

From Order to Chaos, II
Essays: Critical, Chaotic, and Otherwise
by
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General Introduction:
The Worlds of Science

This book is a selection of my research and popular essays, with particular emphasis on works which review or discuss in a general way some scientific or technical question. The papers are all about the world of science, or rather about the different worlds in which a scientist works. In my own work I can see at least four different kinds of things which might be meant when one talks about the worlds of a scientist.

First, I might point to a little society or social grouping composed of scientists and a few associates. This **social world** defines the group in which we work and exchange ideas. This is the audience for our papers, the source of our applause, and our critics and competition. A scientist can go to different places all over the world and see mostly just the usual group of associates. A scientist can be thrown into a new little group, formed in an allied field of scientific endeavor, and immediately recognize the society and the social norms. This little world is close and closed. It defines our successes and failures.

But there is in addition a more intimate social group which defines our work. Much scientific work is done in direct collaboration with other scientists. Many of the papers in this volume have several authors. Typically each author brings a slightly different experience and point of view to the joint effort, so that the eventual product is much better than would have been produced by any single person.

This fact came home to me at the very beginning of my career as an 'independent' scientist. Gordon Baym and I had both been trained at Harvard, he under Schwinger and myself under Paul Martin and Roy Glauber. He had learned how to apply variational methods to the derivation of Green's function approximations. I was working on the development of approximations which built in some thermodynamic and conservation laws. With huge effort over a period of months, I

had derived one or two approximations which fit my criteria. A day after I had described to him what I had done, he showed me how to construct an infinite number of new approximations which fit into the general scheme. The results appeared in part in our book *Quantum Statistical Mechanics* and in part in our paper, which appears as #5 in my publication list¹. Two heads had done a lot more than one.

My scientific life has contained many other very fruitful collaborations. I describe some of these in the introductory essays which head the various sections of this book.

But, we scientists also work in a very different kind of tight little world, the artificial little world constructed by our ideas. Some of my recent work has been related to the development of models for the behavior of avalanches or sand slides. To construct this **model world** we considered a simplified example in which square or cubic grains of sand were stacked in neat piles. If a given pile overtopped its neighbors by more than a specified amount, then several grains would fall onto the neighboring stacks. Clearly, this model represented a totally artificial oversimplification of any picture of the behavior of real sand. Nonetheless, a whole group of us threw ourselves quite wholeheartedly into the study of this little model. For several years at a time, we took this artificial example and pretended it was the whole universe.² We examined this tiny world with the same seriousness that one might examine the history of the British Empire, or a science of the human mind. Our goal was to develop and understand the laws which governed behavior in this tiny closed-off cosmos.

Why should serious people study such inadequate toys? Clearly these toys cannot accurately represent the third type of scientific world, the **real world** in which we work and live. Nonetheless it might be profitable to study such hermetic little model worlds because perhaps the experience developed in the little world can be extended and applied to our real world. Maybe there is something in our model avalanche which can be carried over and give some deep

¹ The publication list can be found at the end of this book. In general, items in this list will be given by a number prefixed by the sign ‘#’

² In this case, the collaborative team included Ashvin B. Chhabra, Mitchell Feigenbaum, Amy Kolan, Sidney Nagel, Itamar Procaccia, Lei Wu, and Su-Min Zhou.

insight into how avalanches work. Perhaps these ideas might even have some practical use in the protection of Swiss mountain villages or to the design of particle detectors. Maybe not. Probably not. But one cannot tell what might be carried to the real world until the model world is examined and understood.

Science also sees another version of the real world, the world of people. People and the society support science. Naturally some return is demanded. One demand is that science generates ideas and concepts which can be meaningful to the public at large and can catch its imagination or satisfy its curiosity. To realize this goal, we scientists must be teachers in the broadest sense. Another demand is that we, from time to time, satisfy the aspirations of society for better technology or for a better understanding of the applications or limits of technology. We can only occasionally help the society in this direction, but our help can be quite crucial. We have served in the development of weapons, of communications, and of health care. We cannot be sure where we will be needed in the future, but we are required to be alert to ways in which we can serve.

The fourth version of the world of the scientist is the most important and the one which we scientists most vividly experience. What is it that one really transfers from the oversimplified model world to the complex world of reality? Clearly the medium of exchange is ideas. One carries some concept of how an avalanche must work. Some idea. And then one takes the idea and asks how well that idea agrees with mundane reality. (But reality is in most cases richer, more beautiful, and more fertile than our imaginations. So in most cases, this comparison enriches rather than just checks our ideas.) The results of all the model building, all the comparison with reality, and all the up and back of scientific exchange is a set of concepts which can then be applied to other situation. Our final outcome then is something which can be added to the **world of ideas**.

The series of essays in this volume relates in some degree to all four worlds.

I cannot imagine anyone who would wish to go through all this material from beginning to end. So let me take the reader for a walking tour through this material so that he or she might plan a particular path which might proving pleasing or useful.

This book is divided into four parts, which I shall describe in inverse order. The last part, **Turbulence and Chaos**, is connected with my most recent work aimed at describing and explaining how chaos and

complexity arises in physical systems. This subject is in the process of development. However, some of the important ideas in it have already become apparent. In one view, this subject starts from a question: Given that the laws of physics are simple and predictive, how can we have a world which is so complex and apparently unpredictable. The question is clearly in the world of ideas. To answer it, one turns to the development of mathematical models and of real physical systems, in both cases looking for simultaneous simplicity and complexity.

This volume's third section describes my introduction to complexity. During the late 1960s, it became fashionable for scientists to look away from the traditional applications of their research to military systems, and to focus instead upon problems which might be relevant to the broader needs of society. In the U.S. National Science Foundation, this relevance boom even gave rise to a new program RANN, Research Applied to National Needs³. In any case, the same social forces which pushed the NSF toward RANN pushed me toward studying the complex of forces which shape the physical and social environment of our urban areas. This part of the book, **Simulations, Urban Studies, and Social Systems**, includes the outcome of this effort. It also includes some editorial pieces written for *Physics Today* which report upon the health and decay in another kind of social system, the one of physics itself.

The second section in this book come from my best and most important contribution to science, my work on the understanding of **Scaling and Phase Transitions**. Here I played a part in the invention of a tiny world, the critical system, and then devoted considerable effort to studying the detailed properties of that world. In this period, many different and very intelligent people focused an amazing amount of effort upon a very closed and partial model of reality. Despite the limited focus of the work, it has had its consequences. For more details about what happened, I ask you to look at the introduction to that section.

The book's first section, **Fundamentals Issues in Hydrodynamics, Condensed Matter and Field Theory**, is

³ This program was in large measure designed and put into place by Joel Snow. Our recent Presidents have had many unkind words for civil servants. In my experience, I have found governmental science administrators to be thoughtful hard-working people. As a group, they have contributed a lot to science without getting much thanks.

devoted to describing the relationship among different models, either in general or in particular examples. Physics contains many different models, which might describe different aspects of the very same physical system. Clearly, one should ask how the different realities caught by the different models fit together. Some of the essays in this section directly confront this general issue, others ask how it is resolved in a particular physical example. A major theme of this section is that physics is really about how models, which give different levels of physical description, may fit together.

Each section in the book is headed by a specific introduction which goes into more details on the questions mentioned here, and outlines some of the contents of the papers.

