Theoretical Paradigms for the Sciences of Complexity

Some Ideas on the Aesthetics of Science

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I may not be a very appropriate representative for the subject of Materials Science here in a conference focusing on technology and the applications of science to human problems . I am not, strictly speaking, a materials scientist in the narrow sence of these words, and much as I admire and applaud the applications of sciense in technology, that is not what I do. I am a theoretical physicist much of whose work has involved trying to understand the behavior of more or less complex materials such as metals, magnets, superconductors, superfluids, and the like. I thought that perhaps you would enjoy hearing, in the brief time I have here, not about these investigations or about wonderful materials of the future —— as far as I am concerned, from an intellectual point of view, the very impractical and obscure low-temperature phases of the mass-3 isotope of helium are at least as fascinating materials as anything the future is likely to bring —— but rather about some of the wider implications of the kind of thing I do. In paticular, I have been an active participant for several years in an enterprise called the Santa Fe Institute whose charter involves action in two main directions;

(1) We believe that the growth points of science lie primarily in the gaps between the sciences, so that we believe in fostering cross-disciplinary research in growth areas which are not well served by the conventional structure of the universities

or the funding agencies, I use the word cross-disciplinary to emphasize that we are not trying to create new disciplines (like materials science or biomolecular engineering), which often rigidify into new, even narrower intellectual straight-jackets, but that we approach problems by cross-coupling between scientists well grounded in their disciplines but thinking about problems outside or between them —— as has often been fertile in materials science, with the coupling of good physicists, good chemists and good engineers.

(2) We believe that there are many common themes in the study of complex systems wherever they occur, from the relatively simple ones which I have encountered in solid state physics or in astrophysical situations, through complex non-linear dynamical systems such as one encounters in hydrodynamics, through bioliogical organization, to complex biological regulatory systems such as the immune system or the nervous system, and on into population biology, ecology, and into human interactions in, for instance, economic systems. Much of our work —— not all of it, to be sure —— fits under the general rubric of the study of Complex Adaptive Systems; systems which by virtue of their complexity are capable of adapting to the world around them.

It would carry me too far afield to describe all of our activities in Santa Fe; one, for instance, which I have enjoyed very much is a program mixing physical scientists such as myself with a group of theoretical economists in the hope of inventing new directions for the science of economics. Rather, I would like to describe a few of the paradigms for dealing with complex systems, in general, which have come from, or are related to, my science of condensed matter physics and which seem to be generalizable to a great many other types of systems. Let me list three of them here and try to describe each in a few words, relate it to the appropriate part of condensed matter theory, and then show how the idea may be generalized.

(1) The Emergent Property of Broken Symmetry.
(2) The Paradigm of the "Rugged Landscape"
(3) Scale-Free Behavior: Critical Points, Fractals, and $1/f$ noise.

There are several other paradigms —— e.g., hierarchical organization, pattern selection, marginal stability, classifier algorithms among others; but surely this is enough ideas for one short talk.

(1) Broken symmetry is actually the basic underlying concept of solid state physics. It seems at first simple and obvious that atoms will want to stack themselves into regular arrays in three dimensions, like cannonballs. Thus one does not recognize that the formation of a crystal lattice is the most-studied and

perhaps simplest example of what we call an "emergent property": a propety which is manifested only by a sufficiently large and complex system by virtue of that size and complexity. The particles,(electrons and nuclei) of which a crystal lattice is made do not have rigidity, regularity, elasticity —— all the characteristic properties of the solid : these are actually only manifest when we get "enough"particles together and cool them to a "low enough" temperature. In fact, there are kinds of particles —— atoms of either isotope of helium, or electrons in a metal, for instance —— which simply do not normally stack at all and remain fluid right down to absolute zero. This illustrates one of the most important facts about broken symmetry; quantum-mechanical as well as thermal fluctuations are inimical to it.

Why do we call the beautifully symmetric crystalline state "broken" symmetry ? Because, symmetrical as it is, the crystal has less symmetry than the atoms of the fluid from which it crystallized: these are, in the ideal case, featureless balls which translate and rotate in any direction, while the crystal has no continuous rotaion or translation symmetry.

