Chapter 19

MURRAY GELL-MANN

"Plectics"

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J. Doyne Farmer: *The first thing that makes me respect Murray is that unlike all his contemporaries, including Feynman, Weinberg, Hawking, and all the other particle physicists, he saw that complexity is the next big problem. The kind of breakthroughs he made in the early 1960s in terms of impact on the world of science are not going to get made in that domain, they are going to get made in this domain. Murray recognized that, and has become more than just conversant with what's going on and with what the problems are.*

MURRAY GELL-MANN is a theoretical physicist; Robert Andrews Millikan Professor Emeritus of Theoretical Physics at the California Institute of Technology; winner of the 1969 Nobel Prize in physics; a cofounder of the Santa Fe Institute, where he is a professor and cochairman of the science board; a director of the J.D. and C.T. MacArthur Foundation; one of the Global Five Hundred honored by the U.N. Environment Program; a member of the President's Committee of Advisors on Science and Technology; author of *The Quark and the Jaguar: Adventures in the Simple and the Complex* (1994).

Murray Gell-Mann: When I was a small child, I was very interested in natural history and linguistics and archaeology. Though I lived in New York City, I managed to find some patches of country where I could become familiar with birds and butterflies and trees and flowering herbs. Even then, I was fascinated by the results of biological evolution and of the evolution of human culture. So it's not unnatural that I would want to try to understand the chain of relationships linking the fundamental physical laws that govern all matter in the universe to the behavior of the rich complex fabric we see around us and of which we are a part.

One way to make the task manageable is to look at the world from the point of view of information. When we do that, we see that the basic pattern is one of complexity emerging from very simple rules, initial order, and the operation, over and over again, of chance. In the case of the whole universe, the fundamental laws of physics constitute those simple rules.

There are various quantities labeled "complexity." In each case, the complexity of a thing is context-dependent — in other words, dependent not only on the thing being described but also on who or what is doing the describing. There's one quantity in particular that I think most deserves the label — what I call "effective complexity." A related quantity, which I have named "potential complexity," is also very important. Neither is yet defined with mathematical rigor, and that's a task I've undertaken. Some of the other quantities that people have called "complexity" are also well worth discussing.

In any case, to refer to the subject on which some of us now work as "complexity" seems to me to distort the nature of what we do, because the simplicity of the underlying rules is a critical feature of the whole enterprise. Therefore what I like to say is that the subject consists of the study of simplicity, complexity of various kinds, and complex adaptive systems, with some consideration of complex nonadaptive systems as well. To describe the whole field, I've coined the word "plectics," which comes from the Greek word meaning "twisted" or "braided." The cognate Latin word, *plexus,* also meaning "braided," gives rise to "complex," originally "braided together." The related Latin verb *plicare,* meaning "to fold," is connected with *simplex,* originally "once-folded," which gives rise to "simple."

Plectics is then the study of simplicity and complexity. It includes the various attempts to define complexity; the study of the roles of simplicity and complexity and of classical and quantum information in the history of the universe; the physics of information; the study of nonlinear dynamics, including chaos theory, strange attractors, and self-similarity in complex nonadaptive systems in physical science; and the study of complex adaptive systems, including prebiotic chemical evolution, biological evolution, the behavior of individual organisms, the functioning of ecosystems, the operation of mammalian immune systems, learning and thinking, the evolution of human languages, the rise and fall of human cultures, the behavior of markets, and the operation of computers that are designed or programmed to evolve strategies — say, for playing games or solving problems.

The Santa Fe Institute, which I helped to found in 1984, gathers together mathematicians, computer scientists, physicists, chemists, neurobiologists, immunologists, evolutionary biologists, ecologists, archaeologists, linguists, economists, political scientists, and historians, among others. The emphasis is on interactive people. Many distinguished scientists and scholars yearn to stray outside their own fields but can't do so easily at their own institutions. We didn't want to locate our institute near Harvard or Stanford, where there's enormous pressure of received ideas — ideas accepted by a whole community and therefore difficult to challenge. In Santa Fe, we can think and talk freely, constrained only by the need to agree with reality.

