REDUCTIONISM REDUX

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It used to be traditional for college courses on the history of philosophy to begin around 600 BC with Thales of Miletus. According to later writers, Thales taught that everything is made of water. Learning about Thales, undergraduates had the healthy experience of starting their study of philosophy with a doctrine that they knew to be false.

Though wrong. Thales and his pre-Socratic successors were not just being silly. They had somehow come upon the idea that it might be possible to explain a great many complicated things on the basis of some simple and universal principle - everything is made of water, or everything is made of atoms, or everything is in flux, or nothing ever changes, or whatever. Not much progress could be made with such purely qualitative ideas. Over two thousand years later Isaac Newton at last proposed mathematical laws of motion and gravitation, with which he could explain the motion of the planets, tides, and falling apples. Then in the "Opticks", he predicted that light and chemistry would someday be understood "by the same kind of reasoning as for mechanical principles", applied to "the smallest particles of nature".

By the end of the nineteenth century physicists and chemists had succeeded in explaining much of what was known about chemistry and heat, on the basis of certain assumed properties of some ninety types of atoms - hydrogen atoms, carbon atoms, iron atoms, and so on. In the 1920s physicists began to be able to explain the properties of atoms and other things like radioactivity and light, using a new universal theory known as quantum mechanics. The fundamental entities to which this theory was applied were no longer the atoms themselves but particles even more elementary than atoms - electrons, protons, and a few others - together with fields of force that surround them, like the familiar fields that surround magnets or electric charges.

By the mid-1970s it had become clear that the properties of these particles and all other known particles could be understood as mathematical consequences of a fairly simple quantum theory, known as the "standard model". The fundamental equations of the standard model do not deal with particles and fields, but with fields of force alone; particles are just bundles of field energy. From Newton's time to our own we have seen a steady expansion of the range of phenomena that we know how to explain, and a steady improvement in the simplicity and universality of the theories used in these explanations.

Science in this style is properly called reductionist. In a recent article in these pages (1) Freeman Dyson described reductionism in physics as the effort "to reduce the world of physical phenomena to a finite set of fundamental equations". I might quibble over whether it is equations or principles that are being sought, but it seems to me that in this description Dyson has caught the essence of reductionism pretty well. He also cite the work of Schroedinger and Dirac on quantum mechanics in 1925 and 1927 as "triumphs of reductionism. Bewildering complexities of chemistry and physics were reduced to two lines of algebraic symbols".
You might have thought that these illustrious precedents would inspire a general feeling of enthusiasm about the reductionist style of scientific research. Far from it. Many science kibitzers and some scientists today speak of reductionism with a sneer, like postmodernists talking about modernism or historians about Whig historiography. In 1992 John Cornwell, the director of a project at Jesus College, Cambridge, on the sociology of science, convened a group of well-known scientists and philosophers to meet there to discuss reductionism. It was at this symposium that Dyson gave the talk on which his eloquent NEW YORK REVIEW article was based. The collected papers of this symposium, NATURE'S IMAGINATION(2), contains articles with titles such as "Must mathematical physics be reductionist?" (Roger Penrose), "Reductive megalomania" (Mary Midgley), and "Memory and the individual soul: against silly reductionism" (Gerald Edelman). A review of this book by the mathematician John Casti, in NATURE, calls these the "good guys in the white hats" as opposed to the unreconstructed reductionists at the meeting like the chemist Peter Atkins and the astronomer John Barrow.

Casti is a fellow of the Santa Fe Institute, a haven for non-reductionist science. Dyson himself remarks that he has a "low opinion" of reductionism. (Coming from Dyson, this really hurts. He played a major role in the development of quantum field theory, which has been the basis of the reduction of all of elementary particle physics to the standard model.) What has gone wrong? How has one of the great themes in intellectual history become so disreputable?

One of the problems is a confusion about what reductionism is. We ought first of all to distinguish between what (to borrow the language of criminal law) I like to call grand and petty reductionism. Grand reductionism is what I have been talking about so far - the view that all of nature is the way it is (with certain qualifications about initial conditions and historical accidents) because of simple universal laws, to which all other scientific laws may in some sense be reduced. Petty reductionism is the much less interesting doctrine that things behave the way they do because of the properties of their constituents: for instance, a diamond is hard because the carbon atoms of which it is composed can fit together neatly. Grand and petty reductionism(3) are often confused because much of the reductive progress in science has been in answering questions about what things are made of, but the one is very different from the other.

