

Paul Gross's Dilemma: An Open Letter to the National Association
of Scholars in Response to Paul Gross's Article on Intelligent
Design in the NAS's September 2003 Issue of *Science Insights*

<http://www.nas.org/publications/sci_newslst/7_4/sci_insght74.pdf>

By William A. Dembski

I have before me a letter dated January 5, 2000 from Bradford Wilson, the executive director of the NAS. It begins, "I really enjoyed your contribution to the recent symposium in the January issue of *First Things*, so much so that I've also decided to invite you to join the NAS. Many of your fellow contributors including Robert George, Jeffrey Satinover, and Father Neuhaus are among our current members, and I think you'd find it well worth your while if you joined ranks with us yourself."

Even though I am sympathetic to the NAS's values and goals, I decided not to accept Wilson's kind offer. The reason was that with individuals like Paul Gross, E.O. Wilson, and Chester Finn deciding the NAS's science policy, I was confident my work on intelligent design would in due course be reviled. Paul Gross's article on intelligent design in the most recent *Science Insights* proved me correct.

Gross no doubt believes that the abuse he heaps on me and my colleagues in the intelligent design movement is warranted. And given only his article, readers will feel justified drawing this conclusion. But there's much that Gross doesn't tell readers in his article. Because Gross addresses my work specifically in his article, I'll concentrate on his criticisms of it. Even so, the same pattern of misdirection and selective presentation of facts that characterize his treatment of my work also characterize his treatment of my colleagues in the intelligent design movement (notably Michael Behe).

The editor John Wenger's introduction to Gross's article and the beginning of Gross's article itself set the rhetorical tone. One reads phrases like "pretense to be science," "poses as a scientific theory," "symptoms of crankhood," and "bogus science." After that Gross describes Robert Park's "seven warning signs of bogus science" and shoehorns me and my work into this taxonomy. Gross rightly notes that I'm best known for my work on a "design inference," which he puts in scare quotes. He then cites the work of Victor Stenger, Jason Rosenhouse, and Richard Wein as having decisively refuted me. He also cites my semi-popular book *No Free Lunch*.

But what Gross doesn't disclose is that the book *The Design Inference*, in which I lay out in full technical detail a method of design detection applicable to biology, was published by Cambridge University Press and peer-reviewed as part of a distinguished monograph series, *Cambridge Studies in Probability, Induction, and Decision Theory*. At the time of publication, the editorial board of that series included members of the National Academy of Sciences as well as one Nobel laureate, John Harsanyi, who shared the prize in 1994 with John Nash, the protagonist in the film *A Beautiful Mind*. The editors and referees of *The Design Inference* were in each case more qualified to judge its merits than the three individuals Gross cites (Richard Wein, for instance, holds nothing more than a bachelor's degree in statistics).

Beyond that, I've responded in detail to the criticisms raised by the individuals Gross cites (my responses are all available on my website <www.designinference.com>). Curiously, in one of his responses to David Berlinski in *Commentary*, Gross refers to my "frenetic responses to critics." Yet neither there nor here does he cite my actual responses. It is enough for him merely to assert (as he does in his *Commentary* letter) that my work is of no "interest for mathematical physics" (more on this in a moment) and that there have been "a dozen point-by-point refutations" of my claims.

But if I have been refuted, why not cite my most formidable critic rather than amateurs like Stenger, Rosenhouse, and Wein. All the critics of my work, when pushed to cinch their critique, end up citing Stanford's Elliott Sober, who critiques my work from a Bayesian perspective. Sober has now published four or so articles either exclusively devoted to critiquing my work or devoting substantial space to it, including a long critical review of *The Design Inference* for the journal *Philosophy of Science* as well as his 1999 presidential address to the American Philosophical Association (which was subsequently reprinted). My point is that Gross could have cited professional critics rather than amateurs; but in doing so he might have given away that there is more to intelligent design than he lets on.

What about Sober's criticisms? I deal with them in *No Free Lunch* and again in *The Design Revolution* (which is forthcoming). In my view, I've answered Sober's concerns successfully. But suppose you think Gross is a more credible witness than I and you don't want to take my word for it. Then consider the following remark by Paul Davies, prolific science writer and a well regarded physicist in his own right: "Dembski's attempt to quantify design, or provide mathematical criteria for design, is extremely useful. I'm concerned that the suspicion of a hidden agenda is going to prevent that sort of work from receiving the recognition it deserves. Strictly speaking, you see, science should be judged purely on the science and not on the scientist." (Quoted in L. Witham, *By Design* [San Francisco: Encounter Books, 2003], p. 149.) Note that Davies made this remark when asked about Elliott Sober's criticism of my work.