Mathematically, the properties of the crystal are only to be derived in the so-called "thermodynamic "or "$N \to \infty$" limit of every large system. Of course, for many purposes a very small cluster of atoms, of the order of a few thousand, can behave in somewhat crystalline ways, but the structure at a finite crystal is not really stable against thermal or quantum fluctuations. Thus the characteristic crystalline properties of rigidity, elasticity,(as opposed to the shear flow of a viscous fluid) and anisotropy(as e.g., birefringence) are true emergent properties, properties which are only properties of the large and complex systems.

It turns out that many, if not most, of the interesting properties of condensed matter systems are emergent broken symmetry effects. Magnetism is a well-known example; so is superconductivity of metals and the very similar superfluidity of the two forms of helium and of neutrons in a neutron star. The anisotropic properties of liquid crystals, useful in calculator displays, are yet another fascinating example.

Broken symmetry is encountered in several other contexts. One important one is in the theory of the "Big Bang" during which, it is proposed, one or more broken symmetry transitions took place in the state of the vacuum, changing the nature and number of elementary particles available at each one, greatly modifying the energetic of these primeval events, and leaving behind one or more forms of debris —— the fashionable one these days being "cosmic strings". The early history of broken symmetry in the vacuum was dominated by the Japanese-American physicist Yoichiro Nambu.

Some scientists have proposed that driven dynamical systems can exhibit broken symmetry effects; I find the analogy between the emergent behavior of equilibrium and of non-equilibrium system less than compelling. Broken symmetry does not generalize in any straightforward way to form a model for the origin of life, for instance; it stands, rather, as an explicit proof of the existence of emergence.

(2) The paradigm of the "rugged landscape" was discussed in connection with another condensed matter physics problem, the rather obscure phenomenon known as"spin glass". It is almost unnecessary to go into the long and controversial history of the spin glass problem itself, exept that it involves the possibility of a phase transition at which the spins in a random magnetic alloy "freeze" into some random configuration. The attempt was initially to find a simple model for the still mysterious behavior of ordinary glass when freezing into a solid-like but disordered state; but it turned out the disordered magnetic alloy had its own different and complex behavior.

The model one uses abstracts the movable spins in the magnetic alloy as having two possible states, up(+) or down(-), like the 0 or 1 of a binary bit. Each spin S_i is presumed to interact in a random fashion "J_{ij}" with many other spins, causing a "frustrated" Hamiltonian

$$\mathcal{H}=\Sigma J_{ij}S_iS_j$$

in wich there are many conflicting terms to optimize and it is not easy to visualize the lowest energy state. J_{ij} is a random variable, equally often positive or negative. It is the values of the energy $\mathcal{H}$-plotted in the multidimentional "configuration space" of the state variable $\{S_i\}$ —— which constitutes the "rugged landscape". The task of finding a low-energy state is one of seeking for deep valleys in this "rugged landscape", but it can be proved that this task is computationally very difficult, because one gets stuck in one of the many different local minima with no hint as to where to go to find a better state.

In fact, this problem is a prime example of one of the most important classifications of computational complexity, the "NP complete" case. It has already suggested an important new algorithm for solving complex "combinatoric optimization" problems which arise in many emergency situations such as complex chip design: the method of simulated annealing.

Combinatorial optimization problems in the presence of conflicting goals are very common in everyday life: almost every personal or business decision, from ordering from a menu to siting a new factory, is of this nature: so of course information on the basic nature of these problems is of great value. Unique to our approach is the recognition of the "freezing" phenomena, the possibility of being stuck indefinitely in a less than optimum solution.

Two places where the rugged landscape point of view is catching on are in evolutionary biology and in the theory of neural networks — "generalized brains". S.Kaufmann has particularly emphasized the "rugged landscape" approach to problems of molecular evolution, both in the original origins of life and in proposing systems of "directed evolution" to produce organisms with particular traits. In the evolutionary analogy, which was poineered by Stein and myself, and picked up by S.Kaufmann, the genetic material is the state-vector $\{S_i\}$ in a multidimensional configuration space. A very important point is that given the "freezing" phenomenon, it may be better to improve by complexifying — adding dimensions to the configuration space — than by optimization within one's obvious capabilities. The reanimation of neural network theory which has recently occurred due to Hopfield's introduction of spin glass like ideas is both so well known and so far afield from my subject that I do not want to go into it more deeply than that.