The poet Arthur Sze wrote, "The world of the quark has everything to do with a jaguar circling in the night." What is the key to understanding the jaguar circling in the night, from the point of view of information? The major insight here is that perceived regularities in the stream of data reaching a complex adaptive system — one that can adapt, learn, or evolve the way living things on Earth evolve — are compressed into models or schemata. Those schemata are subject to change and to replacement by other schemata, so that various alternative schemata compete. When the schemata are used to describe or predict the behavior of the world or to prescribe behavior for the complex adaptive system itself, there are real-world consequences. Those consequences feed back to influence the competition among schemata, and that's how learning and adaptation take place.

The theory of complex adaptive systems, which we're now beginning to develop, should apply to all such systems, wherever they occur in the universe. Just think how many galaxies there are in the universe and how many stars there are in each galaxy. Many of those stars presumably have planets that can support complex adaptive systems. We don't know yet what constraints physical laws impose on the nature of such systems. Must they resemble, to some extent, life on Earth or machines constructed by living organisms on Earth? Or can they take very different forms? We don't know, for example, whether biochemistry on Earth is nearly unique or whether it was just one of many possibilities. In other words, we're not yet sure to what extent biochemistry was determined by physics and to what extent it was determined by the accidents of history.

I mentioned that the effective complexity of the world around us comes from very simple rules and initial order, plus the operation of chance, which is associated with indeterminacy. The most fundamental source of indeterminacy is quantum mechanics, the basic framework for physical law. In contrast to the older classical physics, quantum mechanics is not fully deterministic. Even if the initial condition of the universe and the fundamental law of the elementary particles and their interactions are both exactly known, the history of the universe is still not determined. Instead, quantum mechanics gives only probabilities for alternative histories of the universe. In some situations, those probabilities are nearly certainties, and classical physics is a good approximation, but in other situations the indeterminacy is striking. For example, when a radioactive nucleus disintegrates, emitting an alpha particle, say, the direction of emission of that particle is altogether unknowable in principle before the disintegration takes place — all directions are equally probable.

Even in the classical approximation, with the fundamental law assumed to be exactly known, effective indeterminacy of the future still arises from partial ignorance of present circumstances (which are actually in part the results of earlier accidents) and from difficulty of calculation. This kind of indeterminacy is exacerbated by the common phenomenon of chaos in nonlinear systems, which refers to an extreme sensitivity of the outcome to details of the present situation.

The importance of accidents in the history of the universe can thus hardly be exaggerated. Each of us human beings, for example, is the product of an enormously long sequence of accidents, any of which could have turned out differently. Think of the fluctuations that produced our galaxy, the accidents that led to the formation of the solar system, including the condensation of dust and gas that produced Earth, the accidents that helped to determine the particular way that life began to evolve on Earth, and the accidents that contributed to the evolution of particular species with particular characteristics, including the special features of the human species. Each of us individuals has genes that result from a long sequence of accidental mutations and chance matings, as well as natural selection.

Now, most single accidents make very little difference to the future, but others may have widespread ramifications, many diverse consequences all traceable to one chance event that could have turned out differently. Those we call frozen accidents. I give as an example the right-handed character of some of the molecules that play important roles in all life on Earth though the corresponding left-handed ones do not. People tried for a long time to explain this phenomenon by invoking the left- handedness of the weak interaction for matter as opposed to antimatter, but they concluded that such an explanation wouldn't work. Let's suppose that this conclusion is correct and that the right-handedness of the biological molecules is purely an accident. Then the ancestral organism from which all life on this planet is descended happened to have right-handed molecules, and life could perfectly well have come out the other way, with left- handed molecules playing the important roles.

Another example can be chosen from human history. For instance, Henry VIII became king of England because his older brother Arthur died. From the accident of that death flowed all the coins, all the charters, all the other records, all the history books mentioning Henry VIII; all the different events of his reign, including the manner of separation of the Church of England from the Roman Catholic Church; and of course the whole succession of subsequent monarchs of England and of Great Britain, to say nothing of the antics of Charles and Diana. The accumulation of frozen accidents is what gives the world its effective complexity.

The effective complexity of something is the length of a brief description of its regularities. Those regularities can come from only two sources: the fundamental laws, which are very simple and briefly describable, and frozen accidents.

