

CHAPTER 1

MORE IS DIFFERENT – ONE MORE TIME

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In the Spring of 1967, just before my first term as Cambridge's first—and last—part-time professor, I spent a pleasant month at La Jolla with, among others, some old friends from Bell who had been recruited there by Walter Kohn to give a jump-start to the infant physics department. I had an appointment as Regents' lecturer, the only requirement being to give one more or less public lecture. I think this was the first time I had ever had such an assignment; I was nervous and worked hard on it. Afterwards I heard tell that the lecture was quite incomprehensible, though 30 years later I learned by chance that at least one listener, specifically Christiane Caroli (visiting from Paris), had taken the message seriously. Nearly five years later I wrote up an edited version and, somewhat to my surprise, Science accepted it: this was "More is Different" [1].

This was the late sixties when even established economic verities were being seriously questioned. Such books as Schumacher's "Small is Beautiful" were around, and in England one of the environmental movement's slogans was "more is worse"—to which the reply of the establishment was of course "more is better" (foreshadowing the Reagan era which opponents—and even some supporters—characterized with the slogan "greed is good".) So it was natural to suggest that more was merely different.

Sociologists of science posit that there is a personal or emotional subtext behind much scientific work, and that its integrity is therefore necessarily compromised. I agree with the first but reject the second. I think "More is Different" embodies these truths. The article was unquestionably the result of a buildup of resentment and discontent on my part and among the condensed matter physicists I normally spoke with. 1967 was a temporary maximum of arrogance among the particle physics establishment, riding high in government advisory circles (this was the heyday of JASON and of the RAND corporation), and in possession of

funding at a level which made international travel a commonplace and afforded overheads which made their employment profitable for any university. There was for instance difficulty in getting condensed matter colleagues recognised by the NAS, and many physics departments in major universities such as Yale, Columbia, and Princeton had only token representation of the field of condensed matter.

Viki Weisskopf was by no means the most narrowminded of the nuclear and particle types. “More is Different” is a reply to his article dividing science into “intensive” (for practical purposes, particle physics) vs “extensive” (the rest). I had always considered him a friend (and do now)—he sat on my thesis committee, and much of the early work in my thesis field was his—and this made it particularly hard to take. Looking back, I was right to be disturbed—in avoiding pejorative terms like “fundamental” he was attempting to camouflage a message which was identical to those said less delicately by others: that particle physics was the only truly intellectually challenging specialty, the others, especially the solid state physics (as it was then actually called), dear to my heart, were “mere chemistry”.

My article accepted Viki’s point that science is largely hierarchical in that the subject matter of one science is the “substrate” on which or out of which another builds the objects of its interest. For the essentially emotional reasons described above, my concern was to show that there is nonetheless intellectual autonomy: one cannot assume that the laws of the one science are trivial consequences of those of the other. It was by way of example that I used my newly generalized idea of broken symmetry to illustrate the processes by which this kind of autonomy arises. I wanted to explain in detail the way in which, in at least one sort of situation, truly novel properties and concepts could emerge from a simpler substrate. Broken gauge symmetry, which is exhibited by superconductors, was perfect for my purposes because many of the greatest theoretical physicists had tried to understand the problem of superconductivity before it was broken by the “phenomenologist” John Bardeen. Bloch and Wigner had even supposed that they had “proofs” that no solution of the sort existed. Bloch’s proof was simply based on a misunderstanding of the experimental facts, but Wigner’s error is crucial and generic, and is worth discussing. He based the “superselection rules” on the idea that it would be meaningless to have coherence between states with different numbers of particles, i.e. that ground states are always discrete eigenstates of the symmetries of the problem. This misses the possibility of “quasidegeneracy”, that macroscopic systems can have Goldstone “zero modes” which restore the broken symmetry. Thus the “ground state” is often not best described as such a symmetry eigenstate. This concept was an idea I had encountered in my theory of antiferromagnetism in 1952, and which came into its own in understanding superconductivity.

