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# Workshop on Stability in Superconducting Magnets

**Los Alamos, New Mexico**

**July 25—29, 1977**

University of California

**LOS ALAMOS SCIENTIFIC LABORATORY**
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# Workshop on Stability in Superconducting Magnets

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## July 25—29, 1977

Compiled and edited by

W. V. Hassenzahl
J. D. Rogers

# WORKSHOP ON STABILITY IN SUPERCONDUCTING MAGNETS

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## ABSTRACT

The week-long Workshop on Stability in Superconducting Magnets sponsored by the Los Alamos Scientific Laboratory was a delightful technical success. Experts in theory and practice from all areas of the superconducting community met to discuss the intricacies of the stability problem. Detailed theory, recent data, computer interpretations of both, and engineering or design solutions to assure stability were presented. Emphasis of the workshop was mostly on the aspects of heat transfer necessary to promote stability and recovery. The great unknowns, lurking as a continuous thread throughout the workshop, and still mostly undefined at the end, were the magnitude, source, and nature of the inadvertent disturbances that might lead to localized or general energy perturbations or instabilities. Wilson likened this to an unknown animal to be protected against by a settler in a new wilderness. Of course, the size of the barricade built by the settler or the design precautions embodied in a particular superconducting magnet is predicated upon an estimate of the size of the beast.

For the use of the attendees we have compiled Martin Wilson's talk of July 31; several summaries of the main sessions, as presented by rapporteurs on August 4; brief synopses of some of the working group sessions; a conference review by Wilson; and a list of attendees.

We have intentionally resisted editing these submissions to a structured format or language style to maintain their freshness and technical content. Each individual's personality is conveyed with the message. Wilson has not been afforded the courtesy to rewrite his "testimony" as if it were to become a part of the Congressional Record. Thus, we have preserved his directness; and he talks to you from the page. We trust the transcription is reasonably faithful.

We are pleased to see new research leading to a better understanding of the limits of operation of superconducting devices. It was clear that we've come a long way from the pioneering, Duco cement days of Laverick to the present situation where magnet and conductor designs are based on the measured values of power or energy that are required to drive some conductors normal. This progress makes one hopeful that in the near future there will be extensive applications of superconductivity that are based on knowledge of the factors that make superconductors stable.

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# WHAT ELSE IS THERE TO KNOW?

By Martin N. Wilson

What I am going to do is try and take the pragmatic view of someone who is faced with the problem of designing a coil that is rather outside previous experience but which cannot be too conservative in its design. How do you proceed? What do you know and what are the things that you really don't know? I find in thinking about this problem that it is very helpful to divide the problem into two interacting parts. First is the disturbance, which is a function of the magnet that we have. The disturbance produces a response in the conductor; and, of course, the main response of interest is whether the magnet quenches or recovers. As disturbances, we can think about magnetic disturbances, mechanical disturbances, thermal disturbances, cryogenic problems, ohmic heating, and the like. These can either be abrupt or continuous, and you can either affect a very large volume of superconductor or in the other extreme can affect a point. Second is the conductor response, where we have cryostability theory, Maddock theory, minimum propagating zone theory, and, eventually, we have to come up with a full time-dependent theory. Experimentally, of course, we have the various measurements that people do on conductors. So I'm really going to try and quickly look at it from this dual point of view. In doing so I will try and summarize what has gone before, but really that's a bit much to expect in half an hour, so you will probably find this summary a bit incomplete.

First, I would like to throw in one or two things of my own. Let us then talk about the response of the conductor to a given disturbance and the sort of factors that come into that and have been considered in various people's theories. Two major factors coming into this are thermal conductivity along and perpendicular to the conductor and the cooling. There are other things, for example, the heat generation in the superconductors as a function of temperature, which, of course, involves the degree of thermal contact between the superconductor and the copper and also, as John Stekly mentioned, the variation of the temperature across the section. I am going to assume that is small. Also, we must not forget the time dependence of the heat generation in extreme cases; Hilal went into this in some detail in the past.

Conductivity then, first of all: the simplest case, of course, is zero-dimensional theory and this is what John Stekly started with today. This is a perfectly realistic situation in a magnet and corresponds to a disturbance that afflicts a very large volume; and, therefore, conduction out of the zone that is being affected is small and can be neglected. Secondly, we can have one-dimensional conductivity along the conductor, again a fairly realistic situation in some cases where there is not very good thermal contact between turns in the magnet. And then we can go to two and three dimensions. The difference among these last three really is not very great, in terms of the physics of what is going on. One important difference in three dimensions, as John Stekly pointed out, is that without cooling, you can still have a normal zone that is in equilibrium in three dimensions. You cannot have it in one or two dimensions.

