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U.S.-JAPAN WORKSHOP ON STATISTICAL PHYSICS AND CHAOS IN FUSION PLASMAS

December 13-17, 1982

U.S.-JAPAN WORKSHOP ON
STATISTICAL PHYSICS AND CHAOS IN FUSION PLASMAS

Held at

Thompson Conference Center

University of Texas at Austin

December 13-17, 1982

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Horton

It is time to open the U. S.-Japan Workshop on Statistical Physics and Chaos in Fusion Plasmas. On behalf of the U. S. participants, I would like to extend a warm and cordial welcome to the Japanese participants and the Japanese delegation.

This Workshop is the seventh U. S.-Japan Workshop[1] that has been organized under the planning of the Joint Institute for Fusion Theory, which is the coordinating committee that organizes the exchange workshops between the Institute for Fusion Studies in the U. S. and the International Center for Fusion Theory in Japan. Professor Yoshi Ichikawa was recently named Director of the International Center for Fusion Theory in Nagoya. Professor Linda Reichl, who is Assistant Director of the Center for Statistical Mechanics, the Prigogine group, and I have had the great pleasure of working with Dr. Ichikawa in organizing the Workshop. At this point I would like to ask Dr. Ichikawa to introduce the five members of the Japanese delegation that came with him and also the three participants from the Nagoya, Hiroshima, and J.A.E.R.I. theory groups that are here with us at the Institute for Fusion Studies on exchange visits.

Ichikawa

Just let me introduce our delegation. Professor Hajime Mori from Kyushu University. He has been working on statistical physics for a long time and has been recognized as one of the leading statistical physicists in Japan. Professor Masuo Suzuki from the University of Tokyo, long-time collaborator with Prof. Kubo. Professor Kuramoto, from Kyoto University, Research Institute for Fundamental Physics, this is the famous Yukawa's Institute. He has been Professor at this Institute, working on statistical physics for a long time. Then, Prof. Mitsuo Kono from Kyushu University. He has been working with us for a long time in plasma physics and statistical physics. At this moment, we are having three Japanese scientists stationed at the Institute for Fusion Studies. This is a part of the activities of the Joint Institute between the Institute for Fusion Studies and the International Center for Fusion Theory. This Joint Institute program has been created for the purpose of furthering cooperation between the U. S. and Japan. Dr. Sanae Itoh from Hiroshima University. She is working with Prof. Kyoji Nishikawa, a very active and leading plasma scientist. And Dr. Toshio Tange, also from Hiroshima University. Dr. Kimitak Itoh from J.A.E.R.I. They are working here in Austin for half a year. You see, this is the kind of idea of JIFT of having an exchange -- a constant exchange and collaboration of Japanese scientists and American scientists. So that at the present moment, in Nagoya we have Prof. John Dawson as the Visting Professor. Previously, Dr. Charles Karney spent several months in Japan, and we had very good collaboration with him. Maybe we can now announce one of next year's visitors to Japan. Prof. David Montgomery has accepted the position of Visiting Professor for next year in Japan. We are looking forward to Prof. Montgomery's visit to Japan. Perhaps just this much description will be enough to give you an impression of the activities of the Joint Institute for Fusion Theory (JIFT).

Horton

If you would permit me one observation before introducing Prof. John Wheeler, it would be to call your attention to the unique combination of skills and experiences we bring together here from the classical discipline of statistical mechanics, to the state of the art understanding of fluid turbulence, to the complex self-consistent dynamics of the Coulomb system in an external magnetic field. I ask you to take advantage of our being together here to make this a productive week for Statistical Physics and Chaos in Fusion Plasmas. Now I would like to introduce Dr. John A. Wheeler.

Professor John A. Wheeler joined the U.T. Austin Physics Department in 1976. He is the Ashbel Smith Professor of Physics and the Jane Blumberg Professor of Physics and the Director of the Center for Theoretical Physics.

In addition to the Einstein Prize, the Fermi Award, the Franklin Medal, and the National Medal of Science, this October John Wheeler was awarded the Niels Bohr International Gold Medal for his outstanding work toward the peaceful utilization of nuclear energy. In this regard he has repeatedly demonstrated a profound interest in the work of plasma physicists in the development of fusion power and, of course, in particular, in the work at the Austin and Nagoya Institutes for Fusion Theory. John Wheeler's enthusiasm and clarity of thinking provide inspiration to students and scientists in all areas of theoretical physics. It is with great respect and pleasure that we ask him to share a few thoughts with us on his view on the direction we are going with the undertaking we make here this week.

Wheeler

Professor Ichikawa, Professors Reichl and Horton, and colleagues: For me to be here may be presumptive, because I am no expert in this field. All I can claim is a deep interest in it from the very first day when I slept on the grounds where the Princeton Tokamak Fusion Test Reactor is now under construction. Buildings absolutely emptied by the original Rockefeller Institute for Medical Research and not yet occupied by anything else, and we were starting, thanks not least to Marshall Rosenbluth, a new project in the direction of nuclear energy.

It is especially wonderful that the present meeting brings together leading people from the work in this field in this country and in Japan. The death of Tomanaga, a few years ago, and the death of Yukawa last year, were great losses. I stopped to pay my respects to Mrs. Yukawa in Kyoto just a few days after Yukawa's death. The story of his life has appeared now in English as Tabibito: The Traveler.[2] Anybody who wants to get a perspective of the wonderful tradition, and the wonderful insights that Japan has to offer, will find that book a very great pleasure to read.

In regard to the fusion problem, at lunch time I met the former cabinet minister in this country in charge of that one of all the departments in the United States' government which spends the most money--Health and Welfare--Wilbur Cohen. And he told me he's making a commencement address in two day's time, and in that address one of his recommendations is fusion. The future is in fusion. But we don't have to wait forever. We have a Christmas present coming up. We have the

Tokamak Test reactor about to be ready at Princeton. We, I know, expect much from it, but also, I'm sure, we all expect surprises. And the surprises will teach us new things. But I think we all know those wonderful words of Pasteur: "Chance favors the prepared mind." I think this meeting is a preparation of minds to deal with the unexpected on the basis of sound principles.

But I would be dishonest with you if I did not reveal the real, central interest I have in the topic you take up here. That is, my deep belief that all the laws of physics, at the last analysis, go back to disorder for their explanation. There is not a single one of them, no matter how beautiful--electromagnetism, gravitation, chromodynamics--not a single one of them that does not go back at the bottom, to the same kind of principle as chaos that we see in the second law of thermodynamics. Not one single thing can we predict reliably about any of the individual molecules, yet when we look at the large numbers of molecules, they add up to the second law of thermodynamics with all the precision that anybody could want. You ask a molecule what it thinks about the second law of thermodynamics, and it will laugh at you. It does what it wants. But despite that, everything adds up to this beautiful result.

So the theme and thesis of this continuing endeavor, on my part and several others, is to find a way of penetrating deep enough into the laws of nature to see that every one of them at the bottom is not primordial and precise, but secondary, approximate, and derived, and based in the last analysis on chaos. Nobody who raises the questions I've just sketched can fail to look around at all the landscape around him for examples of law without law, examples of order coming out of disorder. I thought that perhaps you would permit me to say some of the little things I've come across along the way. Every one of the following examples, I'm sure, are known to you all, but to me they are entrancing because of the community of interest between these several kinds of problems and the suspicion, on my part at any rate, that there will be some day developed a large view in which we can look at each of these --at present very different-looking problems--as examples of a larger unity.

The most primitive example we are familiar with is the Boltzmann law which gives us the law for a collection of molecules which are kicking around. We know that the end result is a probability expression for the probability that the given molecule has a given amount of energy, which is given by the familiar Boltzmann exponential. We have the disorder here, we have the law that comes out of the disorder here, but we recognize, as in all of the examples in the end come to, that there is a regulating principle at work. The regulating principle, in this case, is the principle of the conservation of energy. There is only so much energy available for the whole system. So here is the disorder from molecular chaos, and here is the regulating principle that in the end gives the order out of disorder.