What's the point? The point is that serious scholars are seriously debating the merits of my work, and none of this becomes evident from Gross's article. Davies is a serious physicist and regards my work on information as important (I correspond with him regularly by email, and readers can confirm Davies's view of my work for themselves). But instead of citing people of Davies's stature and caliber, Gross refers to the "experts" who "have ignored or scorned" my work on information, notably my work on the Law of Conservation of Information. Who are his experts? Right after referring to them, he cites an article with the following URL: <http://talkreason.org/articles/dembski_LCI.pdf>. Who wrote this article? An anonymous Internet persona named "Erik." I understand that Erik is a graduate student in mathematics in Sweden. Even though Erik too is an amateur, I responded at length to him on my website (at <http://www.designinference.com/documents/2002.08.Erik_Response.htm>). But again, no mention of my response by Gross.

What about the Law of Conservation of Information? Gross claims that it is a new law of nature, that I've introduced it, and that introducing a new law of nature is the mark of a crank science. Each of these claims is false. Certainly if the mere introduction of a new law of nature signified a crank science, then genuine scientists could never discover and introduce any new laws at all. But the more important point for this discussion is that I'm not introducing anything fundamentally new. The very phrase "Law of Conservation of Information" is due not to me but

to the biologist Peter Medawar (see his *The Limits of Science*, 1984). What's more, he used it, albeit in a restricted sense, in the same way I use it.

I also identify this law with a Fourth Law of Thermodynamics. But again, I'm not claiming to introduce anything fundamentally new. If Gross had read my book *No Free Lunch*, where I give a history of this law, he would realize that it goes back at least to the mid 1970s to some speculations by Victor Weisskopf and that more recently it has received careful attention from Stuart Kauffman (see his most recent book with Oxford titled *Investigations*). Kauffman and I are conversation partners. We have debated publicly at the University of New Mexico and spent several days together at a symposium in Santa Fe. He even graciously consented to do an online chat through a professional organization I helped found (the International Society for Complexity, Information, and Design—for the chat transcript, go to <<http://www.iscid.org/stuartkauffman-chat.php>>).

The Law of Conservation of Information or, equivalently, the Fourth Law of Thermodynamics attempts to understand a deep problem in thermodynamics and information theory. An intuitive way to think about the problem is in terms of two CDs, one with random bits and the other with the latest Microsoft Windows operating system. From the vantage of the Second Law of Thermodynamics, these CDs are indistinguishable. And yet informationally they are very different. The underlying problem here goes back to Maxwell and his famous demon, in which the Second Law of Thermodynamics could be reversed given an appropriate information source. The Law of Conservation of Information attempts to come to grips with such information sources.

How has the scientific community received my work? Leaving aside those who reject it without having read it, by and large I find scientists intrigued. I speak around the globe to science faculties (for instance, mathematicians at the Niels Bohr Institute in Copenhagen invited me to speak there about my work on the design inference in the spring of 2004). More significantly, given Gross's unending refrain that intelligent design is crank science, my work is favorably cited in the peer-reviewed mathematical and biological literature. See, for instance,

- D.K.Y. Chiu & T.H. Lui, "Integrated Use of Multiple Interdependent Patterns for Biomolecular Sequence Analysis," *International Journal of Fuzzy Systems*, 4(3) (September 2002): 766–775.

The opening paragraph of this article reads: "Detection of complex specified information is introduced to infer unknown underlying causes for observed patterns [10]. By complex information, it refers to information obtained from observed pattern or patterns that are highly improbable by random chance alone. We evaluate here the complex pattern corresponding to multiple observations of statistical interdependency such that they all deviate significantly from the prior or null hypothesis [8]. Such multiple interdependent patterns when consistently observed can be a powerful indication of common underlying causes. That is, detection of significant multiple interdependent patterns in a consistent way can lead to the discovery of possible new or hidden knowledge." Reference number [10] here is to my book *The Design Inference*.

- W.-E. Loennig & H. Saedler, "Chromosome Rearrangements and Transposable Elements," *Annual Review of Genetics*, 36 (2002): 389–410.

This article examines the role of transposons in the abrupt origin of new species and the possibility of a partly predetermined generation of biodiversity and new species. The authors' approach is non-Darwinian, and they cite favorably the work of Michael Behe and me.

Gross is playing a losing game if he wants to say that my work on intelligent design is not science. If he doesn't like the peer-reviewed scientific articles that currently cite my work—perhaps he thinks they don't make sufficiently extensive use of my work or they were not published with sufficiently prestigious journals or presses—let him consider that there is more in the pipeline and that the charade that methods of design detection are not scientific when applied to biology will grow increasingly implausible.

Before closing I want to say something about Gross's reference to me as a "theologian and mathematician." I hold a Ph.D. in mathematics from the University of Chicago. I was also awarded graduate and postdoctoral fellowships from the National Science Foundation in that field. In addition, I did postdoctoral work in mathematics at MIT, computer science at Princeton, and physics at the University of Chicago. Besides mathematics, I hold a Ph.D. in philosophy from the University of Illinois at Chicago and have done postdoctoral work in that field at Northwestern University and the University of Notre Dame. My area of specialization is the philosophy of science, and I'm currently an associate research professor in the conceptual foundations of science at Baylor University. And finally, I hold an M.Div. from Princeton Theological Seminary.

