Reductionism Is Not Fundamental

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Science progresses one funeral at a time. The future depends on some graduate student who is deeply suspicious of everything I have said. [1]

Preamble

The wondrous Universe is far richer, more complex, and more interconnected than we humans can imagine. This calls into question our reliance — perhaps over-reliance — on Reductionism in constructing, interpreting, and thus treating as real the models we construct in our attempts to understand and explain the Universe. Reductionism may well not be the fundamental pillar of science as we have come to regard it.

At least since the era of the Renaissance and Galileo, Reductionism has been the modus operandi for Western science, especially in the realm of natural philosophy and physics. Reductionism is nothing more than the breaking down of a complex system or problem into simpler components, then breaking down these simpler components into even-simpler sub-components, in principal ad infinitum. Thus, an intractable problem can be reduced to a series of (hopefully) more tractable sub-problems. In chemistry, for example, macroscopic chemical reactions can be analyzed in terms of molecules; molecules can be analyzed in terms of combinations of atoms; atoms, in terms of nuclei and electrons; nuclei, in terms of nucleons (protons and neutrons) [and perhaps pions and other bosons]; nucleons, in terms of quarks and gluons; etc…

The range and scope of Reductionism is enormous, as shown in Fig. 1, ranging from the cosmos itself through galaxies and stars down to subatomic and perhaps even subquark realms. Also, there are various branches that have to be dealt with, such as biology and nonlinearity and emergent complexity. But — what if there are interactions between or among some of those subsystems? In other words, what if the whole is not equal to the sum of its parts — what if, because of interactions and cross-correlations, the whole is greater (or possibly smaller) than the sum of its components?! Then the entire process becomes suspect, and blind application of and reliance on Reductionism could easily result in erroneous models of the Universe. Reliable models of the Universe would correlate more like a complex series of citrus segments, as suggested by Fig. 2, where different fields of endeavor are only semi-localized and the overall structure requires understanding mutual interdependence. The relatively new fields of nonlinear dynamics and its extreme manifestation, modern chaos theory, have shown this to be the situation across a wide range of sciences, both pure and applied.

Along with cross-correlations themselves, other problems more or less connected with Reductionism need to be investigated. These include: (1) Linearity, necessary for Reductionism. (2) Misunderstanding and/or misapplication of statistics, especially connected with correlated, Bayesian probabilities. (3) Lack of vital interplay — or "checks and balances" — between experiment and theory in certain arenas of modern physics. For the latter I'll present several personal encounters where theory or experiment went awry.

According to a significant, vocal minority of scientists, modern physics has lost its way. In the last few years four impressive books have been published concerning this, and simply from their titles one can get a good idea of the intensity and depth of the criticism: *The Trouble with Physics: The Rise of String Theory, the Fall of a Science and What Comes Next*, by Lee Smolin [2]. *Not Even Wrong: The Failure of String Theory and the Continuing Challenge to Unify the Laws of Physics*, by Peter Woit [3]. *Farewell to Reality: How Modern Physics Has Betrayed the Search for Scientific Truth*, by Jim Baggott [4]. *Fashion, Faith, and Fantasy in the New Physics of the Universe*, by Roger Penrose [5]. The first two are primarily criticisms of string theory by physicists who actually worked successfully in that field. The third is somewhat more general, written by a professor of Chemical Physics who later became a respected science writer. And the fourth, perhaps the most damning of all, was written by the renowned mathematician, who places much of the blame on human folly. Thus, I am not alone in my criticisms and suggestions.

