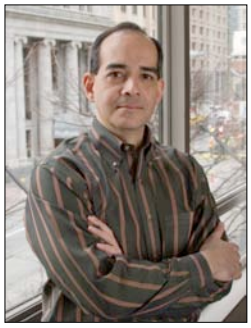
How My Views on Evolution Evolved

by Richard Sternberg



I am an evolutionary biologist with interests in the relation between genes and morphological homologies, and the nature of genomic "information." I hold a Ph.D. in Biology (Molecular Evolution) from Florida International University and a Ph.D. in Systems Science (Theoretical Biology) from Binghamton University. From 2001-2007, I served as a staff scientist at the National Center for Biotechnology Information and a Research Associate at the Smithsonian's National Museum of Natural History (NMNH). I am presently a research scientist at the Biologic Institute, supported by a research fellowship from Discovery Institute; I am also a Research Collaborator at the NMNH. More information about my research and background is available at my website, www.richardsternberg.com.

I began my university education in the early 1980s as a committed Darwinian undergraduate who was a strong opponent of young earth creationism. As a teenager I was repelled by the strain of fundamentalist Christianity that surrounded me in the deep South: the anti-intellectualism, the cultural flatness, and the pessimistic fatalism that seemed then to go with the former two. Equally revolting to me was the accommodationism that I saw in the post-Vatican II Roman Catholic Church, that usually took the form of Liberation Theology on the one hand or a bourgeois moralism on the other. Like some of my peers, I was actively searching for an integrated picture of the world and I just could not find it in what passed for Christianity.

Then three eye-opening events occurred that led me to Darwinism, and immediately thereafter to an implicit atheism. For one thing, I read Darwin's *Origin of Species* and found myself convinced—or mostly convinced—that the author had made his case. For another, I started meditating on the works of Friedrich Nietszche: His writings appeared prophetic, for not only had he accurately diagnosed in my opinion the disease (the "Last Man syndrome" you could call it) whose societal symptoms were (and are) everywhere to be seen, but he had also foreseen the major intellectual trends of the twentieth century. And then I ran across Richard Dawkins's *The Selfish Gene* and mentally devoured it. By the age of twenty, I was an intellectually fulfilled atheist like Dawkins.

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Having had a lifelong interest in all things biological—my goal as a child was to become an ichthyologist—I decided to pursue a bachelors degree in the biological sciences at the University of South Carolina. As an undergraduate I took as many courses having to do with evolutionary theory as I possibly could, and it was there that I

developed to an art a most dangerous habit. I would spend hours in the library *reading*. Not just required materials mind you, but heretical volumes; and not just your run-of-the-mill books that presented some crankish ideas, but the strong plain-brown-wrapper stuff—literature that posed hardcore, sophisticated challenges to the Darwinism that I had so casually imbibed.

Day after day, and sometimes night after night when I wasn't out and about, my attention turned to Richard Goldschmidt's *The Material Basis of Evolution* and *Theoretical Genetics*; Hugo de Vries's *The Mutation Theory* and *Species and Varieties: Their Origin by Mutation*; Søren Løvtrup's *Epigenetics: A Treatise on Theoretical Biology* and *Darwinism: The Refutation of a Myth*; to name only a handful. None of the authors were creationists and none to my knowledge ever mentioned the G-word, but all were evolutionists who were critical of Darwinism and, given their backgrounds in genetics and embryology, able to outline in detail why the "facts" that I "knew to be true" were either misinterpreted or simply erroneous. Who needed *Penthouse*?

Now this self-exposure to the banned doctrines of Mutationism and Saltationism, Aristogenesis and Orthogenesis, Lamarckism and even Teilhardism, was initially only for the satisfaction of prurient intellectual desires. But like all unchecked inordinate leanings, I needed *more*. Not for idle curiosity, mind you, but for the deeper reason that *if* what Goldschmidt and Schindewolf and Croizat and Lima-de-Faria and many others were saying had empirical backing, *then* I had to seriously rethink my position with respect to Darwinian Theory.

Other factors were also goading me to pursue this line of investigation. The 1980s were a time of upheaval in biology. So many revolutionary positions were being staked out in that decade—like pattern cladistics—that I lack the space to mention them. Two, however, stand out in terms of their influence on my thinking.