A whole collection of masses connected up by springs of random strength is a second example of disorder. The strength of the springs vary all over the map, and yet we know that a simple way to describe this is to think of a matrix which is symmetric, yes, but has elements which are given by random numbers. And we know what comes out if we look at the characteristic frequencies, or eigenvalues, and as for the number of eigenvalues of a given frequency, we know that it is given by the basic half circle. We know that the regulating principle here is the requirement that the strengths of the springs of the coupling constants are distributed by the Gaussian law of probability. If we don't have that fed in at the beginning as a regulating principle, we don't get this amazingly beautiful distribution of eigenvalues coming out at the end.

A third example, further removed from our traditional field of endeavor, is, of course, evolution with the mutations going on at a random basis all the time, and then we have the following order of the genera and species of the plant and animal kingdoms, and we know that the regulating principle is the principle of selection and survival.

The travelling salesman is a problem that our friends in the world of mathematics have the great pleasure to deal with. They consider this collection of towns, and if one is considering for example, this country with a collection of cities at various places, then our friends discovered that the poor salesman is supposed to visit one town after another and he is to come back, at the end of his travels, to his starting point and visit his customers all over again. The point of issue is the average distance travelled per customer visited. It turns out that to look at all the possibilities needs, of course, something like N-factorial trials. And when N is this large, it goes beyond the power of any computer system. So our friends in the world of mathematics devote a great deal of attention to this, but I think that we, of the world of physics, would be inclined to approach this question in a different way. First of all, we know that in the world of physics, we like to deal with problems in which the boundary is idealized, so that we deal with a torus. So that we identify the left side and the right side, we identify the bottom and the top of the map. So that we get rid of the problems there. Second, we get rid of the problem of this clustering of cities that is attuned to the distribution of landscape, and we put these towns around with all the randomness and uniformity of density that we think of for the molecules in a gas.

Then we ask ourselves, "How's a physicist to deal with a question like this?" And we say, "Well, the way you do this is with a mowing machine." You take the mowing machine out and mow the grass, and you pick a swath and then this comes around and continues on, and the question then is not the question of orientation, clearly that's unimportant, which way you turn the mower. The question that's important is the width of this swath. If the width is too small, then one has to travel a very long distance before one encounters a city, and then the distance per customer visit is very large. So this distance per customer plotted has a function of L varies as $1/L$ for small L. And if, on the other hand, the distance is made very large, then as the mowing machine proceeds ahead and cuts each stalk of grass in its forward progress, the traveller is going back and forth and most of the motion is longitudinal as compared to transverse. And it's easy to see that the average distance travelled in that limit is $L/3$, and consequently, one can easily see that there is a minimum in the curve, and if one calculates a little more detail, one finds the optimum swath width and its dependence on the density of cities.

No physicist would be so rash as to claim that this gives the best answer, but I think most physicists would believe that this puts the problem within five or ten per cent of the best, and it's a cheap way through. But the interesting feature about it is not these questions of detail. The interesting feature is the question of how order arises in the problem. In this case the regulating principle is the minimization of distance, and the disorder is the arrangement of the cities. Out of the disorder of the arrangement of the cities, by the help of the regulating principle of minimum distance, we are driven to a foliation into bands of stress. Once we have arrived at that idea, then when we go back and look at what our friends in the world of mathematics have done with the problem, we see that they've got the beginning of foliation in here, in this region where there is uniform density. The reason they don't see it in a clear way, is because they have this variety of densities in different places. So this is foliation...well, I won't say anything about the modern subject of stenglasses, which lends to a whole other collection of issues, but I might touch for just a moment on the issue of the operation of the brain.

Here the randomness is a connection between neuron and neuron, and Hopfield[3] has a number of exciting results in this field. He finds that he uses the ends of couplings between neuron and neuron. In this system, some of the neurons are what we might call in particularly simple language, some are plus and some are minus, and each neuron on a purely chance basis, every once in a while looks up and looks around and looks at all the other neurons that its coupled to, and it multiplies the reading of all the other neurons by the coupling coefficient. If the result is positive, this neuron changes whatever its reading was to plus one. If it was already one, it leaves it as one. If the product of all the other numbers on all the other neurons, multiplied by the coupling coefficients adds up to a negative result, then this neuron switches to a negative value. This is a random way of coupling. This is, as Hopfield puts it, throw the neurons together and you let them compute, and they just go on computing. And the result is, of course, if you have a thousand neurons, that's the largest number to which he has traced out results so far, numerically with the help of a computer model. You would have, of course, 2^{1000}, or roughly, 10^{300} possible initial configurations. But as a result of these interactions of the thing, computing randomly chances what a given neuron looks around and sees, changes what these coupling elements are. The thing nevertheless settles down to one or another stable configurations, which are memory states, and these memory states are the new feature. They are the element of order. It is, of course, a most interesting question how the number of memory states varies with the number of neurons. That is a problem for the future. And what is the regulating principle? It is simply this order of computation.

Well, it's a very interesting and exciting thing to be under the influence and stimulus of a group like this. But, naturally, nobody like we who are here can fail to be excited by the things that Harry Swinney does on the flow of fluids in one or another geometrical arrangement. How the flow depends on the parameter velocity of whatever it is, and how the analysis of the motion at one velocity parameter shows, for example, one frequency. Then when the parameter is raised slightly the velocity is increased with two frequencies, and so on, doubling this frequency as one goes on up the road. And this marvelous feature of the approach of the changes that pertain to the parameter value from one to the one before approaching to the Feigenbaum number. Nobody who has these little hand-held calculators fails to try this out on simple examples and to be marvelously impressed by the story we have here.

Of course, we have the issue. Oscar Landford tells us that no Hamiltonian system will show these Feigenbaum numbers, so you get terribly worried. Here they are. It is not only on mathematical models but in the real world you see them, but Landford tells us that no Hamiltonian system will show them. Well, we know what the answer is he gives. That it's dissipation. And, in fact, we have only a small number of degrees of freedom excited. The rest are damped out. And that's how come we can have such beautiful simplicities here. I hope that this very sketchy view will show you how at least one person finds it very exciting to look at this idea of searching for order coming from disorder, and how one person can have a little hope that some day this terrible division that we have in physics today between laws, on the one hand, and initial conditions, on the other hand, will be broken down when we see the whole thing at the bottom is disordered. That just is, when we look at the numbers. And as to the numbers, we see that some of them are primes and some of them are not. It looks like a very sharp distinction. But the issues of computability in recent times have taught us that when we go to very large numbers, the distinction rather washes out. It becomes impossible to test in any reasonable amount of time whether the number is or is not prime. So, in the end, my theme, my hope, my message, and my gratitude goes for those of you who are, and all of us here are, working for this wider view of order and chaos. Thank you very much.

US-JAPAN WORKSHOP PROGRAM
STATISTICAL PHYSICS AND CHAOS IN FUSION PLASMAS
DECEMBER 13-17, 1982 AUSTIN, TEXAS

MONDAY, DECEMBER 13, 1982

2:00 PM	OPENING ADDRESS: JOHN A. WHEELER
2:30 PM	NONLINEAR MAPS CHAIRMAN: W. HORTON
Y-H. ICHIKAWA	STATISTICAL PROPERTIES OF NONLINEAR MAPS (40 MIN)
J. GREENE	BEHAVIOR OF ORBITS IN AREA-PRESERVING MAPS (40 MIN)
A. RECHESTER	STATISTICAL DESCRIPTION OF NONLINEAR MAPS IN THE PRESENCE OF NOISE (40 MIN)
C. KARNEY	LONG-TIME CORRELATIONS IN STOCHASTIC SYSTEMS (20 MIN)

TUESDAY, DECEMBER 14, 1982

9:00 AM	TRANSITION TO CHAOS CHAIRMAN: H. GRAD
R. HELLEMAN	MECHANISMS FOR THE TRANSITION TO CHAOS IN CONSERVATION AND DISSIPATIVE SYSTEMS (40 MIN)
G. SCHMIDT	TRANSITION FROM ORDER TO CHAOS IN CHARGED PARTICLE MOTION IN A STANDING WAVE FIELD (20 MIN)
Y. KURAMOTO	ONSET OF CHAOS IN CONTINUOUS MEDIA: CASE OF REACTION DIFFUSION SYSTEM (40 MIN)
E. OTT	DIMENSION OF STRANGE ATTRACTORS (40 MIN)
	LUNCH BREAK 12:00 - 1:30 PM