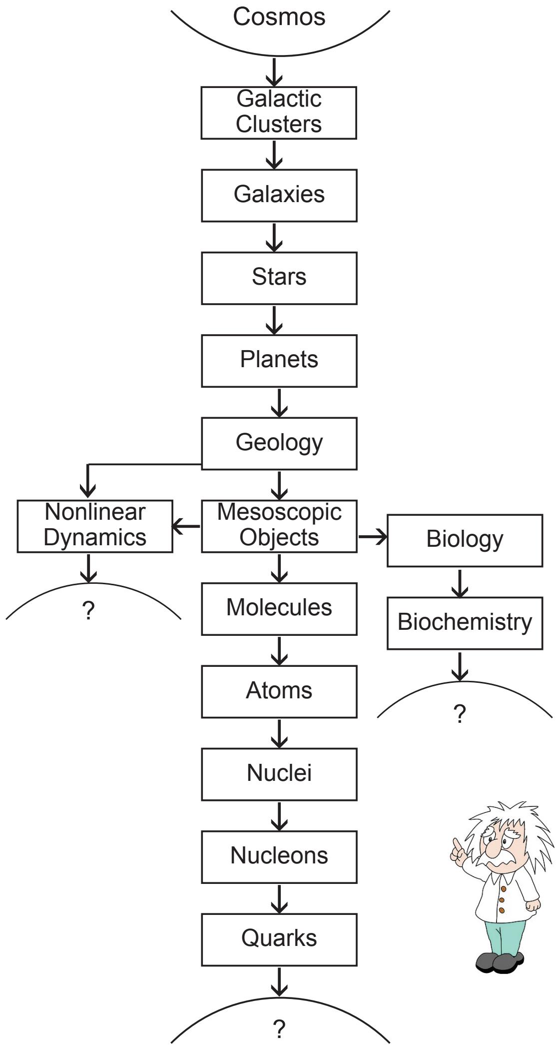

Fig. 1. Stylized scenario for a Reductionist flow diagram.†

Aside from specific critiques of string theory, it would appear that so-called fundamental modern physics could well profit from a critical assessment of some of its current procedures. A striking example is the comparison of the two compendia, "*The New Physics*" [6, 7]. The first, published in 1989, leans heavily toward the theoretical, is highly optimistic about the then current state of physics, and includes several important chapters about novel and progressive topics, including one of the first assessments of modern chaos theory [8]. The second, published seventeen years later in 2006, while an excellent and useful volume, presents relatively little that is strikingly new — its most noteworthy chapters are those on applied physics, demonstrating many impressive experimental accomplishments. However, theoretical progress is far less impressive and repeats many ideas introduced in the first volume. Is it possible that fundamental theoretical physics is spinning its wheels? I must confess that I am undoubtedly biased, coming into science as an experimentalist, then progressing more and more into theory in order to explain my own findings, and finally working almost exclusively on theoretical topics — yet retaining throughout a more or less skeptical experimentalist's outlook. Like many a mathematician who proves a theorem by demonstrating that its negative is false, I'll attempt to answer, or at least examine, the question of what is fundamental by showing that long-accepted procedures are not necessarily fundamental.

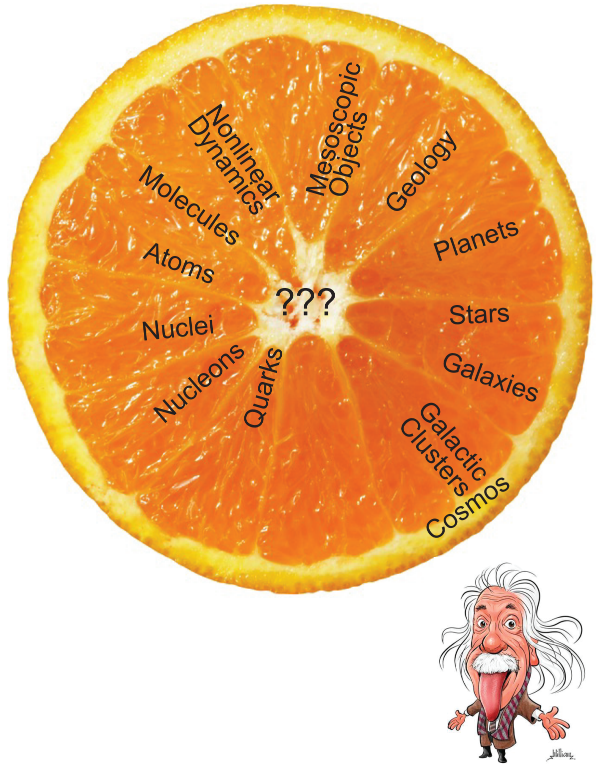

Fig. 2. Stylized scenario for a description of the Universe in which various components have more interactions and cross-correlations than with simple Reductionism.

Nonlinear Dynamics and Modern Chaos Theory

One of the necessary requirements for Reductionism to proceed smoothly is that the various subsystems do not interfere with one another — in other words, that the total system is equal to the sum of its components. This requirement is not met in nonlinear systems. Nonlinear dynamics involves feedback, with multiple cross-correlations, such that the total can easily become greater or less than the sum of its parts. And in *strongly* nonlinear systems (not simply nonlinear perturbations applied to basically linear systems) chaos, its extreme manifestation, can appear. (Ref. [9] is an excellent introduction to chaos theory.)