As time goes on, systems of greater and greater effective complexity appear. That's true for nonadaptive systems, such as galaxies, stars, and planets, as well as for complex adaptive systems, as in biological evolution. Of course, I don't mean that each individual system becomes more complex. Some things get simpler; they may even disappear altogether, as in the case of vanished civilizations. Instead of a steady march toward greater complexity everywhere, there's a tendency for the envelope of effective complexity to expand. We can understand why. With the passage of time, more and more accidents occur, and frozen accidents accumulate. In fact, at any time, there are many mechanisms at work producing self-organization, which results in local order, even though the average disorder in the universe is increasing in accordance with the second law of thermodynamics. Self-organization gives rise, for example, to the arms of spiral galaxies and the myriad symmetrical shapes of snowflakes.

In the case of complex adaptive systems, their schemata have consequences in the real world, which exert selection pressures back on the competition among the schemata, and those schemata that produce favorable results in the real world have a tendency to survive, or to be promoted, and those that are less successful in the real world have a tendency to be demoted or to disappear. In many situations, complexity may offer a selective advantage. It is a challenge to evolutionary biologists, for example, to understand when that is the case.

Light can be thrown on many such questions by making use of computer-based complex adaptive systems, which can be used (1) to provide crude models of natural complex adaptive systems, (2) to supply interesting examples of complex adaptive systems for study, (3) to evolve new strategies for playing games or for solving problems, or (4) to solve problems by means of "adaptive computation."

The study of computer-based complex adaptive systems is already burgeoning, especially as a mathematical discipline concerned with the relation between simple rules and the emergence of complex behavior. That's something worth pursuing in its own right, but even more exciting is the possibility of useful contributions to the life sciences, the social and behavioral sciences, and even matters of policy for human society.

The favorite activity of some of my colleagues, especially my younger colleagues, at the Santa Fe Institute and of their friends around the world is to construct computer models with very, very simple rules — carefully chosen, stripped-down sets of rules that permit complex behavior to emerge. It's a remarkable and somewhat addictive experience to watch that emergence. We have people who are very good at stripping down rules for computer models — the political scientist Bob Axelrod, for example. He also has a flair for persuading his colleagues in political science that such a simplified model is somehow relevant to reality. If I came up with a model of that kind and presented it in a lecture to political scientists, they'd laugh me off the platform. Bob, however, presents it in such a way that social scientists can accept it. For example, imagine a circle of little polities occupying the coast of a Polynesian island with a huge volcano in the middle. The polities interact with one another either by forming alliances or making war. Each one can attack only an immediate neighbor or one that can be reached through an uninterrupted sequence of allies. Somehow Axelrod manages to extract interesting lessons from such a trivial, one-dimensional model.

Someday we'll have a full-fledged mathematical science, with theorems and proofs, that will make it clear, for instance, when new rules merely complicate the picture without adding anything essential to the emergent patterns. The construction of that science lies at one end of the spectrum of efforts to use computers to help us think about complicated systems. At the other end of the spectrum are attempts to think about policy problems that humanity faces in the real world, in connection with human society, the rest of the biosphere, and the relation between the two. In the middle, we have attempts to understand better the operation of complex adaptive systems in the life sciences and in the behavioral and social sciences. When we get away from the mathematical end of the spectrum, the accumulation of accidents of history enters in a very important way. The stripped-down computer models are typically ones that apply, in a general way, to complex adaptive systems on any planet in the universe. They don't contain any historical information about the planet Earth, or about the organisms that inhabit the planet Earth, or about human beings and the institutions we've built.

In the simple exercises that are so popular, one starts with a caricature of one level of organization, and then one often sees a higher level of organization emerge. Starting with highly simplified individuals, you may see the emergence of a society. Starting with highly simplified polities, you may see confederations emerge. Suppose, however, you want a simplified description of human society as it exists on this planet, with all its polities and the various levels — federations, confederations, and so on — that exist, and their various relations with one another, the results of a huge number of historical accidents. These entities are all historical and peculiar to this planet and to human beings. You're forced to start complicating the stripped-down models by adding in other things — especially, new levels of organization — without waiting for them to emerge. You don't wait for the individuals in your model to develop a city or a business firm, and you don't depend on the cities and the firms to invent a nation, and the nations to invent a U.N. You have to put a lot of that in, along with some of the special properties that human beings and their firms, cities, ethnic groups, nations, and international organizations exhibit on this planet. You can no longer be content with the thrill that my friends get when they see one level of organization emerging from another, as simple rules give rise to complex behavior.

