Smale fest 56

## Final Panel:
## R.F. Williams, D. Karp, D. Brown, O. Lanford, P. Holmes, R. Thom, C. Zeeman, M. Peixoto, and Audience Members

**R.F. (Bob) Williams:** Each of our experts will speak for ten minutes, giving their overview of their respective fields. One of the things that might be very good to get is a view of the future. It's been pointed out that maybe that should come more from the younger people here than us gray-beards. So, going in the order suggested by the program, I will start with Karp, who will speak about computations.

**Dick Karp:** First of all, I haven't had a chance to wish you happy birthday, Steve, so happy birthday!

The theory of computational complexity has traditionally been based on discrete, rather than continuous, mathematics. One of Steve's major achievements has been to reorient this topic so that it becomes more interlinked with fields such as topology, analysis and geometry. And, in so doing, Steve has cast the subject in a form more compatible with the way people who actually compute think about numerical analysis and scientific computation. I'd like to trace some of Steve's contributions and indicate their impact and meaning for the field of computational complexity.

Steve's investigation of polynomial zero finding was perhaps his first venture into computational complexity. This topic was well covered by Mike Shub's talk and I won't say anything further about it.

My principal overlap with Steve has been in the the area of linear programming. One of the most fundamental and useful algorithms in existence is the simplex method of George Dantzig. The simplex method solves the problem of maximizing a linear function of many variables subject to linear inequality constraints. This is a fundamental model in many areas of operations research and economics. For reasons that are somewhat mysterious, the simplex method is wonderfully successful in practice. One can construct pathological examples on which the simplex method performs extremely poorly, and yet in practice it can be depended on to perform very rapidly. For many years it was a puzzle to explain the remarkable success of the simplex algorithm.

One natural approach is to put a probability distribution on the space of instances of the linear programming problem, and then show that in some expected sense the simplex method was good. Many people were aware of this possibility, but the two who first made significant progress were Steve and Karlheinz Borgwardt in Germany, using rather different models. Steve, with his penchant for homotopy approaches, chose a particular parametric version of the simplex algorithm and was able to prove that, under a certain type of probability distribution, it ran in polynomial expected time. Later, a number of other investigators proved stronger results under weaker hypotheses with somewhat simpler proofs, but it was Steve who first opened the door through his original analysis.

It's not very clear, of course, whether these probabilistic models really explain the success of the simplex method, because the probability distributions that are assumed may have no relation to the examples that actually come up in practice. But, nevertheless, these kinds of analyses have been a landmark in the study of linear programming.

Later, Steve introduced a very interesting new concept into computation complexity theory: the topological complexity of a function. Roughly speaking, topological complexity corresponds to the number of conditional tests or branches that a program must use to evaluate a given function. Steve was able to relate this quantity to fundamental ideas in topology. He showed that topological complexity is related to the following question: Given a continuous function, into how many open sets must the domain be partitioned in order that, within each part, the function has continuous inverse? This notion of topological complexity provides an important link between topology and the theory of computation.

Of all Steve's work in the theory of computation, his recent paper with Lenore Blum and Mike Shub has the broadest implications. The paper develops a theory of computation over the reals or, more generally, over an ordered ring. The basic point of departure of the paper, which distinguishes it from standard theories of computation, is that a real number is considered as a monolithic entity, rather than an infinite string of binary or decimal digits. Thus, an arithmetic operation such as addition or multiplication of two real numbers is considered to be a primitive step. This is not exactly what happens in a digital computer, but it corresponds pretty well to the way applied mathematicians think about computation. This viewpoint leads to a rich theory which is very different from more conventional theories of computation. For example, within this theory any discrete combinatorial problem is trivially solvable, since one can encode the solutions to its countably many instances into a single real number which can be thought of as simply a constant input to the problem. To solve any instance, all you have to do is extract the appropriate bits of this constant.

So the theory is completely different from standard discrete models, and yet within it one can define natural analogues of the recursive and recursively enumerable sets, the class P of problems solvable in polynomial time, the class NP of problems for which solutions are checkable in polynomial time, the class of NP-complete problems and other basic constructs from computability theory and complexity theory.

The theory has very interesting links with dynamical systems. It turns out, for example, that for most rational maps the basin of attraction constitutes a recursively enumerable set which is not recursive. This is a kind of connection that could not possibly arise within the standard discrete theories of computation.

