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The Gaia Hypotheses: Are They Testable? Are They Useful?

The reader may wonder why, in my title, I refer to the Gaia hypotheses in the plural. I do it because I think that many logically different theories have been put forth under the single banner of "the Gaia hypothesis." Perhaps the Gaia hypothesis is all things to all people, but the differences between these theories are both subtle and crucial. I suspect that a lot of debate has resulted from a simple misunderstanding of which of the multiple hypotheses is on the table at any one time. In the interests of clarity and precision, I propose the following taxonomy of the Gaia hypotheses:

A Taxonomy of the Gaia Hypotheses

Influential Gaia. The weakest of the hypotheses (here I use "weak" and "strong" in reference to the extremity, not the plausibility, of the hypotheses), the influential Gaia theory asserts simply that the biota has a substantial influence over certain aspects of the abiotic world, such as the temperature and composition of the atmosphere.

> The Gaia hypothesis . . . states that the temperature and composition of the Earth's atmosphere are actively regulated by the sum of life on the planet (Sagan and Margulis, 1983).

Coevolutionary Gaia. The coevolutionary Gaia hypothesis asserts that the biota influences the abiotic environment, and that the environment in turn influences the evolution of the biota by Darwinian processes.

> The biota have effected profound changes on the environment of the surface of the earth. At the same time, that environment has imposed constraints on the biota, so that life and the environment may be considered as two parts of a coupled system (Watson and Lovelock, 1983).

Homeostatic Gaia. The homeostatic Gaia hypothesis asserts that the biota influences the abiotic world, and does so in a way that is stabilizing. In the language of systems analysis, the major linkages between the biota and the abiotic world are negative feedback loops.

> The notion of the biosphere as an active adaptive control system able to maintain the earth in homeostasis we are calling the 'Gaia' hypothesis (Lovelock and Margulis, 1974a).

Teleological Gaia. The teleological Gaia hypothesis holds that the atmosphere is kept in homeostasis, not just by the biosphere, but by and for (in some sense) the biosphere.

> . . . the Earth's atmosphere is more than merely anomalous; it appears to be a contrivance specifically constituted for a set of purposes (Lovelock and Margulis 1974a).

Optimizing Gaia. The optimizing Gaia hypothesis holds that the biota manipulates its physical environment for the purpose of creating biologically favorable, or even optimal, conditions for itself.

> "We argue that it is unlikely that chance alone accounts for the fact that temperature, pH and the presence of compounds of nutrient elements have been, for immense periods of time, just those optimal for surface life. Rather we present the 'Gaia hypothesis,' the idea that energy is expended by the biota to actively maintain these optima" (Lovelock and Margulis, 1974b).

This is just one taxonomy of the Gaia hypotheses. One can take issue with my way of classifying them. It can be done many other ways, but I think most will agree that it must be done somehow, because (as the examples above make clear) different people mean different things when they use the same words. Sometimes even the *same* people appear to mean different things when they use the same words. Some of these claims are relatively weak (as

(Author's note: A substantially refined and expanded version of this contribution appeared in the May, 1989 *Reviews of Geophysics* (Kirchner, 1989). That paper makes some points more carefully and precisely than this one, and addresses geophysiology, Daisyworld, and Lovelock's newest book, which was in press at the time of the conference. The version presented here, by contrast, has the advantage of saying things more plainly, and better revealing one practicing scientist's difficulties with Gaia.)

in the influential or coevolutionary Gaia theories, which seem to state just that the biota and the physical environment have something to do with one another), and others are, of course, quite strong stuff. If we all talk about "the Gaia hypothesis," without specifying *which* Gaia hypothesis, we can create a lot of confusion.