First, I read Barbara McClintock's 1983 Nobel Lecture where she expressed her view that the genome is a responsive organelle that can be "shocked" into reorganization, thereby leading to the emergence of new taxonomic groups. It was as if Lancelot Law Whyte's *Internal Factors in Evolution* and Goldschmidt's and Schindewolf's notions of "hopeful monsters," had been validated by McClintock's discovery of "jumping genes." I found her ideas to be exciting, to say the least. No one else seemed to, though. Professors, maybe in an attempt to curb my enthusiasm, provided me

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with papers wherein hypotheses were proposed that made any "smart genome" a theoretical impossibility. Copies of the 1980 papers—"Selfish genes, the phenotype paradigm and genome evolution" by W. F. Doolittle and C. Sapienza, and "Selfish DNA: the ultimate parasite" by L. E. Orgel and F. Crick, both back-to-back in *Nature*—presented the argument that McClintock's jumping genes and all the other repetitive DNA along chromosomes, had no function whatsoever. Excess DNA, ultimately the accidental by-product of replication, could proliferate by stealth because it has little or no effect on the workings of the cell.

The problem with McClintock's hypothesis of a unified genome, so I was told, is the assumption she made that most of the, say, 98% or so of human chromosome sequences not belonging to the gene category, are functional and thus have a phenotypic effect when shuffled around. The evolutionary genetic model of the genome is, in contrast, basically an aggregate of semi-autonomous, independently segregating, and "selfish" units arranged like beads on a string. You can rearrange those beads without consequence, and the strings in between are just filler. *No integrated system there*. So there was McClintock on one side, and the selfish DNA theorists on the other side. I chose McClintock. Throwing academic caution to the wind, I decided to study the functions of "junk DNA" despite being told that it was a futile search.

This brings me to the second event. The 1988 *Science* paper by John Cairns and colleagues on the evidence for Lamarckian-like directed (non-random) mutations in bacteria made its way into my hands. (Remember, I had become a serious literature junkie.) What struck me about the article was not the case it laid out for internally-oriented genetic changes; no, it was the controversy that it

sparked. By all accounts, Cairns *et alia* were wrong—really, really wrong. The best that could be said, some asserted, was that some statistical error had crept into the analysis, whereas others pointed out that directed mutations were impossible in principle. For biased *and* adaptive DNA changes raised the spectre of teleology and that, thankfully, had been defeated by Darwin.

By the late 1980s, then, and while still at the University of South Carolina, all my mental energies were being consumed by such topics as the functionalities of junk DNA, and directed or (as it is now termed) adaptive mutation. I also read all that I could on the nuclear matrix; chromosome organization; genetic phenomena such as "transvection," "position effects," and "paramutation"; and so forth. My professors were quite disappointed in me because I did not exert myself where I should have: at the bench.

The name of the game then as well as now is to produce publishable data, usually by studying a problem that is readily tractable in the laboratory. I, on the other hand, wanted to *think* about outstanding theoretical issues and I frankly found doable research projects—the kind that make a graduate student successful—boring. Only too stingingly obvious were all the faults of mine that were constantly brought to my attention for remediation: my lack of focus, my reading and thinking too much, my not applying myself (in the right way), and my ceaseless reviewing of data instead of gathering it. True, all manifestly true. It was academically a very maladaptive strategy, to borrow a Darwinian phrase. My peers, in contrast, were single-minded and sanely found the problematica that interested me to be a waste of time. Fortunately, a few friends who were undergraduate and graduate students in the Department of Philosophy were only too happy to discuss theoretical biology with me over beers—they were my enablers.

Nonetheless, given today's standards, I was still far from the slippery slope with respect to evolutionary theory. My views then would have fitted broadly within what is now labeled "self-organization theory." And I was more than willing to publicly defend evolutionism from various creationist attacks.

FAST forwarding a bit, the early 1990s found me in a new state (Florida) and at a different state university (Florida International University), where I would earn my Ph.D. in biology (molecular evolution) in 1995. My reasons for choosing FIU were numerous: The campus in Miami was (and is) beautiful, it was relatively close to the ocean, I could combine field and lab research, and I had the explicit go-ahead to study my beloved junk DNA.

While there I compiled enough data from the literature to convince me that the so-called excess and non-coding sequences in genomes are functional, nay multifunctional, and thus that they contain codes at many levels. And although still a strict DNA reductionist—I had no problem accepting the premise that the development and morphology of the marine shrimps whose nucleotides I studied were specified by the genome—my interest shifted to, of all things, taxonomy. Why? Well, I wanted to relate phylogenetic changes in chromosomal sequences, repetitive DNA to be exact, with morphological transitions in shrimps.

The difficulty was that the leading systematists who worked on "my" shrimps had no evolutionary trees to offer me. Yet where some see crisis others see opportunity, and so I slowly began to learn how to tell a telson from a uropod, and a petasma from a regular pleopod endopod. But looking at organisms as wholes—actually holding them and turning those over and noting homologies—began to affect my way of thinking. No longer could I see the specimen as an epiphenomenon of DNA, as I had been implicitly trained to do; rather, I began to see the preserved shrimps as entities in their own right, regardless of whether the blueprint for them was in the DNA. Even so, I knew from repeated trips to the heresy store that buying in bulk is expensive, and thus I layered onto my working thesis (that self-organizing genomes give rise to ordered trends in evolution) the proviso that morphology is real albeit a genetic "output." Hence, the question for me became: How to relate the genome and three-dimensional form? Little did I know that this one question would move me straight to a conceptual No Man's Land.