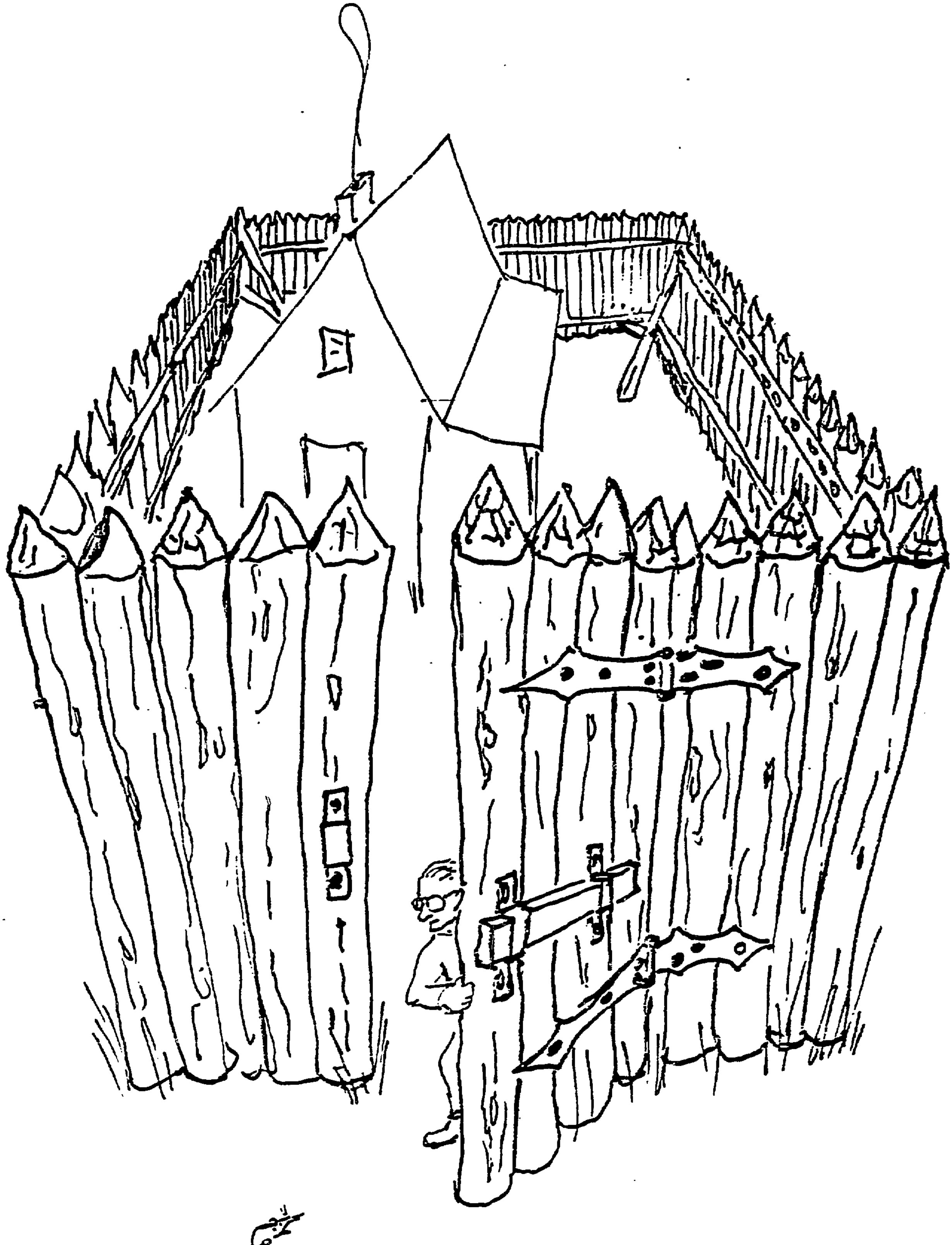
We can combine all different conductivity models with all different cooling models; and, if you want, you can draw yourself a matrix and pick out the case that really applies to your situation. Simplest, of course, is no cooling, which is the adiabatic situation - a perfectly good approximation for resin-potted coils and the like. This can be combined with all the different conductivity models. If you want to put in cooling, then the simplest thing is a constant heat-transfer coefficient. This tends not to be correct in most cases, especially in the boiling case, but it is easy to deal with analytically. Then you can have a heat-transfer coefficient that is a function of temperature and has hysteresis. Finally, if you want to be realistic, and I think you have to be in certain situations, you have to have a heat-transfer coefficient that is not only a hysteretic function of temperature but also is a function of time and the thermal history. That's when things start to get really complicated; but in many magnet situations, that is certainly the case, especially, for example, when you have forced-flow cooling where the heat generation upstream affects the cooling downstream. This is also true in a magnet with constricted channels so that in the time of the disturbance fresh helium can't come in. This is the case that Larry Dresner was talking about.

There is no doubt now that, in certain cases with the three cooling types and all the kinds of thermal conductivity, the idea of a minimum propagating zone is good; and if you do not believe it, let me show you an experimental result. These are some experiments that Iwasa and I did at MIT on the conductor for the U-25 MHD magnet, and here you see a voltage trace versus time of a conductor that was subjected to two different disturbances. The disturbances were very similar. In both cases the conductor carried the same current in the same field with the same cooling and all the rest of it. We were trying to simulate the magnet condition exactly. You can see the trace of voltage versus time and for the lower of the traces you cannot actually separate the heat pulses, but take my word for it that one was bigger than the other. For the smaller pulse you produce a normal zone that eventually decides that it is not quite big enough and will recover with time. In this region you really have a state of unstable equilibrium. You have made the normal region that is just not quite big enough and then it collapses again. A slightly bigger heater pulse, then away you go. Certainly, in the case of A, B, and C, where the heat transfer does not change with time, I think this idea of a minimum zone and being able to determine this point by steady state equilibrium theory is valid. It is not a time-dependent theory, just a steady state equilibrium. You say, "If it's bigger, then it runs away, if smaller, it recovers."

We have the feeling from some of our results that if you put hysteresis in, you can also get an intermediate sort of behavior that we have seen experimentally, and we think we know roughly how it goes theoretically. In that case, you see, the steady state theory does not quite tell you what you need to know. You can still have a minimum propagating zone that will recover, and you will also have something that will try to recover and not quite make it. If the heat transfer is a function of time, and, you know Murphy's Law being what it is, it always gets worse with time. There is no question of it getting better in a sort of constricted situation. You can have this situation where the zone tries to recover because the heat transfer is good, but then the heat transfer runs out on you and away you go. So, as you can see, things are fairly complicated if you go away from the situation of pool boiling with nice big cooling channels. As I said at the beginning, I am particularly interested in the situation where the designer wants to push things to the limit so he wants to take the bare minimum.

So where are we in terms of understanding? I think the analytic models can say a lot to us about minimum propagating zones and so on and can say something about all these dimensions of conductivity, either with no cooling or with constant heat transfer coefficient. And in that way we can really get a lot and a lot of physical insight. For example, you can make some approximations in the situation where cooling runs out with time. I just want to remind you of something I showed at Stanford that rather intrigued me. In the situation where the cooling runs out with time, you can make one analytic approximation; it is not as good as Larry Dresner's but it gives you a feel for what is going on. The total cooling to be supplied in the channel is given by the initial disturbance plus the ohmic heating. You might say, "Well, for a reasonable design let's make those roughly equal. You don't want this to be 100 times that, or vice versa, if you are limited in cooling." If you put that in, you can get an equation for the heating and the temperature rise in the coolant. Let me just tell you the answer. If you want the thing to recover fast enough for the total heat used to be twice that of the heat pulse that you put in, then you must come up with the Stekly alpha parameter, which is the parameter for stability not equal to one but equal to half. This is to say, we have to be overstabilized by a factor of two, which sort of surprises you at first, but bear in mind that these steady state stability theories give you the boundary between the things either just going away or just recovering. So if you are just a bit below that, the thing takes forever to recover, you are at a balance point. You want to be recovering like hell before all the helium boils away. So there your are, a clear factor of two in the sort of cooling area that you have to use if your cooling is limited.