TUESDAY AFTERNOON

1:30 PM	TRANSIENT PHENOMENA AND TURBULENT DIFFUSION CHAIRMAN: F. PERKINS
M. SUZUKI	GLOBAL ANALYSIS OF NONLINEAR TRANSIENT PHENOMENA NEAR THE INSTABILITY POINT (40 MIN)
T. PETROSKY	KINETIC EQUATION FOR SYSTEM WITH TWO DEGRESS OF FREEDOM (20 MIN)
H. MORI	TIME EVOLUTION OF VORTICITY FIELD AND TURBULENT DIFFUSION (40 MIN)
4:00 PM	PHYSICS COLLOQUIUM-RLM BLDG., ROOM 11.204 - 26TH & SPEEDWAY STREETS
E.G.D. COHEN	FLUCTUATIONS IN FLUIDS FAR FROM EQUILIBRIUM

TUESDAY EVENING

7:00 PM - 10:00 PM	US-JAPAN TRACOR BANQUET GREEN PASTURES RESTAURANT

WEDNESDAY, DECEMBER 15, 1982

9:00 AM	KINETIC THEORY-CLUMPS AND RESONANCES CHAIRMAN: H. MORI
R. BALESCU	CLUMPS AND RELATIVE DIFFUSION IN PLASMAS (40 MIN)
M. KONO	KINETIC THEORY OF PLASMA CLUMPS (20 MIN)
P. TERRY	EXPANSION FREE ENERGY EXTRACTION FROM MICROSCALE CORRELATION AND CLUMPS (20 MIN)
T. O'NEIL	COLLISIONS IN A STRONGLY MAGNETIZED PURE ELECTRON PLASMA (20 MIN)
D. MONTGOMERY	MHD TURBULENCE IN STRONG, EXTERNALLY-IMPOSED MAGNETIC FIELD (20 MIN)
	LUNCH BREAK 12:00 PM - 1:30 PM

WEDNESDAY AFTERNOON

1:30 PM	SOLITONS TO CHAOS CHAIRMAN: J. GREENE
L. REICHL	FIELD INDUCED CHAOS IN THE TODA LATTICE (20 MIN)
J. MEISS	SOLITONS IN TURBULENT FLOW (40 MIN)
D. DUBOIS	COHERENCE IN CHAOS AND THE ZAKHAROV EQUATIONS (40 MIN)
A. WONG	EXPERIMENTAL STUDIES OF EVOLUTION FOR COHERENT LANGMUIR CAVITONS TO TURBULENCE (40 MIN)
Y-H. ICHIKAWA	RECENT DEVELOPMENTS IN SOLITON RESEARCH (20 MIN)

THURSDAY, DECEMBER 16, 1982

9:00 AM	ORBITS AND TURBULENCE IN PLASMAS CHAIRMAN: A. KAUFMAN
R. LITTLEJOHN	MAGNETIC FIELD LINE FLOW AND NONCANONICAL HAMILTONIAN THEORY (20 MIN)
J. CARY	STRUCTURE OF VACUUM MAGNETIC FIELDS (20 MIN)
R. WHITE	DRIFT ORBITS IN TOROIDAL SYSTEMS WITH IMPERFECT MAGNETIC SURFACES (20 MIN)
J. KROMMES	STATISTICAL CLOSURE APPROXIMATIONS IN PLASMA PHYSICS (40 MIN)
	LUNCH BREAK 12:15 PM - 1:30 PM

THURSDAY AFTERNOON

1:30 PM	NON-EQUILIBRIUM FLUCTUATIONS CHAIRMAN: Y. KURAMOTO
E. COHEN	KINETIC THEORY OF FLUCTUATIONS IN FLUIDS FAR FROM EQUILIBRIUM (40 MIN)
M. SUZUKI	APPLICATION OF FRACTAL ANALYSIS TO PHASE TRANSITIONS AND OTHER PHENOMENA (40 MIN)
H. SWINNEY	ROUTES TO CHAOS IN HYDRODYNAMIC AND CHEMICAL SYSTEMS (40 MIN)
J. SWIFT	PROGRESS IN CALCULATING SYAPUNOV EXPONENTS FROM EXPERIMENTAL DATA (20 MIN)

FRIDAY, DECEMBER 17, 1982

9:00 AM	CHAOS AND TURBULENCE CHAIRMAN: M. SUZUKI
C. GREBOGI	UNSTABLE-UNSTABLE PAIR PRODUCTION EN ROUTE TO CHAOS AND CHAOTIC TRANSIENTS (20 MIN)
W. HORTON	STATISTICAL DESCRIPTION OF DRIFT WAVE FLUCTUATIONS (20 MIN)
10:30 AM	DISCUSSION CO-CHAIRMEN: M.N. ROSENBLUTH Y. ICHIKAWA

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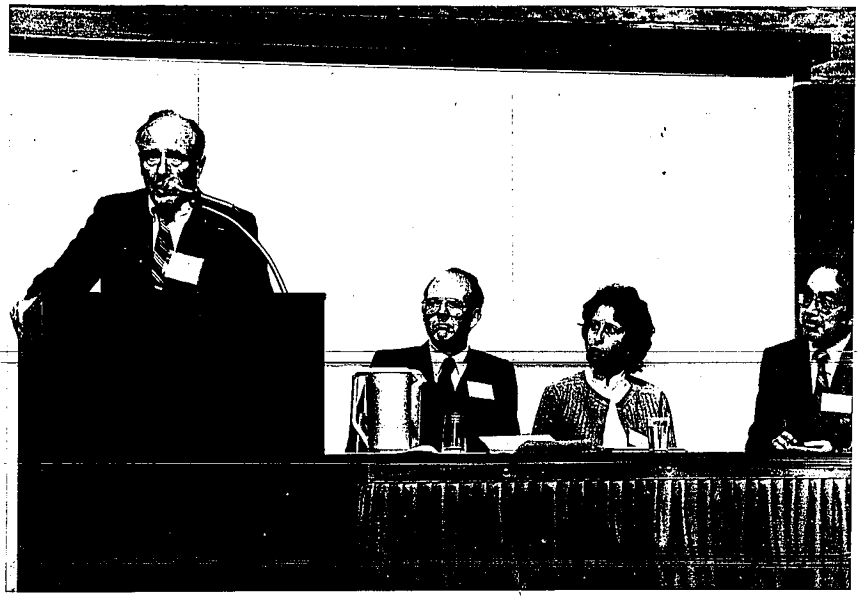
OPENING THE WORKSHOP -

W. HORTON, L. REICHL AND Y. ICHIKAWA WELCOME WORKSHOP PARTICIPANTS.

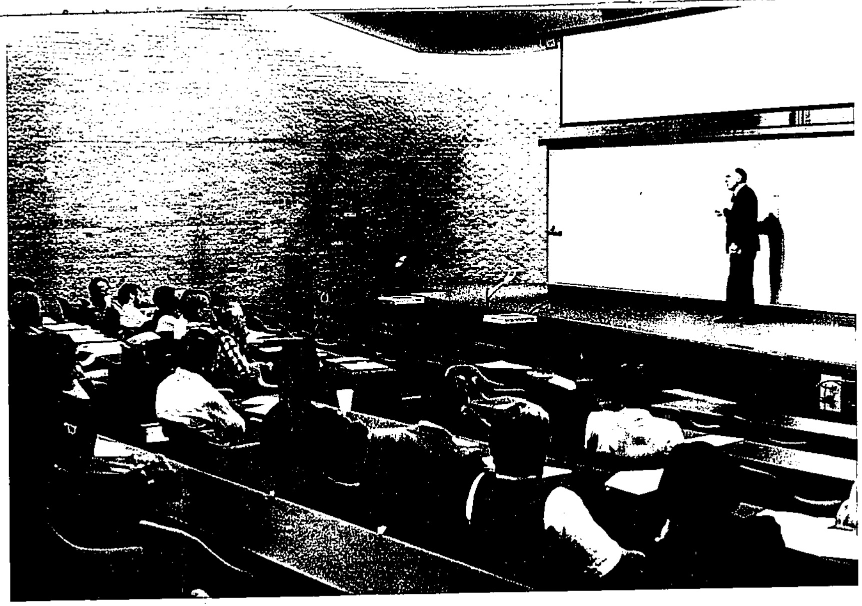
PROFESSOR JOHN WHEELER MAKES A POINT DURING HIS OPENING ADDRESS.