Backing off from Steve's specific contributions, I'd like to mention that there are further untapped connections between the theory of dynamical systems and some important issues in theoretical computer science. For example, there is a lot of interest in the study of pseudorandom number generators -- the kind that are used to generate "random" numbers for Monte Carlo experiments. A pseudorandom number generator is nothing more than a discrete dynamical system with the property that, without knowing its initial state, no polynomial-time algorithm can determine its future outputs from its past outputs. Viewed this way, pseudorandom number generators are very similar to chaotic dynamical systems, with their unpredictability due to sensitive dependence on initial conditions. This connection really cries out to be investigated in a more formal way. Also, more broadly, any computer system or computer network -- the AT&T network, for example -- can be regarded as a dynamical system, and it's tempting to wonder whether some of the instabilities that are observed are related to chaos. Bernardo Huberman at Xerox, Palo Alto, has conducted some investigations along these lines.

I'd like to take the liberty of concluding in a personal vein. To me, the most striking aspects of Steve's character are his courage and his confidence. His courage has been manifested in his political work, as we all know. In mathematics, Steve has always had the courage to plunge into new areas, pick up whatever background he needed, and make an attack on the central problems. In the rest of his life Steve shows the same courage. Few of us, I think, would spend a summer sailing to the most remote island group in the world, just to see if we could do it without getting shipwrecked. This is characteristic of how Steve lives life to the hilt. With respect to confidence, I've learned a lot from Steve. Many of us, as we get up into our 50's and 60's, begin to back away from research a little bit and get interested in serving on departmental committees and doing all those important things that need to be done. It's good that people do that, but it's also a retreat from facing the challenges and frustrations of research. Steve's confidence has never flagged. He remains committed to his research and we're very glad that he is.

So once again, Steve, congratulations. It's been a pleasure to be associated with you.

**Bob Williams:** Our next speaker is Don Brown, who will speak about economics and its relation with Steve Smale's work.

**Don Brown:** Well, it's hard to know what to say after the three talks we heard last Sunday. Debreu, Geanakoplos, and MasCollel talked about Steve's impact not only on my own work, but on the economics profession. I think I'm going to take a suggestion from Howard Oliver and try to talk about problems which we haven't solved, and how some of the work that Steve has done might help us resolve them. In order to do that, I want to put Steve's contribution in a different perspective than that given on Sunday and look at where mathematical economics was prior to Steve's interest in the field in the `70's. The best reference is Debreu's *Theory of Value*, where the central mathematical framework is the theory of dual locally convex linear topological vector spaces. One vector space corresponds to the space of commodities and the other space corresponds to the space of prices. In this model, agents' characteristics are described in terms of convex subsets of the commodity space and concave functions on the relevant sets in the commodity space. The whole analysis, that is, the proof of the existence of equilibria and the proof of the welfare theorems, proceeds within this structure. The primary two theorems which are used for this analysis are the Separating Hyperplane Theorem and Brouwer's or Kakutani's Fixed-Point Theorem.

Now we look at Steve's contribution. From my perspective, and I think the perspective of many people who've studied Steve's work, his primary contribution is not in giving new proofs of existence or the welfare theorems, using different methods of analysis, but his conception of a new framework in which we ought to do general equilibrium analysis. What Steve did was to suggest that linear vector spaces was not the only appropriate framework, but that one should consider economic models as being modeled by manifolds. This conceptual breakthrough, in fact, led to the class of economic models introduced in John's talk, where the Grassmanian, an abstract manifold, is an essential ingredient of the model. What are Steve's methods of analysis? Well, he uses only two results from global analysis, as he says in this article in Volume I of the *Handbook of Mathematical Economics*. One is the Implicit Function Theorem, the other is Sard's Theorem.

If you look at the current policy and the research agendas in economics, say over the last five to six years, you see a tremendous number of commodities. Such models arise naturally when one thinks about the act of production or the act of consumption being contingent on either the state of the world, or the time of production, or the time of consumption. Let me give you a specific example. A question that economists are actively studying now, of great importance, is why it is that countries have developed at different rates. What is it that is similar or dissimilar about the Taiwan experience as opposed to the Korean experience?

Well, we have models of economic growth and these models are most naturally formulated as economies where the space of commodities is an infinite diminishing function space. One wants to know: If you write down a model with an infinite number of commodities (and say you posit a finite number of households and firms) is this general equilibrium model consistent? That is, are the basic propositions of the Theory of Value true in this world? Consistent in the sense that there exist prices that clear markets and the resulting allocation is Pareto optimal? The first proof of consistency in this sense was given here at Berkeley in 1972 by Truman Bewly, who has Ph.D.'s in both mathematics and economics from Berkeley. In Bewley's model, the commodity space is $L^\infty$ and the price space is $L^1$. They're put in duality and the topology on the commodity space is the Mackey topology.