Chaos is *apparent* random behavior in simple *nonlinear* systems, behavior independent of outside, complex influences. Such systems must be described by quadratic or higher-order equations. And chaotic systems can exhibit the "butterfly effect," i.e., extreme, exponential sensitivity to initial conditions. ("Does the flap of a butterfly's wings in Brazil set off a tornado in Texas.") This random behavior is only *apparent* because there is deep order in chaos — were chaos truly random, it would be of less interest. Indeed, in most chaotic systems regions of (cyclic) order and (chaotic) disorder are intimately entwined, even down to infinite magnifications. The geometry of chaos is that of fractals, which have non-integer dimensionality and whose structures are self-similar (or self-affine) mathematically down to infinite magnification, although in nature this usually breaks down due to microscopic structure. The Mandelbrot and Julia sets are the best known fractals.

Although Poincaré hit upon chaos in the late 1800's while investigating the three-body problem, nonlinear mathematics remained intractable until the advent of modern computers and computer graphics. Thus, chaos remained all but unknown until it was rediscovered by Lorenz in 1963, using an early office computer to model turbulence in the atmosphere with nonlinear equations. Since then chaos has had remarkable success across many disciplines, ranging from traffic patterns and stock market fluctuations to biological and chemical systems and mathematics. Quantum mechanics seems to be the only major holdout.

Nonlinear behavior appears to be ubiquitous in nature! Nonlinear dynamicists are fond of quoting, "Calling most of dynamics *nonlinear* is like calling all of zoology other than the study of elephants *nonpachydermolog*y." So why is quantum mechanics, considered by most physicists to be the most fundamental description of physics, still looked upon as the very epitome of a linear science? For example, the very first chapter of Dirac's *The Principles of Quantum Mechanics* [10], the "Bible" on the subject, is "The Principle of Superposition," applicable only to linear systems.

Perhaps because they did not have access to nonlinear dynamics, much less modern chaos theory, the originators of quantum mechanics necessarily had to force it into a strictly linear mold, where it has remained until today. Linear quantum mechanics, especially in its Copenhagen description, requires a number of paradoxical interpretations, such as the both-dead-and-alive Schrödinger's cat and Einstein's abhorrent "spooky-action-at-a-distance" encountered in most interpretations of Bell-type inequalities. It has difficulties even with the random nature of radioactive decay.

During the last several decades I have tried to show that many of the so-called paradoxes have parallels in nonlinear dynamics and chaos theory ([11-14] and references therein). Nonlinear dynamics and especially chaos theory superficially are every bit as counterintuitive as quantum mechanics, but upon deeper investigation, their peculiarities, like those of relativity, are found to be basically sensible and logical. (Unlike the peculiarities of quantum mechanics, which remain paradoxical.) For example, iteration of even the simplest nonlinear map, the quadratic map, produces exponential behavior, which coupled with exponential sensitivity to initial conditions, apes exponential decay remarkably well. Or, nonergodic distributions in nonlinear systems can result in behavior that looks suspiciously like "spooky action-at-a-distance," although it is nothing of the sort. I would be the last person to claim that I have proven that quantum mechanics is fundamentally nonlinear, but I do think that I have raised important questions concerning its being strictly linear. After all, when one reaches the subatomic level, the measurement process most certainly introduces feedback, i.e., fundamental nonlinearities. This is why quantum mechanics becomes only a statistical rather than an exact description of physical states. Thus, the original interpretation of the Uncertainty Principle rather than Bohr's "state in limbo until you measure it" seems a more reasonable interpretation. Nonlinear elements in quantum mechanics would also eliminate the need for the arbitrary — and ever controversial — positioning of the line between the quantum description of a quantum state and the "classical" observer who instigates the so-called collapse of the wave function.

Questionable Applications of Statistics

All of us in principal agree with Born that quantum mechanics yields a statistical description of the data, nothing more. This means that the wave function must be interpreted statistically: The square of the wave function gives the probability of finding a physical system in a particular quantum state. If the wave function consists of a linear combination of eigenstates, then because of the orthogonality of the eigenstates, all cross terms drop out and the squares of the coefficients give the probabilities for finding the system in each respective state upon measurement. This is where the Schrödinger's cat argument comes in, according to the orthodox Copenhagen interpretation. Before measurement the quantum state is in a sort of "limbo" (the cat both/either alive and/or dead!), and upon measurement, the wave function collapses into one or the other, with the designated probability.