“Emergence” was a term from evolutionary biology with which I, like most physicists, was unfamiliar at that time. For over a century biologists had speculated that life “emerged” from non-living matter without any divine (or other)

intervention, “by accident” as Dawkins puts it. In fact, they saw the whole evolutionary process as emergence of the more complex from the simpler. In a sense, MID is just putting the concept of emergence in a physical context and generalizing it. Many biological examples of emergence are examples of its kind of scale change. But if one listens to the great synthesizer of evolutionary theory, Ernst Mayr, talking about emergence even today, it is clear that he is discussing a concept which is a little different, broader, and vaguer. He considers the “emergence” of the functionality of a hammer from the combination of a stick and a stone – what evolutionists call an exaptation – as a valid instance of emergence, while I was focussing on the effect of scale change. Only in the case of scale change, I felt, is there a clearly sufficient argument for qualitatively new concepts to appear.

The reader of MID today should realise that it was written in 1967, before much of the technical apparatus with which we understand the macroscopic, $N \rightarrow \infty$ limit had been created. This was before the renormalisation group had been applied in statistical physics (and more or less simultaneously with the electroweak theory’s publication, and long before its acceptance as the triumph of broken symmetry in particle physics). Kadanoff had only begun to state the idea of universality formally, though many of us, myself included, felt in our bones that something of the sort existed. Universality is the most primitive way of showing that the same macroscopic results can follow from very different microscopic causes.

Even farther in the future (1975) was to come the formal topological theory of order parameter defects such as vortices, flux lines, domain boundaries and the like, which allows the macroscopic system wide and unexpected flexibility in its behavior. Nonetheless, there was quite enough to make it clear that the emergence of new concepts and properties is almost inevitable when one makes up a macroscopically condensed phase. The process by which this happens is straightforwardly clear and has a precise description in terms of the theory of broken symmetry. All of the properties which characterise such phases – crystal structure, metallicity, macroscopic quantum coherence, elasticity, and so on – have no meaning in a world of individual atoms, and logically arise only when one puts together many atoms. One goes first, conceptually, to the limit of a very large system, and then backs off to the finite one that one actually has. Playing this game, I pointed out, is particularly fascinating in nuclear physics, where nuclei containing of order only 100 nucleons exhibit unmistakable evidence of such properties as shape and superfluidity, which in principle are only definable on the macroscopic level.

As I have tried to explain, my main goal was to demonstrate the intellectual autonomy of the higher level phenomena from the tyranny of the fundamental equations which constitute the “theory of everything” (this is a phrase which was to be invented thirty years in the future.) Though the modern theory of chaos in deterministic dynamical systems— the idea of sensitive dependence on initial

conditions— had yet to cross the horizon of a conventionally trained physicist like myself, quasidegeneracy of the quantum state of a macroscopic system has the equivalent effect of divorcing the future from the past as far as rote, mechanical computation using the laws of atomic physics is concerned, in spite of the fact that these are exactly known. LaPlace's perfect computer is not a physically realizable machine, and determinism via the laws of physics is a nonsense.

Nonetheless, a perverse reader could postulate a sufficiently brilliant genius—a super-Einstein—who might see at least the outlines of the phenomena at the new scale; but the fact is that neither Einstein nor Feynman succeeded in solving superconductivity. Imagine how much more difficult it would have been to predict the phenomenon of superconductivity a priori without the actual thing in front of you, than it was for Bardeen, Cooper and Schrieffer to follow experimental hints to guide them to a theory.

It was only in the last few paragraphs of the article that I spelled out a moral for the general structure of scientific knowledge: that scale changes in a wide variety of instances can lead to qualitative change in the nature of phenomena. It is a general rule that emergence of concepts and entities which are novel in some fundamental way occurs at the point where scales change materially. Life takes advantage of the fact that aperiodic macromolecules can carry information in the ordering of their constituents, a kind of broken symmetry going beyond that of an ordinary condensed phase. Again, organisms composed of many cells, with their differentiation of function and of cell types, constitute a leap of imagination relative to protists. The further generalisation to social and economic organisation seems obvious. Money and markets, for instance, are unnecessary when society is organised on the tribal level only, but seem usually to appear spontaneously when larger units or wider regions become organized.