If you want to put in too many special properties, whether at the level of the individual human being or at higher levels of organization, you'd be going far beyond the capacity of any model. First, the model would become too difficult to handle mathematically, and second, once the model ran you'd find it very difficult to understand the results. There's always a trade-off between the advantages of stripping down the rules — so that you get caricatures of human beings, let's say, but you also get operations you can carry out mathematically — and the advantages of putting in something more complex, more sophisticated, more applicable to this planet and to the human race. Of course, as computers get better and better, the whole game will become more sophisticated, but there will still be such a trade-off.

An interesting question about the behavior of complex adaptive systems is, What is required to move from one level to another? In Tom Ray's little artificial world of digital organisms, there are significant jumps, and with more elaborate models we'll be able to see even more significant changes in level of organization.

The tendency of the researchers is to crowd over at the mathematical end of the spectrum, where the rules are simple and they get enormous pleasure out of seeing complexity emerge, but that work will be difficult to use for scientific or policy purposes, and rather easy to misuse. One has to invest some effort in the other parts of the spectrum as well.

Furthermore, one has to proceed with caution, in that much mischief has been done in the world by exaggerating the role of scientific metaphor in human affairs. The science of economics provides an example: people have tried to apply a stripped-down version of economics to human affairs, omitting a great many values, a great many things of importance. You get society in the service of economics, instead of economics in the service of society. The Nazi racial theories are, of course, a horrible example of misapplying metaphors from science. Nineteenth-century ideas of social Darwinism are another example. We have to be careful when we use these stripped-down models — and even when we use more complicated models — not to take them too seriously but rather to use them as prostheses for the imagination, as sources of inspiration, as acknowledged metaphors. In that way I think they can be valuable.

I've never been eager to sell a particular kind of activity to others just because I'm engaged in it myself. I never tried to sell elementary-particle physics to people as a career, and I wouldn't try to sell the study of complex adaptive systems to anybody either. I think what is exciting is human culture as a whole. People may want to be painters or poets or historians or scientists of various kinds — field biologists or archaeologists or plectics theorists or elementary-particle experimentalists or astronomers or whatever. It is noteworthy, though, that people who work on simplicity and complexity — on plectics — are often capable of carrying out practical activities in a great many different fields.

Nevertheless, people doing transdisciplinary work have a lot of problems finding suitable employment, especially in academic life. The reason isn't merely prejudice but also the fact that all the mechanisms for judging excellence are set up in the narrow traditional disciplines. Peer reviewed journals, academic departments, Ph.D. exams, professional societies, and so on, are typically organized along disciplinary lines. Of course, there are always phonies who cower on the boundaries between fields, so people aren't altogether unjustified in being wary of transdisciplinary work. Clearly, we need effective mechanisms for judging it.

In discussing plectics with audiences, I encourage people to see one panorama rather than a lot of separate disciplines: the various meanings of simplicity and complexity; complex nonadaptive systems in the physical sciences; the modern interpretation of quantum mechanics; the simplicity of the fundamental laws of physics — that is, the unified theory of all the particles and their interactions plus the boundary condition at the beginning of the expansion of the universe; complex adaptive systems in the life sciences, in the behavioral and social sciences, and in practical human affairs; computer-based complex adaptive systems, some of which can serve as crude models for natural complex adaptive systems; and so forth.

Also, I have found it necessary to discuss the notion of reductionism. People scream epithets at one another over this issue of reduction. I take what I think is the only sensible position, which is that of course the basic laws of physics are fundamental in the sense that all the other laws are built on them, but that doesn't mean you can derive all the other laws from the laws of physics, because you have to add in all the special features of the world that come from history and that underlie the other sciences. Physics and chemistry stem from the fundamental laws, although even there, in the complicated branches of physics and chemistry, the formulation of the appropriate questions involves a great deal of special additional information about particular conditions that don't obtain everywhere in the universe. In the center of the sun, there is no solid-state physics. In the very early universe, when matter was still mostly a quark soup, there was not even nuclear physics. So even those subjects involve, in a sense, more than just fundamental laws.