But it does not have to be this way. If we really believe that quantum mechanics is a statistical description, then it applies only when there are enough examples to be statistically meaningful. It cannot and does not apply to individual cases. What this means is that if, say, a nucleus can be either in its ground state or in an excited state at 1 MeV above ground, and its wave function has the coefficients $\sqrt{(1/3)}$ for the ground state and $\sqrt{(2/3)}$ for the excited state — this does not mean that an individual nucleus is in some virtual non-eigenstate somewhere between the two (a state not allowed in this particular quantization). What it does mean is that, given a large number of such nuclei, upon measurement statistically 1/3 of them will be found to be in the ground state and 2/3 of them in the excited state.

Similar questionable reasoning can be found in the analyses of many Bell-type systems. A Bell-type inequality imposes a limit on the statistical correlations between "entangled" pairs, two objects that have interacted and as a result retain correlations even after having been separated by considerable distances. Quantum mechanics violates such inequalities, indicating "abnormal" correlations — quite often interpreted as Einstein's spooky action-at-a-distance. (A discussion of Bell-type experiments lies beyond the scope of this essay, but the reader can find detailed explanations in the books, *Quantum [Un]speakables* [15], or references in my previous FQXi essay [14].) Bell-type experiments require the collection of large, statistically meaningful sets of data, but analysts are wont to apply statistical conclusions to individual pairs.

Most practicing scientists, even mathematicians, are not conversant with Bayesian statistics or statistics that contain subtle correlations. A simple but vivid example of this was the hullabaloo raised several decades ago over the so-called Monty Hall paradox. In the TV game show, "Let's Make a Deal," a contestant was shown three closed doors. Behind one was a sports car, and behind the other two were goats. He/she could choose a door and get to keep what was behind it. After the contestant chose a door but before it was opened, the host (Monty Hall) would open one of the other two doors, showing a goat behind it. Then came the kicker: The contestant could open either the original door or switch to the third as yet unopened door. Could he/she improve the odds of winning the sports car by switching?

"Common sense" says it shouldn't make any difference — the odds were simply 1/3, 1/3, 1/3. Yet it turns out the contestant will double his/her odds by switching. A hullabaloo followed this announcement [16], with several respected math professors proclaiming this could not be the case. The answer was — correlated statistics. If the host opened his door at random, then the odds would indeed remain even. However, the host was forced to open a door behind which was a goat! A not so subtle correlation. This shifted the odds.

A perhaps more sober example comes from my experience in nuclear science. Gamma-ray spectroscopy is a powerful tool in nuclear spectroscopy. One can measure the energies of the gamma-rays emitted by a radioactive nucleus and deduce the energy levels, i.e., the structure of its daughter nucleus. The trouble is, these gamma-rays do not come with tags saying, for example, "I de-excite level 5 down to level 3." In the early days of nuclear spectroscopy it was common to use "sums and differences" as an aid to placing gamma-rays in their proper places. For example, if you knew that gamma-ray *A* did de-excite level 5 down to level 3 and gamma-ray *B* de-excited level 5 down to an intermediate level 4, then it would be very tempting to place gamma-ray *C*, having energy = level 4 – level 3, between levels 4 and 3. Alas, such logic led to all sorts of incorrect decay schemes, since *C* might actually de-excite some level not even in this sequence! It turns out the only way to be sure of such a placement is to perform a "coincidence" experiment, i.e., use two detectors and electronically prove that gamma-ray *C* followed gamma-ray *B* within some designated (usually short) time span. The trouble is, the energies of excited states in a nucleus are anything but random numbers. There are all sorts of subtle (and usually unknown) correlations among these numbers, and one can get into serious trouble by not recognizing this fact.

Theory Out on a Limb

Over the centuries one of the mainstays of the Scientific Method has been the interplay between experiment and theory. This has proven to be an important set of checks and balances. When theory gets too far ahead of experiment, it can lead to fantasy. Without theory, however, experiment can become sterile and unimaginative. Each needs the other for meaningful progress to emerge.

According to the four books cited near the beginning of this essay, theory has indeed advanced so far beyond experiment, especially in the realms of cosmology and elementary particle physics, that it has lost sight of reality. I agree with this assessment, but I think it is not a recent problem but one that has been developing over a long period of time. And it has to do with the over-compartmentalization of science, especially in physics.