In the years after MID my own work often involved the theme that complex generic behavior can arise from simplicity. Localisation was in the back of my mind in the original article, and in the '70's I picked it up again, as well as the theme of generating magnetic spins from a non-magnetic substrate; and I came to see these as generic examples of the concept of emergence rather than as isolated ideas. With the spin glass another was added, one which in fact led my group away from physical systems into the "emerging" (in another sense) science of complexity. We became involved in theories of complex optimisation and evolutionary landscapes. John Hopfield even carried the spin glass idea into his theories of brain function.

In the meantime MID was percolating along and acquiring a small following. The first I knew of this was when I received a phone call out of the blue, in summer 1977, asking me to speak at a neurosciences meeting at Keystone CO that winter. When I answered the phone, Gene Yates was relieved to find that I really (and still) existed. He had no idea of what else I had done and was flabbergasted by the prize announcement and pleased that I came to his meeting nonetheless. He put me on a program with an interesting variety of characters— Ralph Abrahams

from the Chaos collective at Santa Cruz, an early General Systems theorist named Art Iberall, and a somewhat hyper neurophysiologist named Arnie Mandell (who later won a MacArthur award for his studies of altered consciousness). It turned out that I thoroughly enjoyed the kind of broad-gauge, openminded discussions I had with these and others of Gene's friends, and when he asked me to a meeting in Dubrovnik on self-organization [2] which he organized in 1980 I was glad to go and meet a cross-section of the fundamental thinkers on biology—Leslie Orgel, Gunther Stent, Harold Morowitz, Steve Gould, Brian Goodwin, and many others, and to continue on my learning curve.

Thus, starting from MID and the spin glass, and an interesting if abbreviated sabbatical in 1981 helping with John Hopfield's course at Caltech on the physics of information, I gradually became socialised into the community of scientists who were thinking about the general themes of complexity, self-organisation, and emergent properties in general. Thus it was very natural for me to attend the first two organising workshops in 1984 and 1985 [3] of what became the Santa Fe Institute, then taking shape in the minds of George Cowan, Peter Carruthers, Murray Gell-Mann, David Pines and other senior scientists associated with Los Alamos. An early organising meeting at Aspen and the decision to help run the first workshops on economics [4] in 1986-7 (using a little expertise picked up in courses my wife and I attended in Cambridge) left me permanently attached to SFI.

By this time MID was really humming along. The title became almost a mantra for the work of the Santa Fe Institute and for the science of complexity in general (whatever that means). SFI started out as an interdisciplinary institute focusing on the subjects which grow out of making connections between existing fields. We found ourselves becoming interested, in a number of instances, in studying the emergence of the more complex level from the simpler one: how, in a number of cases, more complex behaviors resulted from the interactions of a number of simpler "agents". In economics, ecology, immunology, archaeology, neurophysiology we became to an extent captivated by "agent-based modeling"—the use of the computer to demonstrate such phenomena, in particular. This kind of work is by no means the only activity of SFI but it has become something of a trademark.

In a recent article I applied a similar line of reasoning to MID in the opposite direction, towards the origins of our physical universe. The Big Bang involved at least two thermodynamic phase transitions, one of which has been the subject of fairly intensive speculation: the phase transition to broken electroweak symmetry, which has been conjectured to be the cause of an "inflationary" era in cosmology. From the first the question of whether there should not be visible traces in the form of topological defects in the putative Higgs field (monopoles, cosmic strings) has been discussed. But there are even deeper and more subtle questions to be answered. Perhaps the most striking of all instances of emergence is the emergence of the classical world of identifiable, distinguishable objects in

space, obeying causality with a definite sense of time, out of the microscopic quantum description of the universe as a collection of quantum fields describing absolutely identical quanta moving in isotropic, homogeneous space-time. I have conjectured [5] that space-time itself might be an emergent property, born perhaps at the time when gravitational instabilities of the cosmos which eventually became clusters of galaxies began to form. Some cosmologists such as Lee Smolin have gone even further, but so far I haven't joined them.

