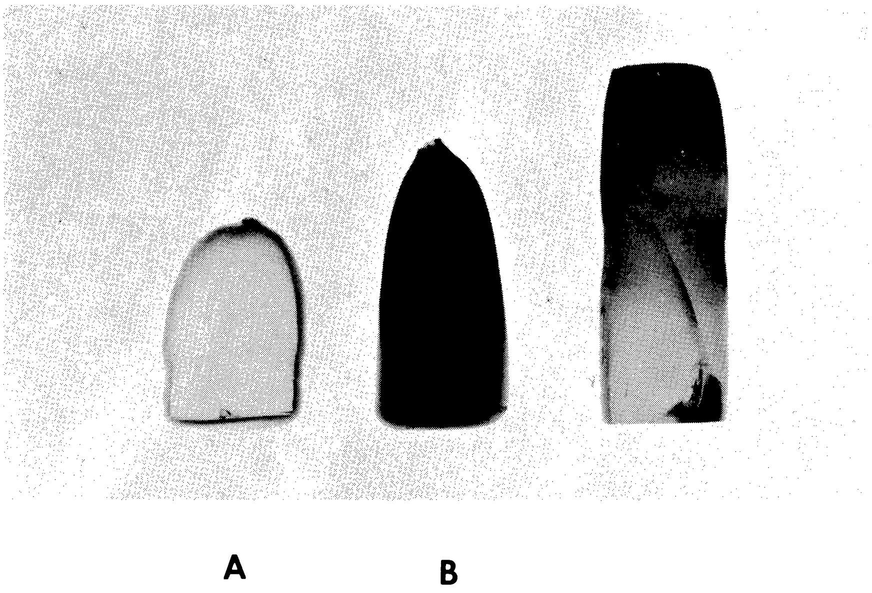

Figure 1

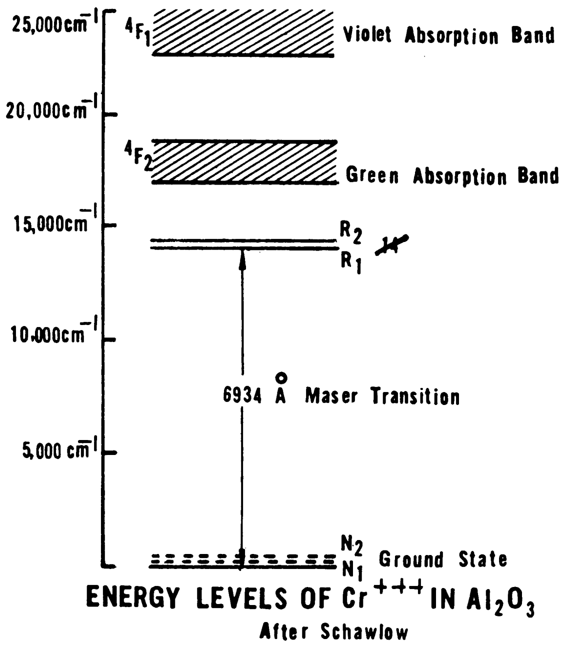

Figure 2

It is unfortunate that we cannot demonstrate this today - but the intensity of light is such that it is quite startling and particularly in view of the fact that the light is on the borderline of the visible, the eye is not very sensitive to this wavelength. If we look a little more at this figure, we see that there are two more states, and one very close to the ground state. In fact they are not to scale, they are closer to the ground state than shown here. The origin of these states is not clear, but they arise only in ruby crystals which contain heavier concentration of chromium, and apparently it is thought they arise from pairing of chromium atoms. You can also get emission to these two states from the R states, just as to the ground state.

However, contrary to expectations, these transitions do not yield an improved laser. The reason I say contrary to expectations is that a much more favorable energy level scheme would be to have, let us say, some 500 wave numbers above the ground state - another state which would serve as a terminal state for the laser transition, and here is why. With these energy separations, you can expect the terminal state, say at about 500 waves above the ground state, to be almost empty, especially at low temperatures. So what we would do then is take a heavily populated ground state and pump electrons into the absorption bands. Then the overpopulation which we would have to achieve at our 1 and 2 states would be considerably smaller in view of the fact that this level, lying at 500 wave numbers, is essentially empty. I would like to show that this condition is satisfied in other known laser materials such as calcium fluoride with divalent samarium or trivalent uranium.

There the energy level configuration is as follows: we have absorption bands which lie essentially in the same place as in the ruby, and we have a metastable state. Then we have the ground state and another state, something like 400 wave numbers above the ground. Now we pump from the ground state into the absorption bands. Then there is another non-radiative transition to the metastable level followed by the laser transition which terminates in a state which is above ground. Normally this state, particularly at low temperatures, is empty and therefore the amount of overpopulation required at this state, in order to produce stimulated emission, is considerably less than in the case of the ruby. In fact, the power required to energize this particular material at liquid helium temperature is something like three orders of magnitude less than that required to energize ruby. We have high hopes that this material will run cw, i.e., in continuous fashion rather than in the pulsed fashion as is the case with ruby. Some people, particularly ourselves, hope to get continuous operation with ruby, but the problem of getting the heat out of the sample at the present time appears to be rather difficult to solve. Well, this is the principle of operation in the laser materials that we know.

How do we proceed about making a laser? We take a piece of material and shape it into the form of a rod. It may be of square cross-section, it may be of round cross-section, it does not matter. The rod is normally about two inches long. The ends of the rod are polished to within a fraction of wave lengths flat and parallel. The ends are plated with silver - one end completely silver plated, the other is semi-transparent. This rod is placed into a flash tube, the flash tube is connected to a condenser bank, the condenser bank is discharged, and out of the rod comes a beam of light which is extremely monochromatic and which is also thought to be coherent. The condition to be satisfied, at least in theory, is the usual one. The reflection coefficient of the ends times the exponential of $-\alpha L$ is equal to 1 where α, as in this case has to be negative so you have net gain per unit length in the rod. This is a well-known oscillation condition. There is something very surprising about ruby. It has to be a single crystal in order to have the particular energy level scheme, and when work was originally started on the maser it was contended that ruby manufactured by Linde was a single crystal. Now about two years later we found out that it is not a single crystal. It consists of a tremendous number of minute crystals which, fortunately for our work, have axes - optical axes, lined up to within a degree of arc.

Now at first thought this would be extremely detrimental to the operation of the laser because you would have a great number of grain boundaries which would scatter light. It is detrimental to the operation of the laser all right, but it still works. It was found, for example, that if you grow a perfect crystal of ruby, which can be done in the case of very small crystals, you can get laser operation in a piece of ruby only 5mm long. This cannot be done with an ordinary ruby rod. Now Figure 3 essentially describes a complete laser - we have the ruby rod right here, and the spiral flash tube is placed above the rod, and the whole thing is placed in the reflecting can, which allows us to get as much light as possible into the rod. The tube is then connected to a condenser bank which is charged by a power supply. In the system we use we have a 385 microfarads block of capacitors which is charged up to roughly 4000 volts. This voltage is not high enough to initiate the discharge so we use a trigger of something like 10,000 or 15,000 volts which starts the discharge in the tube and then, of course, the tube fires. From the figures I gave you, you can compute the energy required to energize this laser. It is roughly 2000 joules with a peak power in the order of megawatts and peak currents of the order of 1500 amperes. The duration of the flash is something like 200 microseconds. The duration of the laser output is perhaps 300 or 400 microseconds, sometimes as long as 1 millisecond.

Figure 4 shows our first laser - and the capacitor bank is right here. The charging supply is right here - the tube

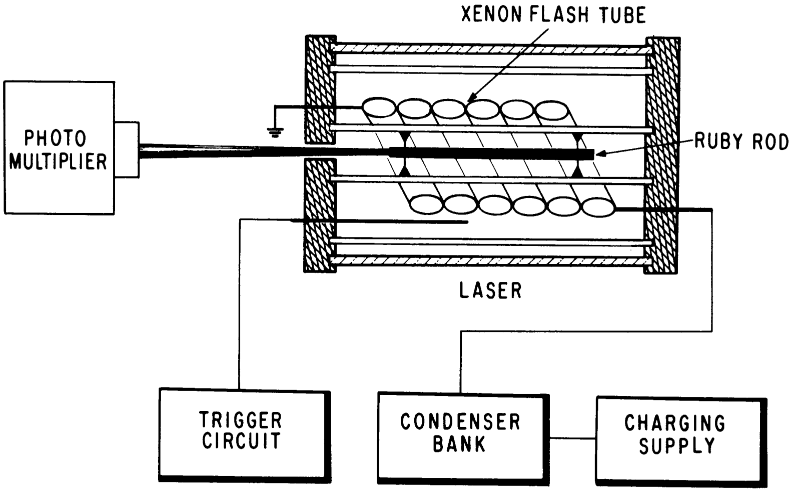

Figure 3

Figure 4

and the rod are in this can. The reason for the hoses at the end is to prevent the light from the tube from getting out because we study the laser beam by reflecting it from this mirror end into a photomultiplier and of course the light from the tube would give us a very peculiar effect with a photomultiplier. It is a very simple-minded system, in fact right now we only use this part for power supply. As you will see in Figure 5 the tube has been placed on a telescope. Now this is an attempt to build an optical radar. I should not say it is an attempt, it has been done by us and by others. It is a device which really will give very good resolution if you consider the size of the antenna, which is the lens. One thing I neglected to mention is that in most cases the device will operate in a plane parallel mode, meaning that the wave front which emerged from the end of the rod is a plane wave, and consequently the beam has very little divergence. The best beam we have observed has a divergence of something like six minutes of arc, this is without any collimation at all. The reason you go to the telescope with a larger aperture is because the diffraction limit is determined by λ/d, λ being the wave length of light, and d the diameter of the objective lens. Now, a laser beam has some divergence, however small. It would be better to use this divergent beam and put it into a telescope with the large objective, in this fashion increasing the d and decreasing the diffraction limit.

Now to give you some performance figures. This unit has been operated at night. The reason is that we do not have a good filter - interference filter, which is very expensive. We can get returns from about 12 miles from a reflecting target. We can get returns from about three miles from a non-reflecting target. There are two telescopes. The laser is connected to one and the photomultiplier is connected to the other. The whole thing is cooled - the laser rod, I should say, is cooled by liquid nitrogen. The liquid nitrogen is boiled and by means of a small compressor we can get a stream of cold air flowing past the rod which cools the rod and in this fashion improves considerably the performance of the laser. The beam goes out and whatever return we get goes through the second telescope into the photomultiplier and of course it goes to an oscilloscope. This is the same power supply we had on the first laser, which as I mentioned consists of 385 microfarads charged to something like 4000 volts.