This confusion can appear in different guises. One of the most serious lies in claiming that evidence for one of the weaker versions of the hypothesis somehow proves the much stronger versions of the hypothesis as well. Some believe, as I do, that the biota affects the physical environment. Some also think, as I do, that the physical environment shapes biotic evolution. Those holding these views are in good company, because scientists have thought these things for over a hundred years. So if I were asked whether I believed in the Gaia hypothesis, referring to *that* Gaia hypothesis, I would say that I do. But does that mean that I believe that the biota is part of a global cybernetic control system, the purpose of which is to create biologically optimal conditions—that is another matter entirely.

Weak Gaia Is Not New

Some might be surprised at my statement that scientists have believed in Gaia—believed in "weak" Gaia, believed that life shapes the physical environment—for over a hundred years. We have all become accustomed to reading that the Gaia hypothesis is a radical departure from the earlier view that the biota simply responds to a fixed physical environment. If that is a radical departure, then some people have been radically departing for a very long time. Consider T.H. Huxley. In 1877 he wrote what could be considered to be the very first textbook in physical geography. In it he wrote, "Since the atmosphere is constantly receiving vast volumes of carbonic acid from various sources, it might not unnaturally be assumed that this gas would unduly accumulate, and at length vitiate the entire bulk of the atmosphere. Such accumulation is, however, prevented by the action of living plants" (Huxley, 1877).

So a century ago Huxley thought the biota was responsible for the chemical disequilibrium of the atmosphere. He not only thought this, but he also thought it was elementary enough, and obvious enough, to put it into a textbook.

Thirty years earlier, Huxley's compatriot Herbert Spencer wrote about the same phenomenon. He not only thought that the biota had shaped the earth's atmosphere; he also thought that changes in the atmosphere had charted the course of evolution (which Spencer called "progressive development," Darwin's *Origin of Species* being still in the future). Spencer called his theory

. . . an entirely new and very beautiful explanation of the proximate causes of progressive development . . . not only do the organisms of the vegetable kingdom decompose the carbonic acid which has been thrown into the atmosphere by animals, but they likewise serve for the removal of those extraneous supplies of the same gas that are continually poured into it through volcanos, calcareous springs, fissures, and other such channels . . . Assuming then that the present theory, supported as it is by the fact that the constituents of the atmosphere are not in atomic proportions, and borne out likewise by the foregoing arguments, is correct, let us mark the inferences that may be drawn respecting the effects produced upon the organic creation. . . .

"If rapid oxidation of the blood is accompanied by a higher heat and a more perfect mental and bodily development, and if in consequence of an alteration in the composition of the air greater facilities for such oxidation are afforded, it may be reasonably inferred that there has been a corresponding advancement in the temperature and organization of the world's inhabitants" (Spencer, 1844).

In other words, Spencer held that the emergence of green plants produced our present abundance of oxygen, and that oxygen made the evolution of higher animals possible. The biota, in other words, shaped the physical world in a way that seems fortuitous for the course of evolution.

My contention is not that Spencer was correct (his view is simultaneously grandiose and simplistic), but that he was, in a sense, *Gaian*. His theory has the key elements: the biota alters the physical environment, which in turn shapes biotic evolution. Indeed, his theory sounds surprisingly similar to contemporary Gaian treatments that portray the creation of earth's oxidizing atmosphere as a cathartic event, necessary for the further progress of evolution.

I certainly do not claim familiarity with the whole history of the evolution of such ideas. The fact that I could find these two "Gaian" references in an afternoon of library browsing, however, suggests to me that such passages may be relatively common. Indeed, the whole field of biogeochemistry, although more cautious in its speculations, is centrally concerned with the same biotic interactions that Gaia alludes to.

So the first two statements of the Gaia hypothesis—what I have labeled *influential* and *coevolutionary* Gaia, respectively—have a long history. This weak hypothesis has such a long history, indeed, and seems so intuitively plausible, that it seems odd to call it a hypothesis at all. Rather than a theory, it seems to be simply an observation that the physical and biotic worlds have something to do with one another. We can, of course, argue about the relative importance of these interactions.