There is an overall theme to the whole book. Each of the first three sections is devoted to 'old' scientific questions, questions which have been asked and mostly answered. One can expect that each field of science starts from questions built upon small pieces of the world, and that in time these questions will become answered as well as the times and means available permit. Then, the subfield gets mined out. Naturally, the scientist must then direct his or her work away from these particular aspects of reality. Naturally, also, when that happens there is some temptation to see and bemoan 'the end of science'. Perhaps this is particularly tempting right now for a physicist since some of our most exciting problems have been either solved or run into major technical barriers.

However, in the introduction to the last section, I shall argue that we have in front of us a mostly uncharted territory, concerning the development of complexity in the world. The ideas in this part of science are connected with understanding the relationship and linkage among the worlds we have explored. Right now, I see not an end for physics but a beginning. Now is an exciting time to be working in physics.

Section A. Fundamental Issues in Hydrodynamics, Condensed Matter and Field Theory.

From Level to Level

This section contains a collection of essays about apparently disconnected subjects in field theory, condensed matter physics, and hydrodynamics. There is, however, a thread of connection among all these different essays. In each case, we ask some kind of question about the relation between different levels of description of the physical world. This is a very natural question for a person trained

as I am, in the area of condensed matter physics. My thesis advisers, Paul Martin and Roy Glauber, continually directed my attention at the relation between a microscopic description of reality and a macroscopic description. Thus a gas is composed of molecules, but it also obeys the laws of fluid mechanics. A microwave cavity contains photons but also an electric field. Or again, a fluid near its critical point is a bunch of molecules, but they also be described by a scale-invariant field theory.

In my imagination I see every physics problem as a kind of little world. Each world has its own rules, which apply to the description at that level. This idea is brought out in the paper⁴ A1, *On two Levels*, which looks for a more macroscopic, hydrodynamic, level of description within a computer game in which simplified particles go through a kind of dance. Naturally, one sees the dancers by looking very closely, while a more lumped description shows the hydrodynamic flow. In this paper the logic is one in which the more lumped description is built up from the more microscopic one. This same point is made again, backwards, in the next essay. Here Paul Martin and I start from the equations of motion of hydrodynamics, and look at local fluctuations to gain a more microscopic description of fluid flow. In this case, one and the same physical description covers two quite different ranges of physical size and physical phenomena.

There is something more to say about this paper. A parallel computation was done by Landau and Placzek, long before Paul Martin and I wrote our piece. I do not recall ever having seen this parallel effort. Nonetheless its existence serves as a reminder that rarely do we produce something completely new in science. Every piece is based upon predecessors, and if we did not do the work, it still probably would be produced by others in substantially similar form. This point was brought home to me in my work on electrons and phonons. Essay A3 in this volume is a review piece, based upon the research described in paper #14 on my publication list. The latter is a joint work by Richard Prange and myself. It arose in that form because Prange and I did substantially identical, independent research on this topic. His work was written up first, and appeared one morning in my mailbox. After I called him and described my

⁴ Each reference to a paper which appears in this volume is given as a letter followed by a number. The letter denotes the section in this volume while the following number gives the placement of the paper in the section.

own thinking about this subject, he generously suggested that we write it up together. During these many years, I have felt in his debt for this fine courtesy.

Paper A4 is partially a reprise of the ideas of A3 but now applied to superfluid motion rather than the normal state⁵. It is illustrative of an important idea about the relation between different levels of experience. The basic microscopic forces and interactions are quite the same in normal materials as in superfluids. However, the nature of the physical state of the two systems are vastly different. The superfluid forms a condensate in which all the particles in the system cooperate to produce a single quantum state. This condensation extends across the entire spatial extent of the system and produces a change in behavior which spans both the micro and the macro levels of description. The superfluid is thus essentially different from its normal state counterpart in both worlds of description.

A qualitative change in the collective behavior of a large group of particles is called a phase transition. Often a phase transition is accompanied by a change in the symmetries shown by the system. The paper on superfluidity shows in one example how phase transitions may manifest themselves equally in the microscopic and macroscopic domains. Paper A5 reviews Kenneth Wilson's work on symmetry changes and phase transitions in systems of quarks and strings. The world of strong interactions is described by a theory, *quantum chromodynamics*, in which quarks are the fundamental particles. The basic question here how can one have a description of quantum chromodynamics in terms of almost free quarks, and yet not directly see quarks in our world. The answer is that the confinement is a concept which describes the macro level, the world of ordinary nuclear physics, while the micro-level is correctly given by quarks only weakly coupled by gluons. This work that Wilson started has grown into the subbranch of particle and nuclear physics called lattice gauge theory.

⁵ Another important thread of my research effort in the earlier part of my career was concerned with understanding the relationship between the microscopic and the macroscopic properties of superconductors and superfluids. These include, for example, the collaborative works with P.C. Martin (#1, #2, and #6) on my publication list, with Vinay Ambegaokar (#4), with Brian Pippard (#20) and with my students D. Markowitz (#10), Igor I. Falko (#16), G. Laramore (#31), and Jack Swift (#32).

This review, A5, is one of many papers I have written to describe and perhaps help explain the work of other physicists. I have always felt enlarged when I could work with the beautiful and deep ideas of others. In fact, many of my papers were intended to be in part or in whole explanations or descriptions of beautiful ideas of others. Thus, for example, my paper #57 on my publication list extends the work of A.A. Migdal⁶, #59, written with J. Jose, S. Kirkpatrick, and D. Nelson is partially built to convince the reader of the correctness of a theory due to Kosterlitz and Thouless⁷, #133 addresses the ideas of Bak, Tang, and Wiesenfeld⁸, while in #69 Mahito Kohmoto and I review the work of Sato, Miwa, and Jimbo. Of course, this recognition of others is not entirely unselfish. I have always held the opinion that people are more likely to recognize your work if you recognize theirs.

In paper A6, Eduardo Fradkin and I explain ideas from the theory of critical phenomena and phase transitions in a way which has proved fruitful in a variety of other fields. This paper shows how 'particles' with very peculiar behavior arises from a treatment of two coupled Ising models. It is one of the first treatments of fractional statistics particles, the so-called anyons, which have had a vogue in condensed matter physics and field theory. This paper has perhaps received a bit less attention than it deserved. It is part of a long series of papers aimed at describing the mathematical structure of two-dimensional statistical mechanics. These will be discussed in more detail in the introductory essay for section B.

In my work, as in the work of most physicists, a major goal has been the building of bridges between different worlds of experience. Science has two traditional windows for looking at the world, through experimental study or theoretical construction. In recent decades a third window has opened, which approaches reality through the construction of computer models. Paper A7 is an attempt to describe some of the great accomplishments of computer-based physics. It argues that the importance of this relatively new tool is that it can be

⁶ A.A. Migdal, Z. Eksper. Theoret. Fiz **69** 810, 1475 (1975).

⁷J. M. Kosterlitz and D. Thouless, J. Phys. C. **6** 1181 (1973), J. M. Kosterlitz J. Phys. C. **7** 1046 (1974). See also the work of V. L. Berezinskii JETP **32** 493 (1971) and JETP **34** 610 (1971).

⁸P. Bak, C. Tang, and K. Wiesenfeld, Phys. Rev. Lett. **59**, 381 (1987), Phys. Rev **A38**, 364 (1988).

effectively used in conjunction with the older tools of analysis and experiment. If I were writing this column now, I would say the same thing in a more negative way by pointing out that computer-based physics can be ineffective when it constructs totally closed worlds, which then cannot be related to the richer perspectives of theory and of experiment. However, now as then, I believe that this new tool can be properly used to either explore some totally new area of behavior, or alternatively to check a precisely formulated idea.