Figure 6 shows a typical output. The tube was fired right here, and as you see it builds up and drops off roughly here. The laser rises quite rapidly - I think the rise time is on the order of microseconds. Then it is essentially flat and then falls off. The pulse duration, as I mentioned is up to a millisecond, normally something like 400 or 500 microseconds. If you increase the horizontal gain, you see that there are small oscillations riding on top of the pulse. By changing the reflectivity of the ends and also the temperature of the rod,

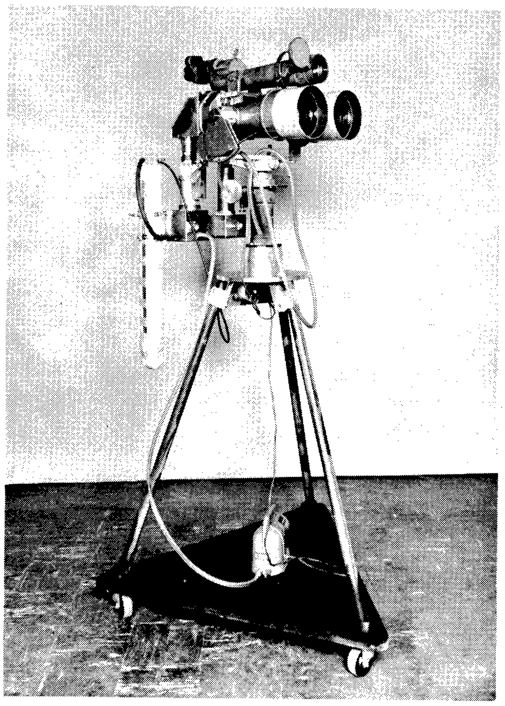

Figure 5

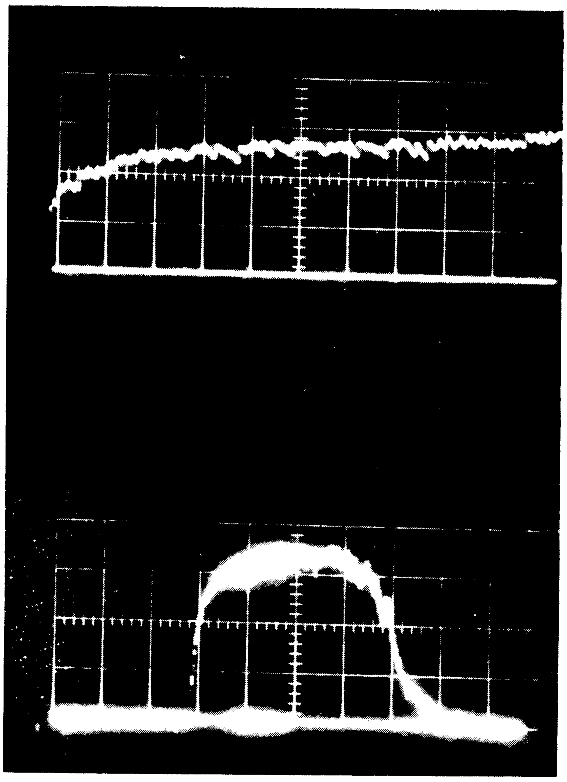

Figure 6

you can make individual oscillation pulses go down to the ground, in which case you get decaying pulses which are on the order of a microsecond. Perhaps this can have application to radar, but as yet this is not clear.

Figure 7 is a somewhat mysterious photograph which is actually a photograph of the end of the laser rod, taken with the camera quite close to the rod and a thick pile of filters to cut down the intensity of emission. As you see, the rod is roughly this large and as you see, it is not emitting uniformly. The reason for the existence of these filaments is that the surface of these two ends is not perfect and at certain points conditions are not favorable for the existence of oscillation. I might mention that due to the fact that the wave length generated by the laser is so much smaller than the dimensions of the rod, the device operates in an enormous number of modes. It has been estimated that at something like 100,000 modes, operate at once. Apparently where the dark spots occur, the conditions are not favorable because of the imperfection on the surfaces of the ends of the rods for further oscillations to occur.

Now the laser can be used to take pictures at long distance, and Figure 8 will show that. The camera was slightly out of focus, but this was taken at something like 200 feet with the laser flash and there is some thought that this might have some applications for aerial photography at night at long distances. And finally, Figure 8 will show that not all laser experiments end up very successfully. What happened is that in an endeavor to cool the rod with liquid nitrogen some of the liquid splashed on the hot flash tube and the resulting chaos occurred.

One logical question would be, what is the use of the laser? Well, one I already mentioned would be radar. Quite a few government agencies are very much interested in this - Signal Corps, Army - they would like to have range finders for tanks, for example, or artillery. Surprisingly, it is not classified. Everybody knows about them, everybody builds them. It is not a very expensive proposition and can be built for several thousand dollars. I guess the condenser bank is the most expensive part. This particular application makes use primarily of the non-diverging property of the laser beam. Conversely, we can focus the laser beam, since it is a plane parallel wave and if you have a good lens, you can focus this laser beam down to a minute point, theoretically of the order of wave length. Now the energy density of the laser beam is such that if appropriate focusing is achieved, we can, at least in theory, get enormous temperatures. I should not say in theory, because we have done this and our estimates are that we had temperatures on the order of 10,000°. The experiment that we have done is one that I hesitate to mention because it is really sort of a stunt.

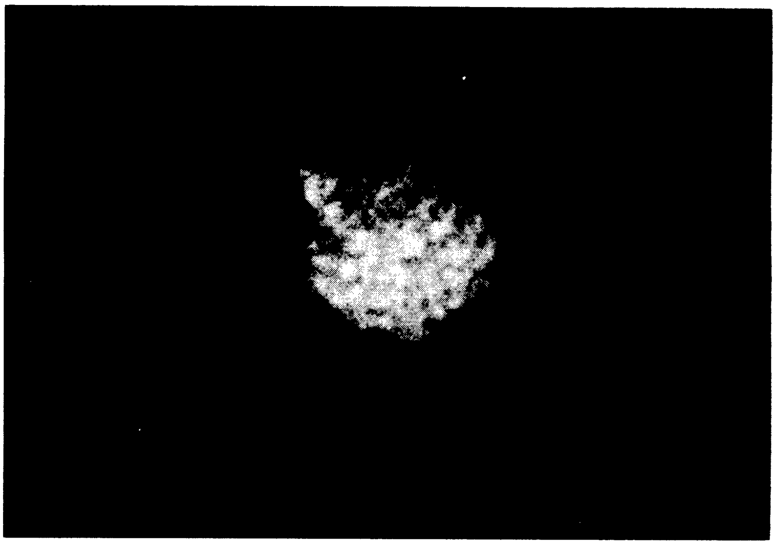

Figure 7

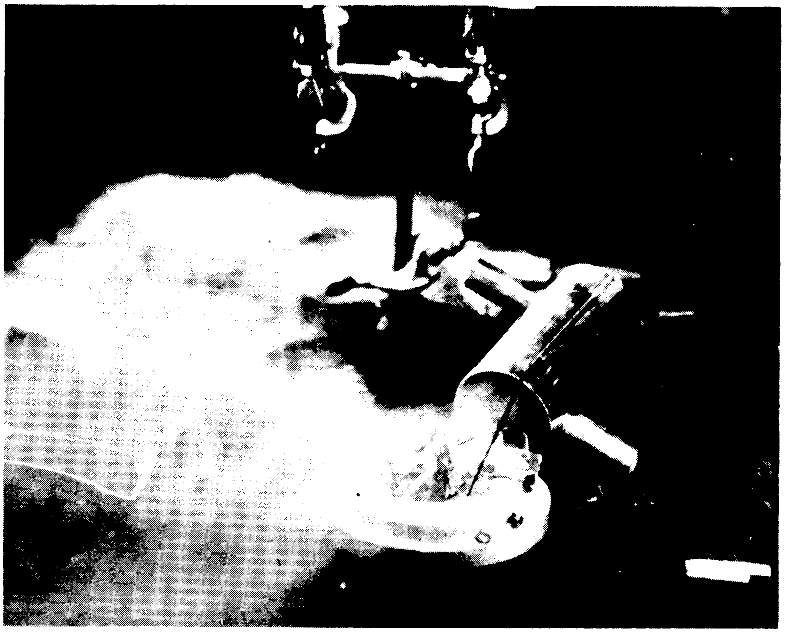

Figure 8

Take a razor blade and an 8 mm focal length condenser lens and fire the laser through this lens on to the razor blade; a single flash of the laser will perforate the razor blade. The perforation is something like 5/1000th of an inch in diameter. Another experiment that we have performed is to take a carbon block - a piece of carbon near the focus of the lens and in that case we vaporized carbon. This required temperatures, I believe is on the order of 10,000° and our estimates of the temperatures, theoretical estimates, agree with the experimental data. This would be one of the possible uses.

One use, very much for the future, is communications. Bell Telephone Laboratories are working very hard on this in view of the fact that the ruby laser has frequencies on the order of 10^{15} cycles. You can immediately see that on such a beam you can carry an enormous number of channels, and I think in the future this device will play a very major role in communications, particularly in space where there is no attenuation of the beam.

I think if you let your imagination roam a little bit, you will see other applications immediately. I might mention the possibilities of constructing units which are considerably more high-powered than the present ones are. So far we have been able to obtain roughly 10 kilowatts peak output. The construction of a high-power laser, let us say with megawatt peak of power, appears to be essentially the problem of building bigger and better boilers - just make the laser bigger and provide appropriate cooling. We think at the present time we have a scheme by which we can construct such a unit. It would be a fairly bulky affair, of course, and require a lot of power, but such a unit would produce peak power of a megawatt or higher with pulse duration of the order of hundreds of microseconds. Now again, if such a laser were focused down, you probably could obtain temperatures on the order of 1,000,000°. I do not know exactly how you would go about checking such a temperature - but if it were produced, it would vaporize the material and the temperature would be self-limiting by expansion, but theoretically, such a temperature could be obtained and to my knowledge they cannot be obtained by any other means. So this is one possible use of lasers. At the present time there is an experiment going on here to try to produce harmonics of light by shining the laser beam on to a piece of quartz. Red light should produce ultraviolet. To my knowledge the experiment has not been successful at this time.