Well, it probably doesn't pay to go much further analytically, and you have to go to the numerical codes either to give you the right shape of the boiling curve, the temperature, or to build in this thing about running out with time. I think that the sort of codes we have, the codes that Larry Tuner and Wilkie Chen told about and Mitch Hoenig for the hollow-cooled conductor, are probably adequate for most of the cases. What we are missing, I think at the moment, is heat transfer in the transient case. I think we need a lot more information because we don't really know - and especially heat transfer when the heated surface is cooling down. One tends to find in transient heat transfer data that people are very keen on what happens when you are trying to heat up the surface; but what we are concerned with generally is the recovery. That is something that we need to know quite a bit more about. What we need first, I think, is a relatively simple model to give us a feel, like Yuki Iwasa's one-seventh rule that we will probably say something about tomorrow. It is going to be fairly complicated and difficult work, but I think it is fairly well mapped out. I think in a couple or three years we'll be able to solve this problem pretty well analytically. Of course, that is not much use to engineers designing magnets today. And my advice to them, as John Stekly



said, is to take a sample of conductor and measure it, so that you will get the right answer. Take a sample with current, in the field, and so on, and then put the disturbance on it that you expect to find in the magnet. Either short pulses or long pulses on big regions or small regions. How does the conductor recover from those disturbances? That is your safety margin.

No more to be said. Well, of course there is. That begs the whole question at the moment. It makes the point that you really can't talk about stability without talking about the disturbance that you are stabilizing against. Really, this seems to me quite a big unknown. For example, in the past we have talked about stability, a conductor that is stable against having the superconductor raised above its critical temperature, about flux jumping, a conductor that is stable against the release of the stored mangetization energy. The problem now is we are talking more generally about the whole magnet situation; especially, we are talking about mechanical energy releases caused by stresses in the magnet and so on. To my knowledge, nobody has actually seen this mechanical energy release, never mind measured it. So it is a bit hypothetical.

It is a bit like going to live in a new, wild and unknown country, building yourself a house, and deciding that you ought to build a fence to keep out the wild animals without being too sure of what these wild animals are. So what you do, if you want to sleep soundly at night, is build yourself an elephant fence because they are about the largest animal that you can think of. And similarly, if you are building a cryostable conductor, you think about the biggest possible disturbance and stabilize against that. And I guess if you do that, you will sleep soundly at night. But, after a few years of living in and paying the mortgage on this inconvenient and rather expensive house, you have to be prepared to wake up one morning and to admit to yourself that perhaps you overdid that design a bit. Equally well, of couse, is that it's no good setting a mousetrap if there are elephants around. So it seems a good idea to try and find out roughly what sort of disturbances there are before starting to build. Find out what the local fauna are.

Now I can think of about three or four ways to learn about the disturbances. You can look at other people's houses, of course, and see whether they have survived or been eaten alive. You can watch out for animals and make a tabulation of what animals are around. Finally, and this will appeal to the theorists among us, you can study zoology and from knowledge of the local terrain and weather you could predict the animals that are most likely to be prevalent in that part of the world.

I want to look at two houses that other people built. One that worked and one that worked tolerably well. First, the U-25 magnet, which I am sure we will hear something about from Bert Wang later in the week. This, as you probably already know, is a large MHD magnet. These are results on some tests that we did at MIT where we plotted the energy, in joules, needed to cause a quench versus the transport current; and this is the theory. The magnet works very well, and the theory seems to work pretty well in this case because the heat transfer doesn't change with time. It's got nice big cooling channels. But don't take the theory's word for it, look at the experiments and it's not so bad. The magnet we know worked well at 900 A; and, therefore, we can infer from this that any disturbances present in that magnet were less than 0.5 J because we learned from our short-sample measurements that if we put in more than 0.5 J, the magnet would have quenched. So that has given us a "less-than" result. Unfortunately, the magnet didn't quench, so we don't know what the "more-than" result is. Similarly, for long times, I can show you the same picture that Stefan showed for his niobium-tin coils, and the long time pulses now go to a steady state power. In that case it was about 7 W. So, if the disturbance is at this point, I can say to you that I know that either 7.5-W steady state power or a 0.5-J pulse would have been sufficient to quench it. Equally well, one could do other experiments and other theory for disturbances that were very large, and then I would be talking about joules per cubic meter or watts per cubic foot. So, there is one that worked well.

Secondly, then, I would like to look at a whole family of magnets for which we do have quenching data. These are the resin-impregnated magnets, such as are used in reasonable-sized solenoids or beam-line dipoles, where we have typically seen training. Everybody has seen training. I think what we are seeing is when you turn the magnets up at first there is some energy release; and I am willing to bet it is mechanical, because these magnets are made from fine filamentary conductors and I don't think they have flux jumping in general. You see training starting at, let's say, 50% of short sample, and you may get up to 90%. What I have done there, using a fairly simple theory, is to calculate the energy needed to cause a quench as a function of the fraction of critical current. Now these are purely theoretical values and are based on a simple theory. Some day we ought to measure it and improve the theory. But at least it gives a magnitude, and what it says is that for these magnets the energy required is $10^{-5}$ J, which is a remarkably small quantity. I was giving a talk about this, and to show what $10^{-5}$ J is like I dropped a pin. Everbody had to be very quiet because you only have to drop a pin 5 mm to release $10^{-5}$ J. It is quite remarkable.

We have Stefan's 10 J for flux jumps in niobium-tin coils. We have somewhat less than 0.5 J for the energy release in U-25, and we have $10^{-5}$ J for energy release in potted systems. We cover an enormous range; and, really, we don't know very much about it at all. I think we ought to get some experimental data. We really want to know more, especially about big magnets and especially about big magnets that quench prematurely. Those are the ones, unfortunately, that you don't hear about too much. Certainly, if anybody ever had the time to spare, it would be a great service to the community to collect all the data on big magnets that quench prematurely and work out the energy needed to make a minimum propagating zone in each case because, I think, they are going to find a very wide range of energies.

Of course, nobody really wants to set out to make magnets that aren't going to work, and so we also ought to study the zoology of the animals we are likely to find. From the noises we hear coming out of coils and also from the power levels of steady state disturbances that you calculate, I think, generally, they seem unreasonably high. I can't believe that you have 10-W magnets without knowing about it. You would see voltage across the terminals and so on. So for my money, it is the abrupt disturbances that are the problem. I think, probably, we understand quite a bit about flux jumps. We can certainly understand what energy per unit volume is. I suppose the big doubt still in our minds is what volume is involved. We tend to do two-dimensional rather than three-dimensional calculations. However, I think we know enough about it to design magnets where flux jumps don't happen, if that is what we want to do. But we can't really say much about mechanical disturbances yet.

I hope we may either add to this list or eliminate a few items during the conference, or, perhaps, go into a little more detail.

- Abrupt disturbances: flux jumping and perhaps external spikes from the power supply; in the magnet area, mechanical slipping, debonding, shear failure, insulation crack propagation, serrated yielding, all giving sudden releases of energy either over large volumes or maybe over small volumes, I don't know.