Thus those who believe in the weak forms of the Gaia hypothesis are carrying on a long and honorable scientific tradition, but one so long, and so honorable, that it may deprive them of the pleasure of being part of a revolution in scientific thought. But what about the stronger versions of the hypothesis? Are they testable, and are they useful? Before I address that question, I must briefly review a bit of basic epistemology.

Criteria for Testability

Much of the debate surrounding any scientific theory, including the Gaia theories, consists of finding and weighing the evidence, for and against. This is the day-to-day business of scientists, and of scientific conferences like the San Diego meeting. We call it testing a theory.

But not every theory can be tested. Now, as a matter of strict logic, a theory that is untestable is far worse than one that is merely false. A false theory, once known to be false, at least helps restrict the sphere of possibilities. It teaches us something, namely that the truth lies elsewhere. Testing an untestable theory, on the other hand, is simply a waste of time. So true, false, and untestable theories are, respectively, "the good, the bad, and the ugly." What must a theory be, to be testable?

First, it must be well defined. Its meaning must be clear and its terms must be unambiguous. Second, it must be intelligible in terms of observable phenomena of the real world. Finally, it must not be tautological. That is, it must not be true simply by definition. Equivalently, it must not encompass all logical possibilities. It must be logically possible for the theory to be false, and there must be some conceivable fact that, if it were in fact the case, would prove the theory false. This is what separates empirical hypotheses from pure logical deductions.

A tautology is a theory that is true no matter what the facts are. A theory should be logically consistent, but it should not be completely airtight; it has to let a little empirical truth in at some point.

Metaphors

Metaphors constitute a whole class of untestable theories. If Shakespeare tells you that "all the world's a stage," could you test his hypothesis? I doubt it. What would you measure or observe to tell whether the world is a stage? What would a world that is *not* a stage look like? If you could complain to Shakespeare about the ambiguity of his metaphor, he might reply, "OK. The entire world is made of wooden flooring, and at the edge of the earth you'll find a few footlights." Now you have a hypothesis. You can now go out and very quickly verify that the world is not a stage, at least in that sense. But of course, in some more poetic sense, the world is indeed a stage. That is what makes metaphors so inviting; at the same time that they are literally false, they are figuratively true.

A metaphor makes a poor hypothesis because it does not specify *in what sense* the metaphor is true. Showing that the world is a stage in one sense does not prove it is a stage in any other sense. Now, "All the world's a stage" sounds a lot like "All the world is a global organism," and some have indeed claimed that the Gaia hypothesis is just a metaphor. My point is not that metaphors are useless—they inspire fruitful speculation—but that they are untestable. Treating a metaphor as a scientific proposition that is factually true or false is simply a waste of time.

Now, some may think that I'm being a terrible spoil sport, that I am far too serious about what should be considered just a metaphor, and that I take the whole Gaia hypothesis far too literally. Perhaps I do. But if Gaia is just a metaphor, why do we keep referring to the Gaia *hypothesis?* Why to we keep talking about *evidence for* or *proof of* the Gaia hypothesis? If it is a metaphor, why do we talk about it as if it were a scientific proposition, as if it were either true or false?

Criteria of Usefulness

Besides testability, another fundamental issue to consider is usefulness. Some theories, although coherent and perhaps even true, are simply not useful in furthering scientific progress. Theories are useful to the degree that they are distinct from related theories. If a hypothesis simply restates other tried-and-true theories, or can be logically derived from them, why bother testing it?

The second major criterion of usefulness is predictive or explanatory power. Theories are useful in proportion to the phenomena they can predict or explain, and—perhaps more importantly—in inverse proportion to what they force you to assume. This is simply Ockham's Razor: *all else equal,* choose the theory that burdens you with the least baggage of unverifiable assumptions. If two theories explain the same data, reject the one that forces you to assume the most. Note that Ockham's Razor does not say that all simple theories are better than all complex ones. It simply says that one should not invoke extraordinary assumptions to explain phenomena that can be understood more straightforwardly.