PROFESSOR ICHIKAWA INTRODUCES THE JAPANESE DELEGATION.



LEFT TO RIGHT:
J. WHEELER, W. HORTON, L.E. REICHL, AND Y. ICHIKAWA

DR. J. WHEELER DESCRIBING A REGULATING PRINCIPLE IN A STATISTICAL SYSTEM.



WORKSHOP PARTICIPANTS WITH PROFESSOR MARSHALL N. ROSENBLUTH IN THE FOREGROUND.


U. S.—JAPAN WORKSHOP
STATISTICAL PHYSICS & CHAOS
IN FUSION PLAMAS
DECEMBER 13–17, 1982
AUSTIN, TEXAS

CONCLUDING REMARKS

Rosenbluth

The first speaker who will tell us how to get to chaos, or at least tell us about the roadmaps to chaos, is John Greene. In the format of this session we shall try to hold the speakers to somewhere between five and ten minutes. Then, I urge anybody who has any doubts at all, or who wants to raise a contraversial point, to please do so, so that we can have a lively discussion.

Greene

I am going to talk about the map problems that were discussed. Roscoe White had the big picture, so I borrowed a few slides, and I will use these. Figure 1 is a picture of what the tokamak looks like. It reads up, and from left to right. The magnetic axis is at the lower left, ψ measures the distance outward, and θ is the angle around the magnetic axis. What one sees here, in this picture of real field lines, is banded regions where there are lots of KAM surfaces, or magnetic surfaces, that provide good containment. Then there are regions which are reasonably banded, but interspersed with good sized magnetic islands. Here there will be moderately good containment. And then there are regions where the magnetic field lines really wander around. There may be little islands in the middle, but they are pretty much irrelevant to containment. This is a region of really bad containment.

A critical point in this is to distinguish regions with islands separated by good KAM surfaces from regions wehre there is full stochasticity throughout. This separates modest containment from terrible containment. One can see from this figure what is well known to everybody who has had experience with maps, that the distinction between KAM surfaces and stochasticity is apparent even with the moderately short orbits used in making this map. In my talk I discussed MacKay's thesis, and that contains the outlines, the beginning of a real mathematical proof, and certainly a good physical demonstration, that

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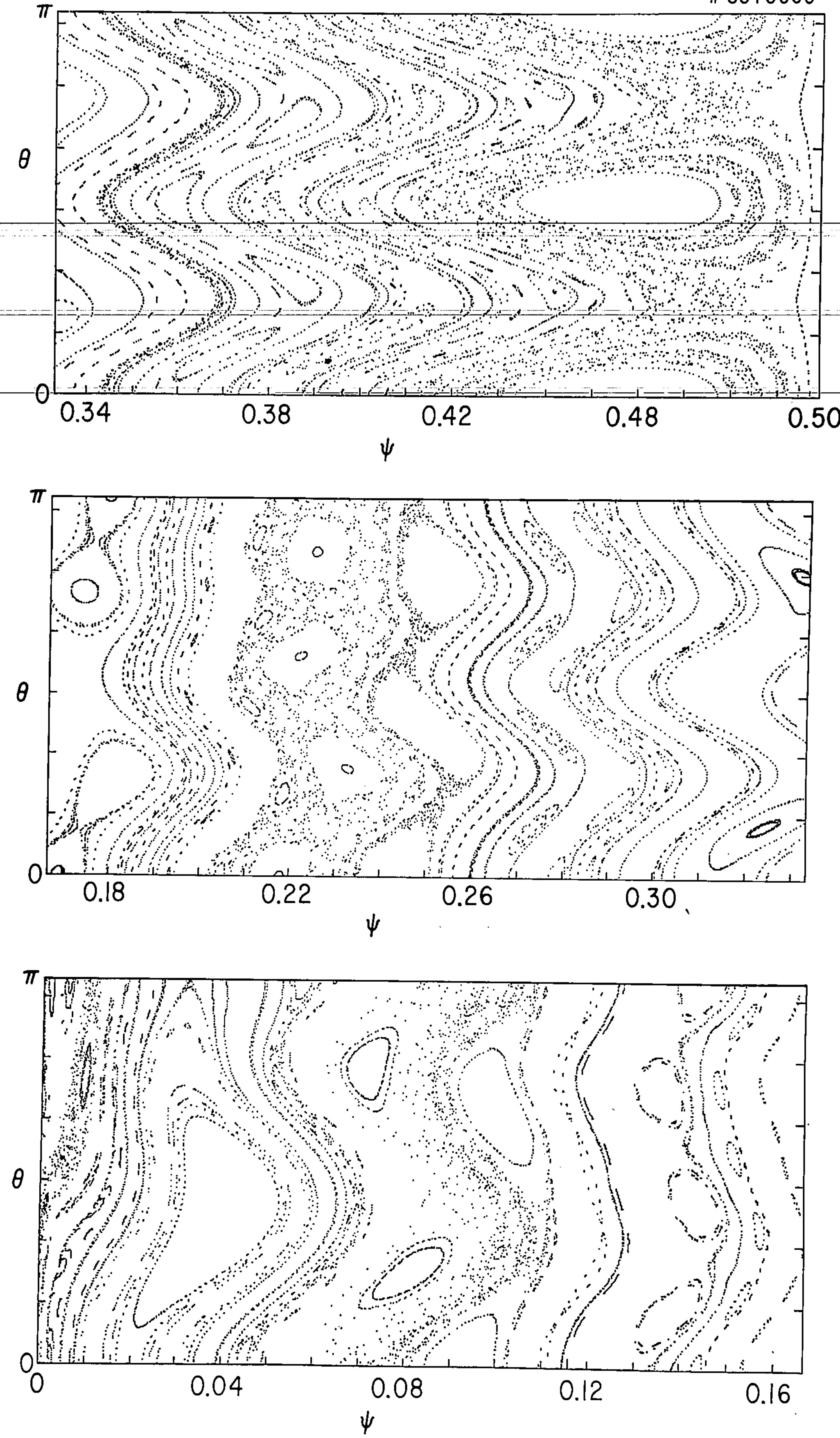
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criticality occurs when the safety factor is calculated around the magnetic axis at the island center. This is represented in Roscoe's second picture, Fig. 2, which I will not completely describe. It does show the central safety factor of islands, and in regions where this is less than six, there are good magnetic surfaces.

Rosenbluth

When you say criticality, John, what happens if you violate it. Critical to what?