- Continuous sources of energy: we can have things like hysteresis loss in the superconductor and eddy currents, mechanical hysteresis, joints, cryogenic heat leaks, and that sort of thing.

But, I say, for my money, it is these abrupt things that are going to be bothering us most of all. And the problem is with those things you can do pessimistic calcualtions. You know you can calculate for the elephants, but you get very bad results then. You get enormous releases in joules, whereas, you know magnets like U-25 work well; and, therefore, you are bound to admit that you know you are being too pessimistic. The question is how pessimistic. My feeling is that what we need to do is to monitor the coils better so that as time goes on we can build up a kind of a conventional wisdom in the community as to what is really going on and perhaps to correlate experimental data with theoretical data. That really is a plug for a little group that I would like to get together during the week, and I hope perhaps at the end of the week we might be able to make some recommendations as to what people with big coils do in the future in the way of measuring them. I mean it is nice to know that a coil works, but it will be much nicer to know with what margin of safety did it work. How big were the disturbances and what actually were you stabilized against? Was there a factor of 100 or was there a factor of 1.1? And, of course, when we really understand this, we can start putting the design of stability on a quantitative basis. Not only can we design the conductor to withstand the disturbances we expect, we may even be able to design the windings, as well, to minimize the release of energy using slippery insulation, improved force support, and so on - the things that suggest themselves as ways of probably reducing mechanical energy release. But we really don't understand enough about it.

## MONDAY, JULY 25, 1977, OVERVIEW

By Stefan Wipf

The theoretical aspects of the problem of stability were dealt with on Monday. Major talks were given by Stekly, Turck, Dresner, and Wipf. These four talks were all concerned with solving the thermal diffusion equation:

$$\alpha_1 \quad \nabla^2 T + \alpha_2 \frac{\partial T}{\partial t} + \alpha_3 \Delta T + \alpha_4 g = 0 \quad ,$$

where $\alpha_i$ are functions of place, magnetic field, time, temperature, and possibly other things. In addition to a diffusion term and a time-dependent term, there is a cooling term for when a coolant is present, and a Joule-heating term. I should also mention that John Chi touched on this problem on Wednesday, and he suggested that work on fission reactors has produced many codes to solve this equation. However, the equation can never be solved in a very general way; one can only develop special solutions. The usual approach, for instance, by John Stekly and Wipf, is to solve the steady state solution, that is, omitting the second term. The talks by both Stekly and Wipf covered very similar ground. Stekly emphasized the operation above the short-sample current density, $j_c$, and expressed the results in terms of what he called "terminal characteristics," the V-I characteristics, when looking at the voltage of the coil. Wipf described stability in terms of the equivalent of a potential well, also called a "basin of attraction," and showed that there are some disturbances that can force the superconductor out of the basin of attraction. Thus, in most cases, the problem has these two aspects, potential well equivalent and disturbances. But basically, it is a steady state picture. One more point, the basin of attraction can be described in terms of what is called "minimum propagating zone" (MPZ); and this is complementary to the more familiar term, "mimimum propagating current." In other words, the minimum propagating current is only defined if the size of the normal zone from which propagation takes place is also given. This size is the MPZ.

Larry Dresner also showed solutions to the same equation. He worked out the minimum propagating current in some very special conductors, cables enclosed in a conduit. He also covered an additional interesting effect, namely, vapor locking. One should keep in mind that vapor locking is not really a disturbance that tries to force the superconductor out of the basin of attraction but rather a sudden change in the shape of the basin itself. One can probably still keep to the definition of stability in terms of basin of attraction and energy releasing disturbances, provided allowance is made for processes that change the basin disadvantageously without actually releasing energy.

In the other talks the possible disturbances that occur in a device were emphasized. Turck solved the equations for magnetic disturbances that, of course, produce flux jumps. The difficulty with magnetic disturbances is that the heat production term becomes a function not only of B and $\dot{B}$, but is itself influenced by a solution to the magnetic diffusion equation, thus making this a very complicated problem. Again simplifications have to be made. In the beginning of superconducting coil manufacture, flux jumps used to be the biggest disturbance. They were of the order of 10 J, and the introduction of multifilament conductors and elimination of flux jumps as a major problem were great steps forward. Flux jumps, however, can happen in any material with a critical current density and a normal resistivity, or simply a resistivity strongly dependent on current and temperature. The criterion for flux jumping depends on a comparison between thermal and magnetic diffusion. If the conditions are right, a flux jump occurs, which means a release of magnetically stored energy in an entropy producing process. Multifilamentary wire of sufficiently fine subdivision approaches a material with a critical current density and a resistivity highly dependent on current and temperature that, if the conditions are right, can exhibit flux jumping. The flux jumps are now much smaller and, in practice, comparable to the other disturbances. That is the problem which Turck has solved in great detail. This is very useful work as it defines the conditions under which flux jumps may still occur.

Martin Wilson, in the fifth major talk, summarized the present state of stability theory. He pointed to future work to be done that should focus on two things, disturbances to which a magnet is actually subjected and the response of the conductor. In the past we may have worried about bigger disturbances than actually occur, rather like the fellow who builds an elephant fence around his house in a country where there are no elephants.

Continuing with the disturbance spectrum, Bert Wang talked about the U-25 coil and an analysis carried through at MIT showing that the U-25 coil at operating current is safe for localized disturbances of 0.5 J. In other words, a disturbance of only 0.5 J would quench it. Therefore, whatever disturbances occur inside that coil must be smaller than 0.5 J since it is operating successfully. The difference between the energy released by the actual disturbances and 0.5 J is the stability margin. The largest disturbances anticipated in the U-25 coil appear to be friction between turns. We had two presentations about frictional disturbances, one by Iwasa and one by John Murphy (whose talk was actually presented on Wednesday). Both plan to measure friction inside superconducting windings, a very laudable effort. Experimental results are not available yet, but the two different experimental setups were described.

Another presentation was by Mawardi, focusing on the following: disturbances are always entropy producing events; the final product is heat. However, prior to the final stage, the energy is in a different form; some of it may be acoustic energy. He reviewed the size and frequencies of acoustic emissions connected with plastic deformation and other processes. This raised the question as to whether it would be possible to make use of acoustic emissions to monitor coils. This led beyond Mawardi's review into the topic for a week-long workshop.

In closing, and still impressed by the discussion on definitions of terms, I want to remind you of John Stekly's beginning sentence. He said he never uses the term "cryostability," and my parting advice is to do likewise.