If I have any quibble with weak Gaia, it is on these grounds. It is not clear that Influential Gaia or Coevolutionary Gaia say anything that was not already said by Huxley, Darwin, and others of their age. Does Gaia say anything new? If not, is there any advantage to restating tried-and-true theories in Gaian language?

The same point can be raised with respect to the strong versions of the Gaia hypotheses, to the extent that they claim to explain why the physical environment and the biota are well matched. Darwin said a long time ago that the biota fits the physical environment well. Gaia reverses the statement, and says that the physical environment suits the biota well. Is there any advantage in standing poor old Darwin on his head? And is that advantage great enough to justify the assumptions we have to make? Natural selection—without any of the embellishments that Gaia offers—explains why the environment and the biota are well matched. Organisms suited to a different environment, having been wiped out long ago, are no longer part of the biota, to which the current environment seems so well suited. Why invoke a global cybernetic control system to explain the good fit of biota to environment, if you can invoke simple natural selection instead?

Homeostatic Gaia

I shall now turn for a moment to homeostatic Gaia, which claims, in essence, that the biota is vital in maintaining the long-term stability of the physical environment. What is stability? Does it mean resistance to change, resilience under change, or bounds on the magnitude of change? The experience of ecologists in the debate over complexity and stability shows that it is hard enough to pin down the meaning of stability or homeostasis in the case of a neatly bounded ecosystem; it is harder still when the bounds are the entire biosphere. One could precisely define homeostasis, but it has never been done. So the first problem is one of definition.

There are many interrelationships between the biota and the physical environment (that is, many feedback loops). Given that any feedback loop must be either stabilizing or destabilizing, it should come as no surprise that some of them are stabilizing. The Gaia hypothesis has prompted a lot of efforts to look for biological mechanisms of homeostasis, and there are some outwardly plausible candidates.

But we should not just look for confirmatory evidence. We should be cautious in characterizing the putative stability of a paleoclimatic record that is sketchy and ambiguous, one whose error bounds could hide quite a bit of instability. More to the point, without knowing what destabilizing biological mechanisms may also be at work to undermine homeostasis—and there is every reason to believe that there are some, and that some are potent—it is impossible to make a balanced assessment of the role of the biota.

Even the most passionate advocates of Gaia will admit that the biota was once one of the most destabilizing forces on earth. The biota was responsible for the drastic shift in the earth's redox potential in the Precambrian period (a shift that made most of the earth uninhabitable for the anaerobic organisms that precipitated it). Indeed, some accounts claim that this event is evidence of the power of the biotic world and the resilience of Gaia.

But there is a fundamental problem here. If the most destabilizing period in earth's history can be cited as evidence for Gaia, and the apparent stability since can also be cited as evidence for Gaia, I'm left wondering what conceivable events could not be used as evidence for Gaia. If Gaia stabilizes, and Gaia destabilizes—those are the only two possibilities—then is there any possible behavior that is not Gaian? Is Gaia, then, simply a theory so flexible (and, by implication, free of specific empirical content) that it can be wrapped around any conceivable paleoclimatic record?

Anyone attempting a Gaian interpretation of earth's history must think hard about this. And it won't do to say that the Precambrian blue-green algae were not Gaian because they were so violently destabilizing. Such a statement is blatantly tautological. It defines Gaia as stabilizing interactions and then asserts that Gaia has a stabilizing effect. Anything defined to be homeostatic has to be stabilizing . . . there would be no other possibility, so there would be no testable hypothesis.

Teleological Gaia

Teleological Gaia asserts that the biota controls the environment, and does so for a purpose. There is a definitional problem here; the purpose of the putative biological control mechanism has never been defined.