(3) Scale-free phenomena. Here is a paradigm which has had two joint inputs, initially far apart but growing closer with time.

The first is from mathematics: the fundamental ideas coming from a number of mathematicians such as Hausdorff but the applicability to real world situations (which, to a natural scientist, is far more important) being discovered by Mandelbrot. As Mandelbrot points out, there are many real world objects which have the property of non-trivial scale invariance — properly called anomalous dimensionality, but which he calls fractality. Such objects look the same geometrically no matter what scale they are observed at; in general that is true only in a statistical sense. For instance, he shows that many coastlines have the same geometry at any scale; the same is true of clouds and of many mountain landscapes. This alone is not enough — after all, a simple continuum in any dimension is scale-invariant. The second property is that the size of the object vary with scale in a non-integer way. For instance, he shows that the length of coastlines depends on the length ℓ of the ruler used to measure them as $L \propto \ell^{-p}$ where $P \sim .2$. Mandelbrot gives many beautiful examples, in his books, of fractal objects, and others have discovered additional ones — for instance, the shape of the breakdown paths in a dielectric subjected to too high a voltage, which is an example of Diffusion-Limited Aggregation (DLA), a process of pattern growth common to many systems, and discussed by T. Witten and others. Another important case is the "strange attractor" observed in chaotic, low-dimensional dynamic systems as discussed by Ruelle and co-workers. But in general Mandelbrot has not approached — at least successfully — the question of why fractals are so common and so important in nature.

A second independent observation of scale-invariance is due to condensed matter theorists, specifically Kadanoff, Widom, and Wilson, in the study of "critical points" of phase transition between different thermodynamic phases, such as liquid-gas critical points, superfluid-normal fluid transitions, magnetic critical points, etc. It came to be realized that, at these critical points, again the structure of the substance — in terms of the fluctuations back and forth between liquid and gas — is scale-invariant in the same sense: there are "droplets" of absolutely all sizes from one molecule to comparable to the size of the entire sample. This is the basic nature of critical behavior and the famous "critical fluctuations" which may be beautifully demonstrated experimentally.

It turned out that some of Mandelbrot's fractals are formally equivalent to critical points — e.g., the DLA system, which leads to a critically percolating cluster. It is the recent suggestion of Per Bak, that a great many examples of fractals in nature are systems at or near a critical point, in that he feels that many kinds of systems, when driven hard enough, will maintain themselves at a critical point. He calls this phenomenon "self-organized criticality" and the idea is the center of considerable controversy but also great interest. In particular, Bak points out that many systems in nature exhibit a kind of random fluctuation or "noise" which can also be thought of as scale-free but in time, not space — the ubiquitous "$1/f$ noise" of many different kinds of systems. (Actually, in general $1/f^{1\pm p}$ when p is a small number $\sim .1$-$.2$.) This kind of noise is technologically very important. Going from the practical and turning to the gigantic, others have proposed that the large-scale structure of the universe may have fractality over some range.

In conclusion, then, let me summarize the point of view I — we, if I may include others at SFI — am taking towards the study of complexity. From the beginning of thought, the system of Pythagoras, the medieval scholasticists, Descartes and his universality of mechanism (and from him stem the ideas of many modern particle physicists, for instance) — it has been a temptation to try to create a universal system — a Theory of Everything. It is precisely in the opposite direction that we search — we try to look at the world and let it tell us what kinds of things it is capable of doing. How, actually, do complex systems behave, and what do these behaviors have in common? The search for the universal must start from the particular.

Some Ideas on the Aesthetics of Science

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The educated layman is used to thinking of science as having aesthetic values in two senses. Often he can recognize the grandeur and sweep of the scientific vision: the cosmological overview of the universe, the long climb of evolution towards complexity, the slow crunch of the tectonic plates, the delicately concentrated energy of the massive accelerator. Also, many visual images from science have aesthetic meaning: images of galaxies, of the complex structures of crystals or of the double helix, the fascinating diversity of organisms and their traces in Nature. What I want to discuss here, however, is the internal, intellectual aesthetic of science, which is often what the scientist himself alludes to when he calls a certain piece of science "sweet" or "beautiful". This is very often a comprehension of internal intellectual connections among diverse phenomena or even fields of science — that the same intellectual structure, for instance, may govern the formation of elementary particles and the flow of electricity in a superconducting wire; another may relate a complex magnetic alloy with the functioning of neuronal circuits. In summary, I will try to describe what the scientist (or, at least, one scientist) finds beautiful in science.