# TUESDAY, JULY 26, 1977, OVERVIEW

By John D. Rogers

The Tuesday presentation and discussions focused upon some rather fundamental items for large coil stability. Behind these was the continually emphasized need for basic experiments to be run on reasonable conductor samples to assure stability under the conditions for its ultimate use. The entire day's work dwelled mostly on heat transfer.

It might be said, as for the entire session, that the physicist has rediscovered heating and heat transfer effects and that the cryogenist and engineer have found that heat transfer applies to the stability of large coils. The great area of uncertainity seems to be exactly what is, how to predict, and the anticipated magnitude of the specific heating mechanisms that perturb a coil or magnet so that heat transfer protection, if needed, will be adequate for stability.

John Purcell emphasized that large coil design must first consider the structural force problem and then other design issues. He indicated that stability is basic to recovery from a Joule-heating condition after a disturbance. Large coils must be of a design to accommodate the initial perturbation that may drive it normal and then to provide the cooldown capability. John felt that actual perturbations to drive large coils normal are not inherently large.

Don Cornish gave an endorsement for $Nb_3Sn$ superconductor. He offered a note of caution from early experience for those who expect enhanced coil performance by going below the HeII transition. He described the complex mirror machine magnet and endorsed the building of a test coil and small conductor sample tests since the heat transfer could not be calculated. Despite the concern, the results on the mirror machine conductor were not unexpected.

John Stekly pointed to large coil design problems and emphasized the significance of heat transfer. He pointed out that in the final analysis he recommended performing a recovery test on the conductor to make sure the design is right.

The panel members touched upon individual magnet design problems.

Bob Bradshaw discussed the difficulties inherent in the Large Coil Project (LCP), namely, long thin channels, low helium inventory, and the peculiar nature of the requirement to have recovery from a normal one-half turn rather than a pulsed heat load. He concluded that an experiment will need to be performed on the conductor to establish the heat transfer.

Henri Desportes described the muon channel magnet of Saclay, stabilized and protected with Al soldered onto Cu-NbTi conductor and with close thermal coupling between coil turns and to a He flow tube. A model coil was recently built and tested, just before the workshop. He stated that their studies showed the advantages of using superfluid He to have tokamak toroidal field (TF) coils be able to accommodate fault mode operation with a 1-ms loss of the fusion plasma.

John Murphy indicated that ac losses for the LCP or TF coils were important and that wire motion must be restrained.

John Stekly outlined the steps of selecting or designing a superconductor. Far down the list in order of design were those elements related to stability, e.g., amount of matrix, heat transfer area, and heat flux; however, he said these should be based upon or checked with an actual conductor heat flux and recovery tested with a good model.

S. T. Wang described the Russian MHD magnet and concluded that the pool bath coolant channels could have been reduced in size, that the expected trends of increased friction heating and frequency of mechanical motion with higher field were observed, and that training decreased with subsequent coil charging. Conductor motions were less than 10 ms in duration.

John Chi felt that stability for the LCP is a thermal design problem and not specifically a heat transfer problem. He emphasized the importance of limiting critical quality and the importance of proper coolant channel design.

General panel or audience observations were:

1. That no one had observed P or T change in a He bath arising from pulsed mechanical effects on large dc coils;
2. That the safety factor of a coil once it has gone normal is independent of the amount of superconductor available, only the normal conductor and heat transfer are significant; and
3. That the fluid at the top of a pool boiling or cooled system is at the equilibrium vapor pressure and temperature and is not "hot," i.e., it is cooler than the liquid below it.

Specific panel comments of worth were:

1. That a forced flow cable in a conduit could allow for wire motion;

2. That small model coil tests for cryostable systems can be extrapolated to large coils except for those items that are ignorance factors arising from size alone and not the superconductor; and
3. That the concern for vapor locking or starving in pool cooling systems may not be a problem if the channels are properly designed.

Moyses Kuchnir discussed the effects of using single-phase helium at 2 atm for protection and gave data on heat loads for times of 300 to 400 ms that drove the conductor to temperatures as high as 550 K. Stabilization of the doubler coils was verified by a hairpin type experiment in a realistic environment. The tests were run with a pulse imposed on a heater while a steady-state current was run through the conductor. The cable of twisted wires was wrapped in a porous insulating wrap. Calculations, including the specific heat of adjacent He, correlated with the data.

William Gilbert described the ESCAR dipoles. Stability was obtained by use of Stabrite soldered Rutherford cable in a porous structure, small 6-m filaments, and, incidentally, carefully constrained conductor inherently obtained to meet the field requirements of the dipole. Shrink rings are over the winding. Training became permanent only if sufficient energy was dumped into the coil.

Michael Superczynski described the NRL homopolar potted coils. Because of poor heat transfer, normalcy propagation velocities are high. Training occurs and short-sample current levels are reached.

Al McInturff described the Isabelle dipoles. Training is observed and supercritical phase, forced-flow cooling is used. The normalcy energy is dumped into the dewar, and the other coils in the circuit can be isolated and discharged at the rate of 2 to 4 T/s.

## WEDNESDAY, JULY 27, 1977, OVERVIEW

By Henry Laquer

Most of the discussions on Wednesday were concerned with heat transfer. This rapporteur has not been able to attend the additional workshops on heat transfer, but perhaps this is not such a bad position to be in after all, since Vince Arp summarized those very well this morning. Also, my point of view in talking about heat transfer can thus be that of the nonspecialist who is mainly interested in using the information, rather than that of the specialist whose job is to produce the required data. In comparing heat transfer to some of the other problems of superconductor stability, it would appear that the carpenters building the stockade have been much more successful in their work than the scouts that are out looking for the animals.

The basic heat transfer data fall into several groups, as Vince Arp classified them in his talk. First are the data on what is called pool boiling; and they really appear to be sufficient. Certainly they are for the film boiling "takeoff" in spite of the additional refinement that may be possible. On the "recovery" branch, on the other hand, one could use more information, especially if temperature-controlled, heated surfaces were employed. It is also clear that superconductor recovery is certainly not a steady state condition, which generally makes data taking difficult.

Now as to transient data, we have useful new results primarily from Yuki Iwasa's pragmatic approach of measuring under increasing and decreasing temperatures and actually looking both at channels and at different surfaces rather than worrying about the full curve. For the magnet user, those are the data that give one insight. An additional new concept on that subject, that I think was made clear by Martin Wilson, was that during the cooldown, especially, it's not the specific heat of the copper that governs the behavior so much as the specific heat attributable to a boundary layer of helium about 0.1 mm thick. That information, as far as I know, definitely did first come out at this conference. It seems to fit the data and it also agrees with one's intuition. As a matter of fact, the boundary layer is a way of adding enthalpy both on the way up and down, so it has implications for stabilization as well.