From the earliest period of my work I have made use of computers. For example, an applied paper⁹ on heat transfer in semi-transparent materials contained a solution to a radiation transfer equation obtained with the aid of a computer. Later on Jack Swift and I, see paper B4, described a computer algorithm which might be used to simulate the approach to equilibrium in a system obeying statistical mechanics¹⁰. Abdullah Sadiq later implemented this idea in his PhD thesis. In more recent years, my students, my coworkers and I have made a major effort to use the computer as a device to generate ideas which could be checked against theory and experimental reality. We have concentrated on efforts on small computers, because we found these to be truly flexible tools for studying a tiny portion of reality.

Paper A7 was written for the Reference Frames column in *Physics Today*. Gloria Lubkin, the editor, had the idea of such a column, describing personal views of the worlds of physics. She and I worked together in producing these columns. I have had much good feeling about them. I think that they are rather successful, in large measure because of her enthusiasm and good sense. This section ends with two columns, A8 and A9, intended to assess some of the worlds constructed by scientists and how they are related to one another. The first does this by looking at physics as a whole; the second looks at an example from hydrodynamics, condensed matter physics, and elementary particle experiment.

⁹ This joint work with Henry Hidalgo is # 8 on my publication list.

¹⁰ This approach use the method of *cellular automata*. Later on, this approach was very much improved by others. See paper A1 for a discussion of some of its implications. After a while I returned to this subject in work done jointly with McNamara and G. Zanetti, #125. We contributed to the further development and checking of the applicability of such models to describe equilibrium and nonequilibrium situations. The avalanche work already mentioned uses another kind of automaton.

B. Introduction to Section on Scaling and Phase Transitions

On the joys of creation.

This section describes the development of ideas of scaling and universality as they relate to phase transitions and critical phenomena. In the late 1960s and early 1970s, a group of physicists and chemists changed the way scientists look at problems in statistical mechanics and related fields. Looking back at this work from almost thirty years later, I still feel proud, pleased, and somewhat surprised that I could play a role in such an achievement.

My story starts when I was finishing up as a graduate student at Harvard in 1960. Kurt Gottfried and Paul Martin both pointed out to me that the problem of 'second order phase transitions' were was quite interesting and not understood at all. Kurt and I even did a calculation, never published, in which we looked for critical fluctuations in three dimensional superconductors. We got, and were discouraged by what is in retrospect the correct answer which is that it is next door to impossible to observe critical fluctuation in three dimensional superconductors. Then, I put the problem aside for a while.

I came back to it during a nine-month period I was spending in the Cavendish laboratory in Cambridge University, invited by Neville Mott to take part in what he called a theoretical jamboree. This kind of jamboree permitted one to have lots of free time for research, so I began to struggle once again with the critical behavior which occurs very near second order phase transitions. Previously many scientists had believed that the Van der Waals-Weiss-Landau mean field theory of this transition was essentially correct. But, by this time, the experimental work of Sasha Voronel and others had shown that the mean field theory was wrong for the classical liquid gas phase transitions. C. F. Keller's experiment¹¹ and Brian Pippard's¹² analysis had shown that it did not work for the transitions in Helium. On the theoretical side, Cyril Domb, Michael Fisher, and the King's college school had proven that the mean field theory did not apply to Ising models either. The time seemed ripe for a new approach.

¹¹ See C.F. Kellers (Thesis, Duke University, 1960).

¹² A.B. Pippard, Proc. Roy. Soc. (London) **A216** , 547 (21953).

I entered the problem by studying Lars Onsager's solution of the two dimensional Ising model. This exact solution of this model of two dimensional magnetism had been announced in the 1940s, but it had never been fully analyzed. Onsager and C.N. Yang had calculated some of the thermodynamic properties, but there was really no explanation of what physics might be demonstrated by Onsager's solution. Here was a tiny little world, just waiting to be explored and perhaps even captured. As far as I could tell, nobody had looked at the spatial correlations built into the Onsager solution. My own background pushed me toward looking these correlations. The work on the connection between hydrodynamics and correlations (A2) were all about the relation between thermodynamic behavior and space-time correlations, especially in the long-wavelength limit. My Green's function studies and much of the other work described in section A was aimed at understanding correlations over large regions in space and time. I was certainly ready to attack correlations in Ising models.

I began to calculate the spin-spin correlation function of the Ising model near its critical point. (See paper B1.) The calculation was long and difficult. It involved the Weiner-Hopf technique for solving integral equations, which made extensive use of complex variable techniques. Fortunately, I had received an excellent training in this method in graduate school with the applied math courses I had taken from George Carrier and Arthur Bryson and with the complex variable course I had taken from George Mackey. After about six months, I had a finished work which I sent off to the Journal of Mathematical Physics.

This paper was potentially very important for this subarea of physics. It contained the germs of much of what was to prove to be the correct theory of second order phase transitions, all worked out in a particular example. The paper also had defects. It was hard to read. The boundary conditions at the edge of the material were handled in a sloppy and incorrect fashion. (I did not believe that the boundary conditions were really relevant to the solution, so I did not inquire very deeply into their correct handling.) And the paper certainly did not proclaim why it was potentially important. It was rejected, twice. And in each rejection it crossed the Atlantic once or twice by slow boat. This paper was written before the days of extensive use of preprints. So, for a period of almost a year, there were only a few people who knew the contents of this paper. It was eventually published, after the second rejection, in Nuovo Cimento.

In the meantime, the field had progressed considerably. There was a conference on second order phase transitions, held at the U.S. National Bureau of Standards, in which the field was reviewed. Several people—notably Fisher, Ben Widom, and Michael Buckingham—described some conjectures about spatial correlations. Widom, in particular, had introduced some 'magic' (the word was used by Martin when he mentioned the work to me) relations among the critical indices. These critical indices are numbers used to describe the order of magnitudes or the various quantities which can be used to describe the near-critical behavior of a system near a second order phase transition. My spin correlation work also involved these critical indices. There were many of them, and I had a hard time remembering them all. So I had developed a mnemonic device, which involved expressing all orders of magnitudes in terms of two independent magnitudes, the natural fluctuations in the spin and in the energy density.

Then, in Christmas week of 1965 I had a sudden vision. A gift from the gods. I had a simple view of how these magnitude relations might be true and general. In modern terms, I had developed a scaling analysis of the critical behavior of Ising models based upon the idea of running coupling constants, i.e. couplings which depended upon the distance scale. (Some idea of this kind was also present in field theory in the work of Stückelberg and Peterman and of Murray Gell-Mann and Francis Low¹³. Unfortunately, I was only barely aware of this earlier work.) The awfully complex and convoluted extension of the Onsager solution which I had previously done could now be explained in terms of a few simple and appealing ideas. And better yet, these ideas could be extended to understanding most problems involving second order phase transitions.