By late 1995 I was located at Binghamton University where I began to spend all of my waking hours on the problem of how DNA relates to morphology. At Binghamton (where I would earn a Ph.D. in theoretical biology in 1998) I was able to study under some of the best minds in systems theory and theoretical biology, and I began to converse with men such as the late, great Ron Brady. Aside from George Klir, who to me was then and still is genius personified, and the estimable Howard Pattee, two influences on my thinking stand out.

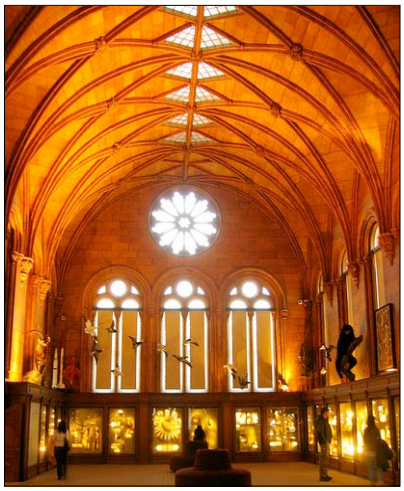
First, I met the apostate Darwinian Stan Salthe, an evolutionary biologist who challenged my still latent molecular reductionism and who introduced me to a radically structuralist way of thinking about organismal form. In a D'Arcy Thompson-like fashion, he called my attention to the fact that many material systems have complex organizations and undergo transformations in the absence of anything like a genome. Then he introduced me to the concept of a "structural attractor": an unchanging type of final cause that *informs* (my words) developmental processes. Evolution from this position is therefore the differential embodiment of attractors over time. My dialogue with Stan convinced me that I did not need to try to reduce morphology to DNA, as they were both ontologically significant at their respective levels. I was almost simultaneously introduced to the ideas of Brian Goodwin, Gerry Webster, Peter Saunders, and René Thom, and I delved into the writings of Robert Rosen, who had suggested to me Binghamton University as place where the freedom to explore new theoretical ideas might still exist (he was right). I was in a wholly different conceptual realm.

No sooner had I set out than I found myself out on the conceptual ocean with no Darwinian shoreline in sight—and like the crew and passengers of the ill-fated SS Minnow on Gilligan's Island, my "three-hour tour" landed me on an uncharted archipelago of ideas.

The second influence came in the form of the works of the so-called idealistic morphologists. For years I had read about how bad these guys were, and how they were dangerous creationists—this was the spin, now known to be highly misleading, that Ernst Mayr and other New Synthesis framers used to dismiss their ideas. I read Naef and Troll and Goethe, to name just some. So by 1997, my thinking was far, far beyond where it had been just a few short years earlier. As an exercise and since I could, I deliberately set sail theoretically, meaning that my aim was to see morphology and the genome from the standpoint of a rather hard structuralism. This seemed at the time like an intellectual pleasure cruise, and my thinking was that I could always go back to the reductionism that I had embraced earlier. But no sooner had I set out than I found myself out on the conceptual ocean with no Darwinian shoreline in sight—and like the crew and passengers of the ill-

fated SS Minnow on *Gilligan's Island*, my "three-hour tour" landed me on an uncharted archipelago of ideas where I've resided in solitude now for over 10 years. Still, I carried out empirical research on the relationships of the world's freshwater crabs, and I had no difficulty reconciling my newly found views with phylogenetic investigations. It was this research that led me to the Smithsonian.

Upon completion of the work that was to be my second Ph.D. dissertation in 1998, I recall one of my committee members telling me that if I was not careful, I was going to lapse into creationism. Seeing as I had no interest in creationism, and seeing as my thoughts had no congruence with any kind of creation narrative, I could only laugh at his warning. "Intelligent design," meanwhile, was almost completely off my conceptual radar—it was only around 1999 that I first encountered the modern version of the idea.



AT the beginning of 2000, I began my postdoctoral fellowship in the Division of Crustacea at the Smithsonian's National Museum of Natural History, to work on the phylogeny of brachyuran crabs. Then in November of that year I was informed that a position was available in GenBank—a computerized repository of DNA sequences—at the National Center for Biotechnology Information, part of the National Institutes of Health, and I was encouraged to apply. The position was that of an invertebrate taxonomist. The job entailed receiving electronic files of DNA sequences from organisms that were not represented in the "tree," a phylogeny-like hierarchy. That meant identifying where a species belongs in the scheme of things and placing it in the appropriate branch. The way the job was structured was that half of one's working time could be spent on research, and the other half on the database.