Other new information appears in the vapor locking analysis from Oak Ridge. Here we have Larry Dresner's suggestion that the effective heat transfer varies with the area that is blocked. In other works, partial vapor coverage will reduce heat transfer in a way that can now be analyzed quantitatively. That brings us to the experimental work at Oak Ridge that straddles the region of pool boiling and counterflow boiling.

Before I go on the the forced flow, we have the finite element computer results of Larry Turner. That is the kind of analysis that also helps one's intuition a great deal, to see the temperature profile with recovery and with runaway all depend critically on the energy input. I personally would like to see John Chi's generalized theory and have a chance to look at it in a little more detail than I was able to do during this workshop. It was more than I could absorb in the time. The same thing is true to a certain extent of Hilal's work. One thing about these large computer programs is that you have to be aware of the hidden decisions that are made. In Larry Turner's case, the decisions are made in the heat transfer curve he used. The shape of that curve makes the decisions on what happens; and, if there are any uncertainties here - well, you get out what you put in.

The third heat-transfer regime that was discussed was the one of superfluid helium. The new part is the work from the group at Grenoble. The suggestion is to put pressure on the helium, not to let it go to the vapor pressure line but to let it go to the lambda line, which then gives a very much improved effective thermal conductivity. That is very important for some systems, and the negative words we had heard earlier from Don Cornish about helium II cooling certainly do not apply to this particular experimental regime. It is always pleasant for the physicist to hear that physics results are now finding some engineering applications and that something can be done with the work that has been in the literature for quite a long time, and that certainly applies to this new usage for superfluid helium.

I am going to discuss the forced-flow session and the forced-flow analysis together in spite of the fact that they were at different times. We heard of Professor Tsukamoto's very generalized analytical results, which again need much study in great detail to be able to use them in designing systems; but there is much information there. The other approach to that problem was in Jim Hoffer's discussion of his computer results; that is the sort of information to reinforce one's intuition; one can visualiz what actually goes on in a system. One or two amperes up or down and the system recovers or it runs away. It is really that kind of hair-trigger behavior. The exact place where that occurs, of course, depends again on what one assumes about the heat transfer, but the qualitative features are there and were verified by Iwasa's experimental results.

On the experimental work, we heard from John Miller on the extensive and ongoing program at Oak Ridge. We also heard from Mitch Hoenig about both new and old results that are, of course, tied to the Oak Ridge program. It is certainly important to realize that the immediately available coolant reservoir is more limited in a forced-flow system than it is in a pool boiling system. On the other hand, transient heat transfer may be enhanced considerably in the forced-flow environment. This, then, would imply improved stability against small disturbances and lower stability against larger disturbances (again that question about the size of the animals). At any rate it would be nice to have magnets operating with forced flow, and I think this will happen in the fairly near future.

We also had a special request paper by Bernd Matthias, which was the first time I have heard him talk about a nonbreakthrough, in this case $V_3Si$, with a transition temperature of 29 K, claimed incorrectly in some recent German patent applications.

# WORKING GROUP ON ELECTRONIC MONITORING OF COIL PERFORMANCE

By Y. Iwasa

## I. THERMOMETERS

- Cannot be used to identify the type of disturbance.
- Are primarily to observe the effect of a disturbance.
- Two Methods:
  1. Localized, easy to mount; have to know beforehand where to look.
  2. Gross, difficult to mount; can indicate the total volume affected.

Types of Thermometers:

Gross

### A. Superconducting Composite Wire

Place superconducting wires alongside the conductor.

Advantages

1. Can tell the total length of conductor affected.
2. Fast time resolution, ms.

Disadvantages

1. Conductor and winding modification required.
2. $T_c$ varies with B.
3. Not sensitive between $T_c$ and 20 K (This can be an advantage - see above.)
4. Possible electrical short problems.

### B. Film Resistor

Place Film Resistor alongside the conductor.

Advantages

1. Can be placed easily in some cases.

Disadvantages

1. Reads average temperature over some length.
2. Lacks strength; may break.
3. Covers the surface and reduces heat transfer to helium.

Localized

A. Thermocouples; carbon resistors; etc.

Advantages

1. Can accurately tell what is happening at key preselected locations; easy to mount.

Disadvantages

1. Have to know where to look. Spatial range limited.

II. STRAIN GAGES

- Can identify mechanical disturbances.
- Best used to check key structural locations.
- Gage itself localized.
- Can measure gross effects.

III. ULTRASONIC

Not practical, in situ.

IV. OPTICAL

- Cannot identify the type of disturbance.
- Primarily to observe the effect of a disturbance.
- Cannot quantify the effect of a disturbance.

V. ACOUSTIC

- Identifies mechanical disturbances.
- Difficult but not impossible to quantify a disturbance or its location, can be done, in principle, with only three detectors.
- If interested only to monitor gross mechanical disturbances, the best method among five methods considered - easy to mount.
- No problems with respect to other sources of noise.
- Easy to mount.
- May cause problems with shorts if mounted directly on conductor.

# WORKING GROUP ON HEAT TRANSFER

By V. Arp

## I. PROBLEMS

- Steady state data probably inapplicable during time $<10^{-1}$ s. Very little transient data available. They are urgently needed.
- Effects of insulating layers:
  1. May be predictable to some extent provided k and $C_p$ of film are known; and
  2. Further study is needed.
- Effects of confining geometries:
  1. In bath cooling the effects are easily predicted, but some correlations developed for other fluid may be applicable;
  2. Needed work is in progress for forced flow; and
  3. Further work is required.
- Advantages and problems of rapid, high-flux heat removal by superfluid are largely unexplored. Further work is required.

## II. RECOMMENDATIONS

Heat transfer studies should be made for the following conditions.

Times: $10^{-3}$ to $10^{0}$ s, most interest
$<10^{-3}$ s, special cases

Materials: Characteristic of copper, bronze, and insulating coatings

Mode: Temperature (not heat flux) should be controlled. dT/dt, both + and -, should be explored.

Geometry: Explore effect of confinement. Try to develop useful correlations with geometrical limits.

Helium: Both single and two phase; both static and forced flow.

The effect of flow on transition and film boiling should also be explored.

## GENERAL OBSERVATION

We who do helium heat transfer measurement for superconducting magnets have tended to isolate ourselves from important heat transfer work done on other fluids.