But if you go back to the finite dimensional case, you get more from the global analytic or smooth approach than you get from the linear vector space approach. Not only do you get existence and optimality, but you also have a way of counting the number of equilibria. The question that needs to be solved, which we have no answer to, is how do we count equilibria when we have an infinite dimensional commodity space? In particular, the following problem is open. In a regular economy, you are able to replicate Debreu's results and Smale's results, that for almost all economies there is a finite number of equilibria.

The only result in the literature is for the special case of additively separable preferences and the commodity space is $\mathcal{L}^\infty$. This is a very unsatisfactory state of affairs. It's a bit surprising that we've been unsuccessful -- probably in part because Steve is now working on other things -- because there is this wonderful extension of Sard's Theorem, due to Smale, in the infinite dimensional setting. But if you argue by analogy that it is Sard's Theorem and the Implicit Function Theorem which suffice for the finite dimensional case, then you would think that in infinite dimensions we should be able to prove the same results.

The problem is that in economic models, the assumptions needed to invoke those theorems aren't satisfied. Typically, in economic settings we aren't able to reduce the analysis to a separable Banach space, which is one of the hypotheses in the Smale-Sard theorems. That's one problem. A more basic problem is that these theorems are usually applied in a Banach space setting where the domains of the functions are open; this is not true in Bewley's model. We hope that when Steve finishes his work in complexity he'll come back to economics and think about the infinite dimensional case.

Finally, in relation to Steve's work on complexity, consider the following "inverse problem" to the problem of existence of an equilibrium. Suppose you observe prices and the income distribution. When can we say that these observations are consistent with some specification of utility functions and production functions in the economy? Remarkably, the answer to this question can be expressed as a family of real semialgebraic equations. At present, the only tool for analyzing the existence of solutions to this family of equations is the Tarski-Seidenberg Theorem. Despite their structure, arising out of the economic model, we know nothing about their complexity, nor do we have practical algorithms for solving them. Maybe Steve can also lend us a helping had on the complexity issue. Thank you.

**Bob Williams:** Thank you. The next expert is Oscar Lanford, who will talk to us about physics.

**Oscar Lanford:** Thank you. I'm both deeply honored and really pleased to be invited to participate in this meeting honoring Steve on his 60th birthday. That said, I feel quite uncomfortable up here. It's difficult to find something useful to say after this very successful meeting.

The general theme that I want to address is the influence of Smale's work on dynamical systems on our fundamental understanding of long-time behavior of dissipative dynamical systems in nature. Now, I should first make clear that my perspective on this is that of an outsider. It's true that I was around Berkeley in the late `60's. I participated in the seminar out of which the 1967 paper grew. Rufus Bowen and I even wrote a paper answering a question that came up in that seminar. But, fundamentally, I didn't understand the subject. I was really working on other things -- statistical mechanics, in those days -- and I only came into the dynamical systems business in about 1975 through an interest in the Lorenz system. So, I came into this enterprise after the very successful period of development of the theory of uniformly hyperbolic sets that went on here in the `60's, and from that perspective what leaps to my mind is the difficulty in making the connection between what was done in that mathematical development and applications to physics. So, I'm going to give a slightly contrarian talk here. I want to try to describe what I think the difficulties were.

In general, it seems to me that the interaction between mathematics and physics and other kinds of applications is difficult and complicated. I liked Nancy Kopell's phrase this morning about the fractal boundary. I also think that this is not the only instance of the difficulties. I've lived through another such interaction -- between operator algebra theory and physics -- and the qualitative similarities in these developments suggest to me that the history is not merely of anecdotal interest.

I think the biggest contribution of the Smale program to physics -- it's already been said here -- was in the way of looking at problems. The geometric viewpoint, the serious use of the notion of genericity, the fact that you could say something useful about differential equations without talking about any particular one, and then in somewhat more detail, the serious exploitation of the fundamentally important notion that many systems have nearby orbits which tend to separate exponentially; none of these ideas was new, but they were in a certain sense legitimated by the developments here. The quality of the output really meant that those ideas had to be taken very seriously by anybody wanting to work on the subjects.

Those are what I think the biggest direct contributions were. Now what about the difficulties? First of all, I think there was an important change in emphasis that had to be made in order to get to the kind of applications I have in mind. That change in emphasis was that for applications to dissipative systems in physics, attractors are very much more important than other kinds of hyperbolic sets. Steve's motivation in `66 and `67 was the search for a general structure theory, and for a general structure theory all kinds of hyperbolic sets are equally important. But if you want to understand turbulence, it seems clear that attractors are much more important than other things. Now, one of the little annoyances in this subject is that uniformly hyperbolic attractors simply haven't turned up in real examples. I still hope that that's a phenomenon having to do with the fact that we tend to look at low-dimensional examples, and that when we get to higher dimensional ones, hyperbolic attractors may appear. But that's the way it is.