It seemed like an ideal job. In February of 2001 I accepted the position and remained there (later as a Staff Scientist) for just over six years. I also became a Research Associate at the National Museum of Natural History. Shortly thereafter I was asked whether I would mind being the editor of the *Proceedings of the Biological Society of Washington*. I was cool about the proposition for a host of reasons—it was, after all, a thankless task that involved work but no pay. It would also cut into my research time. Nevertheless, for reasons that now seem horribly vague, I said yes. Little did I know that my acceptance of the role would have such a lasting impact on the course my life would take.

I had only a cursory knowledge of intelligent design prior to 2004, while working at the NIH and conducting research at the Smithsonian. On the one hand, I knew that various paleontologists and systematists of the past who were, if you will, proto-structuralists, men such as Louis Agassiz and Wilhelm Troll, had no problem relating their "types" to Ideas in the Divine Mind. So I found the concept intriguing from that perspective. On the other hand, I found the concept of Behe's "irreducible complexity" to not be problematic since—no longer a reductionist—I saw every layer of biological organization to be irreducible to lower-level components, regardless of how it evolved. What I could not do, however, was make the conceptual leap from the observation of, say, fish light organs, to an assertion of the existence of a designer. That is to say, I withdrew from the "inference to the best explanation."

The topic of intelligent design did occasionally arise among my colleagues in the museum where, at the time, one could discuss the matter rather dispassionately; and I did have infrequent interactions with a couple of intelligent design proponents. As a consequence, I was invited in 2002 to present a talk on formal causation to a small research conference of scientists interested in ID. I agreed because I wanted to see whether proponents of intelligent design could

present any sound scientific hypotheses. Still, the whole topic for me was a rather "back burner" issue: one that I was not hostile to and yet certainly not sold on. I was of course aware that the design topic has been with us since Anaxagoras and undoubtedly before, and I also thought that hurling expletives at those who wanted to talk rationally about the subject was only an attempt to shut down thousands of years of discourse concerning the matter.

THEN in early 2004 I was contacted by Steve Meyer who told me that he had a manuscript on the Cambrian explosion and the origin of the higher taxonomic categories of metazoans. The central point of the paper, as I recall he told me, was the informational basis of distinctions among phyla. He was considering submitting it to the *Proceedings*, he said, whereupon I said that in order to do so, he would have to be a member of the Society. Soon thereafter—perhaps a few weeks—I received copies of the manuscript and a notice that Meyer had paid his 2004 membership dues. And I read the manuscript. In the draft I had in hand, Meyer surveyed discussions among evolutionary and theoretical biologists regarding the "origination of organismal form," with special attention to neo-Darwinism and self-organization theory, although structuralism was touched upon. I found the paper interesting because here was an intelligent design proponent trying to make his case to a scientific audience. After mulling over the situation, I decided to send it out for peer review.

By coincidence, I had to take a mandatory course at the NIH around that same time on the ethics of peer review, with respect to scientific manuscripts and grant applications. An emphasis of the course was that it is unethical to shelve papers or applications based on the political affiliations of authors, personal disagreements, or because an author's/applicant's views are conflict with one's own. Moreover, the course made it transparent that you do not send manuscripts or grant applications either to those you expect will be presumptively hostile (due to a conflict of interest, maybe), or to those who will give it a pass without reflection. As managing editor, I regarded the Meyer work as within my sphere of knowledge and I was interested to see whether it would stand up to scrutiny. So I asked three biologists and a geologist to review the manuscript—all of whom I knew to be neither young earth creationists nor neo-Darwinians nor affiliated with the Discovery Institute nor particularly sympathetic to the Intelligent design position. I

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gave each the standard time allotted for such reviews that was specified in the letter accompanying the manuscript. Three reviewers responded and were willing to review the paper; all were experts in relevant aspects of evolutionary and molecular biology and held full-time faculty positions in major research institutions, one at an Ivy League university, another at a major North American public university, and the third at a well-known overseas research institution.

After I had collected all the responses to the Meyer manuscript, I paid close attention to the comments. The three agreed that the work was original and would generate interest. None found a compelling, non-ideological reason for rejecting the paper, although all in their own ways made explicit their thought that Meyer's move to intelligent design at the end would be very controversial for readers. Beyond that, the three reviewers had distinct criticisms that each felt had to be addressed. So I sent the manuscript and comments back to Meyer. I thought that Meyer could say "forget it" and look elsewhere, in which case the whole review process would have been an illuminating exercise. Or he could make the changes, the changes might be found to be adequate, and I would have to make a decision. After some weeks, I received a revised manuscript that point-

by-point answered the critical comments, in some cases by making additions to the paper. Finally, I had a completed file before me and thus I could either accept or reject the paper.