All the rest of the sciences depend heavily on particular accidents in the history of the universe: astronomical accidents, geological accidents, biological accidents, accidents of human history, and so on. There's a huge body of information that has to be supplied in addition to the fundamental laws before you get the details of biology on Earth, for example. Just because elementary-particle physics is fundamental doesn't mean you can reduce biology to it, even in principle, unless you adjoin that additional information. Furthermore, in practice, it's essential to study biology at its own level, and likewise psychology, the social sciences, history, and so forth, because at each level you identify appropriate laws that apply at that level. Even though in principle those laws can be derived from the level below plus a lot of additional information, the reasonable strategy is to build staircases between levels both from the bottom up (with explanations in terms of mechanisms) and from the top down (with the discovery of important empirical laws). All of these ideas belong to what I call the doctrine of "emergence."

I've now retired from Caltech, an institution that is often labeled "reductionist," meaning that Caltech researchers usually don't take up in any depth subjects such as linguistics, archaeology, evolutionary biology, and psychology. Typically, they concentrate on fields like neurobiology, trying to investigate the mechanisms that underlie psychology. In that way, Caltech has built up a brilliant record of achievement in certain fields. However, in stressing the search for mechanism, Caltech tends to ignore the other part of strategy, which is to look for empirical rules in complicated fields and build staircases from the top down as well as from the bottom up.

Take Darwin, for example: would Caltech have hired Darwin? Probably not. He had only vague ideas about some of the mechanisms underlying biological evolution. He had no way of knowing about genetics, and he lived before the discovery of mutations. Nevertheless, he did work out, from the top down, the notion of natural selection and the magnificent idea of the relationship of all living things.

At the Santa Fe Institute, we encourage not only the study of plectics but also a number of general habits of research: building staircases from the top as well as the bottom, having the courage to take a crude look at the whole, cooperation among disciplines, and cooperation among different points of view on the same question when they are not logically contradictory. And we would have loved to have Darwin on our faculty.

Christopher G. Langton: There's nothing like having a Nobel laureate around to liven up discussions on almost any topic. Often, however, receiving a Nobel Prize in one field gives the recipient the feeling that anything he or she says on any topic is worth listening to, which is generally not the case — with one howling exception: Murray.

Murray really is an expert in a wide variety of fields and really does know what he's talking about when he launches into a discourse on any one of them. He's probably fluent in as many scientific disciplines as he is in languages of the world, and I've lost count of how many languages he speaks. Sometimes it can be hard to get a conversation going off in a direction that doesn't include a topic that Murray's interested in, but the conversation will certainly never be dull or uninformative. I always learn a lot when I talk with Murray. I also have to say that Murray played a major role in setting up the intellectual atmosphere of the Santa Fe Institute, and he has been a strong advocate of the institute policy of reaching out to and including bright young researchers in addition to the more established older scientists who typically visit here.

Alan Guth: Murray Gell-Mann is certainly one of the three leading particle theorists of the century, along with Richard Feynman and Steven Weinberg. One of Murray's most important contributions was the discovery of the quark model. All particle physicists are now convinced that the so-called strongly interacting particles, which include the proton, neutron, and several hundred other particles less well known to the public, are all made out of fundamental constituents called quarks, and it was Murray who first proposed that. At the time, the evidence wasn't very strong; there were some patterns seen in the mass distribution of particles, but Murray put it all together and came up with the bold proposal that it would all look very simple if we assumed that these particles were made of quarks.

The proposal goes beyond that. It was not just a question of deciding that smaller particles existed — that by itself is kind of an obvious idea — but Murray went on to play an important role in constructing the detailed theory of how these quarks interact with each other, what their properties are, how you can use the properties of these quarks to calculate in detail the properties of the particles that the quarks make up. All that's very important; it's the backbone of our current understanding of particle physics, and Murray's role was absolutely crucial. The quark model became part of what's come to be called the standard model of particle physics, which is now the model that all of us accept.

The standard model is really a conglomeration of pieces that were developed by different people. The phrase "standard model" probably started to be used in 1974 or thereabouts. It's a phrase that caught on gradually, so it's a little hard to know when it was first used. The earliest piece of the standard model is the so-called electroweak theory, which was first published by Weinberg in 1967. The strong-interaction part of the standard model — the part about how quarks interact with one another — is based on papers that came out in 1971, 1972, and 1973, some of which were written by Gell-Mann.