A claim that the atmosphere is a "contrivance specifically constituted for a set of purposes" (Lovelock and Margulis, 1974a) is ill defined without a statement of what the purposes are. This criticism may seem silly, and the purposes may seem perfectly obvious. Clearly, the atmosphere has a number of biologically important functions. Surely the function of the atmosphere is the purpose it was contrived for.

There is a subtle, but serious, error in such a line of reasoning. It is this: if all you know is that the atmosphere functions in some way, how can you say it was contrived? How do you know what its intended purpose was? If you say its intended purpose is the function it serves, then how would you ever know if anything was *not* contrived? Everything has some function, after all. Purpose and function coincide only in contrivances that work well; whether the atmosphere works well, or is contrived at all, is precisely the question at hand. Without an independently defined purpose, teleological Gaia simply says that the atmosphere serves the purpose of doing whatever the atmosphere does.

Optimizing Gaia

The theory I have termed "optimizing Gaia" tries to solve the problem of definition by stating what Gaia's purpose is: Gaia's purpose is maintaining a biologically optimal physical environment. In solving that definitional problem, it creates another. What is optimal for the whole biosphere? We can define an optimal environment for an individual organism in many ways, but what would be optimal for a blue-green anaerobe, a chimpanzee, a pine tree, and a penguin, taken together? Nor does dismissing the notion of optimality, and simply claiming that Gaia creates biologically favorable conditions, solve the problem. What would be favorable, let alone optimal, for the biota, a vast collection of diverse organisms with different, and even conflicting, requirements?

Would it be "better" for the whole biosphere to have more species, more biomass, or more productivity? No matter what the answer, the next question is unanswerable: Why should that be better?

One might respond that what we have now is optimal. But if what is optimal is simply defined by what exists, what content is left in the idea of optimality? The theory boils down to "Gaia created and maintains the world we have now, which is, of course, optimal." (Nor does the simple fact of life's persistence on Earth—great extinctions and all—prove that Gaia maintains biologically favorable conditions. Gaia must mean not just that life did persist, but that it could not fail to persist. Would the environment of a nongaian earth have been "unfavorable" enough to sterilize the planet?)

Thus it is hard to define what we mean by optimality. But we must define it, for as long as the criterion of optimality remains unspecified, optimizing Gaia is clearly a tautological theory, in the rigorous sense that it includes all logical possibilities and does not exclude any possible data. It is a basic theorem of operations research that for any behavior of a system, there is some objective function which that behavior optimizes. For any given behavior, I can write a function that the behavior maximizes. Every conceivable environment is optimal for something, as long as one has complete freedom to specify what the "something" is.

So the concept of Gaian optimization needs a lot of work to save it from tautology. But there is another serious problem. Gaian optimization is internally contradictory. Stability and optimality (for the agent supplying the regulatory mechanism) are mutually exclusive. If an organism is keeping a system stable, the stable point cannot be optimal for the organism. We can see why by looking at the Daisyworld model, in which plants regulate the temperature of a theoretical planet by changing its reflectance.

Consider a world with only white daisies. The daisies keep the temperature stable because if the temperature or solar flux rises above the stable point, more daisies grow, the surface becomes whiter, and the albedo increases. But that means that at a higher temperature, there would be more daisies. A higher temperature would be "better" for white daisies, and the daisies' response *prevents* a temperature increase that would be favorable for them. At the temperature that is optimal for the daisies, there is no stability. At the optimal point, any change in temperature decreases the number of daisies. So if the temperature increases, daisies die, and the temperature increases still further. More daisies die, and the temperature increases still further. And so on. The optimum will be reached only in an unstable transition between the stable suboptimum and total extinction.

What I have described is true of both colors of daisies, and indeed is not specific to the Daisyworld model. You can demonstrate it as a purely mathematical proposition. It is completely general. It is a straightforward theorem of systems analysis that no homeostatic system can be stable at a point that is optimal for the component supplying the homeostasis. If the biota regulates the atmosphere, the atmosphere cannot be optimal for the biota.