Here I should give the reader a glimpse into my own background — and prejudices. Most of my career I was what is known as a "nuclear chemist," a somewhat schizophrenic hybrid that came into being during and after World War II, when physicists couldn't perform the complicated chemical analyses required by the fact that fission produces half the Periodic Table of Elements in microscopic, highly-radioactive quantities. Nuclear chemistry thus developed as a complement to nuclear physics. We do the same kind of research as nuclear physicists but with a chemistry background and outlook. I received my BA in Chemistry from Oberlin College, my PhD from the University of California, Berkeley (in the turbulent 1960's!), and came naively right out of graduate school to the brand new Cyclotron Laboratory at MSU, with joint appointments in the Chemistry and Physics/Astronomy Departments.

For forty-plus years I worked in nuclear science at MSU's National Superconducting Cyclotron Laboratory, much of the time in experimental gamma-ray spectroscopy. I quickly discovered that most of us in experimental nuclear science, whether from MSU or other labs around the world, had only a casual relationship with theory. After all, as experiments became more and more complicated and sophisticated, little time or energy was left to delve very deeply into theoretical conclusions and implications. Selection rules were applied superficially, with but little insight into the nuclear structure involved, and the few calculations performed consisted mostly of the use of weakly-understood canned computer programs run primarily to present pretty pictures of "experiment vs theory." And theorists were of limited help, for they tended to speak a different language, with only modest understanding of exactly what the experimentalists were up to. Communication was more intra- than inter-.

Thus, gradually I found myself having to develop more and more of my own theory, until I was spending as much as time on theory as experiment — and since 2000 I have done very little with nuclear science, working instead with the influence of nonlinear dynamics and chaos on quantum mechanics. I do not consider myself a bona fide theorist but merely an experimentalist who does theory.

Several examples should give a clearer, if limited (to nuclear science) insight into problems introduced by such a lack of communication.

The first concerns both the detection and interpretation of rotational bands in deformed nuclei. Because nucleons must produce their own potential in nuclei — as opposed to nuclei supplying most of the Coulomb potential for electrons in atoms — nuclei far from closed shells, i.e., having many nucleons in only partially-filled shells, become deformed. As a result, they can have excited states in rotational bands, analogous to the way in which diatomic molecules produce rotational bands. These rotational bands de-excite by emitting copious gamma-rays in cascade. Study of these bands has become a favorite for *in-beam* gamma-ray spectroscopy — detecting gamma-rays as the nuclei are being produced by an accelerator beam rather than having to observe them from the decay of stationary radioactive nuclei, which with their lower excitation energies (and strict beta-decay selection rules) produce far fewer gamma-rays. Indeed, hundreds of rotational bands have been observed and published, many having twenty, thirty, or even more members. This all makes for impressive, dramatic figures depicting level schemes in journal articles.

The difficulty is, these rotational bands are all based on so-called "intrinsic" states, i.e., states involving a single or a few nucleons occupying specific nuclear orbitals. If the base state of a rotational band is an excited state, as most of them are, then it has to decay eventually via a far less predictable, usually lower-energy gamma-transition, which can be difficult to detect and characterize. Thus, one has to trade off tedious, difficult experiments producing a few pieces of data for much easier experiments producing spectacular amounts of data. As a result there is a plethora of "floating" rotational bands in the literature — bands that have not been connected to lower-lying states, or perhaps to reality.

This has not prevented the appearance of numerous articles toying with superficial theory. After all, it can be a lot of fun playing with the parameters to fit rotational bands, including centrifugal stretching, Coriolis-force distortions and decouplings, and all sorts of other goodies, including "superdeformation" (nuclei stretched out far beyond what one might normally expect). I have heard many a lecture describing the shape of a specific nucleus as, say, resembling the "depicted watermelon with four extra bumps at each end." This, when the coefficients of the spherical harmonics used to describe a rotational band are comparable with or smaller than the statistical error bars in the experiment! (A good entrance into this literature can be found in [17].)