Greene

Critical to the breakdown of magnetic surfaces, such as occurs around ψ of around .06 to .08 in Fig. 1. The isolated islands in the middle of that region will have a central safety factor less than six. The separating magnetic

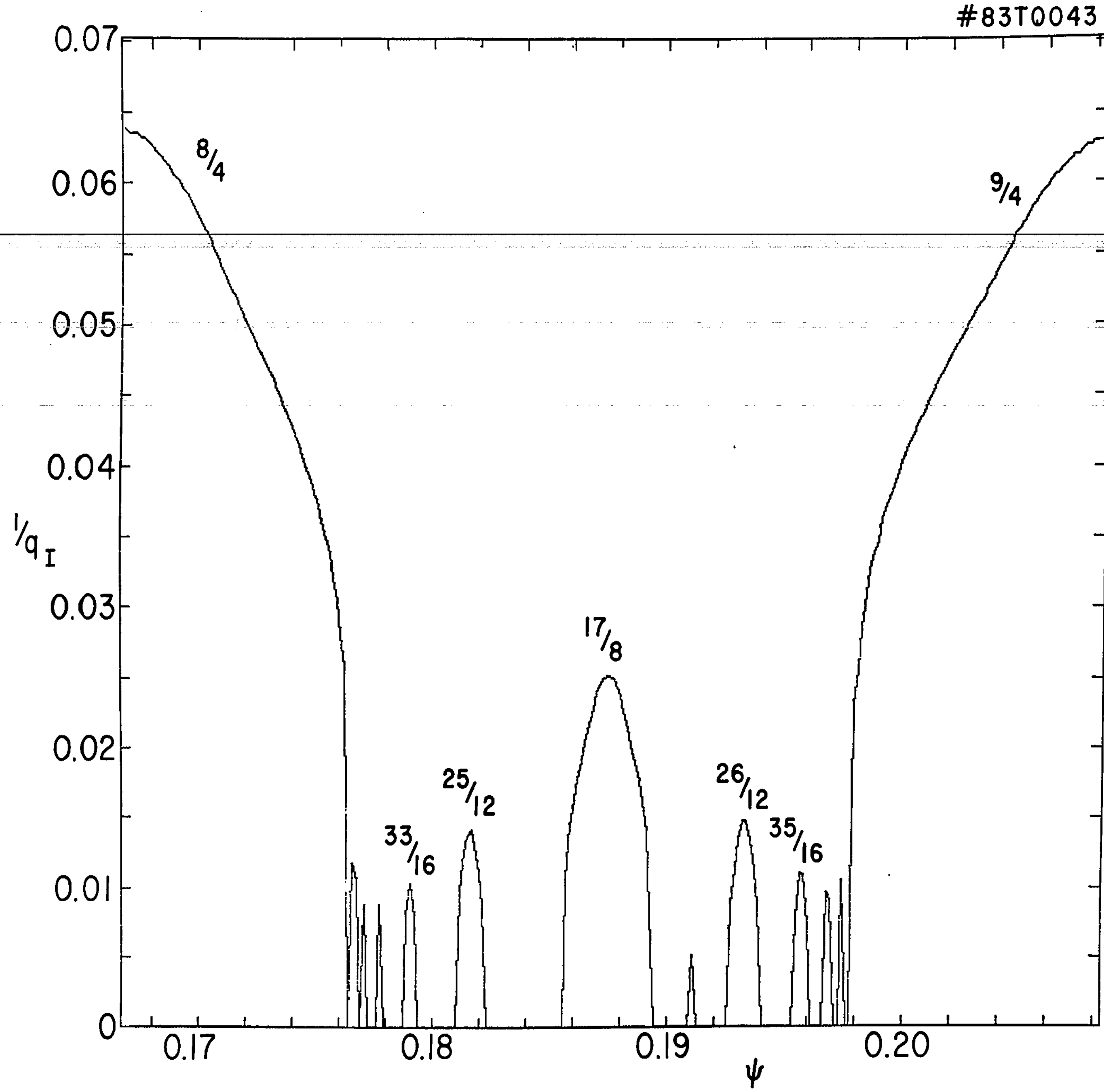
surfaces in the neighborhood disappear as the perturbation is increased so that these islands central q value falls below six. So it is critical for the disappearance of regions of modest containment with reasonably sized interspersed islands. The value of six is a magic number. This calculation is based in a renormalization theory, of the type that Wilson got his Nobel Prize for. These theories tend to run to otherwise inexplicable universal numbers that can be calculated to considerable accuracy. In the strict asymptotic limit the critical q is known to six or eight digits, but in any kind of approximate calculation, the value six is good enough.

Rosenbluth

How does that compare with the traditional way of estimating this sort of nonlinear overlap of magnetic islands? How is MacKay's magic number related to the overlap criterion?

Greene

In some ways, it makes it precise. In Fig. 2, the height, 1/q, is proportional to the overlap criterion when they are both small. With some juggling, Chirikov got an estimate for connected stochasticity from the overlap criterion, which is consistent with the "six" criterion. It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took some juggling and fitting and whatnot to show, but it is correct to within a few percent in the model he worked with. The difference between Chirikov's and MacKay's work is that the closer you look at the overlap criterion, the less mathematically precise it is, whereas, with MacKay's work, the closer you look at it the more precise it becomes. it is possible to do proofs with MacKay's analysis. You can, in principle, prove that there are good magnetic surfaces around $\psi=0.19$ in Fig. 1, since the peaks in Fig. 2 are quite small.

In another talk, Helleman looked at this same problem of breakdown of magnetic surfaces. He also saw this "six" as a magic number. He is struggling to make this number appear a little less magic by a different approach. I won't go into his approach here. You can read his paper when it comes out.

About half the papers on area preserving maps at this meeting were on the subject that I have just described. The other interesting topic in maps is to describe what happens in the regions where the magnetic field is quite chaotic. There is a question of how fast the field lines, or particles, move back and forth through chaotic regions. Yoshi Ichikawa, Charles Karney, Sasha Rechester, and others discussed the details of how time correlations decay. They also consider the type of stochasticity and chaos you get in regions of global stochasticity. This is a very interesting problem, and I think that all the participants would be satisfied with the comment that there are still a lot of questions to be answered.

That is my five minutes.

Rosenbluth

Does anyone want to comment or disagree?

Question

Were Roscoe's calculations done with a static magnetic field? How do you account for the time variations of the field?

Greene

Roscoe is handling all nonstatic, nonideal effects in diffusion through collisions. The magnetic field is quasistatic. It always exists at each time, and everything else is some generalized collision.

Unknown question

Greene

The important thing is not the magnetic field fluctuations, but the collisions. Mathematically, one goes to a limit of infinite time, infinitely long lines, to make proofs of the existance of magnetic surfaces. In the physical problem you do not want to go all the way. So you add collisions to cut off the calculation at a large but finite time. Then collisions and intrinsic stochasticity each contribute to the total diffusion in their own way. I think that it is interesting that infinite time concepts are still useful. The fact that there are good magnetic surfaces, which is an infinite time concept, is a useful way of describing the decent containment around ψ=0.3 in Fig. 1. Intrinsic stochasticity, which is another infinite time concept, is a good way of describing the poor containment around ψ=0.08 in that figure. These concepts are useful even in somewhat collisional regimes, and it is worthwhile working them out and applying them to the collisional regimes.

Rosenbluth

Roscoe, did you give a quantitative criterion of when the time variation is slow enough to be neglected? I mean, you say it is slow, but what has to be less than what?

White

I haven't really studied time varying fields.

Kuramoto

Let me put my discussion as a question of a general nature, instead of a comment. The quesion occurred to me as I was listening to some lectures presented during this meeting. The question is "Why is it that the dynamical models with only a few degrees of freedom often work so well?" Clearly this is a question of a rather fundamental nature beyond the confines of fusion plasmas. I think that no instances have ever been given where the question is answered in a satisfactory manner. If I remember correctly, a similar kind of question was also posed last year at the US - Japan Workshop in Kyoto. I mean by simple dynamical systems, a system like this, for instance the conservative system with 2 degrees of freedom, such as the standard map or possibly a more general type of conservative map. And also possibly a set of partial differential equations of the dissipative type, or a one dimensional map, as we were given by Harry Swinney yesterday. In most cases the physical systems have a tremendous number of degrees of freedom, but why does this kind of simple dynamical description work so well?

At least for dissipative dynamical systems, I think the question has been answered only qualitatively without mathematical rigor. That is to say, for dissipative system, the phase volume contracts to zero, as time goes to infinity. So in the many dimensional phase space, dynamical motion contracts to a certain manifold, which has a very low dimensionality compared to the original phase space. So that is the qualitative reason. Such an argument has been made precise, only near the bifurcation point, as a kind of central manifold theory.