Many people have written about the difficulties which they experienced in getting their ideas accepted by the scientific community. I have never had much problem in this direction. I have always been an establishment figure, and most of my good work has gotten all the recognition it deserved. I had this 'Christmas vision' while I was on the faculty at Urbana. My colleagues there said they liked the work. My collaborator, Gordon Baym, had particularly warm words, while my seniors, John Bardeen and David Pines, nodded approval. I am told my preprint elicited seminars on

¹³ E.C.G. Stückelberg and A. Peterman, *Helv. Phys. Acta.* **26**, 499 (1953). Murray Gell-Mann and Francis Low *Phys. Rev.* **95** 1300 (1954).

the work at Harvard and Cornell. My joy was increased when I spoke to Mel Green, who told me about data that had been presented at the Bureau of Standards conference, which supported my conclusions. I also recalled Paul Martin's statement about 'magic' relations, and then discovered Ben Widom's papers which had just been published in the Journal of Chemical Physics¹⁴. Widom had developed scaling arguments which gave many of my conclusions. Later on I found out that Patashinskii and Pokrovskii had also gotten the right answers at about the same time.¹⁵

My own work was published in the journal *Physics*. It is paper B2 in this volume. My answers in this paper were the same as that of Widom and of Patashinskii and Pokrovskii, but my point of view was somewhat different. There were, in fact, quite a large number of contributors to the development of these new ideas. In hindsight, it is easy to see that we--all of us-- had discovered and invented a wonderful new world. Near critical points, thermodynamic systems show a very special behavior. If you examine the system on distance scales which are much larger than the typical microscopic distances, you see correlations which extend over these very long distances. The system has to chose to fall into one or another thermodynamic phases, and is showing the vacillations in its decision-making. This process of choosing produces a beautiful, closed world with its own rules and its own, internally consistent explanations. The world had been penetrated and could be explained.

The next job was to see whether the ideas that had been developed all jibed with the experimental facts. To do this, a group of us got together at Urbana and ran a seminar aimed at looking at all the known experimental and theoretical data about critical phenomena, to see whether it fit into the newly proposed pattern. This group, W. Gotze, D. Hamblen, R. Hecht, E. A. S. Lewis, V. V. Palciauskas, M. Rayl, J. Swift, D. Aspnes, J. W. Kane and myself, looked at all the available literature and gave seminars on what we had learned. We convinced ourselves that all the data was consistent with the new point of view, and that much of it could be explained by the new scaling theory of critical phenomena. We reported our conclusions in the review paper, which is number B3 in this volume.

¹⁴B. Widom J Chem. Phys. **43**, 3892,3898 (1965)

¹⁵ A.Z.Patashinskii and V.L. Pokrovskii, Soviet Phys. JETP **23** 292 (1966).

At some point in the development of this review paper, I am not sure when, a new idea was added to the concepts of scaling and running couplings. This idea has the modern name 'universality', which I first heard applied in a conversation with Sasha Polyakov and Sasha Migdal in a dollar bar in Moscow. Their use of the word came from descriptions of field theoretical solutions to problems of this kind. The basic idea is that there are only a few different solutions to near-critical problems. Using the solutions, one could group the problems into 'universality classes'. Many apparently different problems have the same solution and belong in the same class. In critical phenomena, the universality classes are in large measure defined by giving the dimensionality of the system and by describing the kind of information which the system must transfer over long distances. As a specific example, liquid gas phase transitions and Ising-model-magnets show exactly the same behavior near their respective critical points.

This universality idea provides a truly new way of looking at problems in statistical mechanics, field theory, hydrodynamics, etc. Instead of solving each problem, one looks for classes of problem each class having a common solution. We essentially used the idea (before it had a name) in organizing our review paper. It had been explicitly used in the previous work on critical phenomena in Helium by Pippard, and had been implicit in the work on Ising models of the King's college school. Bob Griffiths added considerably to the concept by pointing out its relation to the geometry of thermodynamic surfaces. By looking back on my Ising model work, I could see that the universality idea was clearly and closely related to the scaling concepts. The universality concept said that one could classify the different microscopic worlds produced by critical phenomena. This idea made critical behavior itself into a ideal world, beautiful and self-contained.

Over the next few years, I had very considerably pleasure in exploring the corners of our new world. Paper B4, by Swift and myself, describes how to set up a cellular automaton, i.e. a parallel processing computer, so as simulate hydrodynamic equations. Such simulations have been much developed since then. (See, for example, paper E6, which makes use of another automaton.) Paper B4 with Jack Swift is one of a pair done at roughly the same time we were aiming at developing a theory of dynamical critical phenomena. Paper B4 was interesting, maybe even important, but not of much use in critical phenomena. On the other hand B5 describes the work

Jack Swift and I did on developing the beginnings of a theory of transport behavior near critical points. As always, new ideas worked from solid older ones. Here the original ideas were creations of Marshall Fixman and Kiozi Kawasaki. My own development toward these ideas go back to work I had done on transport theory in my thesis (paper 6 with Paul Martin, on my publication list) and to a long series of papers I had done on transport with a wide variety of collaborators.¹⁶

Papers B6 and B7 explain and describe the world of scaling. In particular, paper B6 look back to the older work of Fisher and of Buckingham and describes how their ideas can be incorporated in the new viewpoint. Paper B7 summarizes what we had learned about the phenomenological theory of critical systems, from a slightly more mature perspective than that of the 1967 review paper.

One important result in the theory of critical phenomena is what is called the operator product expansion or short distance expansion (See paper B8). This expansion, due to K. Wilson and myself, is based upon the idea that, at criticality, there are a limited number of different local fluctuating quantities. This idea permits one to express products of fluctuating quantities at criticality in terms of an expansion in the fundamental critical quantities. This idea provides a mechanism for understanding the critical symmetry in a rather deep way. In paper B9, H. Ceva and I explore this expansion for the two-dimensional Ising model.

Meanwhile the field had exploded. Ken Wilson had absorbed the new viewpoint, and magnificently extended it. He introduced two rich new concepts: the fixed point and the space of coupling constants. With these concepts the scaling and universality point of view became a theory: the renormalization theory of critical phenomena (and of much else). The remaining papers in this chapter trace out how the scaling-universality theory and the renormalization group work together. Paper B11 attempts to be a formal review of my contributions to the whole area. I should also point back to paper A5, which describes how the running coupling constant concept found

¹⁶ During the early part of my career, I spent a lot of time working on transport properties of various many particle systems. All this work formed a basis for my understanding of the relation between the macro and micro levels of description. The resulting papers, especially #13 (with D.C. Langreth), #15 (with M. Revzen), and #25 (with J.W. Kane) formed an important part of my education.

application in quantum chromodynamics. Paper B10, a joint effort with Humphrey Maris, describes how some of the new ideas could be presented at a level which is accessible to undergraduates.

I should emphasize once again my tremendous personal gratification in the accomplishments of the whole field. The story I have told has many actors and many excellent accomplishments, among them my own. In the end, we can say that a group of scientists created something beautiful, which was not there before. I saw it happen. I saw understanding and nature come into correspondence. It was great to have been there.

But science does not stand still to admire past accomplishments. Immediately after Wilson had invented the renormalization group, Franz Wegner showed how to phase the renormalization arguments into a formal algebra of coupling constants. Wilson and I independently (see papers B8 and B9) described the complementary algebra of fluctuating quantities near the critical point. My own work, which I did jointly with many collaborators¹⁷ carried forward the understanding of these algebras in the context of two-dimensional critical behavior. Then Polyakov invented conformal field theory, which proved to be a tool which would enable one to understand all the behavior of almost any critical theory in two dimensions. The algebraic technique was refined and perfected by the field theorists. The scientific area of critical behavior exploded and indirectly contributed to the development of string theory.