Another example, based on superficial understanding of a theoretical concept: Several decades ago, "pseudo-spin" was a hot topic in nuclear spectroscopy, treated as a physically real property. This pseudo-spin (not to be confused with other uses of the term, such as the pseudospin of graphene) was originally introduced as a convenience in calculating states of electrons in atoms. For example, when dealing with *f* electrons (orbital angular momentum, $l = 3$), one has to diagonalize at least 7X7 matrices for the orbital angular momentum, but only 2X2 matrices for the intrinsic spin. By using some clever tricks and pretending the electrons have pseudo-spin 3/2 rather than intrinsic spin 1/2, this can be evened out, making calculations much more tractable. When confronted with the much higher spins encountered for nuclear states, the procedure becomes even more appealing, and a number of papers were published on the subject. I once attended a Gordon Conference where scientists who should have known better gave talk after talk diagnosing the properties of "physically-real" pseudo-spin in various nuclei.

Of course, theorists can be equally culpable. Too often they proceed from equation to equation without stopping to enquire what those equations have to do with physical reality. The classic case of this was parity nonconservation in weak interactions. Ever since its relativistic formulation, the theory of weak interactions and beta decay contained quantities known as pseudoscalars, the dot products of axial and normal vectors, which change sign upon reflection. However, nobody had questioned what these pseudoscalars corresponded to physically. Lee and Yang [18] pointed this out, C. S. Wu performed the experiment showing that beta decay had a preferential (left-handed) direction, and the rest is history. (Incidentally, some classical, odd-order systems, such as iterating a cubic map, also have an asymmetry, so weak interactions are not the only ones to violate parity conservation.)

To return to the present. Much of modern theoretical physics, especially in elementary particle physics and attempts to formulate quantum gravity and perhaps resolve the fact that quantum mechanics and relativity remain irreconcilable — much of this, including string theory and its offshoots such as *M* theory, have crawled far out on a lengthy, fragile limb. Far from being possible to test by present day experiments, they are conceptually impossible to test by any foreseeable experiments. Requiring examination at impossibly small Planck distances and incredibly large Planck energies, they are essentially unfalsifiable. This represents a complete breakdown of feedback between experiment and theory. I need not expound on this here, for the reader can find more than enough information in the entertaining and emotionally charged books previously cited.

Conclusion

Reductionism is not fundamental. Nature — and the Universe — is.

The three previous sections have attempted to touch upon this. Much, if not most of nature is nonlinear and involves feedback, making it questionable, if not impossible, to break down models and descriptions of it into ever smaller, noninteracting subsections ad nauseam. Questionable applications of (effectively linear) statistics aids and abets our unthinking dependence on Reductionism. And all of this is compounded by a breakdown in interplay between experiment and theory.

As I have already mentioned, I am a relative "come-lately" to learning and applying serious theory. This has disadvantages — but advantages, as well, for as a mature scientist I could follow arguments with a healthy degree of skepticism. And, to tell the truth, I found that most theorists relied on models and math to a fault, even in basic derivations. For example, why not say that such and such a result came about because of conservation of energy and independence (orthogonality?) of the states rather than because of the (abstract) unitarity of the operators?

Wigner's essay, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences" [19], is often quoted as an excuse for following mathematics blindly. Yet it contains admonitions against this, effectively saying that, as effective as mathematics has been, one should apply it with open eyes to each physical situation. A later essay by Smolin [20] goes much further, pointing out that there is not necessarily a one-to-one mapping between mathematics and every physical problem. Platonist or naturalist, whether we regard mathematics as a separate entity or as an innate component of nature, we all should take this to heart.

Remember, with enough degrees of freedom — with an excess of free parameters to be determined in an equation — we can make almost any theory *quantitatively* represent any physical problem. The Ptolemaic system of epicycles survived for centuries because it was capable of representing — and predicting! — astronomical motions and positions. We sneer at it nowadays partly because it is so cumbersome. The Copernican theory is so much more elegant — and real.

I am all for Occam's Razor and seeking beauty in a mathematical theory. I would prefer a simple, beautiful theory any day over a messy, cumbersome one. But simplicity and beauty alone are not sufficient. Nature appears to oscillate between simplicity and complexity, between neatness and messiness. Nonlinear dynamics — and most certainly chaos — are not so compact and elegant as linear, simple Reductionism. But then quantum mechanics itself is a contender for being one of the messiest, most counterintuitive disciplines yet invented!

Some contemporary theorists declare that their mathematics is so elegant and beautiful that applying it must be the correct thing to do. But remember, math can develop multiple scenarios, and it takes considerable physical insight to know which to apply to a particular physical situation. A few theorists even go so far as to say that experimental proof of their theories is irrelevant. And some invoke philosophical arguments, especially in regard to the anthropological principle. To quote several philosophers I have known: "Most physicists do not make particularly good philosophers."

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