Be that as it may, another thought is that the apparent difficulties and contradictions of quantum measurement theory are the result of attempting to apply, to systems at one scale, the concepts and properties that are appropriate to an entirely different scale: causality, rigidity, and so on. As I put it in another article [6], to an electron, the properties of the apparatus – Stern-Gerlach magnets, slits, and the like – are much more mysterious than the properties of the electron are to us. These objects have the strange property that they can act merely as boundary conditions for the electron, that is they can act on it without changing their quantum state. The complexity of the quantum description of such objects makes it in principle impossible to follow an atomic-scale object once it has interacted with a macroscopic object and hence their wave functions have become entangled.

The original article may have been too concise to express my full meaning. It is not a prescription for ignoring reductionist ideas, and indulging in what is called “holistic” thinking, in which one ignores the physical or biological substrate upon which a given science feeds. Just a few days ago I received a copy of a correspondence arguing about reductionism vs this kind of holism in which I was quoted as supporting both sides, and this was not a unique case.

I think the original article was clear in advocating reductionism in the sense of the assertion that the basic laws of physics, chemistry and biology hold under all known circumstances – magic doesn't happen. That being said, what was a little new is that this does not imply what I called “constructionism”, (more recently, others have called it “strong” or “strict-sense” reductionism): that the consequences of those laws can be worked out in detail or that they seriously restrict the endless possibilities of nature or even our free will – the former being demonstrable, the latter a conjectural corollary.

Then the question arises, whether there is any point in the reductionist program – if you can't work the consequences out in detail, why bother with the underlying laws at all? I gave my own personal answer to this question in the article, that understanding on that kind of level is infinitely satisfying; but there is a practical answer as well. As science becomes more complex and unavailable to the general public, the primitive, Baconian model of science which is taught – or was when I grew up – in high-school textbooks is no longer adequate. Again and again, groups of scientists working in isolation have succeeded in convincing themselves that black is white by the most reliable-seeming “studies”, or even by simply

repeating some set of seemingly rational propositions to each other often enough. Scientists are not immune to self-interest or egregious error.

In reality, academic scientists no longer rely solely on the direct experimental method of one hypothesis, one test to decide whether a given proposition is correct. One way of making sure that science is correct has been emphasised by such writers as Merton and Ziman [7] namely the social structure of science and its character as “organised skepticism”. I would propose that as science matures an equal or greater role is played by tying results in to the exponentially growing web of consistent knowledge, and one of the best ways one can do that is by showing that phenomena in one science can be explained from the basic laws of that science's substrate subject. For instance, genetics became enormously more powerful and believable as we explored the mechanism via structural chemistry and then molecular biology. And when claims of cold fusion by an isolated coterie of specialists hit the headlines, the importance of cross-checking against fundamental knowledge in related fields became apparent – as well as the inefficacy, for correcting error, of “direct”, “benchtop” measurements, by interested parties.

The message for which I was groping in 1967 has become much clearer to me with the passage of time. It was born of the realisation that science is no longer a collection of isolated communities, each applying the Baconian “scientific method” of empiricism and Popper's paradigm of “falsifiability” within its own bailiwick. The Newtonian mode, unification, has taken over from Bacon, and science is becoming an interconnected whole. But in the process of unification we were in danger of being victimized by those who appear to own the most universal, most microscopic laws: those who strive to achieve the “theory of everything” and discover the fundamental particles of which the universe is made. If they owned the fundamentals, they claimed, they could deduce all the rest. I fired the first salvo in rebuttal: that I saw the “theory of everything” as the theory of almost nothing. The actual universe is the consequence of layer upon layer of emergence, and the concepts and laws necessary to understand it are as complicated, subtle and, in some cases, as universal as anything the particle folks are likely to come up with. This also makes it possible to believe that the structure of science is not the simple hierarchical tree that the reductionists envision, but a multiply connected web, each strand supporting the others. Science, apparently, like everything else, has become qualitatively different as it has grown.

I rest my case.

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