During the debate over the Hydrogen Bomb in the early 1950's which eventually led to J.R.Oppenheimer's downfall, he opposed Teller's "crash" efforts to design such a bomb on some combination of technical and moral grounds. But when Stan Ulam, working with Teller, proposed a new configuration, Oppenheimer seems to have withdrawn his opposition, remarking that the new design was "so technically sweet" — *i.e.,* so "beautiful", that it had to be done. This is only a widely publicized incident involving scientists making essentially aesthetic judgments and allowing them to influence their actions ; I happen to feel that it is a disgraceful one, but that is beside the point here. All scientists, I think, who are worthy of their calling, have some aesthetic feeling about it, specifically about what is beautiful science and what is not.

It is this aesthetic component of science which I want to discuss here. I am sure that I shall tread on many toes, nor am I absolutely sure that I have got it right in any case; in fact, I would feel that I have done my job if I simply succeed in opening a discussion. In aesthetic matters there is a widespread prejudice summarized in the saying "each to his own taste", but, in fact, I happen to feel there are real criteria both in the arts and in science.

Let me first dispose of some common layman's misconceptions. The most common would surely be that science is not only value-free but without scope for imagination and creativity. It is seen as the application of a systematic "scientific method" involving wearing a white coat and being dull. I feel that too many young people come into science with this view, and that too many fields degenerate into the kind of work which results: automatic crank-turning and data-collecting of the sort which Kuhn calls "normal science" and Rutherford "stamp-collecting". In fact, the creation of new science is a creative act, literally, and people who are not creative are not very good at it. (Equally, one often finds people miscast in scientific careers who do not realize that the second most important skill is communication: this seems to be a special problem for Japanese scientists. Science is the discovery and communication of new knowledge.)

A second layman's problem is the attempt to project his own aesthetic system into science. I have, several times, been asked by artists, for instance, about striking images which can be made from scientific objects, and, of course, in popularizing science every TV program is eagerly hunting for this kind of thing. Science itself contains a fifth column of practitioners — often otherwise respectable — who like to create pretty images, sometimes by computer tricks, or to emphasize the grandeur of the scientific vista by playing games with large numbers. To play pretty games or to inspire awe with large or small magnitudes is perhaps a legitimate, if tricky, way to enhance popular support for science (but what happens to the equally important but unglamorizable subjects?) but it has little or nothing to do with science itself. It is true that different fields of science attract people who are, to some extent, swayed by subject matter: astronomers do like to look at the stars and contemplate deep space, biologists often seem to enjoy the diversity of forms of life, elementary particle physicists are convinced they alone are plumbing the "really" fundamental, etc. But within each science, and across the spectrum of the sciences, it is still possible to distinguish the "sweet" from the ugly.

A third misconception is promulgated by certain sociologists of science, who seem to feel that science is a purely sociological phenomenon, with no intrinsic truth value at all: that scientists' aesthetic and cultural prejudices create the form which science takes, which is otherwise arbitrary. This is mainly refuted by the fact that science works in a real sense: it grows exponentially because it is useful and effective, which means that it produces, one way or another, a true picture of the real world. These sociologists have studied science being done, which is, of course, a confusing set of interactions among highly fallible people with strong prejudices; but they have not enough insight into the subject matter or into the qualitative differences among fields and among people to recognize the rapid disappearance of the shoddy or dishonest result. It is significant that the average scientific paper is cited less than once in the literature, while some are cited thousands of times: some are right, some are wrong, most are meaningless. To the sociologist of science, observing from the outside, the uncited paper and the "classic" appear equally significant. As we will see, fortunately, the "aesthetic" aspect of science has much to do with values which are also related to its validity and truth, so I am not saying that aesthetics leads scientists to distort the meaning of their work.

I do not deny the regrettable fact that some scientific fields do become detached from the values of the rest of science and lose sight of certain basic reality principles: we have, in the past few months, seen an example of precisely this problem in the field of electrochemistry, which I am told is one of these. But the advancing edge of sciences adhere to unavoidable reality principles.