Just to conclude this talk, let me also mention that although this talk is billed as a ruby laser, which means solid state laser, there are in existence gas lasers. They are somewhat different in principle, not to the extent that you do not have this emission of radiation between the two levels, but they energize in a slightly different manner.

The one and only gas laser in existence employs a mixture of helium and neon - something like 100 parts of helium to 1 part of neon. The helium is excited by electrical discharge, by, I think, a frequency of 27 megacycles/second. The energy is transferred from helium ions to neon ions by collision and then emission from the metastable level of neon to states above ground state. It has been possible to operate this device continuously. It puts out power of the order of milliwatts at the present time, although it should be able to put out on the order of watts. The output is, I believe, in the 1.2 micron region which is near infrared, and the remarkable thing about it is the bandwidths of emission at a frequency of roughly 10^{15} cycles is only 10 kc. This is extremely monochromatic radiation. Just to sketch it briefly, it is simply a tube filled with a mixture of helium and neon and on the ends there are plates which are optical flats and which are adjusted to be parallel. Then there is a coil which energizes the helium gas and essentially that is all there is to it. The power levels obtained, primarily to the small density of ions, compared to solid material, are of the order of milliwatts. This is considerably less than in the case of solid state lasers, but it will run continuously, which the ruby laser will not. There are many other schemes in theory at least, which can be used to energize such a device. For example, chemical reactions can produce whole populations of higher states, at least it is thought so. But at the present time, the only device in operation is the helium-neon laser at Bell Telephone Laboratories.

APPENDIX

LIST OF CURRENT ENGINEERING RESEARCH PROJECTS AT THE UNIVERSITY OF MICHIGAN

Department of Aeronautical and Astronautical Engineering

1. Optimum Linear Filtering of Sampled Signals

   Analytical investigation of optimum interpolation and extrapolation of random processes from non-zero width samples.
   Boris Danik, Doctoral Candidate. (Chairman: F. J. Beutler).
   Doctoral Thesis Research.

2. Use of State-Variable Concepts in the Syntheses of Linear Sampled Data Control Systems

   An investigation of design procedures for sampled data systems which are based on vector-difference equations and specially developed matrix methods.
   Howard M. Estes, Doctoral Candidate. (Chairman: E. G. Gilbert).
   Doctoral Thesis Research.

3. Human Estimation of Time-Varying Probabilities

   An experimental study of how the human operator estimates probabilities associated with a certain binary random process
   Gordon H. Robinson, Doctoral Candidate. (Chairman: E. G. Gilbert).
   Doctoral Thesis Research.

4. A New Computer Technique for the Reduction of Real Matrices to the Diagonal Form of Their Latent Roots

   Jack R. Jennings, Doctoral Candidate. (Chairman: R. M. Howe).
   Doctoral Thesis Research.

5. Dynamic Stability of Short Bodies

   An investigation of the details of the unsteady aerodynamic flow phenomena associated with the flow past the unstable bodies.
   Norman E. Hawk, Doctoral Candidate. (Chairman: A. M. Kuethe).
   Doctoral Thesis Research.

6. Random Motion of the Stagnation Point on a Body as Affected by Turbulence in the Airstream

   An experimental and theoretical study of flow near the stagnation point at subsonic and supersonic speeds.
   Daniel W. Cheatham, Jr., Doctoral Candidate. (Chairman: A. M. Kuethe).
   Doctoral Thesis Research.

7. Boundary Layer Transition on Super-Cooled Bodies

   A quantitative experimental study of the effects of cooling and roughness on transition
   Algimantas J. Kuprenas, Doctoral Candidate. (Chairman: A. M. Kuethe).
   Doctoral Thesis Research.

8. Effect of Turbulence on Heat Transfer in Laminar Boundary Layers

   An experimental and theoretical investigation of the heat transfer from flat plates and cylinders as affected by turbulence in the mean stream.
   Mahlon C. Smith, Doctoral Candidate. (Chairman: A. M. Kuethe).
   Doctoral Thesis Research.

Department of Aeronautical and Astronautical Engineering - continued

9. Studies on the Structure of Detonation Waves

   Detail study of the reaction zone in detonation waves to assess the chemical kinetics in that zone.
   Eliahou K. Dabora, Doctoral Candidate. (Co-Chairmen: R. B. Morrison and W. Mirsky)
   Doctoral Thesis Research.

10. Theoretical and Experimental Investigation of a High Temperature Seeded Plasma

    A study of the properties of a seeded plasma at a temperature of approximately 5000°R using microwave and optical spectroscopy techniques.
    Robert S. Buchanan, Doctoral Candidate. (Chairman: R. B. Morrison).
    Doctoral Thesis Research.

11. Drag Coefficients of Small Reacting and Accelerating Particles in Gaseous Flow Systems

    This project is concerned with determining the drag coefficients of acceleration reaction particles in a gaseous flowing system.
    Clayton T. Crowe, Doctoral Candidate. (Chairman: R. B. Morrison).
    Doctoral Thesis Research.

12. PCM Demodulation

    Investigation of improved demodulation methods for PCM data.
    Earl F. Smith, Doctoral Candidate. (Chairman: L. L. Rauch).
    Doctoral Thesis Research.

13. Analysis of Nonlinear Control System

    A. J. Gregory, Doctoral Candidate. (Chairman: L. L. Rauch).
    Doctoral Thesis Research.

14. Phase-locked Loop Devices

    Frederick L. Bartman, Doctoral Candidate. (Chairman: L. L. Rauch).
    Doctoral Thesis Research.

15. An Investigation of the Pressure Fluctuations in a Turbulent Boundary Layer

    An experimental investigation the purpose of which is to learn more about the structure of a turbulent boundary layer.
    Charles E. Wooldridge, Doctoral Candidate. (Chairman: W. W. Willmarth).
    Doctoral Thesis Research.

16. Investigation of Information and Communication Theory Pertinent to the Signal to Noise Ratios Encountered in Space Communication and Telemetry

    Application of the theory of stochastic processes to problems related to information and communication theory.
    F. J. Beutler. NASA Research Grant.

Department of Aeronautical and Astronautical Engineering - continued

17. A Study of Structural Dynamics Problems Using the Electronic Differential Analyzer

    This is a study of analog computer methods for these problems using finite difference techniques.
    D. T. Greenwood.

18. Space Vehicle Control Systems

    Research on the analysis and synthesis of space vehicle booster attitude control systems.
    R. M. Howe. NASA Research Grant.

19. Development of Aerodynamic Equations for the Simulation of Unconventional Flight Characteristics

    Development of equations suitable for simulation of the flight of orbital and reentry vehicles, and study of means for mechanizing these equations.
    G. Isakson.

20. Study of Vibration of Twisted Rotating Blades

    Development of a practical method for the determination of the coupled bending and torsional vibration characteristics of twisted rotating blades, and a study of these characteristics.
    G. Isakson.

21. Dynamic Stability of Short Bodies

    An investigation of the details of the unsteady aerodynamic flow phenomena associated with the flow past the unstable bodies.
    A. M. Kuethe.

22. Boundary Layer Transition on Super-Cooled Bodies

    A quantitative experimental study of the effects of cooling and roughness on transition.
    A. M. Kuethe.

23. Study of Rocket Sounding Methods of Measuring Upper Atmosphere

    Application of a new theory of rarified gas dynamics to the analysis of the sphere drag and pitot tube corresponding to the atmosphere at altitude range of 8 - 100 km.
    V. -C Liu.

24. Particle Dynamics of Small Reacting and Accelerating Particles in Gaseous Flow Systems

    An experimental and analytical approach to determine the drag coefficients and burning rates of small particles which are subjected to large changes in relative velocity between the particle and its medium.
    R. B. Morrison.

25. Experimental Investigation of Standing Detonation Waves

    A study of the detailed structure of such waves and the influence of magnetic fields.
    J. A. Nicholls.

Department of Aeronatucal and Astronautical Engineering - continued

26. Seeding of High Temperature Gas Streams for Purposes of Increasing Electrical Conductivity

    A study of different seeding elements and gases to assess electrical conductivity for potential application to power conversion.
    J. A. Nicholls.

27. Study on Telemetry and Range Safety Techniques for Advanced Aerospace Vehicles
    To provide analytical background leading to proper applied research effort.
    L. L. Rauch.

28. Dynamics of Homogeneous Turbulence

    Experimental study of energy transfer from large to small scale motion.
    M. S. Uberoi.

29. Sea Surface Roughness

    Determination of spectral density of observed wind generated waves.
    M. S. Uberoi.

30. Statistical Analysis of Two-Dimensional Functions

    Development of optical techniques for the measurement of two-dimensional spectral density
    M. S. Uberoi.

31. Hydromagnetics at Zero Magnetic Reynolds Number

    Velocities induced by passage of current through liquids.
    M. S. Uberoi.

32. Impact of Water Drops on Water
    Study of oscillations and break-up of water surface due to impact of water drops.
    M. S. Uberoi.

33. An Investigation of the Pressure Fluctuations in a Turbulent Boundary Layer

    An experimental investigation, the purpose of which is to learn more about the structure of a turbulent boundary layer.
    Office of Naval Research.
    W. W. Willmarth.

34. Upper Air Research at High Latitudes

    A program concerned with development of a rocket-borne instrumentation package for measurement of upper atmosphere meteorological data.
    H. F. Allen.

35. Jet Interaction Studies

    Experimental and theoretical investigation of jets issuing into a supersonic stream.
    J. L. Amick.

36. Transition Tests in a Low-Turbulence Supersonic Wind Tunnel

    Tests of stream turbulence and of transition on a model in the new laminar flow wind tunnel.
    J. L. Amick.

37. Sphere Experiment for Upper Air Density

    A synoptic method for measuring the density of the upper atmosphere by means of the drag on falling sphere is being extended to higher altitudes and refined.
    H. F. Schulte and J. W. Peterson.

38. Upper Air Composition

    A rocket-borne mass spectrometer for measuring the composition of the atmosphere between 100 and 200 kilometers is being developed and tested.
    E. J. Schaefer.

39. Meteorological Satellites

    A visible and infra-red optical laboratory has been established. Development and evaluation of radiation sensors to be used in the TIROS and NIMBUS meteorological satellite programs is being carried out.
    F. L. Bartman.

40. Solid Propellant Sounding Rocket

    A new two-stage solid propellant sounding rocket capable of carrying 80 lbs. to 225 kilometers is being developed. Four will be flown during the summer of 1961.
    W. H. Hansen.

Department of Chemical and Metallurgical Engineering

1. Study of the Stabilization of Pre-mixed Flames in a Ceramic Tube

   Thomas D. Bath, Doctoral Candidate. (Chairman: S. W. Churchill).
   Doctoral Thesis Research.