In the example in yesterday's talk, Swinney demonstrated very beautiful experiments on the loss of dimensionality in a reaction system. In that case at first we have more than thirty interacting chemical substances. The phase space contracted to a three dimensional phase portrait in the reduced manifold. Then he contracted the dynamics even more to a one dimensional discreet map. For other examples, Don DuBois and myself talked about some problems with partial differential equations which also, at least in some cases, contract to much simpler dynamical systems. Those dynamical systems show successive bifurcations and chaos. For instance, at first we have limit cycles and then the two torus or Hopf bifurcations and chaos. Possibly the chaos in that case is of a rather simple nature. In this way we understand, at least partially, the reason why such a simple dynamical system is very effective in describing the dissipative system.

I do not know the reason why the same type of property should hold for conservative systems. Of course this kind of mapping is based on a one particle picture, but the fact is that physical systems have a tremendous numbers of degrees of freedom. What is the reduction principle for the Hamiltonian system or the general conservative system? What kind of reduction principle works behind these systems? I cannot find the reason. That is the principle question we have uncovered at this Workshop in my opinion.

Ott

I was asked to say something about dissipative dynamics, and I would like to start off by making some general comments about this field. The first comment is that it is a very young field, and it is not really terribly clear what its significant contributions to physical science will be. But it seems to me that there will be significant contributions in a great many areas, and hopefully in fusion plasmas. The second comment is that very recently, there has been explosive growth in the field. I do not think we know precisely where it is going, or what its eventual impact will be.

The studies that have been done generally fall into two categories. One is basic studies, where one asks what are typical things that can happen. The key idea here is to look at the simplest possible systems, say in maps--asking what can happen in simple maps. These simple problems appear to be very abstracted from reality. They are inventions of the theorist. The amazing thing is that what often happens is that these inventions of the theorists, appear to describe a complex physical system. This may be because there appears to be this fact, already pointed out by Kuramoto, that real systems actually reduce in dimensionality. So the simple theorists' models often seem to have applications in experiments.

The other line of research that is pursued is to take a physical system and examine it and apply these ideas. The ideas have been applied with some success. If you have been keeping track of the literature, you see every once in a while, an experiment or theory, where dissipative dynamics, and the general theory of it, is making a very strong impact. Such fields include fluids, solid state devices, laser systems, etc. Given this situation, I think it would be somewhat surprising if the field of dissipative dynamics did not eventually make an impact in fusion plasmas, especially since we know that fusion plasma are full of ugly nonlinear equations.

At this meeting these two lines of research were presented. In terms of physical applications, we have seen experiments discussed, by Swinney and Swift, and the applications related to fluids and chemical reactions. In addition, we have seen what I believe is the first experimental indication of applications to plasmas by Al Wong, in which he reports seeing a period doubling route to chaos in his experimental system. Again, one of the interesting points is that the system he described, is quite complex. The system has Langmuir waves, ion-acoustic waves, and cavitons growing up and going away. So there is a vast variety of motion, an infinite number of degrees of freedom. There is a lot of physics in the system, and yet, on another level, it is all described by a certain very simple one dimensional map. This is a very amazing situation. The same comment applies to Swinney's experiment, where there are 30 chemical reactions going on; 30 nonlinear rate equations describing the system.

Other theoretical studies, by DuBois and Horton describe theoretical solutions of partial differential equations describing turbulence in quite different systems. In one case, Langmuir turbulence, and in the other case, drift wave turbulence. But I think that one could see a great deal of commonality in the approach and in the fundamental ideas.

I might make some observations related to the applications in fusion, as Professor Rosenbluth suggested. One is the fact that these simple ideas may provide a useful way of talking about experiments, even when you do not know exactly what is going on microscopically. For example, let's say you have a tokamak, and you do your experiment and it turns out very badly. You keep doing the experiment over and over again, and you see different things happening at each time. Let's say first you have a good shot, then you have a bad shot, then you have a good shot,etc. . You might say that you have a period two system, and I think you might well justify that by saying "Well, the good shot does something so the impurities dissolve in the walls and thus prepares them in such a way that the next shot is bad. In turn, the bad shot does something that prepares the walls for a good shot." Then you might vary some other parameter, and you might see that the order of the good shots and bad shots just becomes kind of random. Even though you didn't know the physics of what was going on, you might be able to make some progress. Well, what this further points out is that the field may give you a framework for talking about things that you observe that you wouldn't otherwise know how to discuss. So maybe it might have some impact on fusion plasmas in this way, or in the more conventional ways described earlier in connection with plasma turbulence.

As another example we should note that Swinney and Swift were talking about chemical reactions. Fusion plasmas do not have chemical reactions, but they will have nuclear reactions hopefully, and they are described by a similar system of rate equations. Maybe similar phenomena eventually will be observed in fusion plasmas. In general I would say that a suggestion for the future is that fusion plasma people, and plasma people in general, need to know more about this new field. I think it will have an impact on physics in general, and I hope that more plasma physicists will find out about its predictions.

In terms of the talks on general theory that we have heard this week, there were important discussions on the statistical properties of the chaos, the distribution functions and so forth. Rechester discussed both conservative and dissipative systems, and used the same kind of techniques to examine these two types of systems. So there is some common ground between conservative and nonconservative problems. Also, on the statistical nature of fluctuations, Horton used some statistical ideas to discuss the final state of the attractor and the average properties on the attractor in the drift wave model. Grebogi discussed average lengths of the chaotic transients, Lyapunov numbers and other statistical ideas which apply to strange attractors. Similar ideas were discussed by Swift. The Lyapunov numbers and the dimensionality ideas were also used by Dubois and Horton in their studies to gain insight into plasma turbulence.

A final idea that has come up quite a lot in this conference and is related to this problem, is the idea of fractal structures. Strange attractors themselves are fractal structures, and we have seen some very nice experiments illustrating the structure of strange attractors. The strange attractors appear to be surfaces. Actually we know that they must have a dimension higher than two. I just want to point out that in regard to the consideration of the fractal structure of strange attractors, one of the originators of that field is Professor Hazime Mori, here at this conference. Grebogi and myself discussed the fact that fractal structures also occur in the boundary separating the basins of attraction between two strange attractors. In other contexts, fractal structures were discussed by Mori for fluid turbulence, and by Masuo Suzuki for phase transitions. Well, that concludes what I wanted to say. I believe this is a very interesting and important field, and I believe that it will make a significant impact on fusion plasmas.

Mori

Let me take up one of the areas of the workshop program in which I am particularly interested and discuss the relation of this problem to the problem of fluid turbulence I discussed in my talk. One point I would like to emphasize is that turbulence and chaos contain ordered motions of various space-time scales among random motions so that space-time correlation functions exhibit a variety of structures even in fully-developed turbulence.

Weak turbulence with low Reynolds numbers is represented by a strange attractor in phase space, while fully-developed turbulence with high Reynolds numbers is described in terms of a cascade process in fluid space. Although the two approaches to turbulence are quite different from each other, the turbulent diffusion is one of the most important phenomena in both cases and is useful for exploring the physical structure of turbulence.

There are two kinds of diffusion processes. One is the single-particle diffusion

$$\langle [\underset{\sim}{r}(t) - \underset{\sim}{r}(0)]^2 \rangle = \int_0^t ds' \, \phi(s,s'), \tag{1}$$

where $\underset{\sim}{r}(t)$ is the position of a particle at time t and $\phi(s,s') \equiv \langle \dot{\underset{\sim}{r}}(s) \cdot \dot{\underset{\sim}{r}}(s') \rangle$. Another is the relative diffusion of a pair of particles. The single-particle diffusion for large t is simply proportional to t in fully-developed homogeneous turbulence in strong contrast to the relative diffusion for which $L_*^2(t) \sim t^\psi$, where the exponent ψ takes various values from 0 to ∞, representing ordered motions of vortices of various sizes, as discussed in my talk.