Having disposed of the negative, let us ask: can we find a theory of aesthetic value which is at all common between the arts as normally understood, and the sciences ? The arts, of course, have their equivalent of the facile games I referred to in the sciences: sentimental verse, picture postcard art; there is, of course, a great body of aesthetic theory on which I am certainly not an expert ; but I have over a number of years, picked out a number of statements which I think are significant.

In sculpture and painting, the critic Berenson has made much of what he calls "Tactile Values", which seems to mean giving the viewer a sense of personal involvement in the action or motion or scene depicted. A similar, if quite different, statement by a sculptress friend once impressed me strongly: she felt that all successful sculpture, no matter how abstract, referred back to the human body. Finally, also in the visual arts, the use of iconography and symbol is a common bond between ultramodern painters such as Jasper Johns and Frank Stella, and classic painters and sculptors, especially religious art but also classical oriental painting. In the modern paintings, the iconography is self-created by repetition of certain motifs, but it is firmly there. All of these kinds of remarks bring out two theses which I want to put forward and test

(1) That even in abstract art there must be a "content" or "substrate" to which the viewer is expected to relate. Nothing serious is beautiful in a vacuum; in fact, this is thought now to be a property of the human mind: that it can not think, can not perceive, can not communicate except about something: the mere act of communication requires context.

(2) To be beautiful, a piece of art should have more to it than surface content. It should be enriched by more than one layer of meaning. This brings me to a

theory of aesthetics in literature and poetry which very much intrigued me, the ideas of T.S.Eliot and the Cambridge school of critics such as David Daiches. Eliot uses the word "ambiguity" to express himself, perhaps a misleading use of this word which often, in English, means "fuzziness" or lack of clarity; whereas Eliot was always absolutely certain of what he meant. What he really meant was that good poetry should have as many levels of meaning packed into the same words as possible. In his poem "The Wasteland" for instance, there are characters carrying out certain actions on the surface, which is at least clear enough that much of the poem may be read directly as a series of stories. There is also a surface level of absolutely gorgeous use of language. There is, underneath those two levels, a sense of despair at the moral emptiness of the modern world of the time; and still under that, if we read quite carefully, there are a series of references to myth, especially the Grail legends and those involving the Fisher King. On a more obvious level, his play "The Cocktail Party" has quite obvious Christian symbolism superposed on an apparently clever, brittle drawing-room comedy. But in this, in Japan, I am probably telling you nothing new: in the land of the Haiku, the delicate use of ambiguity and cross-reference needs no explanation.

Leonard Bernstein's Harvard lectures give some beautiful examples of this kind of cross-reference or multiple meaning in, especially, Stravinsky's music; I am not an expert on music and can give you no further examples. But a kind of music I do know well, classic American Jazz, is again a case of multiple-layered meaning and multiple reference. Characteristically, the surface meaning of jazz is a sentimental love song or a naive hymn; this is then overlaid with an ironic twist which pokes fun at its sentimentality or simplicity, and possibly also emphasizes a less respectable meaning of the lyrics; and, finally, there is the contrapuntal improvisation which is a pure, rather abstract musical object, only weakly related to the original tune and often bringing in cross-references to other pieces of music: quotes from Souza marches, bugle calls, or even well-known classical pieces.

As far as I understand the concepts of structuralism and of deconstructionism, my point of view is diametrically opposite to these; I have a feeling that these ideas devalue art and, when applied to science, often have the same effect as the sociological relativism which I have already deplored.

Let me then set out the criteria for beautiful science which I am going to try to abstract from these ideas about beautiful art.

(1) Reality principle: The work must refer to the external world, not just to the

contents of the scientist's (artist's) mind. In this I make a real distinction between mathematics and science. Mathematics creates its own world, and because of the long history of mathematics there is a shared substrate of ideas within which cross-reference is possible. But I think any mathematician would agree that beauty in mathematics lies in tying together pre-existing material, rather than in meaninglessly arcane postulational systems.

On the other hand, natural science is the science of <u>nature</u>, not of imaginary worlds; I do not, for instance, feel that cellular automata are part of nature, so that study of their properties must be judged as mathematics, not as science.