Besides, what do we gain by assuming that Gaia has a purpose, or that Gaia optimizes? What more can we predict or explain? If we make such extreme assumptions, but do not gain any explanatory power, Ockham's Razor will slice us to ribbons.

Summary

Some may be either baffled or irritated by the discussion I have presented. Some may be thinking, "Oh come on. I'm just interested in exploring the connections between the biotic and abiotic worlds, and there's nothing wrong with that. My hypothesis is just that the organisms of the biota influence their local environments, that the sum of these influences can be globally significant, and that organisms evolve by chance and are selected by Darwinian processes, in terms of where they survive, whether they survive, and what their characteristics are."

I think that is a great starting point for illuminating research. It probably explains all that the more extreme Gaia hypotheses do, without invoking global entities, imputing teleological intentionality, or assuming optimal control. It is testable at many scales, from the laboratory to the globe. And in its basic outline it is almost certainly correct. Those holding that view are in good company, and are carrying on an honorable scientific tradition that is at least a century old.

On the other hand, those who think that the idea of a global organism is an intellectually appealing metaphor, but not a rigorous scientific theory, will only distract their colleagues by talking about it as if it were a hypothesis that could be tested or proved.

Some think that Gaia is the stabilizing interaction of the biotic and abiotic worlds. That is an interesting possibility. Given that stabilizing and destabilizing interactions are the only two choices, in any particular case there is at least a 50% chance that this theory is correct. Indeed, if Gaia were violently destabilizing throughout the earth's history, we probably would not be here to carry on this debate. In any event, we should explore all the links between the biotic and abiotic worlds . . . not just those that agree with a particular theory. The destabilizing feedbacks are important too.

Does Gaia have a purpose? Does Gaia maintain optimal conditions for life? I do not think these theories are testable. Nobody will be able to test such theories until Gaia's purpose is defined and the meaning of optimality is specified. And nobody will be able to test such theories until it is clearly stated what conceivable result of an experiment could possibly prove them false.

Addenda and Errata

After presenting this paper, I received a number of thought-provoking questions that have made it clear that some issues I addressed needed to be discussed further, and more precisely.

My Central Concern with Gaia

A number of people suggested that I was expecting far too much, that the nature of the hypothesis and the system itself make it unrealistic to expect that the question be answered after only a decade of work. I am not complaining that the question has not been answered, but that it has not yet been asked in a scientifically meaningful way. The central problem is not a lack of information (though good data are by no means abundant here) but a lack of something to do with the information. Until we can frame a scientifically coherent and significant question, we will not know what the answer means, or even whether we have found it.

Why Untestable Hypotheses Are Ugly

Steve Schneider asked why untestable hypotheses are "ugly." He suggested that the nuclear winter theory, although not testable, was useful in molding our approach to international security.

There are two types of untestability. Theories about nuclear winter are untestable in practice; "ugly" theories are untestable in principle. (In fact, the nuclear winter theory is eminently testable. We are all at risk of becoming involuntary participants in a full-scale, uncontrolled, irreproducible experiment . . .)

Hypotheses that are untestable in principle are those for which every conceivable experiment can be shown, on logic alone, to have only one possible result. Consider the hypothesis, "Once perturbed out of steady state, the system will exhibit transient behavior until it again settles down into steady state." That will always be true of any behavior of any system. Showing that it is true in a particular case, in a particular system, cannot give you any information about the object of study.

"Always true?" one might ask. Yes indeed. The system is only perturbed out of steady state if it begins some sort of transient behavior (if there were no transient behavior, it would still, by definition, be in steady state). Similarly, when transient behavior ends, the steady state begins, by definition. Note that the hypothesis does not say a new steady state must be reached, but only that if it is, it will occur at the end of transience.