Some new correlations of forced, two-phase channel flow were presented by Sydoriak and are given below. The critical, nucleate-boiling heat fluid, q, given by the equation fits 131 data points in seven publications with a standard deviation of 13%.

$$q\left(\frac{W}{cm^2}\right) = 27\lambda\ G^{0.23}\ L^{-0.24}\ \left(\frac{D}{D+B}\right)^{0.69}$$

$$\times\ \beta^{-2.65}\left(1+32\beta^{-2}\right)^{-2.04}\ (\kappa\tau)^{0.062}$$

cgs units

$B$ = 0.15 cm, one heated wall
0.18 cm, two heated walls
0.62 cm, tubes
$G$ = mass flow/unit area, g/cm$^2$s
$\beta$ = liquid/vapor density ratio
$K$ = thermal conductivity (wall), w/cm K
$\tau$ = wall thickness, cm
$\lambda$ = latent heat of vaporization J/g
$D$ = plate separation or diameter, cm
$L$ = heated length, cm

Assumes quality = 0.0 at inlet

Calculations are under way that show the extent to which the pumping speed must be increased to overcome vapor block in the channels of a magnet. For example, if complete vaporization occurs at the inlet of one channel and the LHe bath temperature is best for critical heat flux (4.65K), then the circulating pump will need to provide an average mass flux of 2.2 G to have a flux of G in the vapor-filled channel.

SUPERFLUID

A. Nonlinear heat transfer coefficient.
B. Heat transport through helium.

$q_{max}$ 1 W/cm$^2$ (boiling)
4 W/cm$^2$ (subcooled)
(larger) (forced)

C. The major question that must be answered is: "What is the magnitude of time-dependent heat pulses, high amplitudes, and short times that can be accommodated by transient heat transfer, which may be as great as 20 $W/cm^2$?"

If the design constraints permit the use of heavily stabilized conductors (large copper to superconductor ratios) with low overall current density, then heat transfer will not be a problem and existing heat transfer data may be adequate. But, if one wants to understand the stability of existing adiabatically stable conductors or magnets and to design closer to the stability limit, then relatively little of the available helium heat transfer data is unambiguously applicable.

# REPORT OF WORKING GROUP ON COIL MONITORING

By M. Wilson

## I. OBJECTIVES

The group decided to concentrate exclusively on the problem of measuring the size of energy disturbances inside an operating superconducting magnet. It was hoped that in future large magnet tests, more measurements of this kind will be made so that conductor stability design can be put on a more quantitative basis.

## II. METHOD OF WORKING

After some preliminary and broad ranging discussion the group decided to divide in three smaller groups that would examine the three most apparently feasible techniques.

A. Electrical measurements - voltage, magnetic field, etc.
B. Coolant measurements - helium boil off, pressure, etc.
C. Structural measurement - acoustic emission, strain, etc.

Under each heading the groups set out to evaluate the following factors:

A. Areas of application - magnet modifications required,
B. Sensitivity - minimum energy pulse detectable,
C. Spatial resolution - volume afflicted by energy pulse,
D. Time resolution,
E. Calibration,
F. Types of disturbance that can be detected, and
G. Practical experience so far.

## III. ELECTRICAL MEASUREMENTS

### A. Voltage Taps

Commonly employed in testing magnets, voltage across a coil section is

$$V = IR + \frac{d}{dt}(LI) + \frac{d}{dt}(MI) + \frac{d\phi}{dt} ,$$

where the first term might come from the transient normal zone, the second term from a current fluctuation or movement within a section, the third term from current fluctuation or movement of neighboring sections, and the fourth from flux jumping. It was agreed that a dI/dt term should be subtracted from all signals, i.e., by means of a Rogowski coil. This is necessary to eliminate extraneous effects of power supply, line fluctuations, etc. Thus, we have

$$V = IR + \frac{IdL}{dt} + \frac{MdI}{dt} + \frac{d\phi}{dt} .$$

The mutual inductance term will be smaller than the self-inductance term in the section where the disturbance occurs. We ignore it. For the remaining terms, generally

$$\int VIdt = \text{work done}.$$

For example, take a wire of length $\ell$ ; moving a distance, d; while carrying I in a field, B.

$$\begin{aligned}\text{Work done} &= B\ I\ell d \\ &= I\Delta\phi = \int VIdt.\end{aligned}$$

The problem is that this work may not all be dissipated as heat, e.g., in mechanical motion.

Work done = change in elastic energy + dissipation.

### B. Magnetic Measurements

Motion of the magnet with respect to a search coil will induce a voltage in a search coil. We must again subtract LdI/dt to guard against transients on the line. Greatest sensitivity to small local movements will be obtained by putting the search coil close to the coil. Sensitivity and discrimination against extraneous noise may be greatly improved by dividing the search coil into sections that are connected to respond to different harmonics, for example, Fermilab's experience is that coil motion shows up most strongly in the sextapole field.

## C. Orders of Magnitude

Energy = VIt

Practical detection sensitivity is ≈$10^{-3}$ V (after bucking out dI/dt, etc.) at a current ≈1000 A, say, time as observed in coils so far ≈10 ms. Therefore, the minimum energy pulse detectable is

$$E = 10^{-3} \times 1000 \times 10^{-2} = 10^{-2} J\ .$$

Rough calculation show that energy pulse from conductor movement could last 0.1 ms, so that the 10 ms seen so far may be characteristic of the detector response.

## WORKING GROUP ON
## FACTORS RELATED TO COIL STABILITY

By M. Wilson

I. COIL DESIGN
  A. Mechanical - Coil Rigidity
  B. Electrical - Turn to turn and coil to ground insulation
  C. Thermal - Heat transfer, thermal diffusivity, cryostability, and cooling channel design

II. GAS-COOLED POWER LEAD
  A. Temperature, flow rate

III. QUALITY CONTROL AND HUMAN FACTOR
  A. Conductor, coil fabrication, and assembly; cleanliness and human errors; etc.

IV. INSTRUMENTATION AND CONTROL
  A. Gas-cooled power lead - Temperature, voltage, and flow sensors
  B. Quench condition - Energy removal and dumping energy uniformly in the coil
  C. Instrumentation malfunction and fail-safe circuit design

# COMMENT ON STABILITY

By John Purcell

1. Stability is a great deal more than just heat transfer.
2. The biggest question in magnet stability is the size of perturbations that exist. Past evidence points to very small energy releases in pure tension coils such as solenoids and tokamak toroidal field coils.
3. Stability and coil protection are separate items and must be considered separately.
4. History of coils that are in operation give a good check of any stability theories. If the theory says the coils do not work, then perhaps the theory should be reconsidered.