Petty reductionism is not worth a fierce defense. Sometimes things can be explained by studying their constituents - sometimes not. When Einstein explained Newton's theories of motion and gravitation, he was not committing petty reductionism. His explanation was not based on some theory about the constituents of anything, but rather on a new physical principle, the general principle of relativity, which is embodied in his theory of curved spacetime. In fact, petty reductionism in physics has probably run its course. Just as it doesn't make sense to talk about the hardness or temperature or intelligence of individual "elementary" particles, it is also not possible to give a precise meaning to statements about particles being composed of other particles. We do speak loosely of a proton as being composed of three quarks, but if you look very closely at a quark you will find it surrounded with a cloud of quarks and antiquarks and other particles, occasionally bound into protons; so at least for a brief moment we could say that the quark is made of protons. It is grand reductionism rather than petty reductionism that continues to be worth arguing about.

Then there is another distinction, one that almost no one mentions, between reductionism as a program for scientific research and reductionism as a view of nature. For instance, the reductionist view emphasizes that the weather behaves the way it does because of the general
principles of aerodynamics, radiation flow, and so on (as well as historical accidents like the size and orbit of the earth), but in order to predict the weather tomorrow it may be more useful to think about cold fronts and thunderstorms. Reductionism may or may not be a good guide for a program of weather forecasting, but it provides the necessary insight that there are not autonomous laws of weather that are logically independent of the principles of physics. Whether or not it helps the meteorologist to keep it in mind, cold fronts are the way they are because of the properties of air and water vapor and so on which in turn are the way they are because of the principles of chemistry and physics. We don't know the final laws of nature, but we know that they are not expressed in terms of cold fronts or thunderstorms.

One can illustrate the reductionist world view by imagining all the principles of science as being dots on a huge chart, with arrows flowing into each principle from all the other principles by which it is explained. The lesson of history is that these arrows do not form separate disconnected clumps, representing science that are logically independent, and they do not wander aimlessly. Rather, they are all connected, and if followed backward they all seem to branch outward from a common source, an ultimate law of nature that Dyson calls "a finite set of fundamental equations". We say that one concept is at a higher level or a deeper level than another if it is governed by principles that are further from or closer to this common source. Thus the reductionist regards the general theories governing air and water and radiation as being at a deeper level than theories about cold fronts or thunderstorms, not in the sense that they are more useful, but only in the sense that the latter can in principle be understood as mathematical consequences of the former. The reductionist program of physics is the search for the common source of all explanations.

As far as I can tell, Dyson's objections are entirely directed at reductionism as a research program rather than as a world view. He regrets that Einstein and (in later life) Oppenheimer were not interested in something as exciting as black holes, and blames this on their belief that "the only problem worthy of the attention of a serious theoretical physicists was the discovery of the fundamental equation of physics". This is pretty mild criticism. Dyson does not question the value of the discovery of the fundamental equations (how could he), but only tells us that there are other things in physics to think about, like black holes. This is like a prohibitionist who is against gin because, good as it is, it distracts people from orange juice. And I am not sure that Dyson is even entirely right about Einstein and Oppenheimer as examples of the danger of the appeal of reductionism.

I recall as a Princeton graduate student going to seminars at the Institute for Advanced Study, where Dyson was a professor and Oppenheimer the director.

Oppenheimer always sat in front and carried on a detailed technical dialogue with the speaker, whatever the topic might be. He certainly seemed interested in everything what was going on in physics, not just at the reductive forefront. In fact, even in the 1920s and 1930s, when he was doing his best research, Oppenheimer's work had much less to do with finding fundamental equations than with calculating the consequences of existing theories. By the time I met him, Oppenheimer's own research had pretty well ended, and I can believe that he explained this even to himself in the way that is cited by Dyson; but I suspect that the truth is that he had just become too famous and too busy to have time for research.

Einstein is another story. He had never immersed himself the way Oppenheimer did in the physics research of others. The physicist-historian Gerald Holton showed some years ago that Einstein was not significantly influenced by the specific experimental results of Michelson
and Morley that is often described as the crucial clue that led to special relativity. I think that Einstein objected to black holes not because he found them uninteresting but rather for precisely the reason that I and many others find them interesting; they suggested an incompleteness in his beloved general theory of relativity. Physics in the reductive style had served Einstein magnificently well until the 1920s, and he was not so much wrong in trying to continue in this vein as he was in assuming that the appropriate subjects for fundamental research would continue to be what they had been in his youth: gravitation and electromagnetism. He became narrow, endlessly pursuing the false goal of unifying gravitation and electromagnetism, and cut off from exciting work on cosmic rays and elementary particles and quantum field theory that eventually led to the unification of the standard model. His real mistake is one we all risk: he became old.