What is "ugly" about that hypothesis is that it claims to be revealing aspects of the system under study, when in fact it is just defining the words *steady state* and *transient*. Because the result of the experiment was obvious strictly as a matter of logic, the experiment and the hypothesis have no empirical content. What is truly "ugly" about these sorts of hypotheses is that they are misleading, and in the minds of the unwary they are entrancing; one believes one understands the system very, very well, because one's predictions are always confirmed.

Other "ugly" theories violate the criterion of intelligibility. Most of the pseudoscientific blather currently clogging the media is untestable because the proponents will never say exactly what they mean in terms of empirically observable things (I speak here with the prejudices of a practicing scientist). One hears a lot about "essences" and "vital forces," but never anything independently detectable. At best, such theories do not explain observable phenomena, they just give them new names. At worst, they give one a very idiosyncratic view, which nobody else can verify or falsify, of personal experience. They let us paper over our ignorance by explaining away puzzling phenomena with unobservable spirits and vapors that are assumed, but cannot be proven, to be responsible for the otherwise inexplicable facts. (It is precisely on this point that science and religion part company, in deference to their fundamentally different precepts and purposes.)

Why Newton's Laws and Natural Selection Are Not Tautologies

I received many comments such as, "All of science is built on tautologies. Newton's law, $F = ma$, is a tautology. Survival of the fittest is a tautology. You're holding Lovelock to a standard that you wouldn't apply to Newton or Darwin." The common wisdom behind this objection is so pervasive, so persuasive, and so subtly (but seriously) fallacious, that I must spill a little ink here to straighten things out.

All definitions and purely logical deductions are tautologically true. Science is built on a system of definitions and deductions, so science is built on tautologies, but science has to consist of more than tautologies if it is to say anything about the real world, instead of about our words and how we define them. Mathematics is a very structured, elegant, and powerful system of definitions and deductions, and it is the workhorse of modern science, largely because it helps us to deduce consequences (e.g., experimental predictions) from assumptions without error. But even the best mathematics, all by itself, will tell you absolutely nothing about the real world. We also need a set of assertions about what the mathematics means in real-world terms. Mathematics is the language of science, but we still must have something to say.

Newton's $F = ma$ is just a definition. The left hand side could be called "gzork" rather than "force" and it would make no difference. With only $F = ma$, Newton can only play the engrossing game of restatement. "You give me a measurement of mass and acceleration, and I tell you what the 'force' is." $F = ma$ is—according to some of our educators—what Newton is famous for. But if Newton had stopped at $F = ma$ he would have been just as forgettable as all the non-Newtons in scientific history. What made Newton famous is that he came up with an independent measure of force. He not only asserted $F = ma$; he simultaneously asserted $F = Gm_1m_2/r^2$, and then (here was the brilliant part) he asserted that G is a universal constant, the two forces are equal, and mass is an intrinsic property of matter that means the same thing in both equations.

Those latter assertions are not definitions; they are a statement about what Newton believed to be true about the real world. They should have been warranted for three centuries or twelve orders of magnitude, whichever comes first. (Scale, not time, caught up with Newton, but that's another story).

With only definitions, Newton could only have pinned new labels on old facts. But because he understood both what force was (that which accelerates mass), and what controlled the gravitational force between planetary bodies (mass and separation), Newton could make a new statement about how the world works (the motion of planets is controlled, in a very specific way, by their relative positions and masses). And so he could explain Kepler's descriptions of planetary orbits, and the rest is history.

(As an aside, consider the difference between Newton's approach and the heuristic reasoning of the ancients about things consisting of "earth" "wanting" to return to their "proper" home. This bit of teleology can be restated in nonteleological fashion as things acting *as if* they wanted to return to their proper home, but note how much farther Newton was able to go by not thinking teleologically at all. Newton, having what can only be termed a considerable capacity for generalization, saw that if apples fell out of trees, the moon must be "falling" too (despite the obvious difference in their apparent trajectories) and the earth must be "falling" the opposite way to meet them (despite the complete lack of apparent motion on its part). The success of ignoring teleology and appearances entirely makes me skeptical of the usefulness of simply trying to recast Gaia as the biotic world acting *as if* it wanted to create a nice home for itself . . . but perhaps this says more about my background in physics than about Gaia or the methods of science.)