Now, I'm certainly proud of my theological background and enjoy writing on theological topics, especially on what I take to be the theological implications of my work on intelligent design. But I keep my technical work on design detection separate from its theological implications. The former I publish through standard academic outlets. The latter I publish through religious outlets. Gross, however, makes sure this distinction is missed. Thus he quotes some of my most flamboyantly theological writings and suggests that these are representative of my work on intelligent design. Not so. As a public intellectual, I'm perfectly in my rights to explain what I take to be the broader implications of my work. But I always insist that the actual mathematical and scientific work must stand on its own merits. I'll be the first to admit that intelligent design is an ambitious program and that it may not pan out. But it needs first to be fairly discussed. Gross, in his article for *Science Insights*, attempts to keep that discussion from ever starting.

One of my friends at Oxford, a senior scholar who works in the history of physics, has watched the vituperation and storm of controversy that surrounds my work and continually counsels me to keep my polemical streak in check. Yet when I referred him to Gross's article, here's what he wrote:

My goodness, Bill, this is loaded with extreme polemical language almost from the first sentence. I find it so biased that I simply cannot get beyond the first page. That the editor is proud to present this polemical babble is astonishing. If this is the best that the "scientific establishment" can do, then that establishment is culturally decadent. It confirms what I have worried about for a long time: that science today simply does not have the cultural depth, the conceptual and linguistic resources, to conduct civilized scholarly debate about its foundational commitments and assumptions. Thomas Huxley would be deeply embarrassed by this article. If you have to deal daily with this kind of low polemic, there is a real danger of being dragged down to their level. I am more sympathetic than ever with what you have to deal with.

Notwithstanding, I'm almost tempted to sympathize with Gross. He finds himself in a hard dilemma. Because he is convinced that my colleagues and I are cranks and frauds, he is dead set

on continuing his low polemic (witness the book he has forthcoming with Barbara Forrest, tententiously titled *Creationism's Trojan Horse: The Wedge of Intelligent Design*, to be published, no less, by Oxford University Press). On the other hand, the successes of intelligent design on the scientific front (like the citations of my work on design detection in the scientific literature) are bound only to increase. Gross's dilemma is therefore this: he is committed to destroying a position he finds insufferable (i.e., intelligent design) but to do so he must employ a critique that is indefensible (i.e., charging that intelligent design is not real science).

Let me therefore recommend that Gross reevaluate his strategy in light of its dismal chance of success. Gross admits that there are multiple millions of people who find it plausible or even self-evident that the world was designed by intelligence. These multiple millions now have a voice in the academic and scientific world, effective enough that Gross and his colleagues have to spend a lot of time writing articles and even whole books attacking intelligent design (and in some cases, like Robert Pennock, they even make an academic career attacking it).

What's more, Gross admits that the "creationist" Santorum Amendment barely missed incorporation into the federal education bill, and it is "still there" in the Report language. Why not quote the language instead of merely applying a pejorative label? It's certainly short enough:

It is the sense of the Senate that (1) good science education should prepare students to distinguish the data or testable theories of science from philosophical or religious claims that are made in the name of science; and (2) where biological evolution is taught, the curriculum should help students to understand why this subject generates so much continuing controversy, and should prepare the students to be informed participants in public discussions regarding the subject.

This is the Senate version. Senator Ted Kennedy endorsed this language, and it passed the Senate by a vote of 91-8. Why didn't Gross tell his readers about that. Was he afraid they would agree with the senators that the amendment was reasonable?

The situation looks bleak if you think Darwinists ought to have consolidated their victory decades ago. But perhaps Gross will find a way out of the dilemma. A good place to start might be by positing a grand conspiracy. To start the ball rolling, perhaps someone will accuse me of bribing the editors and referees at Cambridge University Press to slip my book *The Design Inference* past the Cambridge Syndicate.

What about Bradford Wilson's invitation that I join the NAS? I'll be happy to do so provided *Science Insights* opens its pages to me and those colleagues of mine in the intelligent design movement whom Gross misrepresented in his article (that includes Michael Behe, David Berlinski, Phillip Johnson, and Jonathan Wells). The NAS's motto is "Reasoned Scholarship in a Free Society." Reasoned scholarship in a free society demands that both sides to a controversy be heard. Darwin himself agreed. In the *Origin of Species* he wrote: "A fair result can be obtained only by fully stating and balancing the facts and arguments on both sides of each question." In Gross's article, the NAS readership has heard only one side.

This original online source for this letter is at <www.designinference.com>.