I have, myself an aesthetic prejudice in favor of science which takes nature as she is, not that which studies artifacts made by the scientist himself such as gigantic accelerators or fusion machines. I accept that this is personal, not universal, and that clever technology can be beautiful to many people.

(2) Craftsmanship is always an element of beauty, in science as in art. The act of creation must be non-trivial and it must be done well. Much ultra-modern art fails on this score, as a visit to, say, the L.A.County museum can easily convince one. The lucky fellow who happens on a new substance or a new effect may win a prize, but we, as scientists, do not really value his contribution unless he

displays other characteristics: Edison, as scientist, is not a model we really admire. In science, however, one often finds that the discoverer does not necessarily craft his discovery optimally: BCS theory, for instance, was first expressed in its ugliest form, and only refined by Bogoliubov, Nambu and others into a thing of beauty. We accept this as the nature of the beast, and it is perhaps not unknown in art; for instance, the Dutch school discovered counterpoint, but Bach exploited its possibilities beyond their abilities.

(3) Next is the principle of maximal cross-reference, i.e. my "ambiguity" equivalent. This refers both to different levels of meaning and to breadth of reference in the real world. I will talk about examples later, but perhaps I can continue with the BCS theory as a relatively simple one. Once re-expressed in Bogoliubov-Nambu form, it became almost evident that BCS could be a model for a theory of elementary particles as well as of its "surface" meaning, theory of superconductivity. Once expanded by Gor'kov in Green's function form, it not only allowed many new insights into the phenomenology of superconductivity, but acquired a second meaning as not just a "model", parametrized theory but a "microscopic", computable theory. And, finally, Bohr, Mottelson and Pines extended the idea to nuclear matter, and Brueckner, Morel and myself to the anisotropic superfluid ^3He, bringing, in the end, two enormously fertile and

unexpected references into the picture.

Where does the beauty reside? Of course, not entirely in the original paper which solved the problem of superconductivity, although indeed that was a well-crafted, very exciting paper. Not in any single object or work: not even any historian of science will be capable of dissecting the entire web of connections brought forth by the phrase "BCS". Perhaps, in some abstract sense, in the citation network: who cites whose paper and why ? Science has the almost unique property of collectively building a beautiful edifice: perhaps the best analogue is a medieval cathedral like Ely or Chartres, or a great building like the Katsura detached palace and its garden, where many dedicated artists working with reference to each other's work jointly created a complex of beauty.

(4) I want finally to add one criterion which is surely needed in science and probably so in art: a paradoxical simplicity imposed on all the complexity. There is the famous story of Ezra Pound editing T.S.Eliot's "Wasteland": that he reduced its total length by nearly half, without changing any of the lines that he left in, and greatly improved the poem thereby.

In science, even more than in art, there is a necessity to achieve maximal simplicity, not just an aesthetic preference. The subjects with which we deal, and the overall bulk of scientific studies grow endlessly; if we are to comprehend in any real sense what is going on, we must generalize, abstract, and simplify. Together with the previous criterion, this amounts to a very basic dictum for good science, not just beautiful science. We must describe the maximum amount of information about the real world with the minimum of ideas and concepts. In a way, we can think of this as a variational problem in information space: to classify the maximum amount of data with the minimum of hypotheses. Of course, this is just "Occam's razor" of not unnecessarily multiplying hypotheses, which in fact has been given a mathematical formulation in modern computer learning theory by Baum and others. In this case, our aesthetic concept is severely practical as well.

Again returning to our canonical example of BCS theory, in its original formulation it was not at all clear what the minimal set of hypotheses was: whether the crucial feature was the energy gap, or the zero-momentum pairing idea, or what? With refinement, which came in response to the Russian work and to the Josephson effect, gradually we discarded details and recognized that the one core concept is macroscopic quantum coherence in the pair field, which when coupled with a fermi liquid description of the normal metal leads inevitably to one of the versions of BCS theory. The beauty of the theory lies in the immense variety and complexity of experimental fact which follows from these two concepts. But without the existence of all that variety of experimental fact, and of the painful, exhilarating process of connecting it in to the main mass, the concept alone seems to me to be a meaningless, relatively uninteresting mathematical game. It is in the interplay, the creative tension between theory and experiment, that the beauty of science lies.