I decided to accept the Meyer article for publication. Personally and professionally I had nothing to gain from the appearance of the paper—I was not associated with the Discovery Institute, I had resigned from being editor of the *Proceedings* back in October 2003, and only wanted to get back to research. Nor did it seem that I had much to lose. My thinking at the time was that, at best, the paper would generate discussion and possibly even be followed up by a "contra-Meyer" article: maybe a genteel debate against the backdrop of the question of the emergence of taxonomic units. The worst that could happen, I naively thought, was that I would be scolded for having published a work that some might think ventured into philosophy. And to give you a measure of my miscalculations, I seriously believed the article would be mostly ignored: Scientists tend to ignore works they don't approve of and besides, the venue was the *Proceedings*, not *Science* or *Nature*.

Rarely did anyone attack me or the Meyer article on the basis of its actual content.

Needless to say, my predicted outcomes were incredibly, unbelievably wrong. I won't go into the details because many of the events that transpired are now a matter of public record. What I will say, however, is that rarely did anyone attack me or the Meyer article on the basis of its actual content—in fact, those who *did read the paper* and who did object to Meyer's case for design at the end of it, tended to be civil in their correspondence. No: The two kinds of angry responses that I received invariably began with a statement about how "the offended party" did not or refused to read such "pseudoscientific" or "creationist" nonsense, followed either by a denunciation of my assumed motives or how the paper was a tool of some vast right-wing conspiracy to which I supposedly belonged. And when the situation in the museum really became nasty, I could have used a stopwatch to mark the seconds from the start of a conversation about the Meyer article, to the tirades about Christians, "fundies," Republicans, George Bush, etc. that so often ensued.

Most of these foot-stomping shouting-down episodes I chose to avoid, for if I did not avoid them I would then hear a rant about abortion rights and the need for stem cell research and how we must leave Iraq now and how the Bible is the most dangerous book ever written and how the US is, was, and ever shall be the rightful home only for "progressives." Three things I could not abide, though:

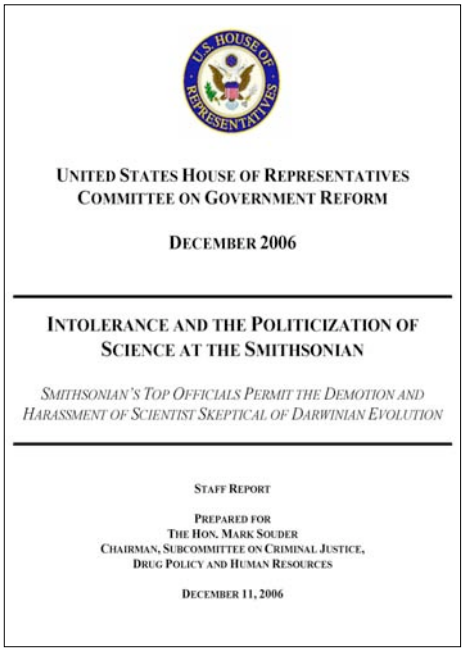
First, an act of legerdemain occurred through the cooperation of the *Biological Society of Washington* (BSW) and the *National Center of Science Education* (NCSE). The NCSE supplied talking points to the BSW leadership and pushed the BSW to issue a statement that implied editorial malfeasance. Once the BSW did so, the NCSE then pointed to the statement as independent confirmation of wrongdoing.

Second, government officials on government time using government computers were e-mailing to the world what were at best libelous rumors and often invented tales. My attempts to provide documentation supporting my side of the situation were suppressed.

Third, after a failed attempt to have me fired from the NIH—which would have succeeded had Capitol Hill not intervened—the Chair of the Invertebrate Zoology Department in the museum told me not just that I was on the wrong side of the political spectrum and thus a threat to many, but that if anything went wrong in the museum—a manuscript missing, a purse lost or stolen: anything awry—I was going to be blamed. My research was severely curtailed and placed under the supervision of an opponent, who was given complete control over what I could and could not do, and what I could and could not write and publish. My keys were demanded from me, and I was ordered not to go back to my office—which permitted the museum then to blame me for not coming in to my office on a regular basis and adding alcohol to the specimen jars that remained

in there. I could go on and on. It was surreal—like a David Lynch adaptation of a Kafka novel.