For weak turbulence just after the onset of turbulence, the time correlation function $\phi(t,t')$ exhibits a lot of structures, representing ordered motions, such as a roll structure of the Benard convection and unstable periodic orbits of a strange attractor. For simplicity, let me take the Lorenz model (X,Y,Z) for the Benard convection. Then the two velocity components in the (x,z) plane perpendicular to the roll axis are given by

$$u \sim X(t) \sin(\pi a x/H) \cos(\pi z/H),$$

$$w \sim -aX(t) \cos(\pi a x/H) \sin(\pi z/H), \tag{2}$$

where H is the width of the liquid layer and a is a positive number. The time correlation function $\phi(t,t')$ can be written in terms of the time correlation function of X(t),

$$C(t) \equiv \langle X(t)\, X(0) \rangle - \langle X \rangle \langle X \rangle \; . \tag{3}$$

The time correlation function C(t) is determined by a nonperiodic orbit of the Lorenz strange attractor. It is, however, very difficult to calculate C(t) analytically. In order to see how statistical properties of chaos are characterized by periodic orbits (i.e., order in chaos), let me take the one-dimensional map $x_{t+1}=f(x_t)$, $t=0,1,2,\ldots$ with

$$f(x) = \begin{cases} \beta x + \beta^{-2}, & (0 \leqq x \leqq \beta^{-2}) \\ -\beta x + \beta, & (\beta^{-2} < x \leqq 1) \end{cases} \tag{4}$$

and $\beta=(\sqrt{5}+1)/2$. This is one of the most typical chaotic maps which satisfy ergodicity. The time correlation function of a nonperiodic orbit of this map turns out to take the form

$$C_t \equiv \langle x_t x_0' \rangle / \langle x_0 x_0' \rangle \tag{5}$$

$$= \left[(-1)^t - \frac{2}{3}\sqrt{\frac{5}{3}}\sin\left(\frac{2\pi}{3}t\right) \right] e^{-\gamma t}, \tag{6}$$

where $\gamma=2 \ln \beta$ and $x_0' \equiv x_0-\langle x_0 \rangle$. This has two oscillations of periods 2 and 3. Periodic orbits of all periods exist for this map, and they are all unstable. The first and the second term of (6) arise from the neighborhood of a periodic orbit of period 2 and that of a periodic orbit of period 3, respectively. In general, an ergodic map has an infinite number of unstable periodic orbits and its statistical properties have structures determined by these periodic orbits, in particular, by those of small periods.

Summarizing the above, turbulence is a chaotic ordered motion which consists of vortices of various sizes in the case of fully-developed turbulence and which consists of periodic orbits of various sizes in phase space in the case of weak turbulence. The time correlation function of a nonperiodic motion and the turbulent diffusion reflect these ordered motions explicitly, exhibiting a variety of structures.

I hope that such a viewpoint is also useful for exploring plasma turbulence and chaos. For instance, the Hasegawa-Mima equation for a plasma under a magnetic field has an energy-spectrum density proportional to k^{-3}. This is similar to the energy-spectrum density of the two-dimensional enstrophy cascade which is most coherent among cascade processes of fluid turbulence, and would imply that the relative diffusion asymptotically obeys the exponential law. It is also well known that the magnetic flux is stretched and folded in time in the magneto-hydrodynamic regime. This may be formulated in a similar way to the vortex stretching of fluid turbulence and would lead to the relative diffusion of trapped particles which obeys the power law with a finite ψ.

The chaotic behavior of conservative systems is, in general, more complicated than that of dissipative systems. For example, the diffusion process in two-dimensional area-preserving maps is more complicated compared with that in extended one-dimensional maps. In fact it should be a very interesting and challenging problem to clarify what structures of the time correlation function of a nonperiodic orbit are brought about by hyperbolic orbits, elliptic orbits and islands. That is all of my comments.

Balescu

The papers presented at this workshop are good evidence of a high degree of interest in the problem of microstructures in a turbulent plasma. The problem is fascinating in itself and is probably of interest to fusion through its bearing on the shape of turbulent spectra and on all aspects of transport mechanisms.

It is more and more evident, as also noted by Grad, that a turbulent plasma is not to be considered as a uniform perfectly chaotic medium, like white noise, but rather has a lot of structure in it - of course, not permanent, but rather dynamic structure. This situation is favored, as compared to a neutral fluid, by the fact that the plasma is made up of charged particles with long-range interactions.

These structures can be grossly classified into two groups. In the first group I would put "semi-macroscopic structures" the typical example being solitons.

Solitons have an almost coherent structure, and their driving motor is the ponderomotive force. Whenever there is an electric field which starts fluctuating and modulating, the resulting ripples will be transferred to the density profile. The result is the typical plasma soliton, or "caviton", in which a maximum of electric field is associated with a minimum in the density profile. These structures have been studied for several years, and many numerical simulations were performed. But I think one of the highlights of this workshop was the experiments of Wong in which he showed visible evidence for these structures as well as of their dynamical evolution. One saw the electric field break down by nonlinear decay, develop harmonics and finally end up in turbulence.

On the theoretical side we heard of several mathematical studies at this conference. Ichikawa told us the latest news of the "exotic" solitons he has studied for several years, which have such attractive aesthetical shapes. Reichl reported on a still different type of solitons in Toda lattices.

Closer to plama physics, an interesting idea which was started a number of years ago by Kaupmann, models certain types of plasma turbulence as a gas of solitons. A similar idea was presented here by Meiss and Horton,an approach which I believe to be very much worth further study. This work was related to drift wave turbulence. We heard about a similar problem in Langmuir turbulence by DuBois. All these routes should be further developed in the future.

The second group of turbulent structures involves microscopic, much less coherent features, exemplified by the "clumps". Although the clumps are now "ten years old", there is still some controversy or some uncertainty about their real physical nature and interpretation. From the discussions at this workshop, it appears that a few facts about the clumps are solid, whereas others are less certain.

What is certain is that a pair of particles in a turbulent plasma stays together for a much longer time than if they were independent. The correlation of their trajectories which comes about from the presence of fluctuating electric fields makes them stick together for a certain time. At this point some people would ask: on one hand you say that particles stick together, and on the other you show that their trajectories diverge exponentially! Well, there is no contradiction, if you remember that there are three regimes in the relative diffusion, as confirmed at this workshop by Suzuki. The particles first start diffusing very slowly as t^3, then there is an exponential speeding up of the diffusion, after which the clump disintegrates and the particles diffuse independently as t^3. The clump actually spends most of its lifetime in the first regime.

A second solid fact is that when you look at the pair correlation function in a turbulent plasma, you easily show that it is strongly enhanced at small separations in phase space. This result, first obtained by Dupree was recalled in my talk at this workshop.

Other aspects of clump theory are less universally acceptable. Can one speak of a clump as of a macroparticle? The idea is very tempting, but is not proven. Kono's paper at this workshop goes in this direction, but much more work is required.

Let me now formulate some personal opinions on the open questions and needs in this field.

First, on the theoretical side, I believe a new direction should be pursued. Till now all the work in this field was very formal. People tried to show that the Vlasov equtaion leads to something that looks like a clump. But I think the time has come to calculate specific effects, like calculations of spectra, or anomalous transport coefficients, etc., and show in what way the clumps affect the real plasma quantities. I believe that the work presented at this workshop by Terry and Diamond is a very important first step in this direction. This valuable work should be developed in more and more detail.

From the numerical point of view, I also believe there is a need of doing much more detailed work. The only published numerical experiments on clumps, at least until now are those of Hui and Dupree, which are already eight years old. With the advent of Crays, generally available for plasma research, one should be able to do much better.

The third aspect is even more interesting because it is completely open: "is there any experimental evidence for clumps?" Here we have absolutely nothing, except for some indirect evidence reported by Wong, showing a lowering of the threshold of ion-acoustic turbulence, as predicted by Boutros-Ghali and Dupree. From the discussions at this workshop, I think we may have good hope that before long Wong, and others, will come up with experiments specifically designed for the direct visualization of clumps.

Grad

I should start by answering Marshall's specific requests. What is the relevance to plasma physics of the studies presented at this workshop? And this is a remark that I have been saying for over 20 years now, and it keeps coming up in different contexts; that almost any phenomenon, especially pathological phenomenon, that exists in mathematics or physics will have some example in plasma physics. So if you want to study all phenomenon, study plasma physics.