Then there was what I regard as a major red herring dragged across the path of things, and this was the precise notion that was given of genericity. I said that I thought that the use of genericity considerations was a real contribution to the subject. However, there was a precise definition of genericity, that a generic property was one which held on complement of a set of first Baire category. Now, there are also measure theoretic notions of genericity, and those are less attractive mathematically. In the first place, these things tend to be quantitative rather than qualitative. They're also intrinsically finite dimensional, because when I say measure theoretic I really mean with respect to a Lebesgue measure, therefore less in the spirit of global analysis. Nevertheless, in instances where topological and measure theoretic genericity considerations are in conflict, I think we've learned, in the past 10 years, that the measure theoretic ones are probably a better guide to what you're likely to see. So that was something that needed to be sorted out.

Then there was the question of stability. Again, a big emphasis in the investigation of uniformly hyperbolic sets was the study of structural stability, and we've had to learn, over the years, to live with very unstable systems. This, to my mind, started with thinking about the Lorenz system, where the fine details of the topology change, so to say, continuously as the parameters are changed. Nevertheless, in the Lorenz system there's still a reasonable amount of stability, such that the average behavior of the system seems to change nicely as the system is changed. Rather more dramatic things happen in things like the quadratic family and, we believe also, the Hénon mapping. To take the example of the quadratic family, what we believe is that there is an open dense set of parameters for which there is an attracting cycle pulling in almost all orbits (in the measure-theoretic sense), but that, nevertheless, there is another set of parameter values with positive measure for which typical orbits behave chaotically (and so, in particular, there is no attracting cycle). That's a very bad instance of nonstability, and it's been necessary to learn to cope with that.

The next thing I'd like to say is that one of the great successes, or one of the things which is particularly suggestive for applications, in the uniformly hyperbolic enterprise, is the Ruelle-Bowen Ergodic Theorem. I also think that this is an instance of the interaction between mathematics and physics at its best. As I remember the history, and I haven't had a chance to check this carefully, Bowen started by trying to study the measure of maximal entropy, and Ruelle, because of his interest in making a theory which reflected typical behavior of systems with attractors of zero measure, focused on the role of Lebesgue measure. This led to a different maximization problem which, in the end, with hindsight, seems to be more natural mathematically than the maximum entropy problem. I think also it's important to note that the work of Benedix, Carlson, and Young for the Hénon system is one of the very important new developments which shows that things like the Ruelle-Bowen Theorem work in the context of things like the Hénon system which really do seem to turn up in examples.

Then I wanted to make two very general and therefore nearly empty remarks about the general question of applicability of these things. The first remark is that the potential applicability of a result, at least in the short run, seems to be badly correlated with its mathematical depth. I don't mean negatively correlated, but badly correlated. The very deep stability analysis of uniformly hyperbolic sets, which was brought recently to its very satisfactory close with the work of Mañé and Palis on the stability conjecture, seems to me to be an instance of a very important result which probably doesn't have direct applications. I contrast this to the Ruelle-Bowen Theorem and the recent developments by Benedix, Carlson, and Young which seem to me also deep, also important, and close to applications.

There's a second observation which has been implicit already, and that is that, to paraphrase Ruelle, the confrontation of real mathematics with real physics -- and I'd substitute "applications" for "physics" except that the word "real" has a special implication in physics here -- this confrontation is likely to influence the mathematics at least as much as the physics. It was true both in $C^*$-algebras and in dynamical systems theory that this kind of confrontation between different approaches to things has proved to be profitable for both sides.

And finally I'd like to close with mildly pessimistic remarks on the difficulty of interaction between two vigorous disciplines with different objectives and different habits of thought. For that purpose I'd like to show you two of my favorite quotes on this subject, and I've put them on a transparency. These quotes are not ones that I found myself, and I think the origin is relevant. The first one many of you have probably seen; it's the motto for David Ruelle's book on mathematical statistical mechanics. It's a quote from Franz Kafka: "Richtiges Auffassen einer Sache und Missverstehen der gleichen Sache schliessen einander nich vollständig aus." It really shouldn't be translated, but let me try to tell you approximately what it says. It says that the correct view of something and misunderstanding of that same thing do not completely exclude each other.

The second quote is even more pessimistic, but nevertheless I like it very much. It's a quote from Peter Debye, which I learned from a book of Richard Hamming, and it says that "If a problem is clearly stated, it has no more interest to the physicist." It's very tempting to add a gloss, but I think I'll end with that.

**Phil Holmes, delivering remarks on "Dynamical Systems, Paradigm and Detail"**

A number of people whose work I learned about in the early `70's, who really got me interested in moving in the direction of "mathematical engineering" are here, and I'd like to say thank you to them.