2. The Effect of Viscosity on the Rate of Oxygen Transfer in Fermentation Systems

   Gary F. Bennett, Doctoral Candidate. (Chairman: L. L. Kempe).
   Doctoral Thesis Research.

3. Optimization of a Member of the Class of Petroleum or Chemical Processes Involving Recycle Operation

   Edmund D. Blum, Doctoral Candidate. (Chairmen: D. F. Rudd, G. B. Williams)
   Doctoral Thesis Research.

4. Applications of the Theory of Irreversible Thermodynamics to the Correlation and Prediction of Electric Transport Phenomena

   Dale Briggs, Doctoral Candidate. (Chairman: E. E. Hucke).
   Doctoral Thesis Research.

5. Mathematical Simulation of Two-Phase Flow in a Gas Storage Reservoir

   James Briggs, Doctoral Candidate. (Chairman: K. H. Coats).
   Doctoral Thesis Research.

6. The Effect of Temperature on the Reactions between Iron and Alumina-Silica Refractories

   John Brokloff, Doctoral Candidate. (Chairman: L. H. Van Vlack).
   Doctoral Thesis Research.

7. Energy Distribution Involved in the Adsorption of Benzene on Solid Surfaces

   Jasper Brundege, Doctoral Candidate. (Chairman: G. Parravano).
   Doctoral Thesis Research.

8. Radiation-Induced Decomposition of Paraffin Hydrocarbons

   Brice Carnahan, Doctoral Candidate. (Chairmen: D. L. Katz, J. J. Martin).
   Doctoral Thesis Research.

9. Pressure Drop in Vertical Pneumatic Transport of Solids

   Ja-min Chen, Doctoral Candidate. (Chairman: J. L. York).

10. Natural Convection Heat Transfer in Low Prandtl Number Fluids

    Paul Chu, Doctoral Candidate. (Chairmen: J. J. Martin and F. G. Hammitt).
    Doctoral Thesis Research.

11. Application of Dislocation Etch-Pitting to Study of the Effect of Substructure on the Creep Properties of Nickel

    A. P. Coldren, Doctoral Candidate. (Chairman: W. C. Bigelow).
    Doctoral Thesis Research.

Department of Chemical and Metallurgical Engineering - continued

12. Control of a Packed Bed Tubular Reactor
    John O. Cowles, Doctoral Candidate. (Chairman: D. R. Mason). Doctoral Thesis Research.

13. Determination of the Causes for Variable Liquidation Temperature in a Portion of the Fe-Cr-Ni-Nb System
    Thomas Cullen, Doctoral Candidate. (Chairman: J. W. Freeman). Doctoral Thesis Research.

14. The Equilibrium Composition of Sulphide-Oxide Inclusions in Chromium Steels at Sub-Liquidus Temperatures
    James M. Dahl, Doctoral Candidate. (Chairman: L. H. Van Vlack). Doctoral Thesis Research.

15. The Mixing Process in Jet Pumps
    John M. Dealy, Doctoral Candidate. (Chairman: J. L. York). Doctoral Thesis Research.

16. The Determination of Solubility Products of Certain Alloy Nitrides in Liquid Iron
    Donald Evans, Doctoral Candidate. (Chairman: R. D. Pehlke). Doctoral Thesis Research.

17. Heat Transfer and Pressure Drop Performance of Turbulence Promoters
    Larry Evans, Doctoral Candidate. (Chairman: S. W. Churchill). Doctoral Thesis Research.

18. The Kinetics of Temper Embrittlement
    James Ford, Doctoral Candidate. (Chairman: C. A. Siebert). Doctoral Thesis Research.

19. Two Phase Fluid Flow in Porous Media
    Robert Gorring, Doctoral Candidate. (Chairman: D. L. Katz). Doctoral Thesis Research.

20. Hydration of Propylene Using a Cation Exchange Resin Catalyst
    John Hiestand, Doctoral Candidate. (Chairman: K. F. Gordon). Doctoral Thesis Research.

21. Thermodynamic Properties of Hydrocarbons at Low Temperatures
    Millard Jones, Doctoral Candidate. (Chairman: D. L.Katz). Doctoral Thesis Research.

22. A Study of a Perforated Pulse Column
    Stanley Jones, Doctoral Candidate. (Chairman: M. R. Tek). Doctoral Thesis Research.

23. Fluid-Dynamic Behavior of Fluidized Solids
    Robert Kadlec, Doctoral Candidate. (Chairman: G. B. Williams). Doctoral Thesis Research.

Department of Chemical and Metallurgical Engineering - continued

24. Formation of Included Phases by Liquid-Liquid Injection
David P. Kessler, Doctoral Candidate. (Chairman: J. L. York).
Doctoral Thesis Research.

25. Light Reflection Properties of Rough Paint Surfaces
Hannes Kristinsson, Doctoral Candidate. (Chairman: W. C. Bigelow).
Doctoral Thesis Research.

26. Addition of Hydrogen Sulfide to Olefinic Bonds under the Influence of Gamma Radiation
N. C. Kothary, Doctoral Candidate. (Chairmen: J. J. Martin, L. E. Brownell).
Doctoral Thesis Research.

27. Dynamic and Thermodynamic Behavior of Batch Systems Involving the Generation of Heat
Bernard Hulwicki, Doctoral Candidate. (Chairman: D. R. Mason).
Doctoral Thesis Research.

28. Study of Boiling Refrigerants inside Plain Tubes with Internal Turbulators
J. G. Lavin, Doctoral Candidate. (Chairman: E. H. Young).
Doctoral Thesis Research.

29. The Surface Step Flow Mechanism in the Sintering Process
V. J. Lee, Doctoral Candidate. (Chairman: G. Parravano).
Doctoral Thesis Research.

30. Thermodynamic Properties of Hydrocarbon Mixtures
David Mage, Doctoral Candidate. (Chairman: D. L. Katz).
Doctoral Thesis Research.

31. Pool Boiling Heat Transfer between Immiscible Liquids
Gilbert M. Marcus, Doctoral Candidate. (Chairman: K. F. Gordon).
Doctoral Thesis Research.

32. Separation of Liquid Mixtures by Fixed Bed Adsorption
Alfred J. Martin, Doctoral Candidate. (Chairman: G. B. Williams).
Doctoral Thesis Research.

33. Polymerization of Propylene on Molybdenum Oxide Catalysts
William McCarty,Doctoral Candidate. (Chairman: G. Parravano).
Doctoral Thesis Research.

34. Investigation of the Effects of Strain on the Surface Energy and Plastic Properties of an Alloy System
Herbert McClammy, Doctoral Candidate. (Chairman: E. E. Hucke).
Doctoral Thesis Research.

Department of Chemical and Metallurgical Engineering - continued

35. Deformation Characteristics of Metals
    E. J. Meyers, Doctoral Candidate. (Chairman: M. J. Sinnott).
    Doctoral Thesis Research.

36. The Determination of Liquid-Liquid Equilibria in the Ni-Fe-Mg System
    W. J. Mitchell, Doctoral Candidate. (Chairman: R. A. Flinn).
    Doctoral Thesis Research.

37. Hydrodynamic Scale-up of Gas Storage Reservoirs
    Russell Nielson, Doctoral Candidate. (Chairman: M. R. Tek).
    Doctoral Thesis Research.

38. Phase Transformation of Cobalt Oxides
    Henry O'Bryan, Doctoral Candidate. (Chairman: G. Parravano).
    Doctoral Thesis Research.

39. Ternary Semiconductor Compounds
    Daniel O'Kane, Doctoral Candidate. (Chairman: D. R. Mason).
    Doctoral Thesis Research.

40. Heterogeneous Equilibria between Metallic Solids and Gases
    John Piazza, Doctoral Candidate. (Chairman: M. J. Sinnott).
    Doctoral Thesis Research.

41. Heat and Mass Transfer between Liquid Phases
    Robert Pease, Doctoral Candidate. (Chairman: K. F. Gordon).
    Doctoral Thesis Research.

42. Thermodynamic Properties by Spectra-adsorption
    Philip Rice, Doctoral Candidate. (Chairman: D. V. Ragone).
    Doctoral Thesis Research.

43. Investigation to Determine the Interrelationship between Temperature and Phase Composition upon Phase Distribution in Equilibrated Microstructures
    Otto Riegger, Doctoral Candidate. (Chairman: L. H. Van Vlack).
    Doctoral Thesis Research.

44. Concurrent Flow of Two Immiscible Fluids through a Packed Bed
    Robert Rigg, Doctoral Candidate. (Chairman: S. W. Churchill).
    Doctoral Thesis Research.

45. Mass Transfer and Fluid Dynamic Properties of Dual-Flow Distillation Trays
    Gordon Ringrose, Doctoral Candidate. (Chairman; G. B. Williams).
    Doctoral Thesis Research.

46. Immunity of Nickel-Base Alloys to Damage from Overheating
    John P. Rowe, Doctoral Candidate. (Chairman: J. W. Freeman).
    Doctoral Thesis Research.

Department of Chemical and Metallurgical Engineering - continued

47. Zone Fractionation of High Polymers

    Arnold Ruskin, Doctoral Candidate. (Chairman: G. Parravano).
    Doctoral Thesis Research.

48. Scale Initiation on a Surface

    Bernard Schorle, Doctoral Candidate. (Chairman: K. F. Gordon).
    Doctoral Thesis Research.

49. Catalytic Oxidation of Hydrocarbons

    Thomas Schriber, Doctoral Candidate. (Chairman: G. Parravano).

50. Study of the Heat Capacity of Gases at Constant Volume

    Richard Schwing, Doctoral Candidate. (Chairman: J. J. Martin).

51. Spray Fractionation by Flashing Liquids

    W. Leigh Short, Doctoral Candidate. (Chairman: J. L. York).
    Doctoral Thesis Research.

52. Heat Transfer Coefficient in a LTV

    J. R. Sinek, Doctoral Candidate. (Chairman: E. H. Young).
    Doctoral Thesis Research.

53. Diffusion and Mixing in Porous Environment

    James Skinner, Doctoral Candidate. (Chairman: M. R. Tek).
    Doctoral Thesis Research.

54. An Investigation of the Effects of Substitutional Iron on the Mechanism of Plastic Deformation of Magnesium Sulfide

    Yancey Smith, Doctoral Candidate. (Chairman: L. H. Van Vlack).
    Doctoral Thesis Research.