Much of the criticism of reductionism is really only criticism of reductionism as a program for research. A good example is an argument by the moral philosopher Mary Midgley. In her article in the collection based on the Jesus College symposium, she asks "What, for instance, about a factual statement like 'George was allowed home from prison at last on Sunday'? How will the language of physics convey the meaning of 'Sunday'? or 'home' or 'allowed' or 'prison'? or 'at last'? or even 'George'?" This criticism would strike home if there were physicists who were trying to use physics for such a purpose, but I don't know of any.

It is not just that (as emphasized in this symposium by Atkins and the philosophers Paul and Patricia Churchland) prison and people and thunderstorms are too complicated for us to be able to predict their behavior by following the motion of the elementary particles of which they are composed. It is also a matter of what interests us. The buzzword here is "emergence". As we deal with more and more complicated systems, we see phenomena emerge from them that are much more interesting than a mountain of computer printout describing the motion of each particle in the system ever could be. Mind is a phenomenon that emerges from the biology of complicated anomalies, just as life is a phenomenon that emerges from the chemistry of complicated molecules. We are interested in whether George is happy to be out of jail in a way that is different from our interest in his nerve cells, and we are interested in his nerve cells in a way that is different from our interest in the electrons and protons and neutrons of which they are composed. But phenomena like mind and life do emerge. The rules they obey are not independent truths, but follow from scientific principles at a deeper level; apart from historical accidents that by definition cannot be explained, the nervous systems of George and his friends have evolved to what they are entirely because of the principles of macroscopic physics and chemistry, which in turn are what they are entirely because of principles of standard model of elementary particles.

It is not so much that the reductionist world view helps us to understand George himself as that it rules out other sorts of understanding. Every field of science operates by formulating and testing generalizations that are sometimes dignified by being called principles or laws. The library of the University of Texas has thirty-five books with the title "Principles of Chemistry" and eighteen books with the title "Principles of Psychology". But there are no principles of chemistry that simply stand on their own, without needing to be explained reductively from the properties of electrons and atomic nuclei, and in the same way there are no principles of psychology that are free-standing, in the sense that they do not need ultimately to be understood through the study of the human brain, which in turn must ultimately be understood on the basis on physics and chemistry. Henry Bergson and Darth Vader notwithstanding, there is no life force. This is invaluable negative perspective that is provided by reductionism.
I suppose Midgley might retort that she doesn't know any anti-reductionist philosophers who think that there are free-standing principles of psychology. Maybe not, though many of our fellow citizens still think that George behaves the way he does because he has a soul that is governed by laws quite unrelated to those that govern particles or thunderstorms. But let that pass. In fact, I suspect that Midgley shares the world view of grand reductionism, but holds it "not honesty to have it thus set down".

At any rate, Midgley has to reach in some peculiar directions in her search for horrible examples of reductionism. One of her targets is B.F. Skinner, the late arch-behaviorist and master pigeon trainer. I share her dislike of Skinner's refusal to deal with consciousness in his work. But why does she quote him in a critique of reductionism? I am not aware that Skinner thought very much about science like evolutionary biology or neurology which might provide reductive explanations for principles of psychology. I always thought that Skinner's problem was not reductionism, but positivism, the doctrine that science should concern itself only with what can be directly observed, like behavior. Positivism generally leads away from reductionism; for instance, at the beginning of this century it led the influential Viennese physicist-philosopher Ernst Mach to reject the idea of atoms, because they could not be directly observed.

Perhaps I do know why Midgley chose Skinner as a reductionist villain. Skinner excluded consciousness from his view of the mind, and consciousness poses the greatest challenge to reductionism. It is difficult to see how the ordinary methods of science can be applied to consciousness, because it is the one thing we know about directly, not through the senses. Peter Atkins gave a splendid in-your-face reductionist polemic at Jesus College which I thoroughly enjoyed reading. "Scientists, with their implicit trust in reductionism, are privileged to be at the summit of knowledge, and to see further into truth than any of their contemporaries". Give 'em hell, Peter! But it seems to me that Atkins is not sufficiently sensitive to the problems surrounding consciousness. I don't see how anyone but George will ever know how it feels to be George. On the other hand, I can readily believe that at least in principle we will one day be able to explain all of George's behavior reductively, including what he says about how he feels, and that consciousness will be one of the emergent higher-level concepts appearing in this explanation.