René Thom remarked in his lunchtime speech yesterday on the name "catastrophe theory;" maybe it was an unfortunate choice of a name. When I was just finishing my Ph.D. in experimental mechanics in 1973 I saw a small poster on the announcement board in the engineering department. It said that David Chillingworth was going to give a series of lectures on catastrophe theory, and I thought, "My God, that sounds exciting! I'll go and listen." So, that name had an influence on my life.

Shortly after that, David Rand and I started working on nonlinear oscillations together, and we wrote a paper about the Duffing equation which was sent to Christopher Zeeman to referee. He wrote a referee's report that was longer than the paper and much more interesting. It was very encouraging. And then I guess there was a conference that Rand and other people organized at Southampton in `76 which Nancy Kopell, John Guckenheimer and Jerry Marsden attended. Shortly after that I came to Berkeley and met Smale and other people. This was some time after all the action had happened, but I was fascinated by all this stuff and thought, "Surely we can begin to apply it to some real problems," whatever that means. So let me, in the next few minutes, try and distill a bit of my own experience.

I think it certainly is true that applied mathematicians and engineers, perhaps engineers more than physicists, tend to be obsessed by particular problems, by particular methods, and become almost subject to the tyranny of techniques; those of us who work in institutions with institutes of technology are particularly subject to this. So a very important thing has been the larger view, the idea of paradigms, not perhaps versus methods, but paradigms *as well as* methods; of looking at generic families, of trying to come up with general properties, of going to the big picture. I think that certain pieces are very important fall-out for applied mathematicians and applied scientists in general. One of them is that to understand the problem, it's often better to embed it in a much bigger family of problems. If you want to understand a behavior in the neighborhood of a bifurcation point, it might be better to look at a more degenerate bifurcation than the one that you're actually having your problems with: a larger family. One can use universal unfoldings, look at the symmetric problems which preserve various instances of the symmetry and so on. This idea of embedding your own particular problem in a much large context is very important. However, in terms of how to do that and how to look at generating a larger structure, I think perhaps people in dynamical systems are beginning to appreciate more the role of problems in this. After all, it was a specific problem, a specific example of the Van der pol equation, which led Steve to the creation of the horseshoe. Specific problems really do tell us interesting things. They tell us important things to look for.

Now on the issue of interdisciplinary work, and the kind of tensions between the disciplines, I like Michael Fisher's remark: "There is no interdisciplinary work without the disciplines." It's very important for most of us, I think, to do sort of normal disciplinary science most of the time. And I think it's going to be increasingly important in this interdisciplinary work that collaborative groups from different disciplines are going to play a large role. But these groups must not become politicized; these groups should not be embalmed in the amber of institutes. They should be allowed to form and unform and dissolve, and whenever you start to talk to a colleague in another department, you should not hire a secretary, really.

There's a very nice constellation here at Berkeley which isn't, as far as I know, involved in this conference. There's knot theory and DNA-topology, very interesting collaborations of this kind between, in some sense, the purest mathematicians and the most applied people. There is much more to and fro than formerly. Now, Jim Gleick would like to see this as a new science -- chaos theory. But one can argue, I think, maybe not too cynically, that what is scientific isn't very new. It really started with Poincaré, and Smale's paper has been around for a long time. What's scientific isn't very new, and what's new is perhaps not very scientific. However, I think all this sort of frantic activity will lead to a useful residue. You know, there will be a lot of useful things coming out of it. But I think one has to keep one's feet on the ground if one, like me, is a transient; or at least keep one's feet in a discipline. You really have to try to be honest with the terms of some particular problem if you're trying to solve the problem. You cannot afford just to make huge, broad descriptions; you make those and *then* you try to justify them in specific problems.

From that point of view there's one more little instance of the way in which I think dynamical systems theory has had an important influence on applied mathematics and on mechanics. (It hasn't always been a good influence.) Maybe some of the other panel members might like to address this. The notions of genericity, of universal unfoldings, the idea of a classification theorem and catastrophe theory -- they had an enormous influence from the point of view of coming into a problem and saying, "Aha! I recognize these sorts of universal features." And it became clear that mathematicians could perhaps play a larger role in *modeling*, in the creation of models, than they have done in the past. We've had this image of the mathematicians as kind of garbage collectors who clean up after the physicists. As Oscar Lanford remarked, a consequence of that is that by the time the cleaning up has been done, the physics is "solved" and the physicists have moved on. So, from that point of view it's very nice to see mathematical principles used in a more intimate way in building models. However, one mustn't let the principles, the paradigms, kind of run away with everything. One must pay attention to detail. I think I've said enough.