With open hunting season having been declared, I presented my case to the U.S. Office of Special Counsel (OSC). OSC attorneys found evidence to corroborate my claims of retaliation and harassment and concluded that "[i]t is... clear that a hostile work environment was created with the ultimate goal of forcing" me out of the Smithsonian. But the OSC could not proceed beyond its initial investigation because it determined that it lacked jurisdiction. Later a broader investigation was launched by Congressional subcommittee staff, which resulted in a finding that there was "compelling evidence" that my "civil and constitutional rights were violated by Smithsonian officials."



UNITED STATES HOUSE OF REPRESENTATIVES
COMMITTEE ON GOVERNMENT REFORM

DECEMBER 2006

INTOLERANCE AND THE POLITICIZATION OF SCIENCE AT THE SMITHSONIAN

SMITHSONIAN'S TOP OFFICIALS PERMIT THE DEMOTION AND HARASSMENT OF SCIENTIST SKEPTICAL OF DARWINIAN EVOLUTION

STAFF REPORT

PREPARED FOR
THE HON. MARK SOUDER
CHAIRMAN, SUBCOMMITTEE ON CRIMINAL JUSTICE,
DRUG POLICY AND HUMAN RESOURCES

DECEMBER 11, 2006

Those who want to read the gritty details of what happened to me can visit my website, www.richardsternberg.com.

NOW the question that has constantly been posed to me after August 2004 goes something like this: "So where do you *really* stand on the intelligent design issue?" The response that is expected by the person with the query usually must conform—so I gather—to a set of facile mental categories: He or she wants to know whether I'm actually a "fundamentalist" or not. It's kind of like being asked a political question such as "Are you in favor of universal health care?" where the rejoinder must be categorized as a statement for Marxism on one side, or *laissez faire* capitalism on the other. So when I answer: "Well, that depends," and commence droning on about my true views, what commonly strikes my eyes is a pained look of frustration on the face of my interlocutor after he or she has moved beyond the initial yawning and blank, bored-as-hell-gaze. My response—that I look at the whole ID issue from the standpoint of neo-Pythagorean neo-Platonism—is apparently often seen as an evasion by means of high-sounding metaphysical labels or an attempt at obfuscation, judging from the countenances and frowns I have received. Mine isn't the desired answer. But should my by now annoyed and fatigued conversation partner press the point, I pour stiff drinks for us and proceed to spell it out.

It goes something like this. By "neo-Pythagorean" I mean that I think the universe—including every object in it and all relations between and among those objects—has its basis in logico-mathematical structures. The reason that mathematics is so effective in capturing, expressing, and modeling what we call empirical reality is that there is an ontological correspondence between the two—I would go so far as to say that they *are* the same thing.

This is not to say that I naively hold that everything is reducible to numbers or equations, for certain phenomena may participate in and thus depend upon structures and yet in some way go beyond the latter. But that is beside the point. The point is that one can spend decades working up "formal realms" like some abstract branch of topology, something that might seem to be of absolutely no scientific value whatsoever, only to later find that the sets and formulae and transforms encompass not only some perplexing space-time process, but even allow verifiable predictions to be made. This is astounding. Consider an example from biology, mollusk shells. The patterns and colors that decorate seashells are diverse but they have been shown to conform to specific logical principles, a few simple and elegant mathematical rules that underlie the observed diversity. To me this strongly suggests that the cosmos and everything in it has a profound order, an intrinsic intelligibility.

Through the logico-mathematical rules that

circumscribe all possible mollusk shell patterns or diatom "glass houses" or flower shapes or symmetry groups, we can grasp the transcendent

forms behind these phenomena. Hence neo-Platonism, a philosophy that includes the existence of non-historical prototypes that *inform* all levels of physical reality and determine the entities and processes therein. The formal structures are tantamount to prototypes or Platonic forms to my way of thinking. Not necessarily identical in all respects, but close. These forms can be understood as *designs*, and that is apparently how Plato himself understood them. His student Aristotle, in the *Metaphysics*, distinguished four kinds of causes: the material, the formal, the efficient, and the final. Some view a Platonic form as corresponding to what Aristotle called a formal cause, an abstract pattern that a physical entity manifests. For me, though, a better way of understanding designs or forms is as *paradeigmata* or "models" as discussed by the philosopher Proclus in his *Commentary on Plato's Parmenides*. This is not to imply that I think that there is an ideal eye or vertebrate limb "out there" or that the *paradeigmata* are the same as, say, the blueprint of a building. Not at all; rather, I conceptualize these forms as being like an equation for a triangle—with every realized triangle reflecting that formula.