55. Rate of Scale Deposition on a Heated Surface

    Glen Smith, Doctoral Candidate. (Chairman: K. F. Gordon).
    Doctoral Thesis Research.

56. Thermodynamic and Kinetic Properties of Buthenium Dioxide Surfaces

    Jack Sommerfeld, Doctoral Candidate. (Chairman: G. Parravano).
    Doctoral Thesis Research.

57. Study of Heterogeneous Polymerization Kinetics of the Alpha Olefins

    Philip Spiegelman, Doctoral Candidate. (Chairman: G. Parravano).
    Doctoral Thesis Research.

58. The Solubility of Alkali and Alkaline Earth Elements in Liquid Iron-Carbon Alloys

    David Sponseller, Doctoral Candidate. (Chairman: R. A. Flinn).
    Doctoral Thesis Research.

59. The Effect of Changes in Thermoelectric Power on the Catalyzation Activities of Cobalt Ferrite

    Robert G. Squires, Doctoral Candidate. (Chairman: G. Parravano).
    Doctoral Thesis Research.

Department of Chemical and Metallurgical Engineering - continued

60. Effect of Stress on Surface Energy of Solids
    Charles Stickels, Doctoral Candidate. (Chairman: E. E. Hucke).
    Doctoral Thesis Research.

61. Two-Phase Flow through Vertical Pipe
    James Street, Doctoral Candidate (Chairman: M. R. Tek).
    Doctoral Thesis Research.

62. Magnesium Solubility in Iron-Base Alloys
    Paul Trojan, Doctoral Candidate. (Chairman: R. A. Flinn).
    Doctoral Thesis Research.

63. Mass Transfer Process Involving Simultaneous Adsorption and Chemical Reaction
    Lalit H. Udani, Doctoral Candidate. (Chairman: K. F. Gordon).
    Doctoral Thesis Research.

64. Factors Influencing the Formation and Behavior of Froth
    Robert Van Duyne, Doctoral Candidate. (Chairman: K. F. Gordon).
    Doctoral Thesis Research.

65. Electric Mobility Measured as a Function of Temperature and Composition in a Low Melting Liquid Metal Alloy
    John Verhoeven, Doctoral Candidate. (Chairman: E. E. Hucke).
    Doctoral Thesis Research.

66. Plastic Deformation of Minor Amounts of Non-Metallic Phases in a Ductile Metal Matrix
    Robert Warrick, Doctoral Candidate. (Chairman: L. H. Van Vlack).
    Doctoral Thesis Research.

67. Mechanism of Elevated Creep Strength in Titanium-Bearing 18-8 Stainless Steels
    Jerry White, Doctoral Candidate. (Chairman: J. W. Freeman).
    Doctoral Thesis Research.

68. Some Aspects of the Influence of Gamma Radiation on Chemical Reactions
    Lowell Yemin, Doctoral Candidate. (Chairman: J. J. Martin).
    Doctoral Thesis Research.

69. The Mechanism of Bond Rupture Resulting from Activation of Halogen Atom in an Alkyl Halide
    Chia-Lua Wu, Doctoral Candidate. (Chairmen: A. A. Gordus, J. J. Martin).
    Doctoral Thesis Research.

70. Investigation of Relative Microbiological Response to Varying Types and Methods of Application of Ionizing Radiations to Anaerobic Bacterial Spores
    U.S. Army Quartermaster Corps, sponsor.
    L. L. Kempe.

Department of Chemical and Metallurgical Engineering - continued

71. Effect of Bacterial Ecology on Canned Food Spoilage
National Institutes of Health, sponsor.
L. L. Kempe.

72. Oxygen Transfer in Fermentation
National Science Foundation, sponsor.
L. L. Kempe.

73. Study of Trace Chemicals in Waters of the Great Lakes
Cooperative study with U.S. Bureau of Commercial Fisheries
L. L. Kempe.

74. Irradiation of Foods
L. E. Brownell.

75. Properties of Refrigerants
K. H. Coats.

76. Properties of Metals at High Temperatures
NASA, ASME, WADD, International Nickel, U.S. Pipe & Foundry, sponsors.
J. W. Freeman.

77. Study of Crystallization in Evaporated Solutions
K. F. Gordon.

78. Radioactive Transport through Real Atmosphere
S. W. Churchill.

79. Transport Properties
E. E. Hucke.

80. Process Metallurgy
Valley Research, sponsor.
E. E. Hucke.

81. Water in Contact with Gas
American Gas Assoc.,sponsor
D. L. Katz.

82. Thermodynamic Properties
NSF, sponsor.
D. L. Katz.

83. Sintering of Metal Oxides
G. Parravano.

84. Fractional Crystallization
G. Parravano.

Department of Chemical and Metallurgical Engineering - continued

85. Kinetics of Metal-bearing Catalysts
G. Parravano.

86. Thermodynamics Activities
D. V. Ragone.

87. Microstructures
L. H. Van Vlack.

88. Ductile Metals
L. H. Van Vlack.

89. Deformation of Manganese Sulfide
L. H. Van Vlack.

90. Equilibrium Studies
Esso Research & Engineering Co., sponsor.
G. B. Williams.

91. Heat Transfer through Extended Surfaces
Wolverine Tube, sponsor.
E. H. Young.

92. Thermodynamic Properties of "Freon" Refrigerants
du Pont Co., sponsor.
J. J. Martin.

93. Solubility of Gases in Liquid Metals
Robert D. Pehlke.

94. Activity of Uranium in Liquid Alloys
Phoenix Project.
Robert D. Pehlke.

95. Thermodynamics of Liquid Metallic Solutions
Atomic Energy Comm.
Robert D. Pehlke.

Department of Civil Engineering

1. The Qualitative Measure of Highway Accessibility

   A measurement and analysis of the factors of distance, time and effort in selected urban trips with reference to the highway system and spatial distribution or origins and destinations.
   Clinton L. Heimbach, Doctoral Candidate. (Chairman: John C. Kohl).
   Doctoral Thesis Research.

2. The Role of Mass Transportation in Urban Planning

   An analysis of the demands for various forms of urban mass transportation and their relationships to the nature and intensity of land use.
   John C. Kohl.

3. A Background Planning Study of Michigan Aviation.

   This project is being extended to attempt the application of certain assumptions implied in the factual data gathered during the initial study in 1959-60.
   J. C. Kohl.

4. Effects of Inelastic Action on the Behavior of Structures during Earthquakes

   An analysis of the behavior of structures during earthquake, and the effect of inelastic properties of the structures upon the response.
   Spiro S. Thomaides, Doctoral Candidate. (Chairman: Glen V. Berg).
   Doctoral Thesis Research.

5. The Coupled Modes of Vibration of a Stepped Cantilever Shearbeam

   A study of the coupled translational-torsional modes of a stepped shearbeam, with application to the earthquake response of a tall building with a setback.
   Glen V. Berg.

6. Computer Methods in Structures Analysis

   A development of practical and efficient methods for the analysis of complex structures by high speed digital computers.
   Glen V. Berg.

7. Energy Consumption by Structures in Strong-Motion Earthquakes

   A study of the effect of energy dissipation upon structural response to earthquake, including a spectral analysis of the energy relations for elasto-plastic systems responding to recorded strong-motion earthquakes.
   National Science Foundation, sponsor.
   Glen V. Berg and Spiro S. Thomaides.

8. Waterhammer Analysis with Inclusion of Friction

   An analytical and experimental investigation which includes friction terms in the partial differential equations of waterhammer.
   Chintu Lai, Doctoral Candidate. (Chairman: V. L. Streeter).
   Doctoral Thesis Research.

9. Instability of the Free Surface of a Steady Turbulent Flow, and Criteria for Formation of Roll Waves

   Analytical and experimental investigation of formation of roll waves in an inclined channel.
   James Wiggert, Doctoral Candidate. (Co-chairmen, E. F. Brater, and V. L. Streeter).
   Doctoral Thesis Research.

10. Investigation of valve stroking as a means of controlling waterhammer pressures.

    V. L. Streeter.

11. Propagation of Oscillatory Waves in a Triangular Channel

    A study having the primary purpose of finding methods of predicting the amount of wave energy which will be transmitted from deep water through a channel perpendicular to the shore line.
    Khalil Beitinjaneh, Doctoral Candidate. (Chairman: E. F. Brater).

12. A study of the effectiveness of methods of controlling shore erosion.
    E. F. Brater.

13. Design of Highway Bridges

    A unique internship training program for qualified undergraduates and graduates interested in bridge design. Basic project has been in existence since Feb., 1952.
    Leo M. Legatski and Robert B. Harris.

14. Wind Studies on Mobile Homes

    The determination of the actual forces created by winds which mobile homes must resist, consideration of suitable tie-down devices, and evaluations of construction details to insure the integrity of the mobile home during wind storms.
    Robert B. Harris.

15. A Study of Prestress Losses in Prestressed Concrete Beams

    This research will include an examination of the validity of measuring creep on axially loaded specimens and applying such measurements to eccentrically loaded specimens will be examined.
    R. M. Patel, Doctoral Candidate. (Chairman: L. M. Legatski).
    Doctoral Thesis Research.

16. Plastic Strength of Steel Columns under Biaxial Bending

    Boyd Ringo, Doctoral Candidate. (Chairman: Bruce G. Johnston).
    Doctoral Thesis Research.

17. Strength of Nonuniform Columns in the Inelastic Range

    Determination by use of the digital computer of the actual ultimate strength in the inelastic range of nonuniform columns made of a material similar to structural aluminum.
    Rafi Hariri, Doctoral Candidate.
    Doctoral Thesis Research.

Department of Civil Engineering - continued

18. Shock Tube Vibration Studies

    Kirtland Air Force Base, sponsor.
    S. C. Tang, Doctoral Candidate. (Chairman: Bruce G. Johnston).
    Doctoral Thesis Research.

19. Design Studies on Large Size Shock Tubes

    Studies of the causes of failure of existing shock tubes have been made and the response of a shock tube to a traveling shock wave is being studied.
    Kirtland Air Force Base, sponsor.
    Bruce G. Johnston.

20. The Sand Filtration of Algae Suspensions

    This research involves the study of sand filtration as a process for the extraction of dispersed particles from their suspending medium.
    C. R. O'Melia, Doctoral Candidate. (Chairman: J. A. Borchardt).
    Doctoral Thesis Research.

21. A Study of the Nitrogen Cycle in a Natural Flowing Stream through the Application of Controlled Oxidation-Reduction Potentials

    Henry Dirasian, Doctoral Candidate. (Chairman: J. A. Borchardt).
    Doctoral Thesis Research.