In the meantime, with many collaborators, I contributed to the solidification of the gains that had been made in understanding critical phenomena. For example, Franz Wegner and I (see #43) developed concepts related to continuously varying critical indices.¹⁸ N. Berker, R. Ditzian, Gary Grest, Michael Widom and I applied these concepts to other statistical mechanical systems. This work was

¹⁷ Among these coworkers are H. Ceva, Alan Brown, Michael Widom, Ad Pruiskien, and Gary Grest

¹⁸ I lost a bet involving a bottle of bottle of Scotch (or Vodka) to A.A. Midgal on this issue. I had bet that all two dimensional critical indices were rational numbers. I figured I could not lose, since it is usually very hard to prove that a number which arises in the context of some physical problem is not rational. But then Rodney Baxter proved that in the eight-vertex model, indices vary continuously. I paid off. The Midgal's junior (A.A.) and senior (A.B.) generously repaid with an 'irrational' vodka bottle which had been bent into an impossible shape in their home workshop.

subsequently carried forward and extended with many other people¹⁹ In another series of papers, A. Houghton, M.C. Yalabik and I explored the consequences of the real space analysis of renormalization near critical points.

The understanding our little community has generated has, in the course of time, spread far beyond the study of second order phase transitions. The scaling and correlation ideas form a crucial part of the phenomenology of particle physics and reappears in astrophysics in, for example, analyses of fractal distribution of matter in the universe. Classical Mechanics and hydrodynamics contain many scalings, with the behaviors in turbulence and near the onset of chaos being most like those in critical behavior. In materials science, since the work of P. de Gennes, B. Mandelbrot, and T. Witten, people are always looking for some sort of scaling or universality. We have come far by looking at the small corner of physical behavior very near critical points. In part, we have accomplished our trip by following Onsager's solution and squeezing out every generalization that could possible be found in that rich reservoir.

Beyond the export of our specific technical methods, we have also exported a point of view, encompassing the way in which one might look at the structure of physics. One image of this structure is that each little world of phenomena is really based upon the physical laws which describe a more fundamental level of reality. This image leads one to a reductionist outlook. Then one would say that the true goal of physics should be to reach deeper and deeper toward the basic laws which describe the fundamental interactions in the world. However, the study of critical phenomena and other topics in condensed matter physics pushes one toward another and complementary image of nature. This view was expressed in P.W. Anderson's thoughtful paper 'More is Different'²⁰ in which he pointed out that every level of reality can have its own deep and fundamental laws. In studying a particular little model world, the goal of the scientist is threefold: First, to expose the fundamental laws in their most general form and to show how they work out in the specific system in hand. Second, to show how this particular

¹⁹ My collaborators during this period include most of a generation of Dutch Statistical Mechanics-- Ad Pruisken, Marcel den Nijs, and Bernard Nienhuis -- and other coworkers including J. Banavar and Morgan Grover.

²⁰ P. W. Anderson Science 177 393 (1972)

level of experience is related to other closely connected parts of reality. And third, to take the ideas generated in the study of this one particular part of the world and apply them to other portions of the world. The study of critical phenomena is not in any way unique. However, it is a particularly successful and beautiful example of the generation of deep ideas about the simplified world of critical fluctuations, about how those fluctuations are defined by the microscopic behavior, and about how these ideas manifest themselves in macroscopic behavior. The ideas generated were exported widely and illuminated, in an unexpected fashion, many other areas of science.

C. Simulations, Urban Studies, and Social Systems Models and Arguments

Science includes the critical application of ideas to real world situations. In the 1950s, 60s, and 70s I found many applied problems very interesting. While I was still in college, I worked for the Guided Missile Division of Republic Aviation Corporation, helping with the proposal stage of the design of robot aircraft. In graduate school and thereafter, I worked for the AVCO corporation doing heat transfer calculations related to the design of guided missiles and space probes. I found the latter job extremely instructive. It enabled me to interact with some scientists whose paths I would not normally have crossed, including Hans Bethe, Jim Keck, and Arthur Kantrowitz. I was very impressed by the intellectual vitality of the work at AVCO, particularly the branch in Everett, Massachusetts.

But after a while, I became convinced that this kind of work was socially unproductive. During the Eisenhower years I had been quite uncritical about most of our (U.S.) foreign policy. However, while I was a postdoc in Copenhagen, our government did things I felt that I could not explain to the 'foreigners' around me. The Bay of Pigs invasion of Cuba was the example that I best remember. Having no explanation for others, I became less convinced that our role was always a good one. One defining event for me was sitting in the cafeteria at lunch time at AVCO's Missile Division in Wilmington, Massachusetts discussing the dangers to the world of nuclear weapons. I even wondered whether it was prudent to pick up stakes and go someplace like Brazil. This discussion was taking place during the Cuban Missile Crisis. On the way to work that day, I had driven by Boston's Logan airport which had a small section covered with Strategic Air Command bombers, all probably loaded with nuclear weapons. At that time, I could not see why the US could not tolerate

the same kind of proximity of enemies that the Soviets had to endure. So I began to lose sympathy with US Cold War policy. This progression in my thinking continued through the period of the beginning of the Vietnam War. Naturally, I could no longer work in the missile industry.²¹

So, for a while, my applied interests had no good outlet. But then, starting in about 1967 I began work on the use of scientific techniques to describe and control urban social and economic phenomena.

I got started in this work through the efforts of Dale Compton who was then head of the Coordinated Sciences Laboratory at Urbana. He introduced me to Jerrold Voss, an urban studies specialist. I worked on their project for studying tendencies in urban real estate prices and land use in the little town of Kankakee, Illinois. I do not know what I or they expected to gain from my involvement in this work. I brought essentially no knowledge whatsoever to the project. I do not know any economics or sociology. I got involved in the computer side of the project, supervising the computer specialist in the Urbana laboratory. In this computer work, we absorbed great piles of historical data about real estate prices in Kankakee and tried to display them in some way which would enable us to understand what was going on. Little by little I gained some rudiments of information about the urban scene, and some small knowledge of computers. (The actual computer at the Coordinated Sciences Laboratory²² was large and far too complex for me to program.) I did not contribute much but I was proud, and somewhat flattered to be included in their work, and its writeup, C1 in this section.

Then, I moved to Providence, Rhode Island, and Brown University. At that time there was a great national push toward understanding the dynamics of urban development. Jay Forrester of MIT had developed a computer model of urban change, which took a very

²¹ To complete my story: The lunch-table discussion ended when one of the executives suggested, with some annoyance, that we should all get back to work. We did.

²² This Coordinated Sciences Laboratory (CSL) work eventually replaced my work in the missile industry. Life does have its ironies. This laboratory was almost fully supported on Department of Defense contracts. As a result of complaints about the Vietnam war by people like myself, after a while it became impossible for labs like CSL to use military funds to contribute to civilian research.

simplified view of a urban society, produced a quantification of that view in terms of a computer model, and then used the output of that model to prescribe social policy. I did not like the policy prescribed. He basically suggested that by removing housing for poor people the city could free up land which could be used for industrial expansion, to--in Forrester's view--the long-range benefit of all. In my view, this policy made the poorer section of society pay for the economic development which would occur in the city, and tried to sanctify that policy by giving it the blessing of apparently-objective computer output. Consequently, I set out to use the modeling tools that Forrester had developed to reach conclusions which were more to my liking. Brown University was a good environment for this because they had a computer system which was set up so that anyone could use it. I went to work reproducing Forrester's model in a more flexible computer environment.

Brown University contained a group of people interested in urban public policy. Eventually, the group of urban research coworkers at Brown would include Benjamin Chinitz, Graham Crampton, Bennett Harrison, Susan Jacobs, John Tucker, and Herbert Weinblatt. We took as one of our tasks the incisive criticism of Forrester's work. The parameters and structure of his model had never been verified against real data. We could have tried to disprove the model by looking at the world. However, that would be difficult or impossible since many of the social concepts in the model are very hard to quantify. Instead, we chose another route. (See C2 and C3). Our group changed the focus of the model from a one-city application to a national application. The nation was viewed as being completely made up of cities. We insisted on keeping track of the poor people. This social group would be tend to be squeezed out of the city by a local application of the housing-removal strategy, and would be just squeezed by a national application of that policy. We also tried to develop a more objective (or perhaps just different) set of criteria for the success of the policy. Our first result was that while not changing the model at all, we could reach opposite conclusions from that of the Forrester group. By our criteria, the poor population was highly squeezed with little overall gain to any population segment in the urban society.