Obviously, I am not going to give a summary. These are some random remarks about chaos. There are three topics that I want to discuss very briefly. One of them is the meaning of chaos. It has several different meanings. I want to pinpoint that. The second one is how do we go about simplifying situations when they look to complicated to even think of, and there have been various advances in this meeting. I want to point to some of them. The third topic that was suggested to me was closure of kinetic equations and similar complicated formalisms.

First of all, about the meaning of chaos. This is essentially trivial, but I have to keep repeating it to myself to understand it, so maybe it is worth repeating to others. You have to distinguish the chaos, which has an intuitive meaning. And that intuitive meaning is very close to randomness or stochastic, and induces you to make approximations like random phase and assuming that there exists a diffusion coefficient unless you compute it and things like that. Now there is a technical meaning of chaos, which is related to mathematical concepts such as the law of large numbers, that gives rise to ordered structures which persist for some time, but cannot be predicted for long periods of time. And that is qualitatively an entirely different phenomenon. Also, it is obvious that you have to be very careful about using stochastic type approximations in treating phenomena which have this ordered behavior, which is still very complicated. Being very complicated is not enough to say that something is random or stochastic.

There is a third example that is better known in fluid turbulence, where to the eye, if you look at a movie or something in fully developed turbulence, you will see something that looks like a strange attractor; you see very well defined structures that are very hard to predict over long periods of time, but if you look at the spectrum, it is very smooth. The spectrum is very smooth whereas for strange attractor you would expect to see nothing but spikes. So I think there are at least these three qualititatively different things which are very complicated, frequently called chaotic; but should not, except possibly in the first case, be called random.

Now the second topic, as I said, was how do you simplify some of these very complicated mechanisms that come up. It has been fifteen years now since I first started to worry about the impact of these strange structures, the KAM surfaces and other things on toroidal confinement. As a guiding principle, I'll try to quote Alfred North Whitehead, who said somewhere, "Seek simplicity, but distrust it". And I think that that is still a very good guide, governing principle , toward what we should be looking for. Let me give a very trival example, but in a slighly different concept of what is probably the most important simplifying tool of mathematicians and mathematical physicists. If you have a large number, 10^4, in some physical problem, you simplify the problem by saying, let n go to infinity. That won't be precisely correct, but it is much easier to handle. Or occasionally, n = 1. I am trying to make it sound rational. Obviously WKB is one of the most fabulous success stories. Let me give you a more trivial one. The invention of the irrational number, which is letting n go to infinity. That made arithmetic much simpler. If you think of what the laws of arithmetic are, for a computer, which has ten decimal places, they are very complicated. The only way you can get simple laws of arithmetic is by letting n go to infinity. And it is a pretty good approximation, except it is not always valid. Now, the whole point that I am approaching, is that there are cases in which this method doesn't work. In which letting n approach infinity makes things more complicated, not simpler. It is not the way to solve the problem, and this is the correct qualitative interpretation of the KAM theory, philosophy, everything connected with it. And this of course has been confirmed by hundreds of calculations, some presented here. The point is, if you follow it for a thousand iterations you get a certain qualitative picture, if you follow it for a million iterations, it is completely different, if you follow it for a billion iterations, it completely changes. And this goes on ad infinitum, it will never settle down, so you have to, if you want to use that map, use a number that has some relation to the physical problem you're interested in. If it is a storage ring, it may be 10^{12}. If it is a plasma confinement device, it may be 10^4 or 10^6, depending on what you're looking at. And it does not simplify to let n go to infinity. Or at least most of the time. Now how else can we simplify a problem like that? Let me give examples. One thing is by being crude or approximate or ad hoc, and these are some of the most important things that have been done.

For instance, the paper given by Roscoe White, which shows that we are beginning to look at the few pathological islands or resonances which are important, selectively eliminating everything else. Now that choice will be different depending on your experiment, and on your judgement, on deciding how to do that. To decide how much apology is relevant. As another example, there is the paper given by Rechester, that is connected with many other people, make it stochastic. Throw in something stochastic even if it may not be exactly there. Now, if you let the stochasticity go to zero, you will regain this infinite sequence of more and more complicated things. So that is a way, it is not clear exactly what the limits of usefulness are, if that is a way to go, then you have to put in the right amount of smearing, or of stochastic behavior (Of course the problem is not stochastic.) in order to give you reasonable results. Up to now I think it is primarily impirical, the selection of parameters of that sort.

Another example like that was the paper of Petrovsky. It was a different type of approximation. Not a stochastic one, but he did smear out certain things which allowed him to get a relatively simple answer. It obviously cannot be the answer, if you want results to 12 decimal places, or you want to study that problem for very long periods of time. On the other hand, it could be extremely important for certain parameter ranges. Somewhat different, but along these lines, the very interesting paper by Cary, of improving the magnetic field by eliminating, one by one, some resonances. It is sort of an ad hoc method. I believe that it cannot be convergent. On the other hand, I believe it can be extremely useful practically. Just like in an accelerator, shimming the magnetic field, without that the accelerators wouldn't work, if you didn't have some adjustments there. On the other hand, you wouldn't know, in the case of the accelerator, that shimming the fields does not give you confinement forever. It just gives you the confinement you need to justify the experiment. As another example, this wasn't presented here, there is a code by Alvin Balis , that is a three dimensional tokamak with ripple, for those to whom this means something. He has a diagnostic tool in there, which allows you to magnify resonances, and study them with a microscope, or to smear them out, if you want to get an answer to the problem. What are the transport coefficients? Then you will smear out the resonances, except for maybe one or two. If you want to study what is really going on, you have a choice. There are analytic techniques, of course, for doing the same thing. That would in a sense, summarize the philosophy I've been describing.

Let me turn to item number three. Closure of kinetic equations. First of all, this process of simplifying, reducing an n particle system, where n is 10^{23}, or a system of equations with n independent variables, etc., to a much simpler form, does not have a universal solution. Closure is useful only in a few special cases. It works in a certain limit as you approach fluid dynamics, or nearby a parameter range in which fluid dynamics is valid. It works if you approximate the Boltzman equation, or are nearby rarified gas. The BBGK hierarchy does not close, in general, and truncations and such are ad hoc, if you carry 5 terms, that will give you a slightly better approximation, maybe than 4. If you carry a hundred terms, that will be still better, etc. But you do not get this dramatic one term beautiful results. And this has been known for quite a bit of time, and incidentally, I discussed this briefly with Eddie Cohen here, after all, we're a half an hour away by subway, but we rarely speak to each other except in places that are at least as remote as Texas. And we agree on that point. And it is necessary, in general, if you do not approximate fluid dynamics, or approximate the Boltzman equation, to carry everything, the whole hierarchy, if you use it properly, and you cannot depend on getting a kinetic equation which will be good for a very large range of problems. You have to pick a problem, and appropriately approximate things so that you get your answer. And as a matter a fact, this was the subject, approximately, that I spoke about at the analytic continuation backwards in time of this meeting two years ago[4], and the examples that I had at that time were not very good. The principles were there, but I believe that Eddie's example is the first one that I would feel confidence in that the answer is probably correct, as well as the basic principles.

There is a very large literature of approximate closure methods. No one has done very much testing as to the relative validity of the methods. And therefore, I am looking forward to the rather large computer program that John Krommes intends to do. He is investing a lot of time in seeing to what extent the DIA is useful, and I think that should be very revealing, whatever the answer is. Of course, it may save us some future time in studying it, if it is no good. It may show that some of the analytically simpler methods are almost as good, or it may show that this is a maiden's prayer. Let me conclude.

One of the questions was what should be done next year--is it worth going on? Everyone insists that of course it is worth going on. We have only begun to touch the significance of these subjects. This is an oversimplification. There seems to be two types of people here, those who look through microscopes, and examine with great detail, and with great precision, the properties of special problems; and those who take meat axes, and introduce ad hoc approximations, and get results that are probably much more useful to people who want to build tokamaks or anything else. Now the interaction between these two has been brought out very clearly, and is perhaps one of the most important aspects of the Workshop. And I would hope that next year that there will be as many interesting and exciting ideas as came up at this meeting.

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