We don't regard the standard model as the final theory; it's too complicated, too diverse in its description. Most particle theorists assume that the standard model is a low-energy approximation of a richer, fundamentally more simple theory. We have been looking for that more simple theory. Gell-Mann has played a role in that search, too; he wrote some of the important papers about grand unified theory — the unification of the electroweak and the strong interactions — when grand unified theories were first discussed. He also worked on some of the other ideas, like supergravity and superstrings.

Lately, Murray's gone off and done things I don't understand at all; he's left particle theory now, and he's working on complexity. Complexity remains a mystery to many particle theorists.

Lee Smolin: Murray is the greatest living American theoretical physicist. His contributions to elementary-particle theory were dramatic and very important. They came out of a tremendous imagination — the idea of strangeness, the idea of quarks, the idea of the eightfold way, the idea of SU(3).

SU(3) is the idea that all the known particles would be different manifestations of one kind of particle, and they'd be unified by a symmetry. A symmetry means a way of taking you from one particle to another particle — replacing one by another, in an experiment. The result of a symmetry is that the experiment is not much changed if you replace one particle by another. Murray's proposal was that there could be such a symmetry involving all the particles that were then known. This was in the early 1960s. The particles are of course not identical, but the idea is that the things that distinguish the particles would arise from smaller and less important effects than the things that made them similar, which could be explained with the notion of a symmetry. Symmetry is a profound idea that has been the driving force in elementary-particle physics since then. I'm not sure the idea is completely right, in the sense that it may have outlived its usefulness. But it's been the dominant idea since the 1960s.

Recently, Murray's been interested in more mathematical ideas. He played a big role in the establishment of the standard model; he was one of a number of people who pushed the idea that another sort of symmetry, called a gauge symmetry, could account for the forces that bind the quarks into the proton and neutron — this was quantum chromodynamics. He didn't invent supergravity, but he was important in its development. He invented a form of it with John Schwartz, and they played an important role in pushing the idea. Again, he didn't really make contributions to string theory, but he helped to push the idea. He also materially kept John Schwartz and some other string theorists alive and working as physicists for many years while nobody else was interested in strings. The fact that, after all this, he's become interested in the ideas of complexity is wonderful, because he's right: physics needs a new direction, and the direction should have something to do with the study of complex systems rather than with the kind of physics he did most of his life. The fact that after spending a life focused on studying the most elementary things in nature Murray can turn around and say that now what's important is the study of complex systems is a great inspiration, and also a great tribute to him.

What Murray is saying is that the important new ideas in science will come not from further development of particle physics in the direction of finding the perfect fundamental theory of everything, but in understanding why our universe is complex, and understanding how to mix the science of the fundamental with the science of the complex. It's a striking indication of his originality and intelligence that he's been thinking that way for a long time.

Murray also has ideas about the foundations of quantum mechanics and the interpretations of quantum mechanics and cosmology which are interesting, which have influenced a lot of people. I don't actually agree with these ideas — I have different ideas of my own — but certainly his ideas have played a big role in this field.

Martin Rees: Great man. Clearly someone who has had remarkable success in predictions about particle physics over his career, and whose current work with the theoretical physicist Jim Hartle is influencing one of the main schools of thought in quantum gravity.

What Murray Gell-Mann appreciates is the contrast between the simplicity of particle physics and the complexity of the world around us. Quite different styles of thinking are needed for these kinds of phenomena. As a cosmologist, I like to describe the history of the universe in three parts. The first part is the first microsecond, which is difficult to understand because the basic microphysics is uncertain, involving extreme conditions that we can't replicate in accelerators. After the first microsecond, the universe becomes, in a sense, an easy place to understand; we can make calculations about primordial helium, deuterium, lithium, and so on, and about the spectrum of background radiation. But that simplicity ends after a few million years, when the first structures condense out from the universe. In the third part of its history, the universe becomes a complex place, and it remains a complex place thereafter, not because the basic physical laws are uncertain but because the manifestation of the laws in nonlinear structures are very complex.

Everything from meteorology to biology is essentially complex manifestations of simple laws. Most theoretical cosmologists are concerned with the early universe, where the laws are simple and there are no structures. That's a subject which is akin to particle physics, one side of Murray's interest. But the kind of cosmology I do (what some people call cosmogony, the study of the origin of the structures and of why the universe is the way it is) involves the emergence of complexity after the first few million years, after the fireball cools down. The nature of the subject then becomes different. We can't expect to encapsulate everything in a few simple equations, as in particle physics. We can't aspire to much beyond a qualitative understanding of some key processes. In that sense, it's more like the environmental sciences than like particle physics.