22. The Use of Biological Organisms in Controlled Environments for the Extraction of Growth Factors from Sewage Effluents

    This work would have application either for municipal or industrial wastes or in a smaller package form for space flight or in other confined environments.
    W. E. Gates, Doctoral Candidate. (Chairman: J. A. Borchardt).

23. The Use of Algae in the Treatment of Sewage Effluents

    This study involves the application of various species of algae to the treatment of sewage. It is an attempt to improve efficiency of present secondary treatment through the application of a tertiary phase.
    J. A. Borchardt.

24. The Use of Electronic Devices for Counting and Enumerating Algae Samples

    This study is designed to find an electronic technique which will replace the various laborious microscopic counting techniques currently used to evaluate the problems of algae density in water supplies and stream studies.
    J. A. Borchardt.

25. Cooperative Study on Quality of Great Lakes Waters

    This study is an attempt to develop techniques for measuring trace compounds in Great Lakes waters. This is a cooperative project with U.S. Bureau of Fisheries.
    Lloyd L. Kempe.

26. The Use of Diatomaceous Earth Filters in the Extraction of Algae Cells from the Effluents of Tertiary Sewage Treatment Processes

    This study involves the development of reversible filter techniques, including diatomite recovery, for extraction of algae cells from lagoons or other process effluents.
    J. A. Borchardt.

27. K Factors in the B.O.D. Determinations of Whole or Macerated Algae Cells

    Study of rates at which whole or mascerated algae cells are stabilized under aerobic conditions when used as a substrate by natural biological forces.
    J. A. Borchardt.

28. The Study of Breakpoint Chlorination through the Use of Electrode Potentials

    Breakpoint chlorination has become a standard tool for control of drinking water quality. Little theory is available regarding the process involved. This study is being carried forward to add further basic knowledge to the practice of water disinfection.
    J. A. Borchardt.

29. The Use of Ion Exchange Resins for the Removal of Bacterial Cells through Adsorption.

    This study is an attempt to define the design criteria necessary to establish adequate beds of ion-exchange material for the removal of bacterial cells from water suspensions through adsorption.
    L. L. Kempe and J. A. Borchardt.

Soil Mechanics Laboratory: W. S. Housel, Director

1. Power Plant Structures and Miscellaneous Facilities
   The Detroit Edison Company, sponsor.

2. Cellular Dock Construction - Northwest Extension to Plant No. 2.- Inland Steel Co.
   Arthur G. McKee and Company, sponsor.

3. Cleveland Electric Illuminating Building
   McGeorge, Hargett and Associates, sponsor.

4. Soil Investigation - 1956 Addition to Springwells Station
   City of Detroit Dept. of Water Supply, sponsor.

5. Indiana Harbor Ore Yard and Extensions
   The Youngstown Sheet and Tube Company, sponsors.

6. Raw Water Booster Plant - Department of Water Supply, Detroit, Michigan
   Ayres, Lewis, Norris and May, sponsors.

7. Foundation and Subsurface Investigation - Metropolitan Syracuse Sewage Treatment Plant
   O'Brien and Gere, sponsor.

8. Plant Site - St. Joseph, Michigan
   Huron Portland Cement Company.

9. Plant Site - Cleveland, Ohio
   Huron Portland Cement Company.

Department of Civil Engineering - continued

10. Soil Investigation - Grand Haven Municipal Power Plant
    City of Grand Haven Board of Public Works, sponsor.

11. Consumers Power Company Port Sheldon Plant
    Commonwealth Associates, Inc., sponsor.

12. Storage Capacity of Ore Yard
    Interlake Iron Corporation, sponsor.

13. Michigan Consolidated Gas Company Office Building
    Yamasaki - Leinweber, Smith, Hinchman and Grylls, sponsor.

14. 1959 Addition to Lake Shore Station, Cleveland, Ohio
    Cleveland Electric Illuminating Company, sponsor.

15. Michigan Pavement Performance Study
    Michigan State Highway Dept., and U.S. Department of Commerce Bureau of Public Roads, sponsor.

16. Soil Investigation - Toledo Edison Company Bay Shore Station, Toledo, Ohio
    The A. Bentley and Sons Company, sponsor.

17. Duluth Terminal and Elevators
    F. H. Peavey Company, sponsor.

18. Soil Investigation - Riverview Terrace Housing Development
    Cleveland Metropolitan Housing Authority, Hays and Ruth, Barber, Magee and Hoffman, sponsors.

19. Soil Investigation - Buchanan Pumping Station Southeastern Oakland County Water Authority
    Ayres, Lewis, Norris and May, sponsor.

20. Miscellaneous Investigations
    Raymond Concrete Pile Company, sponsor.

21. City of Oregon Water Treatment Plant, Oregon, Ohio
    Finkbeiner, Pettis and Strout, sponsor.

22. Design of Swimming Pools - Metropolitan Beach
    Huron- Clinton Metropolitan Authority, O'Dell, Hewlett and Luckenbach, sponsors.

23. Foundation Design - Cleveland Civic Center
    Outcalt - Guenther and Van Buren, Barber, Magee and Hoffman, sponsors.

    Methods of Measuring the Stress Conditions in the Earth Mass at Rest.

    The development of a method for measuring stress conditions in the undisturbed soil mass in the field will be a new contribution to this area of technology.
    John R. Evans, Doctoral Candidate. (Chairman: W. S. Housel).
    Doctoral Thesis Research.

Department of Civil Engineering - continued

Pavement Design Based on Pavement Evaluation and Performance of Canadian Airfields
G. Y. Sebastyan, Doctoral Candidate. (Chairman: W. S. Housel).
Doctoral Thesis Research.

Investigations of Fundamental Principles of Pavement Design, Including a General Recapitulation of Present Pavement Design Procedures
G. R. Ingimarsson, Doctoral Candidate. (Chairman: W. S. Housel).
Doctoral Thesis Research.

Structural Properties of Clay
A. L. Desai, Doctoral Candidate. (Chairman: W. S. Housel).
Doctoral Thesis Research.

Transportation Institute:

1. Ford Fund Traffic Accident Study
   An operations analysis of driver, vehicle and environment relationships.
   Bruce D. Greenshields.

2. Driving Behavior and Traffic Accidents
   National Institutes of Health, sponsor.
   Bruce D. Greenshields.

3. Traffic Flow Research
   The development of means of recording highway characteristics.
   Ford Fund
   Bruce D. Greenshields.

Department of Engineering Mechanics

1. Vibrations of Buckled Circular Plates about their Static Equilibrium Configuration

   An investigation of the vibration of circular plates after they have buckled due to thermal or other causes.
   Bertram Herzog, Doctoral Candidate. (Chairman: E. F. Masur)
   Doctoral Thesis Research.

2. Secondary Buckling and Related Phenomena in Plate Theory

   An investigation into secondary buckling and boundary layer phenomena of rectangular buckled plates.
   C. H. Chang, Doctoral Candidate. (Chairman: E. F. Masur)
   Office of Ordnance Research, sponsor.
   Doctoral Thesis Research.

3. On Buckling of Spherical Shells

   A study of the instability phenomenon of spherical shells subjected to external pressure.
   D. A. Beaty, Doctoral Candidate. (Chairman: E. F. Masur)
   National Science Foundation, sponsor.
   Doctoral Thesis Research.

4. The Effect of Shear Deformations on the Buckling and Post-Buckling Behavior of Circular Plates

   Boundary layer effects and collapse phenomena in the theory of plates and their modification through the inclusion of the effect of shear deformations.
   J. E. Taylor, Doctoral Candidate. (Chairman: E. F. Masur)
   Office of Ordnance Research, sponsor.
   Doctoral Thesis Research.

5. Vibration of Systems in which Two Distinct Modes Have the Same, or Nearly the Same, Natural Frequency
   As an example, torsional and longitudinal vibrations of ship propulsion systems are excited, in coupled form, by the propeller and sometimes the two modes of motion have nearly the same natural frequencies.
   G. H. Stickney, Doctoral Candidate. (Chairman: J. Ormondroyd)
   Doctoral Thesis Research.

6. Large-amplitude Motion of a Compressible Fluid in the Atmosphere
   A. J. Claus, Doctoral Candidate. (Chairman: C. S. Yih)
   Army Research Office (Durham), partial sponsor
   Doctoral Thesis Research.

7. Three Studies in Hydrodynamics
   Wei Lai, Doctoral Candidate. (Chairman: C. S. Yih)
   Army Research Office (Durham), sponsor.
   Doctoral Thesis Research.

Department of Engineering Mechanics - continued

8. Flow of Nonhomogeneous Fluids in Porous Media and Hele-Shaw Cells
   Joseph Matar, Doctoral Candidate. (Chairman: C. -S. Yih)
   Army Research Office (Durham), sponsor
   Doctoral Thesis Research.

9. Large-amplitude Homentropis Flows in the Atmosphere
   William O'Dell, Doctoral Candidate. (Chairman: C. -S. Yih)
   Army Research Office (Durham), sponsor
   Doctoral Thesis Research.

10. Flow Characteristics

    Research on hydrodynamics applying to analysis of flow transitions occurring in the fuel-supply components of missile and conventional engines.
    H. J. Smith, Director
    National Aeronautics and Space Administration, sponsor.

11. Theory of Failure at Elevated Temperatures

    This is a study of the condition for failure of metals at various temperature and under complex stress states, particular attention being given to the circumstances under which ductile and brittle fractures occur.
    D. R. Jenkins, Principal Investigator. R. M. Haythornthwaite, Director.
    S. K. Clark, Acting Director.
    Wright Air Development Division, Air Research and Development Command, U.S. Air Force, sponsor.

12. Structural and Dynamic Characteristics of Pneumatic Tires

    This work is an attempt to apply the theory of elasticity to structures made up of textile cords embedded in rubber, and hence to pneumatic tire structures in both the static and dynamic case.
    S. K. Clark, Principal Investigator, R. A. Dodge, Director
    Firestone Tire and Rubber Co., Goodyear Tire and Rubber Co., B. F. Goodrich Tire Co., U.S. Rubber Co., General Tire and Rubber Co., sponsors.

13. Stratified Flows and Hydrodynamics of Rotating Fluids

    C. -S. Yih, Director.
    Army Research Office (Durham), sponsor.

14. Free-surface Instability

    C. -S. Yih, Director.
    Technical Association of the Pulp and Paper Industry, sponsor.

15. The Dynamics of Buckled Panels

    An investigation into the mechanism of buckling of plates and shells and their post-buckling behavior from a dynamic point of view.
    E. F. Masur, Director.
    National Aeronautics and Space Administration, sponsor.