**René Thom:** Well, I would think that we would be very naive if we believed that everything which is objective knowledge can be reduced to strict quantitative mathematization. It is obvious that there are a lot of domains in science which, up to now, resist mathematization. And it's not enough to invoke this magical word "complexity" to indulge, to excuse, our probable inability to dominate this part of science. I think one has to try to consider the borderline between the part of science which is *really* mathematized in the strict sense, and which reduces practically to physics and perhaps a part of chemistry. I don't think biology has much more to offer in terms of strict quantification, except perhaps some genetics, some mathematical genetics, but this is not, at least to my view, fundamental. And so I believe that, on the other side, we have the whole amount of knowledge which is described in ordinary language. Ordinary language allows us, for instance, to speak about causality, to speak about cause. And the concept of cause is not part of the dynamical systems paradigm; up to now there has been now ay of introducing it inside the system of classical dynamics, nor in any kinds of mathematical modelization. So, my program, as I see it, will be to consider this borderline science in which mathematization, strictly speaking, fails. Still, we can try using the flexibility offered by quantifiable dynamics in order to, in some sense, extend the intelligibility offered by ordinary language -- translating the intelligibility associated to causality into some construction of quantifiable dynamics. This is the general program as I see it. And, myself, I am trying to develop this kind of thing essentially in biology, in the so-called theoretical biology. Of course, people might argue that this is not science. Well, perhaps this is not science; perhaps it is only philosophy, perhaps it is bad philosophy. And nevertheless, I believe this approach is worth trying, and I would not be ashamed to indulge in advertising my own work. [He holds up a book and shows it to the audience.] Those among you who know about it may at least try to understand what it does, and may come to some sound judgement on this analysis.

**Christopher Zeeman:** Thank you very much, René. Today we have biological applications as well as physical applications, so perhaps I should say a word or two about biology. Nancy gave a beautiful talk this morning. At the end, she said it wasn't actually connected with the actual biology itself; it was more an understanding of the equations and the roots of biology. I've had a fair amount of experience over the years talking to biologists and medical people and psychologists and neurologists. The reason why I've talked is because I started out as a pure mathematician and I was released and enabled to become an applied mathematician thanks to the great, creative efforts of Steve and his school, and René Thom, in making qualitative dynamics available. That enabled me to begin to speak to the biologists. But, of course, what they say to me is, "Do you really have to learn all that junk? Is it worth investing so many hours of our lives into learning all this mathematics if it can't be of any use to predict something?" Now, if you actually go to the papers of Steve and René, they don't actually do a thing in biology. What they do is address other mathematicians in hoping that they might actually go and take the gospel to the biologists. It's very difficult. Already chaos is being used in biology. For example, I was talking to Roy Anderson the other day, and he said if you count the number of HIV viruses in the bloodstream of an infected HIV person before he actually gets AIDS, then this count will be chaotic. The reason being is that the virus increases and then the antibody system gets hold of it and decreases it. Meanwhile, another one evolves in a slightly different shape and that increases, and then the antibody repeats that and so on, up and down, until one emerges which beats the antibody system. And then you have it taking hold of the antibody system, and then you're over into AIDS, of course. So, it looks like a chaotic attractor. But we're back to the big problem: Can you predict anything? All you're saying is, this chaotic attractor is a description, an after-the-event description. They link the data and look at it, and maybe they reduce it and draw some beautiful picture, that is, a description. But they still come back and say, "Predict something." And that is very educative, because then you have to be really cunning if you want to use qualitative dynamics.

I'll give you an example of a little prediction I made once: Just the observation that if a fish's brain could be modeled by differential equations, then the attractors obey histeresis. Now, I predicted that if the fish was on his nest in a protective mood, and an invader came in and he flipped into the aggressive mood and chased the invader out, until he flipped back to the protective mood again, he will obey histeresis, and that would mean two boundaries to the territory: there would be an inner boundary where he attacked and an outer boundary where he stopped attacking. I've said it to you in 30 seconds. But it was a totally new idea to biologists that the territory should have two radii. Then they went off and did the experiments and they found a fish in a lake in Toronto that did have two radii. The inner radius was 13 centimeters, and the outer radius was 18 centimeters. That led to a numerical experiment, which was then confirmed with these two numbers, and so it's a very concrete biological experiment in which you actually predict. The conceptual idea was just to have the courage to think of a huge dynamical system representing the brain. So, I think one has to be cunning with these dynamical systems, and then all you've gotten are simple predictions that you can actually persuade the biologists to do.

Another lesson that I learned was if you're going to have a piece of applied mathematics, and you've got some standard model that you want to apply it to, and you want to transfer the qualitative properties of this model onto your piece of applied mathematics, then you've got to have a diffeomorphism between them. Now, that hits right at the basis of structural stability. Because structural stability is based on topological equivalence as opposed to differential equivalence, and therefore I have a fundamental criticism of structural stability based on topological equivalence, because it doesn't hand you those equivalence or differentiable models. So I would struggle to develop theories in which you remained within the differentiable category that you're modeling, and then even in these rather flexible biological areas, you can still make qualitative predictions.