# CONFERENCE OVERVIEW

By M. N. Wilson

This is my personal view of where we are on stability. It is very nice to be able to report to you that I think that the profession of Superconductor Stabilizer has advanced another point on the road toward respectability. I remind you that most professions start off somewhere between a mixture of cookery and witchcraft and slowly advance toward respectability. We can look on it rather as in the medical profession.

To see this process taking place: when I started in superconductivity as a fresh young lad, some time ago, I remember the first paper I read was by Charlie Laverick about instabilities in short samples. The conclusion I drew from the paper was that the right kind of stabilization that we should all be working on was Duco cement. It was a great problem to us in England because we did not actually know what Duco cement was. But, anyway, wires embedded in Duco cement were pretty good; and Stefan told us about epoxy stabilization, and so on.

Well, in science, of course, stability means numerous things and I suppose the first hint of respectability came with cryostability, and really that was pretty good. It made possible a lot of magnets in the low current density region; and I suppose that really the big magnets for which the trade, in general, can be proud, were made possible by cryostability.

Then, along came filamentary superconductors for those of us who were not able to manage with the low current density. We had to say, "We'll have to kill the problem rather than to get around it." We, of course, were quite convinced that the problem was flux jumping, and probably basically it was; and we brought out filamentary superconductors to make high current density magnets.

As time goes on, these theories have been improved and ramifications added. We've had the Maddock and James wing added to the low current density side and the Turck self-field stability tower put on the high current density side. The two theories have grown up without really looking across this gap between them and thinking about it and seeing what the other one was doing.

This is a gap in current density in which a lot of magnets could well work, and we have largely ignored it. I certainly ignored it myself. I mean, when people said to me, "I want to build magnets and I will use filamentary superconductors; but I will put a bit of helium cooling in there as well, just in case.", I said, "Well, what are you supposed to be doing? Do you want cryostability or filamentary stability? Make up your mind. Don't try to do both. There is no point." There was no point because we really couldn't analyze it and we couldn't put any numbers to it. And, of course, there were problems at the high current density end, and now the story can be told.

When Peter Smith and I tested our first filametary conductor magnets, the perfomance was quite apalling. We went through a sort of crisis of confidence where we used to say to each other, "If we really did not know these conductors were intrinsically stable, we would have to admit that they were just as bad as all the others." Of course, the problem was mechanical movement; and it has been with us ever since.

Now I think we have the basis for numerical respectability in the gap between the two, and also extending up here in terms of mechanical effects. In fact, we've had it for five years, but we didn't know it. It was presented by Stefan Wipf at Annapolis and was universally ignored, I guess. It contained a simple mathematical approximation to a simple case and simple figures, and so on; and perhaps that was too simple for us. Now we have some very high powered computer programs; and we can look at the transient response of the conductor and the disturbances, and so on. Now everybody is very interested. If you look at the transient responses in Iwasa's data, you see that, in general, they come to a flat part where they decide either the superconductivity is going to be recovered or it is going to break away. That is the Minimum Propagating Zone, which is amenable to quite simple-minded analysis. It tells you most of what you want to know about the situation, which is whether it is going to recover or expand. It is the continental divide or the 12 noon or whatever you like between one regime and the other and is, therefore, the most important thing. At the time, as I say, we were all too stupid or preoccupied to notice. Perhaps there is a lesson for the future there.

What next? Well, working on from this theory we have a fairly new way of looking at things that I would claim could be applied across the whole range of stability theory, from the highest current density magnets to the lowest.

But I think it will probably prove most useful to us in the in-between, the region that is not cryostabilized, but is not right up at the top end either. The region you generally find yourself in is when you start taking account of the economic pressures and all the rest of it when you come to design a big magnet. The idea of a disturbance of the magnet giving rise to a response in the conductor will either, depending on its local environment and cooling and all the rest of it, decide whether to quench or to recover. This is giving us, I think, a really quantitative basis for design of these magnets. As an example of the way I think things ought to go, I would like to again tell you about the MIT Hybrid Magnet.

During the tests, by trying to monitor these disturbances, we came up with a sort of a figure that we think is about right--well, within a factor of 10 anyway--for the disturbance. We thought that the maximum disturbances we saw were about 100 MJ. We measured the conductor by putting disturbances in a short sample. I'll say a bit more about short samples in a minute. We found that we could put in, I think, two or three joules before the conductor quenched. So we now have a feeling of what the margin of safety is. It is not a cryostable magnet. If you drive the whole thing normal, it will quench; but it is somewhere between the two. For the first time we have a numerical feeling, and this is very useful to us because the next type of magnet has to fit in a small cryostat. It must have a high current density. What we decide in the conductor design, whether we say we'll make it stable at 500 MJ, 110 MJ or 1 MJ, of course, is a matter of individual judgement, and all the rest of it. But, nevertheless, at least we got something. Something to go on.

So what about the future? Well, obviously, with respect to disturbances, it will be useful if we could calculate them in advance before we design the magnet, and maybe one day we will be able to. We need to think a lot more about what is going on. We need to have more data about the properties of structures, on things like the coefficient of friction and bond strength between conductors and that sort of thing. In the meantime, I think we ought to build up some kind of a body of conventional wisdom as to what disturbances are in magnets that look like that. At least, it will give us something to go on in the meantime. And to that end we ought to be thinking very closely about monitoring coils. I was very pleased with the results of our workshop on monitoring coils. I think that is taking the problem out and giving it an airing. I don't think we got all the answers, but I think lots of people are now starting to think about it.

In terms of response, let's improve on our computer programs. They are already pretty good, but let's really make them model the situation exactly. It is not too difficult with the computer. You can do it; you can put in all the nonlinearities and all the experimental data, turn the handle, and get out an answer. The answer must agree with experiment and it must have the right data. We need better data to feed the computers, especially transient heat transfer data. That is quite definite. I think that is the big error. It is not the computation; it is the heat transfer. Of course, we need experiments; and I think that we are going to see in the future a new kind of short sample measurement. That is the kind of measurement that takes a sample of the conductor in a representative environment of the magnet winding. That is to say, it is surrounded by many turns of the same conductor and is long enough. How long and how many turns depends, of course, on the size of the minimum propagating zone. It must be bigger than that. Then you subject it to disturbances and see what it will stand. So, in the future we may, for example, specify conductors by saying, "Well, my conductor can carry 1000 A at 6 T and is stable against disturbances up to 1 J or to steady state disturbances up to 10 W or whatever." I think, when all this is done, we can probably finally throw away the witch doctor's mask and magic potions and exchange them for the solid waistcoat and watch chain of a respectable profession.

# REGISTRATION

Workshop on Stability in Superconducting Magnets
July 25 - 29, 1977
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