This first work can be construed in two ways. One truth was that you got out of such models just about what you put into them. They were mostly a way of recording your preconceptions. The second truth was that such preconceptions could be fluently incorporated

into models of this type and one could, in fact, reach any conclusions that one wanted. Hence, at one level, this work could be construed as a criticism of all model-building in which ones preconceptions were not tested against data.

I am not sure that we listened to our own criticism fully. We went on to build other models which more accurately recorded our own prejudices (see paper C4) and points of view. But, after a while the point we had made began to sink in. If these models really represented little more than we could say in words, why not leave out the computer? The construction of this sort of computer model seemed to be a rather pointless endeavor. For this reason and others, I moved away from urban studies.

Nonetheless the experience had taught me much. My collaborators had valuable insights into the functionings of a city and I was pleased to absorb some of these. And I had learned a new technique: the use of the computer to model experience. The computer would prove crucial in the next stage of my career, in which I would work on hydrodynamics and chaos.

My experience in urban work extended beyond research. I worked (unpaid) for the Rhode Island State Planning Department as chair of a committee which constructed criticisms of all proposals for federal funding of public programs. The point of such a committee is to see that everyone talks to everyone so that there is no unnecessary unhappiness engendered by such proposals. Since I enjoy talking, this was a fun job for me.

At the same time, I taught courses for undergraduate majors in urban studies at Brown. We discussed modeling, and the spatial structure of cities, and social policy and lots of other things. Such teaching was a rewarding experience for me, particularly because I could no longer do the kind of teaching which dominated my early career. At Urbana, I spent a lot of time teaching graduate students either in large classes or one-on-one as their graduate thesis adviser. But by 1971 or so, this kind of teaching became much less attractive. Within a few years, a large fraction of the jobs which had been available to physics graduate students dried up. Our students were not finding appropriate work. Consequently, graduate-school teachers like myself felt their work to be unnecessary.

The climate for employment of advanced-degree students in physics had basically changed because of the Vietnam War. Many physicists and other scientists were critical of the policy of the government.

Military contractors looked for potential workers who would not criticize governmental aims and policies. Gradually engineers replaced physicists in the 'defense' industries. Fewer physicists hired meant fewer graduates needed, meant fewer teachers needed to train the graduate students, means fewer physicists hired In any case, there was little satisfaction in training large numbers of graduate students in this period.

However, the job of training urban studies specialists is not very satisfactory either. As pointed out by Alan Altshuler²³, one problem of the urban specialist is that there exists no large base of real knowledge, information, or technique which would enable the trained person to better prescribe urban planning policy than the ordinary intelligent citizen. The specialist has nothing special to add. As you can imagine this situation is rather discouraging to the teacher, who then has nothing special to teach.

So my urban public policy phase gradually withered away. When I accepted a job at the University of Chicago in 1978, I knew that the City of Chicago and its University were both far too professional to allow an easy outlet for my urban interests. After I came to Chicago, I devoted myself single-mindedly to teaching and research in the physical and mathematical sciences. But some residue of my social interests remained. Within the framework provided by an occasional column in *Physics Today* I tried to comment upon the social and economic context in which physicists work²⁴. Three more columns are included at the end of this Section on the urban scene and public policy. These columns are indeed about the relation between the physics community and public policy. I enjoy writing the columns, and I even think that many people enjoy reading them. But Altshuler's criticism of the urban planning specialist can equally well be extended to most columnists. The question to ask is: "By what process have you become qualified to offer us advice?"

D. Turbulence and Chaos Questions without Answers

In the last dozen or so years, my interests have turned to yet another field of science: the description of complexity. My own view of this

²³Alan Altshuler, *The Urban Planning Process*, Cornell University Press, Ithica (1965).

²⁴ The reader has probably noted that I have also taken advantage of these introductions to pontificate in this direction.

field starts with a major intellectual problem: We know that the laws of physics are rather simple in structure. Newton's laws or the Schrodinger equation or even string theory is described by a rather simple system of equations. One's expectation might be that such simplicity in formulation should lead to simplicity in outcome. However, all our experience in life contradicts any expectation of simple outcomes. The world is wondrously complicated and bewilderingly diverse. How can it be that from simple beginnings one gets complex endings.

This problem is not illuminated by much of the traditional practice of physics. Most systems picked for study by physicists are picked precisely because they have simple outcomes. Kepler's orbits or the quantum harmonic oscillator or the simple pendulum have been extensively studied and are used as examples just because they have simply predictable outcomes. Even the many-body systems studied in critical phenomena have simply predictable outcomes. But the world tends not to be predictable in the same way. Once again our life experience suggests opposite outcomes from those of the exemplary physical systems. A pot of boiling water, or a double pendulum, or any person will not behave in expectable or predictable ways. Why should our professional experience with physics belie our civilian experience as human observers of the world.

But the conundrum is worse yet. Most of the systems traditionally studied by physicists remain equally complicated throughout their history. A microwave cavity, any pendulum, or any electrical circuit with passive linear elements does not gain extra complexity as time goes on. However, if you take a hot mass of rock of an appropriate size and throw it into orbit around the sun, you might observe it to gain all kinds of organizing features. Oceans and continents will develop and move around. Mountain ranges will grow and decay. Great deserts will spread out and then be covered with ice. Little cells of organization will emerge, become more complex, and form into trees and us. Is all this development of complexity and organization outside the laws of physics and of science?

Our prejudice as scientists is to say *no! Physics encompasses all*. But our job as scientists is to see and understand. How is it that simple laws can give complex outcomes? How can organization develop from blind law and naked chance? A major thread of development in the modern physical, mathematical, and biological sciences has been devoted to answering this class of questions. My own work on this area has been modest in outcome, but quite exciting and meaningful

to me. These two last sections of the volume are devoted to work on the borders of this great subject.

For me, the story starts in about 1982. I am working at the University of Chicago, doing phase transitions and critical phenomena, and being somewhat dissatisfied with my work. In the great days of the 1960s I was all excited with this subject, and its creative possibilities. Later on, I got considerable joy in seeing what Fisher, Wilson, Wegner, Nelson, Polyakov, and many others had created based upon the earlier work. But, by this time the freshest joy of discovery, either directly or through the work of others, has begun to go out of the field. Then, at a crucial moment, Bob Gomer, a colleague in Chemistry, asks why I am devoting so much work to a particular model system. His implication is that the model is not so real as to be of practical interest, and perhaps not so deep as to have real intellectual interest. My reply seems superficially convincing, but I know that his implied criticism is right. So, I resolve to learn something new.

The new subject I find is *dynamical systems theory* the study of the time development of relatively simple systems. There are portions of the subject which are absolutely beautiful closed topics, not as deep as critical phenomena but roughly similar in structure. Two of these beautiful topics are *iterated one-dimensional maps* and the *onset of chaos*. The first is about problems in which a system is described by a single variable, say its degree of excitation, and jumps from one value of the excitation to another via discrete time steps. The second is about a kind of continuous transition which might occur when a system first shows chaotic behavior. In 1982 or so, my students Albert Zisook, Michael Widom, Scott C. Shenker and I set out to learn about dynamical systems theory. Our work is was filled with great thrills of invention and discovery and then the partial disappointments of finding that our creations had been preempted by previous workers. Some of the structure of this subject is reviewed in papers D1 and D2.