All of this may seem overly philosophical and quite unfamiliar, judging from the reactions of some people to my views (even after the drink has been downed!). To the charge that this is all well and good as metaphysics—and ancient metaphysics at that—but not science, I respond that while structures transcend the physical world, ***we can apprehend them through logic, mathematics, and the scientific analysis of empirical phenomena.***

One of my favorite examples of this apprehension of form is documented by H. Frederik Nijhout in his 1991 book, *The Development and Evolution of Butterfly Wing Patterns*. Nijhout describes the independent discovery of the butterfly wing pattern "groundplan" by the Russian B. Schwanwitsch and the German F. Süffert. Not only does this abstract nymphalid groundplan express pictorially the genetics and ontogeny of wing patterning, but it has even been formalized in such a way that predictive computer models have been generated. So from a scientific analysis of the panoply of butterfly wing spots and stripes, and through logic, a sort of periodic table of patterns is discernible that has more than just a heuristic value. One need not confuse the groundplan with a Platonic form, of course—I certainly do not—to recognize that *structure* has been grasped in this instance. And this allows two things to be accomplished. On the one hand, one can discuss development, evolution, and genetics without having to resort to *post hoc ergo propter hoc* ("after this therefore because of this") historical narratives that, as someone has said, are only limited by one's imagination. On the other hand, it permits descriptive and theoretical rigor to be introduced mainly by way of testable models. In other words, the neo-Platonism that I am referring to combines the vertical perspective of Platonism with the horizontal experimental approach of Aristotle.

If I'm still part of a dialogue—that is to say, if the other person has not gotten up and walked away perplexed or aggravated—then *THE* question is usually asked: "Where exactly do the Platonic forms, or the transcendent structures as you call them, come from?" On that question I can safely say that Platonism and structuralism, especially the latter, enable one to remain agnostic.

Yet neither precludes holding that the structures emanate from *Nous* (mind) or *Logos* (intellect). In fact, to posit that the cosmos is intrinsically intelligible because it reflects in some way Intelligence Itself would place one in good philosophical company, including such pagan "saints" as Iamblichus, Philolaus, Plotinus, Proclus, Theon of Smyrna, the venerable and virginal Hypatia, and much later, Gemmistos Plethon.

In the grand scheme of things, then, there is no incompatibility whatsoever between subscribing to neo-Pythagorean neo-Platonism as I do and intelligent design in the broad sense; quite the contrary. Intelligent design of this variety clearly has roots separate from the Bible. (And, needless to say, those who try to present the former as only the modern offshoot of scriptural literalism or of "red state" cultural ignorance are guilty of gross historical illiteracy.) Thus, my position asserts that the cosmos is fundamentally intelligible in such a way that it can be logically, mathematically, and scientifically recognized to be such; and moreover—following Proclus—that the universe emanates from *Nous* (mind). So in this sense my thinking is compatible with intelligent design broadly defined.

I earlier pointed out how a few simple and beautiful mathematical rules can account for the entire panorama of seashells we find in nature, corroborating the insight of structuralism that biological organisms reflect certain underlying forms. But the problem for structuralism is that although seashell structures may reflect simple mathematical rules, there is nothing simple about the way seashells are actually built in nature. A mollusk requires incredibly fine-tuned instructions on how to assemble its shell. What is true for mollusks is true throughout the biological world. The underlying forms may be simple and elegant, but the instructions for assembling the structures are not. To instantiate that form in a living thing requires layer upon layer of specifications that determine gene organization, how genes are expressed, how proteins interact in the cell, how cells come together in embryogenesis, and much else. So development depends not only on structures that downwardly shape the embryo, but also on a plethora of information-rich codes that spell out material components and how they can interact.

Before my partner-in-dialogue can depart, I interject that I am not a Paleyian in the sense that I believe in or look for some machine-like design. It is true that organisms display many machine-like features. But the machine metaphor is less than fully satisfying in biology. One difference between organisms and machines is that biological entities can be self-assembling whereas machines need to be assembled by another agent,

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even if that agent is another machine. We see this when proteins, added to a test tube, spontaneously arrange themselves into filaments, lattice-like patterns of organization, quasi-crystals, and so forth. Some cite these operations as examples of self-organization in the biological world, and they certainly are. But the very fact that these "smart proteins" have the instructions and specifications for limited self-assembly built into them also provides evidence of their detailed design.

Another reason why I avoid Paleyian or anti-Paleyian thinking is that it leads inexorably to statements about what God would or would not do, *as judged by human engineering standards*. I cannot emphasize the number of times that I have listened to evolutionary biologists theologize on the basis of some *gnosis* they have concerning divine actions. One case stands out in particular. Francis Collins, director of the National Human Genome Research Institute at the NIH, showed a small group that included me a presumably "dead