The point is that science contains definitions—but not *only* definitions. Equivalently, the definitions must be reciprocal.

Which brings me to natural selection. Is "the survival of the fittest" a tautology? First of all, natural selection concerns dominance of the next generation's gene pool by the fittest, rather than merely survival of the fittest, but the tautology could exist in either case, so I will indulge in the man-on-the-street parlance. One often hears the claim that "the survival of the fittest means just the survival of those who survive." But evolutionary biology does not have to be done that way, and that is not the way competent scientists do it nor the way Darwin meant it to be done.

If one seeks to prove that the fittest survive, but assumes that the fittest survive in defining or measuring fitness (in terms of who survived), a tautology is created, one that an unwary scientist could find very seductive. Some evolutionary biologists do measure so-called fitness coefficients by measuring chances of survival. They must assume that natural selection works, and so cannot—and do not—use those measurements to prove that natural selection works. (Measuring fitness coefficients or selection coefficients is really an attempt to pin down what contributes to fitness; thus these researchers assume that Darwin got the basic scheme right—but those who forgot that they were assuming this could get themselves into trouble.) To prove (without tautology) that natural selection works, an independent definition of fitness is necessary.

For example, the survival advantages of black moths on sooty trees are intuitively obvious, so one can predict (without needing the results of the experiment itself to define fitness) that blacker moths will dominate the gene pool when trees become sootier, as happened in Britain as industrialization spread. When blacker moths do in fact become dominant (remember, it did not have to come out this way; if Darwin were wrong, bright yellow moths could have become common instead), one has begun to collect evidence that natural selection (in admittedly unnatural conditions) really works.

Why Models Cannot Prove the Gaia Hypothesis

Some think that Gaia can be proven with models (e.g., Lovelock, 1983).

A model, like any other statement in mathematical language, can only derive conclusions from assumptions. It cannot show that either the assumptions or the conclusions are empirically realistic. Gaia is an assertion about the real world, not about models. The fact that Gaian mechanisms stabilize a model implies nothing about whether Gaian mechanisms are in fact stabilizing the real world. A model and the real world can give the same behavior for different reasons.

Now, that last statement is not quite fair; any testable prediction made from a hypothesis can be right for the wrong reason. That is why the logical positivists (who, I think, carried the whole thing way too far) stressed the importance of disconfirmatory evidence.

The point here is that in most cases the results from Gaian models are not the kind that can be tested against the behavior of the real world. Consequently, success for the theory is often measured, not by a good match between the model behavior and the real world, but by a good match between the model behavior and the behavior predicted by the theory. That kind of success is guaranteed (barring math or logic errors) because rather than comparing theory and data, this "test" instead compares a theory (in words) with itself (in math).

My point is not that models are useless (I use them all the time myself), but that we must do more than build models. Models can be used to deduce the consequences of Gaian thinking, but not to test the empirical realism of the Gaia hypothesis.

Why I Mention Teleological Gaia and Optimizing Gaia

I understand that Jim Lovelock's thinking has evolved considerably since 1974. I addressed at length the more far-out versions of strong Gaia, not because I thought he was still stuck in 1974, but because I think some of his followers and some of the media are still stuck in 1974, because I don't think we're doing a very good job of getting them unstuck from 1974, and because I think 1974 is (at least in this sense) a bad place to be stuck.

The common perception is that Gaia means that "the earth is alive" or that the biosphere is trying to make itself a nice home here. Because many people do not understand the risks of treating poetic statements as scientific propositions, the public at large thinks that scientists are busy trying to figure out whether the earth *really is* "alive." I don't think that perception helps any of us.

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