Eventually, we found our own area which could serve as an entry to this research field. We built upon the work of Mitchell Feigenbaum, who had first understood the onset of chaos via a renormalization group calculation. His work was built upon the tools I had previously known: scaling, universality, and renormalization. When we began, there were two kinds of onsets which had been studied both of which involved a period doubling route to chaos. We looked to other routes

and learned about them through the development of renormalization calculations. Some treatment of this subject can be found in paper D3.

Thus, our strategy was to enter this new field by considering systems which produces time-sequences of data-- like annual populations, or economic outputs, or daily stock prices-- and to see how such a sequence may develop chaotic attributes. These systems were picked to be just complicated enough so that we could fully encompass them with the computers we then had available. (Here was the point at which I would see the computer knowledge gained by studying urban problems pay off for me in genuine, hard science.) Papers D3 and D4 describe the results of this kind of analysis. All the examples studied are just simple mathematical toy systems picked to be analyzed with computer and then with techniques like the renormalization group. In this way, we began to understand the development of complexity in toy examples.

However, these examples also proved to be unsatisfactory in the long run. They were both too simple and too complicated. At onset of chaos they were simple. In their chaotic regimes they could show a behavior which, when studied in detail, was bewildering diverse and did not seem to lead to useful and interesting general principles. We were stuck once again.

The best source of theoretical inspiration is the real world, and the best window to that world is provided by experiments. The systems studied by experimentalists have a much better chance of organizing themselves than computer toys. The real world has much, much more time to organize than our toy examples. A computer might analyze 10^8 or so very simple events per second. (In looking at our models, we looked at much fewer events, perhaps a few thousand time steps in every second of computer analysis.) However, a real system--say a cubic centimeter of gas--will have 10^{32} or so collision events per second. Naturally, the real system will have a greater opportunity to 'get organized'. Conversely, our computer models will have to be picked with very great care if they are to give any inkling of true natural phenomena.

However, with luck or skill, one can relate model systems to real-work outcomes. One very pleasing example of how to do so is given in paper D5, authored by Jensen, Libchaber, Procaccia, and Stavans. This work is devoted to showing how a real system, a pot of liquid mercury heated from below, first develops chaos. The onset in this real system follows one of the universal routes to chaos, the so called

quasiperiodic route, which was extensively studied by the Chicago theoretical group. This paper itself is mostly experimental in content. The system studied is the brainchild of Albert Libchaber, its actualization is due in large part to Joel Stavans. Libchaber understood how to adjust the system to push it into the domain investigated by the theorists. And pleasingly enough, after adjustment the system behaved in exactly the fashion predicted by theory.

It is important to recognize that our understanding of the physical world is due in very large measure to the experimental scientist. Pure thought gets us only so far. The worlds which the theorist may construct are only powerful and rich because they are modeled on the real worlds that nature produces. And our only access to such real worlds is through the studies of the experimental scientist. Without experimental check, the universality of the onset of chaos is a theorists' possibility. The experiment shows that it is a reality.

In addition to illustrating the craft of the experimentalist, this paper also has a pleasing theoretical content. According to the renormalization theory, near onset dynamical systems produce a characteristic structure in which very delicate and small structures are produced of excitation versus time. The incipient chaos is, in fact, built into structural details which persist to arbitrary small scales. The word for such a delicate and scale invariant geometrical structure is a *fractal*. This word was coined by Benoit Mandelbrot, who brilliantly argued that such structures were rather pervasive in nature, and then introduced many clever and incisive techniques for producing and analyzing them. Our description of the experiment was based upon a study of the properties of the fractal produced at onset in this hydrodynamic system. This in turn pointed back to a previous paper on the nature of fractals of this type, D6 and to my concern about the proper use of the fractal concept. (See D7). (See D8 for a summary of some research on multifractals.)

Paper D6 is in some ways an embarrassment to me. It is an excellent expository work which in fact helped make a particular form of analysis of fractal behavior very fashionable. The form of analysis is called multifractal or multiscaling analysis and follows from the work of Mandelbrot²⁵, Hentschel and Procaccia²⁶ and others. The analysis

²⁵B. Mandelbrot J. Fluid Mech **62** 331 (1974).

²⁶ H.G.E. Hentschel and I. Procaccia, Physica **8D**, 435 (1983).

closest to the one in our paper was done by Georgio Parisi and Uriel Frisch, published in a slightly obscure place²⁷, but explained to me in some detail by Parisi. Unfortunately I either forgot or did not understand his explanation, so that we did not appropriately cite this earlier work in ours. Fortunately, an erratum²⁸ and a bit of publicity²⁹ set the record straight.

I was involved in a few more research projects in dynamical systems theory, one with Charles Amick, Emily Ching and Vered Rom-Kedar, another with Mahito Kohmoto and Chao Tang (see #91), and still another with Oreste Piro and Mario Feingold (see # 127). This work was pleasing, but we were never again able to use dynamical systems theory to achieve the generality or experimental relevance of the work on the mercury cell. We were stuck once more.

But we did follow another theoretically motivated line of research, based upon simple mathematical models of extended physical systems. In 1981, Thomas Witten and Leonard Sander³⁰ developed a models of the dynamical aggregation of cluster called DLA. This resulting clusters exhibited a kind of universality along with scaling and fractal behavior. At Chicago, we followed up on these ideas and applied them to other model systems. ³¹

But, then we turned back to experimental systems. With David Bensimon, who was my student, Boris Shraiman, a postdoc, and Albert Libchaber I started looking at fluid flow, which can produce a genuine and rich complexity in both space and time. The example they suggested is the flow of two liquids confined in a very narrow space between two glass plates. This setup is called a Hele-Shaw cell. It is capable of producing both a very simple behavior and also a rich complexity. A portion of the theory of such systems is outlined in the review paper D9 in this section. This paper was followed by

²⁷Georgio Parisi and Uriel Frisch On Turbulence and Predictability in Geophysical Fluid Dynamics and Climate Dynamics M. Ghil, R. Benzi, and G. Parisi, editors (North Holland, Amsterdam ,1985).

²⁸T.C. Halsey, M.H. Jensen, L.P. Kadanoff, I. Procaccia, B. Shraiman, Phys. Rev A**34** 1601 E (1986).

²⁹ Barbara G. Levi, Physics Today April 1986 p. 17.

³⁰Thomas Witten and Leonard Sander, Phys. Rev. Letters, **47**, 1400 (1981).

³¹ Some of the original work can be found in the publication list. See,#89 (Alexander, Domany, LPK, and Bensimon) and #102 (LPK and Liang) as well as #98 (Bensimon, Shraiman, and LPK).

many others³² as we once again constructed theory in response to the experiments of the Libchaber laboratory.

Another closely related experiment is discussed in paper D10.

Paper D11 describes how concepts drawn from dynamical systems and other chaotic situations can be come part of undergraduate teaching. I have spent a very considerable period of time designing courses which use computers to teach about chaos. This teaching is a fitting and satisfactory outcome for the research directed at chaos and dynamical systems.

E. Complex Patterns

Section introduction: From Correlation to Complexity.