I think, in looking to the future, that mathematical biology will be *the* great domain of the next century. I once went to a series of conferences run by C.H. Waddington, the father of Dusa McDuff. I remember, René, these were from `67 to `71, and they were in Italy, for four successive years. And in the first year, Waddington selected pure mathematicians, applied mathematicians, physicists, chemists, and biologists. And the first year, each one spoke to the next one. But the fourth year, the biologists were speaking directly to the pure mathematicians, to the fury of all between them. And I think that's what's going to happen in the next century. The pure mathematicians are going to speak to the biologists, and the really good biologists know exactly what's unknown and what's unsolved. And I remember Crick saying, at one of those meetings, that the three great unsolved areas of biology are evolution, development from the egg to the embryo, and intelligence. These are the three marvelous areas which just call out for geometrical modeling and dynamical systems well into the next century, so perhaps for our children and our grandchildren, that would be one of the most exciting areas of mathematics.

**Mauricio Peixoto:** Well, in a more pedestrian vein I would like to add a few words, related to the area of dynamical systems, about my contacts with Steve. The gist of it I have already said at that banquet the other night. In 1960 Steve spent six months in Rio de Janeiro. During that period he talked about mathematics with very few people, and I was one of them. Besides, he was still learning many classical things about differential equations, and this added some kind of charm to our conversations. One thing that impressed me very much was his instinct for the simple and for the fundamental, his knack for distinguishing what is relevant from what is not. I remember I learned from him, at that time, the meaning of the expression "red herring," which he used frequently. In the early `60's I was very impressed, for example, by the balance and sure-footedness Steve exhibited in handling the problem of the closing lemma, avoiding a head-on confrontation with it. Also, a fact that has not been mentioned here is Steve's enormous influence on the mathematics of Brazil. (See Figure 1)

To close, I think that I express the sentiment of this audience by thanking Steve on his 60th birthday for the wonderful mathematics that he has produced and which, in some way, many of us were fortunate enough to share with him.

**Bob Williams:** We've had a certain success. We've had seven people give 10 minute talks, and it fits in an hour.

**Nancy Kopell:** I would like to follow up on the remarks that Christopher made about biology, almost all of which I agree with, because I think, in fact, that that is going to be a very exciting area in the future. I was talking this morning about biology, but in fact most of what I've been doing in that 10 year span I didn't talk about. It was talking with biologists and working on mathematical models involving trying to understand particular "wet" neural networks. And in that case, dynamical systems plays a very fundamental role which I can summarize in a sentence or two. The point is that, for many large neural networks, one really does not understand the fine details of what's going on in the neural networks. One has no hope in work with networks of understanding every cell and every synapse. But one does understand a certain amount of the gross structure, and what one tries to do is reason from that, in a qualitative way, to what the consequences of what you know are, and that creates various predictions which the experimentalists can then go out and find -- and they have -- which also helps guide them to find more of the fine detail of what's going on in the networks. And it turns out that the particular neural networks I'm describing happen to be for production of rhythmic motion like walking and chewing and swimming, etc.; and the gross structure there is large systems of coupled oscillators. And it turns out that if one takes this gross structure into account, one starts building a large framework within which one can reason about the experimental facts that can then be obtained and placed on that framework. This is beginning to excite a fair number of experimentalists who are joining us in this whole big project. So I see that as a very central use in biology of the whole framework of dynamics.

**Eric Kostlan:** Yes, I would like to speak to political work in the `60's. I think the `60's was perhaps the local maximum in the supply of political involvement by the public and particularly by the academic community. And it's ironic; I think you'll find that the `90's is maybe the local maximum of the demand for political involvement by the academic community. We've talked a lot about confidence and about courage. Well, with recent world and economic events within the developed nations, as well as problems in the underdeveloped or developing nations, it's very difficult to have confidence. But at least I hope that we have courage, and I would like to see the `90's as a period where the academic community rediscovers its global political obligations.

**Mike Shub:** I would like to say one word about the distinction that Oscar drew between the measure theory results and the theorem characterizing structural stability. I would say that from one perspective, the techniques and the entire structure theory developed in pursuit of the theorem on hyperbolicity and structural stability are all being carried forward today: stable manifolds, unstable manifolds, spectral decomposition, Markov partitions, and the resulting measure theory that was done by Sinai, Bowen, and Ruelle. Independent of whether or not that particular theorem turns out to be relevant, the structure is absolutely crucial to the understanding of what is coming now.