In their articles in the symposium report the neuroscientists Gerald Edelman and Oliver Sacks make what I think is too much of the supposed antireductionist implications of new ideas about the brain. In his article written with Giulio Tononi, Edelman describes his "Theory of Neuronal Group Selection", according to which the brain doesn't operate according to a preset program, but rather to one that evolves through a sort of natural selection during the life of the organism. He then argues in another article in this collection that "the kind of reductionism that doomed the thinkers of the Enlightenment is confused by evidence that has emerged both from modern neuroscience and from modern physics. I have argued that a person is not explainable in molecular, field-theoretical, or physiological terms alone. To reduce a theory of individual's behavior to a theory of molecular interactions is simply silly.... Even given the success of reductionism in physics, chemistry, and molecular biology, it nonetheless becomes silly reductionism when it is applied exclusively to the matter of the mind".

Edelman is a very distinguished scientist, and his "neural Darwinism" may well be a great advance in the theory of the mind; but when he discusses the basis of a scientific world view, I don't see what is the big difference between natural selection over millions of years producing a mental operating system that is fixed at birth or natural selection proceeding over
millions of years and then continuing for a few decades after birth. Neural Darwinism may rule out some reductionist theories of the mind of the sort that are based on analogies with artificial intelligence, but it does not rule out the hope of other thoroughly reductionist views of mentality.

When Edelman says that a person cannot be reduced to molecular interactions, is he saying anything different (except in degree) than a botanist or a meteorologist who says that a rose or a thunderstorm cannot be reduced to molecular interactions? It may or may not be silly to pursue reductionist programs of research on complicated systems that are strongly conditioned by history, like brains or roses or thunderstorms. What is never silly is the perspective, provided by reductionism, apart from historical accidents these things ultimately are the way they are because of the fundamental principles of physics.

Roger Penrose strayed some time ago from his successful research in mathematical physics to think about the mind. As in his earlier books, he argued at the Jesus College symposium that "classical [that is, prequantum] physics seems incapable of explaining a phenomenon so deeply mysterious as consciousness". I gather that Edelman agrees with Penrose because he finds the determinism of classical physics uncongenial. Determinism is logically distinct from reductionism, but the two doctrines tend to go together because the reductionist goal of explanation is tied in with the determinist idea of prediction; we test our explanations by their power to make successful predictions. This must be what Edelman means when he speaks of modern physics (i.e., quantum mechanics) as refuting Enlightenment ideas of reductionism.

Of course, everything is ultimately quantum-mechanical; the question is whether quantum mechanics will appear directly in the theory of mind, and not just in the deeper-level theories like chemistry on which the theory of the mind will be based. Edelman and Penrose might be right about this, but I doubt it. It is precisely those systems that can be approximately described by pre-quantum classical mechanics that are so sensitive to initial conditions that, for practical purposes, they are unpredictable. In quantum mechanics isolated systems are governed by an equation (the Schroedinger equation) whose solutions are strictly speaking fully deterministic, never chaotic. The famous uncertainties in the positions and velocities of particles discovered by Heisenberg do not arise in isolation systems but only when we let such a system interact with a measuring apparatus.

There is another reason for some of the opposition to reductionism, and specifically to the perspective provided by grand reductionism. It is that this perspective removes much of the traditional motivation for belief in God. This is especially true, for example, of one of the great reductionist episodes in the history of science: first Darwin and Wallace explained the evolution of adaptation as a consequence of heredity and natural selection; then twentieth-century biologists explained heredity as a result of genes and mutations; and then Crick and Watson explained the genetic mechanism as a consequence of the structure of the DNA molecule, which with a large enough computer could be explained as a solution of the Schroedinger equation.

Vaclav Havel has described the corrosion of religious faith as a reason for his own reservations about much of science. In a 1987 article he complained that "abolishes as mere fiction even the innermost foundation of our natural world; it kills God and takes his place on the vacant throne....' Then, last year, in a widely quoted speech he added that "we may know immeasurably more about the universe than our ancestors did, and yet it
increasingly seems that they knew something more essential about it than we do, something that escapes us."(5)

On the other hand, some people are attracted to reductionist science for the same reason. This is an old story. Thales' ocean had no room for Poseidon. In Hellenistic times the cult leader Epicurus adopted the atomistic theory of Democritus as an antidote to belief in the Olympian gods. I think that Midgley is absolutely right in arguing that scientists are often driven in their work by motives of this sort. Of course, none of this bears on the question of whether the reductionist perspective is correct. And since in fact it is correct, we had all better learn to live with it.