In the previous section, I discussed how apparently complex patterns could be produced by dynamical systems. My own study of complexity has been going through a kind of transition in recent years. I have begun to realize that the dynamical systems usually studied often show only a very limited and partial complexity. Often these systems produce complex time patterns, but the complexity is limited to one or a few chaotic functions of time. The true richness of natural systems go well beyond few-variable chaos. This richness has been emphasized by the people at the Santa Fe Institute, and most particular Murry Gell-Mann³³ and Stuart Kauffman³⁴. Specifically, Stuart argued that a sufficiently complex dynamical system would naturally develop a rapidly increasing complexity. Structures would emerge with rapidly proliferating complexity. Details of history would determine exactly which structures were produced. However--in this view--the production of structure was an inevitable occurrence in a system with a sufficient number of variables and interconnections among them.

³² This work eventually grew into a very big activity, finally involving extensive interactions with people in the Math Department at Chicago. So far, I had the pleasure of collaborating with Bensimon, Shraiman, Libchaber, Giovanni Zocchi, Bruce Shaw, Wei-shen Dai, Su-min Zhou, Peter Constantin, Todd Dupont, Ray Goldstein, Michael Shelley, and Andrea Bertozzi in papers on the Hele Shaw cell.

³³ M. Gell-Mann. *The Quark and the Jaguar*, W.H. Freeman, New York, 1994.

³⁴ Stuart Kauffman, *At Home in the Universe*, Oxford University Press, New York, 1995. Stuart Kauffman, *The Origins of Order* Oxford University Press, New York, 1993.

Real extended systems encourage complexity. Most truly interesting dynamical systems (you and me for example) involve dynamics in both space and time. The spatial dimension permits a structured complexity of two different sorts. First there is translational invariance. This invariance permits structures to repeat again and again at different point in space. But in a chaotic system, nothing quite repeats--instead it is reproduced with different variations each time. For example, all the people in the world are in one sense similar structures, but they have interesting and important individuality that in the end, make life interesting. Another aspect of this structured complexity is that it exists on all length scales. There are people, and organ systems, and tissues, and individual cells, and the working parts of cells, and individual molecules. Each of these different scales shows its own, unique, organized behavior--Each scale's behavior is repetitive and complex and history dependent.

Recently, condensed matter physics (following biology and many other disciplines) has moved into the study of such multi-structured complexity.

As part of one kind of study of this subject, I have been working on turbulence. In my whole career, and particularly at Chicago, I have been blessed by many coworkers who have led and immeasurably enriched various aspects of my research. Theoretical visitors, especially Procaccia, and a whole rich collection of graduate students and postdocs have made my research what it is. But, probably, I learned most from my collaboration with Albert Libchaber.

For several years, Libchaber and I worked as a team in studying complexity. He led the way in visualizing and realizing complexity, I tried to apply theoretical tools to understanding what he had done. We studied three systems together. I have already mentioned the onset of chaos in the mercury cell and the Hele Shaw system. But, the richest case which we studied together was convective turbulence. Turbulence is chaos in both space and time. When a relatively large volume of fluid is heated from below, the fluid on the bottom expands and tends to rise. Thus the fluid is set into motion. At high heating rates the motion becomes unstable and turbulence results. This kind of system is called a Rayleigh-Bénard cell. Our work is summarized in E1, E2 and E3.. My work in this area has been the product of collaboration with many people including Bernard Casting, G. Gunaratne, F. Heslot, S. Thomae, Xiao-Zhong Wu, S. Zaleski, and S. Zanetti, Anton Kast, and Masaki Sano. An additional study was done by Dan Rothman and myself as paper E4. This paper involved using

a very simple kind of computer model, called a cellular automaton, to produce the kinds of swirling motion seen in the Rayleigh-Bénard cell. It was a very pleasant kind of *tour de force* and produced lovely pictures and patterns.

This turbulence work is and was a dream shared by Libchaber and myself. We hoped we could and would really understand how a hydrodynamic system developed a very rich complexity. Something about this dream came to reality. Our work did expose many aspects of the behavior in the cell, including many aspects of the complicated geometry produced by the motion. (The cover of this book shows four different pictures of this geometrical structure.) The experimental work cast into doubt the general applicability of the Kolmogorov theory to systems of this kind. This theory involves a cascade of energy from larger scales to smaller ones. The cascade produces some kind of fractal structure. But then, as always, the work reached a barrier. We had gotten all the knowledge we could from the experiments on convective turbulence, and it was time to move on. Albert and I turned to different approaches to complexity, he in biological systems, and me through the mathematical sciences.

In papers E5 and E6, I turned away from real turbulence and instead studied a simplified model of the cascade process built into the Kolmogorov theory. This work involved a system simpler than fluids, but one which nevertheless showed a rich structure on a wide range of length scales. Of course, because this model does not begin to show the richest kind of complexity and structure, it is in the end slightly unsatisfactory. (Please see #168, #170, and #171 for more detailed writeups of my collaborations on this subject with Roberto Benzi, Detlef Lohse, Jane Wang, and Norbert Schörghofer.)

One kind of structure repeated again and again in hydrodynamic systems arise from the propensity of these systems to produce mathematical singularities. Papers E7, E8, and E9 describe the universal structures which arise from this kind of singularity mechanism. But once again one gets nothing like the complexity shown by the natural world, so the analysis feels incomplete and somewhat unsatisfactory. At least this is somewhat the feeling I get from rereading the papers. When the chase is on, investigations like these are always great fun and wonderfully satisfying. There is a neatness and completeness in the classification of the universal properties of relatively simple similarity solutions, which stands in contrast to the complexity and messiness of the real world.)

Systems in Statistical Equilibrium can only show a small amount of complexity. This limitation on complexity is built into the equilibrium statistical mechanics of Boltzmann, Gibbs, and Maxwell. Their statistical mechanics formalism is one in which the probability of a given configuration is proportional to an exponential of the Hamiltonian divided by minus the temperature times the Boltzmann constant. Such a weight prevents the most complex configurations from dominating the system. However, if a system is not in statistical equilibrium, this exponential weight no longer holds. Something much more complex may emerge. The next series of papers, is based upon particles which cannot go to true equilibrium because their interparticle collisions do not conserve energy. Among the physical systems which show these properties are glass balls or ordinary sand. We use the phrase 'granular material' to describe these systems. Papers E10, E11, and E12 are all aimed at elucidating the properties of these theoretical models of 'sand' in the simplest possible geometries. In fact, we observe that the granular material does have a far richer and more structured behavior than the usual equilibrium systems. Their main characteristic is that portions of them may freeze or slow down into a glassy state. This slowing or freezing then serves as a memory of the history of the system. Sand castles are examples of such memory.

Memory is the subject of the next paper, E13, which aims at explaining how non-equilibrium phenomena can serve as part of a solid state memory device. None of these works actually reach to the levels of complexity familiar from everyday life, but they are all essays in the right direction.

Scientific work on turbulence and complexity is far from completed. Science cannot yet answer, or even properly formulate, questions related to how complexity does actually arise in the world. I started saying that computers can do 10^8 or so calculations per second while a bit of gas sees 10^{32} events/second. But biological systems are even more richly complex. In the course of evolution there might have been 10^{55} or so collisions among atoms in biological systems. This provides lots and lots of steps in which different levels of complexity might have developed. We certainly do not understand the development of complexity. In fact, we do not even understand how the patterned formed by the clumping of matter as it is pushed

around by the motion of a fluid.³⁵ But, unanswered questions and partially formulated problems are the bread and butter of science. I am pleased to see that my table is still full.

³⁵This has been a subject of study for me in recent years with my principal collaborators being Mario Feingold, Oreste Piro, and Peter Constantin, Itamar Procaccia, and Emily Ching.