**Oscar Lanford:** Yeah, I entirely agree. The point I was trying to make, however, was that the interface, the match between what mathematicians find exciting to prove and what people in areas like physics find directly useable, is a complication.

**Christopher Zeeman:** It's true, though, isn't it still, that quantum theory is relatively incompatible with our math?

**Oscar Lanford:** Oh, I don't know.

**Moe Hirsch:** Let me say a few words about the role of mathematics in science.

Contrary to some philosophers of science, accurate description of reality is not the only role for mathematics in the natural sciences. Mathematics offers something which is not fully appreciated by people oriented towards the harder sciences, harder in all senses: namely, the possibility of achieving *insight*. From simple mathematical models it often happens that one can obtain useful insight into a natural system situation of great inherent complexity, even though the model may be useless for predicting reality.

An interesting and perhaps the first example of this is Reverend Malthus' 1798 theory of population growth, *An Essay on the Principle of Population as it Affects the Future Improvement of Society*: While food supply grows linearly, human population will increase geometrically in the absence of war, pestilence and famine. He gloomily concluded that there's no point in trying to improve the lot of common people, because if you do they will increase exponentially and starve to death anyway.

This theory was not predictive, as neither experiments nor observations were possible. It was neither verifiable nor falsifiable. It was all in the subjunctive mood: If you were to do this, then such-and-such would happen. The numerical examples were not realistic. An interesting question is: Why did Malthus' work have (and continue to have) such an enormous impact?

I think for two reasons. In the first place, his quantitative reasoning, although almost hopelessly simplified as a representation of reality, is nevertheless both precise and seemingly related qualitatively to reality: *something like it* seems clearly true about the real world. Secondly, the mathematical argument is extremely *robust*: The exact formulas for the exponential and linear growth functions are irrelevant -- all you need to get his conclusion is that one grows much faster than the other. Thus Malthus' mathematics provided a strong, unforeseen link from a biological hypothesis to a sociological conclusion. While his conclusion is surprising, the mathematics it is based on is unassailable, and his model is robust. Thus he provided a new *insight* into a complex real world situation where accurate measurement and prediction was impossible.

This is an interesting way of using mathematics in a situation where quantitative considerations are paramount and strongly suggestive, but there is insufficient theory or observation to derive exact formulas. Instead of precise equations, a robust *class* of equations is resorted to, no one of which is accurate, but which are plausible as a class. In this way, mathematics offers insights to the natural sciences that probably cannot be obtained in any other way.

**Christopher Zeeman:** The reply to Malthus is just that population growth is a sigmoid curve.

**Moe Hirsch:** That's a different biological theory than he had. You're attacking it on biological terms.

**Christopher Zeeman:** Right.

**Karen Uhlenbeck:** This last discussion calls something to mind that I want to bring up. I just want to comment that having spent the last few years of my life conversing with theoretical physicists, I agree with Nancy and with Christopher. I think there's a point about mathematics which is actually missed by both mathematicians and people in other disciplines. I actually got this out of a conversation with Jim Glimm, to whom I was probably pointing out that physicists never admit that mathematicians do anything, in the sense that they ignored the theory of distributions because all you really need to understand is delta functions. And they didn't know gauge theory; we knew it, but they created it all. They didn't need us. But, in fact, if you observe closely, they actually do use the pedagogical models that we develop. That is, it's okay to sort of figure out groups for yourself and rediscover SU(3) and SU(4). But you can't afford to waste all that time with your students, so that they rethink all of group theory for themselves and so forth. And people in other disciplines don't re-do the mathematics in a way that it can be taught. It's just a set of rules. And, in fact, people do take over our pedagogical methods very thankfully. They don't give us credit for it very often but, in fact, you will notice, if you watch people in other disciplines, that they will take on the logic of the presentation that mathematicians use. And I think mathematicians actually are closer to a sort of traditional model of combining research and teaching. I personally think that that's one of the real strengths of mathematics. In this conversational dialogue between my people and other disciplines in mathematics, I really felt that I wanted to bring this point forward. I really think we should be grateful for the fact that we're a little closer to students than other scientists, and I think we are valuable to the community in a way that is rarely recognized. I agree that sometimes we present things in ways that are impossible to understand, but so do other people.

**Christopher Zeeman:** The point is that scientists have to take 99% of their facts on trust; they have to believe other people's experiments. But we take nothing on trust.

**Phil Holmes:** You don't read through the proof of every theorem you're going to use each time you need one?

**Christopher Zeeman:** I used to when I was young.

Before we close, could I say one word? I would like to say to Hirsch, because Moe has so often been coupled in great results with Steve, and was responsible for the primary organization of this thing: Moe's a sweetie.