Department of Engineering Mechanics - continued

16. Instability of Solids

    A broad study of the instability of solid bodies obeying elastic, visco-elastic, elastic-plastic, or other material laws.
    E. F. Masur, Director.
    National Science Foundation, sponsor.

17. Constant Spring Modulus from -200°F to 1000°F

    Purpose is to design a spring whose characteristics are independent of temperature.
    J. Ormondroyd, Director.
    Wright Air Development Center, sponsor.

Meteorological Laboratories:

1. The Vertical Diffusion from a Continuous Point Source

   A theoretical analysis of an important aspect of atmospheric dispersion processes.
   R. E. Munn, Doctoral Candidate. (Chairman: E. W. Hewson).
   Doctoral Thesis Research.

2. Internal Discontinuities in Transitional Situations

   Research to see if internal discontinuities in meteorological parameters do in fact exist and can be explained mathematically.
   E. W. Bierly, Doctoral Candidate. (Chairman: E. W. Hewson).
   Doctoral Thesis Research.

3. Atmospheric Pollution by Aeroallergens

   An interdisciplinary study of air pollution by ragweed pollen by meteorologists, engineers, botanists, allergists, and public health statisticians.
   E. W. Hewson.
   National Institutes of Health.

4. AnInvestigation of the Dependency of Visual Resolution on Wind and Temperature Conditions in the Surface Layer of the Atmosphere over Snow, Ice and Frozen Ground

   An experimental investigation designed to give information for estimating visibility conditions in high latitude regions.
   D. J. Portman.
   Gold Regions Research and Engineering Laboratory.

5. Atmospheric Diffusion in Transitional States

   Theoretical and field research on basic problems in air pollution.
   E. W. Hewson and G. C. Gill.
   National Science Foundation.

6. Atmospheric Diffusion Near Big Rock Nuclear Power Plant, Charlevoix, Michigan

   Research on air pollution problems near a nuclear power plant located on a lake shore line.
   E. W. Hewson.
   Consumers Power Company.

Department of Engineering Mechanics - continued

7. An Investigation of Convective Transfer Processes Based on Satellite Photographs

   The purpose of this investigation is to study the relationship between cellular cloud patterns photographed by the TIROS satellites and the flux of heat and moisture from the oceans as determined from standard synoptic observations.
   E. S. Epstein and D. L. Jones. NASA.

8. Development of Laboratory and Demonstration Equipment for Meteorological Instruction

   Specific models and demonstrations are proposed for construction as teaching aids to present meteorological concepts to students from the elementary school level through college.
   F. R. Bellaire and D. L. Jones.
   National Science Foundation.

9. Ecology-Water Quality Changes in Lake Michigan and Lake Erie

   The nature and cost of present changes of water quality of Lakes Michigan and Erie will be studied to ascertain the degree to which analysis of past chemical, biological, physical and meteorological conditions will explain eutrophication of the Great Lakes.
   D. L. Jones and C. F. Powers
   National Institute of Health.

10. Rain Scavenging of Particulate Materials from the Atmosphere

    An experimental evaluation of scavenging mechanisms by rain.
    A. N. Dingle. U. S. Atomic Energy Commission.

11. Agglomeration of Cloud Particles and Analysis of Project Hi-Cue Radar Observations

    Cooperative field studies designed for comprehensive observation and analysis of shower-type storms.
    A. N. Dingle and F. C. Elder.
    Air Force Cambridge Research Center.

12. A Study of Turbulence in the Surface Layer of the Atmosphere with Optical Techniques

    Horizontal light beams are used to investigate various characteristics of turbulence in stratified shear flow of different natural surfaces.
    D. J. Portman. National Science Foundation.

13. Meteorological Aspects in the Analysis of Community Air Pollution as a Factor in Lung Cancer and other Degenerative Diseases

    A study of air pollution as a possible factor influencing mortality rates in cities.
    George H. Milly, Doctoral Candidate. (Chairman: E. W. Hewson)
    Doctoral Thesis Research.

Department of Engineering Mechanics - continued

14. The Vertical Diffusion from a Continuous Point Source
    A theoretical analysis of an important aspect of atmospheric dispersion processes.
    R. E. Munn, Doctoral Candidate. (Chairman: E. W. Hewson).
    Doctoral Thesis Research.

15. Atmospheric Pollution by Aeroallergens
    An interdisciplinary study of air pollution by ragweed pollen by meteorologists, engineers, botanists, allergists, and public health statisticians.
    National Institutes of Health.
    E. W. Hewson and J. M. Sheldon.

16. An Investigation of the Dependency of Visual Resolution on Wind and Temperature Conditions in the Surface Layer of the Atmosphere over Snow, Ice and Frozen Ground.
    An experimental investigation designed to give information for estimating visibility conditions in high latitude regions.
    D. J. Portman.

17. Atmospheric Diffusion in Transitional States
    Theoretical and field research on basic problems in air pollution.
    National Science Foundation.
    E. W. Hewson and G. C. Gill.

18. Atmospheric Diffusion Near Big Rock Nuclear Power Plant, Charlevoix, Michigan
    Research on air pollution problems near a nuclear power plant located on a lake shore line.
    Consumers Power Company.
    E. W. Hewson.

19. An Investigation of Convective Transfer Processes Based on Satellite Photographs
    The purpose of this investigation is to study the relationship between cellular cloud patterns photographed by the TIROS satellites and the flux of heat and moisture from the oceans as determined from standard synoptic observations. The information contained in the photographs will be characterized by statistics which will then be related to the synoptic data
    NASA. E. S. Epstein and D. L. Jones.

20. Development of Laboratory and Demonstration Equipment for Meteorological Instruction
    Specific models and demonstrations are proposed for construction as teaching aids to present meteorological concepts to students from the elementary school level through college.
    National Science Foundation.
    F. R. Bellaire and D. L. Jones.

Department of Engineering Mechanics - continued

21. Ecology-Water Quality Changes in Lake Michigan and Lake Erie

    The nature and cost of present changes of water quality of Lakes Michigan and Erie will be studied to ascertain the degree to which analysis of past chemical, biological, physical and meteorological conditions will explain presently observed water quality. Relationships found will be applied as a prognosis for future conditions expected in Great Lakes waters.
    National Institute of Health.
    D. L. Jones and C. F. Powers.

22. Rain Scavenging of Particulate Materials from the Atmosphere
    An experimental evaluation of scavenging mechanisms by rain.
    U.S. Atomic Energy Commission. A. N. Dingle.

23. Agglomeration of Cloud Particles and Analysis of Project Hi-Cue Radar Observations

    Cooperative field studies designed for comprehensive observation and analysis of shower-type storms.
    Air Force Cambridge Research Center.
    A. N. Dingle and F. C. Elder.

24. A Study of Turbulence in the Surface Layer of the Atmosphere with Optical Techniques

    Horizontal light beams are used to investigate various characteristics of turbulence in stratified shear flow of different natural surfaces.
    National Science Foundation. D. J. Portman.

Department of Industrial Engineering

1. Dispatching Policies of Production Control in Job Shop Plants

   Philip J. Thorson, Doctoral Candidate. (Chairman: Wyeth Allen)
   Doctoral Thesis Research.

2. An Industrial Development Program for British Columbia

   Ralph E. Boston, Doctoral Candidate. (Chairman: Wyeth Allen)
   Doctoral Thesis Research.

3. Analysis of Economic Markets as Markov Processes

   Frederick T. Sparrow, Doctoral Candidate. (Chairman: Wyeth Allen).
   Doctoral Thesis Research.

4. Evaluation of Spatial Relations and Empirical Plant Layout Criteria by Digital Computer

   Richard C. Wilson, Doctoral Candidate. (Chairman: Wyeth Allen).
   Doctoral Thesis Research.

5. Simulation of Production Control Systems

   John Wyman, Doctoral Candidate. (Chairman: Richard V. Evans).
   Doctoral Thesis Research.

6. Methods Time Measurement Association

   A study of basic elemental motions to set pre-determined standards.
   Walton M. Hancock.

7. Management Games

   Industrial simulation in gaming as applied to training and research.
   Richard V. Evans.

8. Technical Assistance Program in Ordnance

   Simulation in organization concepts to rewrite general directions.
   Clyde W. Johnson.

9. Work Simplification in Hospitals

   Teaching and research in Industrial Engineering techniques covering hospital administration in all phases.
   Clyde W. Johnson and Richard W. Berkeley.

Department of Mechanical Engineering

1. A Study of Laminar Flow in Tubes Subjected to a Sinusoidal Time Dependent Heat Flux

   A study of oscillating heat transfer to laminar flow through tubes with heat capacity.
   David Ayres, Doctoral Candidate. (Chairman: V. Arpaci).
   Doctoral Thesis Research.

2. The Influence of Some Physical Properties on Machinability

   An analytical and experimental study of the influence of the physical properties of a material on its machinability.
   Alex Henkin, Doctoral Candidate. (Chairman: J. Datsko).
   Doctoral Thesis Research.

3. The Formation and Interaction of Bubbles in the Boundary Layer Region for Flow over a Heated Flat Plate

   A study of bubble boundary layers in sub-cooled forced convection boiling from a flat plate.
   Latif Jiji, Doctoral Candidate. (Chairman: J. A. Clark).
   Doctoral Thesis Research.

4. The Response of a Heat Exchanger to a Fluid Flow Transient

   Dynamic response of heat exchangers having a sinusoidal time variant heat source.
   Robert Keller, Doctoral Candidate. (Chairman: J. A. Clark).
   Doctoral Thesis Research.

5. The Ratio of Electrical to Thermal Conductivity in Mixed Crystals of Mg_2Ge-Mg_2Si

   A study on the optimization of the ratio of electrical to thermal conductivity in mixed crystals of Mg_2Ge - Mg_2 used in thermoelectric generators.
   Richard LaBotz, Doctoral Candidate. (Chairmen: D. R. Mason, G. J. Van Wylen).
   Doctoral Thesis Research.

6. The Effect of Vibration on Friction

   To determine and test the theoretical causes of reducing friction through vibration.
   Kenneth Ludema, Doctoral Candidate. (Chairman: J. R. Frederick).
   Doctoral Thesis Research.