Let me give a few examples of beautiful science to try to clarify my ideas further. To begin with, let me hop entirely outside my own specialty and recall an incident from a recent book by Francis Crick. He was describing a dinner meeting at which Jim Watson was to be the feature speaker, and he describes Jim being plied with sherry, wine and after-dinner port, and then struggling with a presentation of their joint work on the double helix. The practical details he got through, but when it came time to summarize the significance, he just pointed at the model and said — "It's so beautiful... so beautiful". And, as Crick says, it was. Why?

As a model of one of the true macromolecules of biology it did, of course, embody brilliant technical advances and insights, and in addition as a structure itself it contains the creative tension of simple repetition yet complex bonding.

But of course, he meant far more than that: that with the structure in hand, it was possible to first envisage that the detailed molecular mechanism of heredity, and of the genome determining the phenotype, could eventually be solved. At that point not much further had been solved — one was just at the stage of proving that the obvious mechanism of DNA replication on cell division was really taking place, by quantitative measurements of DNA amounts — but that the original piece of the puzzle lay there in that model was hardly to be doubted. Crick and Watson, to their credit, did see — and did, especially Crick, later participate in and formulate — the whole complex of ideas that was likely to arize from their work. Crick and Brunner called this the Central Dogma, and the role played by macroscopic quantum coherence in BCS theory is played by the Central Dogma in this theory.

The "Central Dogma" is, of course

(1) DNA → DNA
(2) DNA → mRNA transcription and
(3) mRNA → protein gene expression
(3) implies a code

Some of this was already known in a vague way: that genes determined protein sequence, for instance, so if the gene was DNA, DNA → protein was obvious — but was it? Crick points out that Watson and he were the first to make up the standard list of 20 amino acids, as a response to their realization that a code must exist. Some of the most beautiful — because simple — scientific reasoning in history went into the determination of the code. Enough said — Jim Watson's alcoholic musing was right.

(2) Again, to go outside my specialty, one of the truly beautiful complexes in science is the gauge principle of particle physics: the realization that all four of the known interactions are gauge interactions, in which the form of the forces coupling the particles follows from symmetry and not vice versa. A very nice discussion of this area is to be found in C.N.Yang's scientific autobiography, written as an annotation of his collected papers.

Mathematicians will tell you that they invented gauge theory anywhere from 50 to 100 years before the physicists in the form of something called "fiber bundles". I do not take this seriously — see my remarks about the "reality principle". A theory as a mathematical object is simply a statement about the contents of someone's mind, not about nature. Another point worth noting is that quite often the physicist — or other scientist, as in the case of probability theory

— invents his own mathematics which is fairly satisfactory for his purposes, and only later finds the relevant branch of mathematics — as with Einstein and non-Euclidean geometry.

Gauge was first used as a formally symmetric way of writing Maxwell's equations, and formal manipulation with it played a significant role in early attempts to produce a "projective" unification of gravity and electromagnetism. But the gauge idea proper stems from the work of Dirac, Jordan and others in reformulating quantum electrodynamics. What was done was to combine the early ideas of Wigner and Weyl on the role of symmetry in quantum mechanics with the "locality" principle of Einstein's general relativity. Quantum mechanics connects symmetry and conservation laws: time-invariance=energy conservation, rotation-invariance=angular momentum, etc; but from the Einsteinian point of view, the elementary interactions must allow only local, not global, symmetries. The appropriate symmetry principle for charge conservation is phase-invariance of a complex field; but to make phase-invariance local we must introduce the gauge field A and write all derivatives as $i\hbar \nabla - \frac{e}{c} A$. The dynamical theory of the vector potential A is then just electromagnetic theory. This is the message of gauge theory: out of three concepts one gets one.

Conservation laws, symmetries, and interactions are not three independent entities but one.