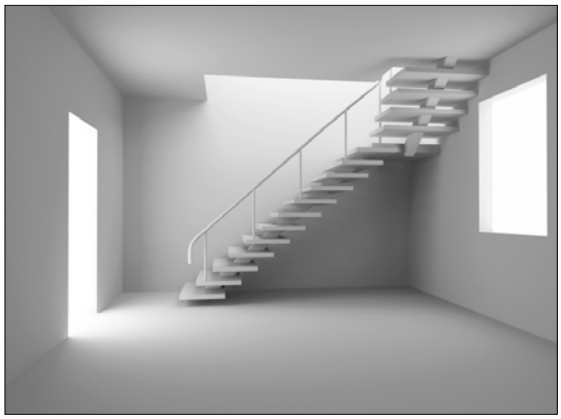
gene," a pseudogene. Now his line of argumentation went something like this:

A. We *know* this pseudogene has no function and therefore no purpose;

B. We *know* also that God would not make functionless, purposeless objects;

∴ God had no role in the creation of the pseudogene—it was a random event.

Based on my conversation with Collins, it became apparent to me that his god is a strict, nineteenth century utilitarian who would, if he deigned to create, manufacture only highly efficient and minimalist entities. His deity would only provide evidence of his handiwork by means of *Bauhaus*-like architectures, as *Baroque* or *Rococo* designs would be, well, excessive and wasteful. A purposeful, intelligently designed cell would, judging from his points, resemble ever so much Fritz Lang's *Metropolis*. And since what we so often observe are over-the-top excrescences and strings of DNA that just don't seem to have a purpose—bad, sloppy "design" to Collins's way of thinking—we know, *we know*—as a scientific fact no less—that the genome is randomly cobbled together in length and breadth and all the way up and down. (We're not off the hook, though, for this god of his, while having no divine knowledge of a cell, is nonetheless the supreme moralist who is keeping track of our slightest sins.)



The reason I mention this is that so often the anti-Paleyian rhetoric that is rampant in the literature on evolution slips and slides into god talk. This god talk is invariably anthropomorphic, meaning that its content runs something like "I would not have done it this way, therefore god would not have done it this way." It is also an extension of a decadent eighteenth and nineteenth century Christianity wherein the most arid deism was combined with a most unattractive and legalistic moralism. I for one have no knowledge concerning how God would or would not have created, or concerning His specific artistic tastes, nor does any other scientist. Certainly alignments of DNA sequences cannot tell us this. To be fair, however, Paleyian creationism is just as irresponsible in this respect. Its conception of God seems to be the same as the one just mentioned, save that the aim is to demonstrate that what is asserted by a Darwin, Dawkins, or Dennett to be a cosmic Ed Wood production is actually a Cecile B. DeMille "cast of thousands" work. Regardless, theologizing should not be palmed off as solid empirical investigation and that is why I eschew thinking along such lines.

THIS past year I decided to become a research fellow with the Biologic Institute in Seattle, made possible by a research fellowship from the Discovery Institute. Certain Darwinist detractors will no doubt shout "I told you so!" upon hearing news of this, since some of them alleged—falsely—that I was a closet IDer working for the Discovery Institute at the time the Meyer paper was published in 2004.

The truth is that I only received a research fellowship from Discovery to work at Biologic *after* I resigned from the NIH in 2007. I had grown profoundly tired of intellectually swimming upstream at both the Smithsonian and the NIH. Condemnation for my past "indiscretions" coupled with attempts to pigeonhole me in existing creationist/evolutionist categories, not to mention the refusal by many to hear me out, sharply circumscribed my freedom of intellectual and scientific inquiry. I was tired of being scrutinized and monitored and questioned and blamed. I was tired of being told that my structuralism reduced to "creationism" by those who have no understanding of either. (By the way, my taxonomy peers at the NIH did not behave this way and, at least from my perspective, we all got along splendidly.) Hence, I made the decision to leave Washington DC, to

move to Florida, to think my thoughts in peace, and try to get back into research. But then the possibility of working with Biologic presented itself last year.

The foremost question I raised before accepting the position concerned my intellectual freedom: Would I have greater freedom at the Biologic Institute to pursue the scientific evidence wherever I thought it led? Or would I face a whole new series of litmus tests, this time from intelligent design proponents rather than from doctrinaire Darwinists? I was assured that no such limitations would be imposed. My next query turned to the kind of research that would be asked of me, and I was assured that would be left up to me. After weighing the various options, I said yes. And those promises have been kept.

My current research focuses on increasing our understanding of the causal relationships between the genotype and the phenotype, or DNA specifications on the one hand and the morphological groundpattern on the other.

Specific research projects include examining so-called “non-coding” chromosomal sequences for the codes that they contain, and the ontogenetic information that they bear. Most and perhaps all DNA in complex genomes such as those of mammals are transcribed, although only a small fraction of these specify proteins. The complex organization and controlled expression of these non-translated, RNA-encoding elements suggests that an array of encrypted “texts” awaits discovery.