There is one limitation of the scientific world view that I am glad to acknowledge. Science may be able to tell us how to explain or to get what we value, but it can never tell us what we ought to value. Moral or aesthetic statements are simply not of the sort which it is appropriate to call true or false. I think Midgley would agree, but I am not sure whether Atkins would, and certainly many others would not. According to the British press, the Bishop of Edinburgh recently argued that, since people are genetically preconditioned toward adultery, the Church should not condemn it. Whatever you think of adultery, it is simply a non sequitur to draw moral lessons from genetics. Ronald Reagan made the same silly mistake when he argued that abortion should be banned because science has not yet decided whether the fetus is alive. Whatever definition of life scientists may find convenient, and at whatever point in pregnancy a fetus may start to match that definition, the question of the value we should place on (say) a newly fertilized human egg is one that is entirely open to individual moral judgment. (Not that this is the only issue in the debate over abortion, or even the one that necessarily motivates opponents of abortion.) Science can't even justify science; the decision to explore the world as it is shown to us by reason and experiment is a moral one, not a scientific one.

None of the participants in the symposium at Jesus College seems to have addressed the really urgent problem confronting reductionism: Is it worth what it cost? After all, there are many competing reasons for doing science. Some research (e.g., medicine, much of chemistry) is done for practical purposes, or for use in other fields. Some of it (e.g., medicine again, especially psychiatry and psychology, human evolution) is done because we are naturally interested in ourselves. Some of it deals with things (e.g., the mind again, black holes, superconductivity) that are so weird and impressive that we can't help trying to understand them. Some research is done because we suspect the phenomena that we study (e.g., superconductivity again, turbulence, sex ratios in animal populations) will have explanations that are mathematically beautiful. All of these types of research compete for funds with research that is done because it moves us closer to the reductionist goal of finding the laws of nature that lie at the starting point of all chains of explanation.

The problem facing science is not (as most of the Jesus College symposiasts seem to think) that the reductionist imperative is putting the rest of science at risk. Few if any of us who are interested in the search for the laws of nature doubt the validity of the other motives of search. (I suspect that eventually I will come to feel that research on cancer or heart disease is more important than anything else.) The problem is that some people, including some scientists, deny that the search for the final laws of nature has its own special sort of value, a value that also should be taken into account in deciding how to fund research.

At present, the search for final explanations takes place chiefly in elementary particle physics. But research on elementary particles has become very expensive, because the laws of nature
are revealed more clearly in the collisions of particles in high-energy accelerators than in what is going on around us in everyday life. Cosmology is also important here. As John Barrow reminded the symposium, in order to understand the world we need to know not only the laws of nature but also the initial conditions. Some theories hope that the initial conditions may ultimately be derived from the laws of nature, but we are a long way from that goal. Cosmological research too is very expensive, requiring observatories like Hubble and COBE and AXAF that are carried above the earth's atmosphere on artificial satellites.

For budgetary reason this sort of research is slowly coming to a halt in the US. The Supercollider project was canceled, partly because the arguments that such research is best done at existing laboratories or through international collaboration; but the same Congress that killed the Supercollider went on to cut research funding of other national laboratories, and the present Congress has shown no eagerness to cooperate with Europe in building their next accelerator near Geneva, the Large Hadron Collider.

In the debates over these funding decisions, an important part was and is played by scientists, including some physicists, who oppose spending for elementary particle physics. In part, these scientists take this position because they hope to see this money spent on research in their own fields, a hope that was disappointed when the funds saved by canceling the Supercollider disappeared into the general budget. But also at work is a perfectly sincere lack of appreciation of the reductionist tradition in science, a tradition that in our time is embodied in the physics of elementary particles and fields. It is good that reductionism is discussed by talented people like those who met at Jesus College, but I wish that their discussion could have been more to the point.


(2) This book contains interesting articles on the foundation of mathematics and on the other subjects, which I will not discuss here because I want to concentrate on reductionism in the natural sciences.

(3) Grand and petty reductionism correspond more or less to what the evolutionary biologist Ernst Mayr has called "theory reductionism" and "explanatory reductionism" in his article "The limits of Reductionism", Nature 331 (1987), p.475.

(4) "Politics and Conscience", in Vaclav Havel, or Living in Truth (Faber and Faber, 1987), p.138. I should add that Havel approves of some aspects of modern science: the anthropic principle and the Gaia hypothesis. This is cold comfort to the working scientist; Havel misunderstands the anthropic principle and overrates the Giai hypothesis.