Next — from the physicist's side, this is where the mathematicians make their unjustifiable claims — Yang and Mills realized that the gauge theory was not unique, in that the symmetry involved did not need to be Abelian; but such a theory introduces gauge fields which carry the conserved quantity. After many false starts it became clear that the appropriate theory for strong interactions was color gauge theory, quantum chromodynamics based on the group $SU(3)$. Here yet another thread was brought in by T'Hooft, Gross, Politzer and others: the proof that gauge theories of this sort are asymptotically free and hence renormalizable. To make a long story short, with yet one more beautiful idea, that of broken symmetry, we now contemplate a world in which all four basic interactions are gauge theories: the three-dimensional $SU(3)$, the $2+1$ dimensional $SU(2)\times U(1)$ of the electroweak theory, and the 4-dimensional gauge theory which is Einstein's gravity. Whether the fact that the dimensions add up to 10, an interesting number in string theory, is significant is still much under discussion. One can hardly not, even at this stage, sit back and marvel at the beauty and intricacy not just of this simple structure but of its history and its cross-connections to many other ways of thinking.

One could go on following almost any thread of modern science and find an equivalent beauty at the center of it. One more instance will allow me to be a bit self-indulgent: topology, dissipation, and broken symmetry.

This starts with four apparently independent but individually beautiful pieces of work. First, the dislocation theory of strength of materials, when Burgers Taylor and others first invented the concept of the dislocation or line defect of the crystalline order, then — using very modern-sounding topological arguments — proved that it was topologically stable, and finally showed that motion of dislocations was the limiting factor in the strength of most materials.

Second chronologically was the beautiful work of Jaques Friedel's grandfather, G.Friedel, in identifying the defects in liquid crystals — specifically the "nematic" liquid crystal, so-called because the defects appeared threadlike. Third was the domain theory of ferromagnetism, and especially the beautiful sequence of work of Shockley and Williams showing how the motion of domain walls — planar defects where magnetization rotates — accounts for hysteresis loops in magnetic materials. Finally, there are the gorgeous conceptual break-throughs of Feynman, and then Abrikosov, where Feynman, in particular, invented the superfluid vortex line and showed that it could account for the critical velocity of superfluid helium, while Abrikosov described the vortex state of superconductors and I later pointed out that motion of the vortices implied resistance.

Oddly enough, it was the discovery of superfluidity in ^3He which triggered the realization that these were all the same phenomena. Almost simultaneously, Volovik and Mineev, and Toulouse and Kleman developed the general topological theory of defects in condensed phases, encompassing the physics developed over 100 years prior to 1975 in a single structure, classifying the possible topologies of maps of real space into the space of the order parameter of the condensed phase. For instance, for liquid helium the order parameter has a free phase so one must map space onto a circle; if that map is non-trivial, it implies that at some line in space the order parameter vanishes. This means that the defects are vortex lines. Then Toulouse and I made the general connection between motion of topological defects and the breakdown of a generalized rigidity of the system, implying dissipation: which couples together all these energy dissipation mechanisms. The great generality of this kind of structure has been exploited in the theory of "glitches" in the spinning neutron stars or pulsars: giant slippages of the vortex structure implied by the superfluidity of the neutron matter in such a star, beautifully isomorphic with the "flux jumps" which are the bane of superconducting magnet designers.

More or less at the same time, topology became fashionable in elementary particle physics, with the revival of the "Skyrmion" model of the fermion particle, and the fashion for "θ vacua" and "instantons". This is one of these fascinating cross-connections, although the topological ideas have not yet had their satisfactory resolution in particle theory.

As I already said, pick up almost any thread near the frontiers of modern science and one will find it leading back through some such sequence of connections. For example, an equally glorious story can be made of the separate investigations which, together, make up the present synthesis called "plate tectonics".

But if there is beautiful science, is there also ugly science? I regret to say that this also exists and often flourishes. It does so most commonly when a field falls out of effective communication with the rest of science; one often find fields or subfields which have lost contact with most of science and survive on purely internal criteria of interest or validity. The behaviorist or Skinnerian school of experimental psychology was a notorious example; I suspect that these days we are seeing an exposure of the entire field of electrochemistry to the pitiless light of real science. And certain recent incidents in the field of superconductivity have inclined me to believe that there is an isolated school of electronic structure calculators who have been avoiding contact with reality for some years.

Finally, there is, of course, pseudoscience, which will always be with us: parapsychology, "creation science", "cognitive science", "political science", etc. —Crick once made the remark that one should always be suspicious of a field with "science" in its title. I leave you with the final thought, that the essentially aesthetic criteria I have tried to describe for you may often be an instant test for scientific validity as well as for beauty.