Murray Gell-Mann is someone who has emphasized this contrast but who appreciates the scientific challenges of both. That's one thing which is very admirable about him. Particle physicists have often been ultra-élitist, regarding their subject as being the highest paradigm, towards which all other sciences should strive. Murray is now emphasizing clearly that many other sciences are equally difficult and challenging, because of complexity. There is continued debate about whether some sciences are more fundamental and difficult than others, and it may be a mistake to regard the most fundamental sciences as being the most mathematical ones. Particle physics is actually a rather atypical science, in that it's the only science where you can expect things to be exactly described by a few equations. You don't expect continental drift to be described by a few equa tions; you expect a few unifying ideas.

In the particle-physics community, there are an enormous number of practitioners chasing a few key problems, and so if someone like Gell-Mann in the old days (or Ed Witten now) comes up with a key idea, lots of bright people follow its consequences very quickly. In astrophysics and cosmology, the ratio of bright people to problems is much lower. What that means is that often the good ideas not only don't get worked to death, they don't even get followed up enough. The frontiers are more extensive and less intensively developed, as it were.

J. Doyne Farmer: The first thing that makes me respect Murray is that unlike all his contemporaries, including Feynman, Weinberg, Hawking, and all the other particle physicists, he saw that complexity is the next big problem. The kind of breakthroughs he made in the early 1960s in terms of impact on the world of science are not going to get made in that domain, they are going to get made in this domain. Murray recognized that, and has become more than just conversant with what's going on and with what the problems are.

What's impressed me is that when I heard Murray give his first few talks on complex systems, I thought he was missing the boat. Then I heard him speaking about it a few years later, and I thought he was accurately describing the boat. Murray is doing the field a great service by lending his name in support of it, and championing the cause, and he's also doing a good job of articulating what the cause is.

Daniel C. Dennett: Murray strikes me as having excellent instincts, scientifically. It's odd for me, as a philosopher, to praise scientists for having excellent scientific instincts, but I'm impressed with the fact that when he leaps into a controversy, his take on it is usually pretty apt. It always fascinates me to see how often fine scientists have a blinkered view of the world which prevents them from seeing the virtue of a certain approach. No blinkers on Murray.

Stuart Kauffman: Murray is enormously smart, sensible, and knowledgeable. He may know more things than any other single human being. He has played an extremely important role at the Santa Fe Institute in two or three guises. First of all, Murray's taste in science is good. His taste in people is good, too, even though he sometimes has a hard time expressing approval. He's been a continuous source of pressure toward broadening the institute and getting it to take on a wider range of issues.

Secondly, Murray has lent enormous prestige to whatever the sciences of complexity will be. He's laid his reputation on the line in helping to found the institute and being out there as a public spokesman for what we're doing. Thirdly, while Murray has obviously dominated physics for years, in the emerging sciences of complexity he hasn't made major contributions of an original kind. What he has done is to assemble what are essentially other people's ideas into his own coherent framework.

Marvin Minsky: What is there to say? He's wonderful. He's right up there with Feynman as one of the great thinkers. He knows a lot about many things, including artificial intelligence. But I think his major contribution is inventing new kinds of insults. For instance, if somebody says something that isn't exactly perfect — Murray has developed one of the best inventories of put- downs that exists. I hear he's getting mellower. That would be a terrible loss for civilization. A collection of anecdotes about his remarks about other people would be priceless.

Paul Davies: Murray Gell-Mann is one of the towering figures in twentieth-century physics. He'll go down in history as the founder, or one of the cofounders, of the idea of quarks, the elementary constituents of the nuclear particles. It's only in recent years that he's become known for his work on complexity theory. What he's done is to recognize the fact that there are two ways of studying the world. There's the reductionist path, in which you try to break things down into their most elementary constituents — quarks, or maybe something deeper, like superstrings. The other path is the path of synthesis, the path of looking at the complex organizational arrangement of things and recognizing that there's a whole science of complexity, with laws and principles emerging at successive levels.

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