7. Heat Transfer in the Internal Combustion Engine

   A study to determine the magnitude of the heat transfer in an internal combustion engine, its variation from point to point on the combustion chamber surface, and the effect of this heat transfer, its magnitude and position in the cycle on the indicated output of the engine.
   Donald Patterson, Doctoral Candidate. (Chairman: G. J. Van Wylen).
   Doctoral Thesis Research.

Department of Mechanical Engineering - continued

8. An Analytical and Experimental Investigation in Three Dimensional Laminar Boundary Layers

   A study to determine exact solutions of the three-dimensional, incompressible laminar boundary-layer equations.
   Ward Winer, Doctoral Candidate. (Chairman: A. G. Hansen).
   Doctoral Thesis Research.

9. An Experimental and Analytical Study of Boundary Layer Stall Phenomena

   To predict analytically the occurrence of boundary layer separation in corner regions and compare the analysis with actual experiment.
   John F. Barrows, Doctoral Candidate. (Chairman: A. Hansen).
   Doctoral Thesis Research.

10. A Study of the Effects of Vibrations on Heat Transfer on a Flat Plate

    A study of heat transfer phenomena from surfaces oscillating with respect to the mean flow of the main body of a fluid.
    Victor Blankenship, Doctoral Candidate. (Chairman: J. A. Clark).
    Doctoral Thesis Research.

11. Studies on the Structure of Detonation Waves

    Spectrographic assessment of chemical reaction progress in the combustion zone and effects of transport properties on the combustion mechanism.
    Eliahou Dabora, Doctoral Candidate.
    Doctoral Thesis Research.

12. A Study of the Effect of Mean Stress on Fatigue in Order to Develop a Method of Applying Fatigue Strength Reduction Factors to Nominal Stress Components and to Relate the Method to the Allowable Stress as Determined from the Goodman Diagram.

    Robert Little, Doctoral Candidate. (Chairman: Charles Lipson).
    Doctoral Thesis Research.

13. Ignition Studies of Lean Fuel-Air Mixtures Involving Conventional Fuels

    To study ignition of conventional fuels in a constant volume bomb.
    John Steiner, Doctoral Candidate.
    Doctoral Thesis Research.

14. An Anslysis and Study of the Design, Construction, Prescription and Clinical Application of Adaptive or Assistive Devices Utilized in the Treatment and Rehabilitation of Severely Disabled Individuals

    This research project utilizes personnel from medicine, anatomy, physiology, psychology, therapy and engineering.
    Office of Vocational Rehabilitation of the Department of Health, Education, and Welfare, sponsor.
    J. R. Pearson, R. C. Juvinall, and H. F. Schulte, Jr.

15. Investigation of the machinability of titanium alloys using a unique high speed machine tool.

    L. J. Quackenbush.

Department of Mechanical Engineering - continued

16. Research on Cutting Fluids

    This study includes analysis of the mechanisms taking place in actual metal cutting, studies of friction and wear in special set-ups where metal cutting is not involved, and tests of the principles which will be evolved for blending oils and additives for the specific environment peculiar to some machining operations.
    Sun Oil Co., sponsor. L. V. Colwell.

17. The Influence of Some Physical Properties on Machinability

    This project involves both an analytical and an experimental study of the influence of the physical properties of a material on its machinability.
    National Science Foundation, sponsor. Joseph Datsko.

18. Material Forming -- Determination of Forces, Power and Maximum Deformation Possible from a Material's Tensile Properties

    Beginning with the true stress-true strain hardening equation, analytical expressions have been developed and experimentally verified that make it possible to calculate the forces and power necessary for a particular forming operation; the yield strength of the part after it is formed; the maximum deformation that a material can be subjected to in a specific forming operation without requiring an annealing cycle.
    J. Datsko.

19. The Combustion of Lean Fuel/Air Mixtures in Spark Ignited Piston Engines

    A study of the factors that limit the use of lean mixtures in engines including the phenomena of ignition, flame propagation, and the composition of the exhaust products.

20. Pool Boiling in an Accelerating System

    A comprehensive study of the effects of a force field such as that resulting from a vehicle acceleration on heat transfer with particular attention to the process of boiling heat transfer.
    J. A. Clark.

21. The Response of Laminar Incompressible Fluid Flow and Heat Transfer through Transverse Wall Vibrations

    The effect of vibrations on natural convection heat transfer is investigated by this research.
    J. A. Clark.

22. Nucleate Boiling Boundary Layers in Forced Convection Flow

    This is a study of boiling in confined channels having application to nuclear reactors.
    J. A. Clark.

23. Dynamic Response of Heat Exchangers

    This research studies the thermal response of heat exchangers resulting from a time varying flow or coolant.
    J. A. Clark.

Department of Mechanical Engineering - continued

24. Pressurized Discharge of Cryogenic Liquids from Containers

    This research is directed at a problem associated with rockets using liquid oxygen in which the heat transfer processes in the oxygen storage tanks is investigated during the time the liquid oxygen is discharged.
    J. A. Clark.

25. Heat Transfer at Zero Gravity

    The purpose of this study is to investigate how the processes of boiling heat transfer may be described in systems in the absence of gravity.
    J. A. Clark.

26. Digital Computer Use in Power Plant Design

    To conduct a research study furthering the use of digital computer equipment in an analytical manner in the design of steam power plant.
    G. V. Edmonson.

27. An Investigation of Elastic and Plastic Properties of Metals during Irradiation.

    An investigation of radiation damage in metals.
    J. R. Frederick.

28. An Investigation of the Basic Mechanisms of Friction

    To determine the effect on friction of elastic and plastic properties of metal surfaces in sliding contact.
    J. R. Frederick.

29. Investigation of the Fundamental Mechanisms of Ultrasonic Welding

    A study to improve the quality of welds, applying ultrasonic welding to materials and thickness not now considered weldable by this method and developing an index of weldability.
    J. R. Frederick.

30. Machinability of Ferrous Metals.
    J. C. Mazur.

31. Behavior of Stack Gas Plumes

    To determine methods of controlling the gas plumes from the stacks at the Seward Station of the Penn Electric Company after the proposed changes in the capacity of the plant have been made.
    Clay Porter and F. K. Boutwell.

32. Forced Convection Boiling Heat Transfer in Heated Rectangular Channels

    Michigan Memorial Phoenix Project and Ford Foundation Project, sponsors.
    J. A. Clark.
    Doctoral Thesis Research.

33. Three projects on System Simulation and Optimization utilizing such processes as automatic program generation, simple learning, and other artificial intelligence techniques.
    F. H. Westervelt.

Department of Naval Architecture and Marine Engineering

Fundamental Study of Ship Hull Form and Propulsion, Scale Effect Studies of Ship Model Test Problems

U.S. Maritime Administration, sponsor.
R. B. Couch and others.

Department of Nuclear Engineering

1. Magnetohydrodynamics of Plasmas with Large Internal Fields
   W. Everett (R. K. Osborn)
   Largely supported by Radiation Lab., Department of Electrical Engineering.
   Doctoral Thesis Research.

2. Transport Theory for Relativistic Plasmas
   A. Ozizmir, (R. K. Osborn)
   Supported by Radiation Lab., Department of Electrical Engineering.
   Doctoral Thesis Research.

3. Photon Transport Theory
   E. Klevans, (R. K. Osborn)
   Supported by Radiation Lab., Department of Electrical Engineering.
   Doctoral Thesis Research.

4. Slow Neutron Scattering by Liquids
   S. Yip (R. K. Osborn)
   Doctoral Thesis Research.

5. Space-Energy Coupling Due to Thermal Effects in Fission Reactors
   J. Olhoeft, (R. K. Osborn)
   Sponsored by Westinghouse
   Doctoral Thesis Research.

6. Diffusive Effects on Neutron Thermalization
   P. F. Zweifel, R. K. Osborn, G. Gyorey.

7. Statistical Theory of Matter
   P. F. Zweifel, R. K. Osborn.

8. Microscopic Transport Theory
   R. K. Osborn.

9. Neutron Ocoustodynamics
   C. Kikuchi, S. Yip, R. K. Osborn.

10. Neutron Physics Programs
    These programs are to develop neutron scattering as a tool for research in crystal structures, ferro- and ferri-magnetic materials, and properties of liquids.
    C. Kikuchi, J. S. King, R. K. Osborn, D. H. Vincent, B. F. Zweifel.

11. Radiation Solid-State Physics Programs
    These programs include the study of solid-state chemistry of vanadium by electron-spin resonance, properties of glasses, effects of radiation in semiconductors and semiconductor devices, and solid-state detectors.
    AFOSR, NSF, NASA, Phoenix Project, and IST - sponsors. C. Kikuchi.

Depear Engineering Department - continu inued

12. Resonance in Radiation Effects

    These programs are to search for and investigate effects that depend critically upon the wave length of electromagnetic radiation in the low energy x-ray region, such as radiation on the enzyme catalase, and the mechanism of F-centre production in alkali halides.
    H. J. Gomberg and A. A. Gordus .
    AEC, sponsor

13. Reactor Theory and Analysis Programs

    These programs are to investigate the effects of physical parameters upon reactor performance to determine new methods of measuring reactor parameters and reactor system behavior.
    W. Kerr, R. K. Osborn and B. F. Zweifel.

14. Theoretical Plasma Research

    This includes photon transport theory in dielectric media in the presence of external fields, relativistic transport theory for plasmas, and magnetohydrodynamics
    R. K. Osborn.

15. Study of Liquid Metal Heat Transfer and Power Reactor Systems

    These investigations are on liquid-metal heat transfer and associated problems of pumping and handling liquid metals.
    Phoenix Project and NASA, sponsors
    F. G. Hammitt.

16. Solid-State Chemistry of Vanadium

    The object of the investigation was to study changes of vanadium oxidation states produced by ionizing radiations.
    AFOSR and Phoenix, sponsors.
    R. Borcherts, Doctoral Candidate. (Chairman: C. Kikuchi).

17. Effects of Ionizing Radiations on Iron Group Ions in Paramagnetic Salts

    G. Wepfer, investigator; C. Kikuchi, supervisor.

18. Cyclotron Resonance in Semiconductors

    NASA, sponsor.
    J. Sickle; Stelakatos, investigators; C. Kikuchi, supervisor.

19. Effect of Nuclear Radiations on the Transport Properties of Semiconductors

    NSF and IST, sponsors
    Oswald, Lee, investigators; C. Kikuchi, supervisor.

20. Radiation Effects in Sapphires

    AFOSR, sponsor.
    Azarbayejani, investigator; C. Kikuchi, supervisor.

21. Neutron Scattering in Solids

    Chen, Tang, investigators; C. Kikuchi, supervisor.