DIRECT AND LATERAL STRATEGIES FOR DEBUNKING POSTMODERNISM[1]

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Abstract

A relatively recent trend in postmodern literary criticism is the use of concepts from mathematics and physics. I discuss the validity of this approach by focusing on three texts: K. Hayles' *Chaos Bound* (Hayles, 1990), P. Gross and N. Levitt's *Higher Superstition* (Gross and Levitt, 1994) and A. Sokal's "Transgressing the Boundaries" (Sokal, 1996a). The first text is an example of serious use of "mathematical metaphors" in literary criticism; the second text is a "frontal" attack on this approach; the third text is an example of a "lateral" attack, which also makes use of parody.

I. Introduction

Academia as a battlefield of ideas is an old and cherished metaphor. And, in the long history of the wars conducted on this battlefield, the two opposing armies have been separated very clearly for at least two centuries: the natural sciences vs. the humanities. While the battles have been violent, they also are infrequent; in fact the usual attitude of both natural scientists and humanists has been to ignore the "other guys".

There are exceptions to this rule, though they are not very common. A very notable and recent one is the current trend in postmodern literary criticism, which makes heavy use of chaos theory, thermodynamics, cybernetics, quantum mechanics and more. This type of work is exemplified, for instance, by (Hayles, 1990; Argyros, 1992).

The response of the natural scientists to this attention has generally been negative. Complaints that have been circulating along the informal circuit were expressed (very forcefully indeed) by biologist P. Gross and mathematician N. Levitt in *Higher Superstition: the Academic Left and its Quarrels with Science*(Gross and Levitt, 1994). The book roused heated feelings among both natural scientists and humanists; see for example the

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following reviews (Shallit, 1994; Pike, 1996; Martin, 1996). Gross and Levitt take to task a wide variety of humanities scholars, whom they group together into what that they call the "Academic Left" (no particular connection with the *political* Left is intended). Prominent members of the Academic Left are postmodernists and, particularly, postmodern literary critics who, according to Gross and Levitt, use mathematics and physics concepts without understanding them. Gross and Levitt mince no words in their criticism of such approaches.

> … The academic left is embedded in a nearly inviolable insularity, which extends and intensifies that of traditional humanists. The classicists and historians of whom C.P. Snow spoke famously in *The Two Culturesand the Scientific Revolution* were excoriated for their self-satisfied ignorance of the most basic principles of science. Today we find ourselves, as scientists, confronting an ignorance even more profound – when it is not, in fact, simply displaced by a sea of misinformation. (Gross and Levitt, 1994, p.7)

If in the martial context of our "academic battlefield" metaphor, Gross and Levitt's book is the equivalent of a major frontal offensive, then Alan Sokal's 1996 paper "Transgressing the Boundaries: Toward a Hermeneutics of Quantum Gravity" (Sokal, 1996a) is a Trojan Horse. Sokal, a physicist at New York University, wrote a parody of postmodern usage of mathematical physics ideas. Sokal's paper "was liberally salted with [physics and mathematics] nonsense … [but] sounded good and … flattered the editors' ideological preconceptions." (Sokal, 1996b). In this quote, Sokal is referring to the editors of *Social Text*, (a New York based journal which publishes postmodern work) who accepted the paper for publication, without knowing that it was a parody. A little after his "Transgressing the Boundaries" was published in *Social Text*, Sokal announced that the paper was a parody in "A Physicist Experiments with Cultural Studies," a follow-up article published in *Lingua Franca* (Sokal, 1996b). The ensuing controversy was even more heated than that raised by *Higher Superstition*, with articles by leading scholars from both the natural sciences and the humanities appearing in, among other places, *Social Text*, the *Bulletin of the American Mathematical Society*, the *New York Times*, *Le Monde*, and the Internet. For a sampling of reactions to Sokal's experiment, see the article “Mystery Science Theater” (*Lingua Franca*, 1996); here are a few of the issues raised therein.

1. Does the current use in postmodern studies of mathematics and the natural sciences make sense?
2. Was Sokal's experiment (?) ethical?
3. Was it really an experiment, a prank, or something more sinister?
4. Whatever it was, did it prove something, and if yes, what exactly?
5. Are postmodern scholars entitled to "appropriate" material from other disciplines?
6. Is there a hierarchy of the scholarly disciplines (natural sciences, social sciences, humanities, etc.)?

The first question is, in my opinion, the most interesting one; it may also be the easiest (*easiest*, not *easy*!!!) to answer, at least on a case-specific basis. And this is how I want to start my modest commentary: by considering in some detail specific examples of the use of mathematical and physical ideas in postmodern literary criticism. All the examples that I discuss are from N.K. Hayles' *Chaos Bound* After I discuss these examples and the book in general, I will relate them to Gross and Levitt's criticism of *Chaos Bound*[2] Finally, I will try to compare Gross and Levitt's analysis to Sokal's approach.

First, a few definitions. I use the term natural sciences loosely, to also include disciplines such as mathematics, engineering and computer science. This is clearly an abuse of the term (for instance, it is really questionable whether mathematics is a science, let alone a natural one) and is done only for reasons of brevity.[3] When I refer to *dynamical systems*, or simply *systems*, I mean the variety described by either difference equations (e.g. $x_n=2x_{n-1}$, n=0, 1, 2, …) or differential equations (e.g. $\frac{dx}{dt} = x(t)$, $t=[0, \infty)$). Finally, I use “postmodernism” and “postmodern” as undefined terms. I suppose most of us have a rough idea of who the principal postmodernists are; for instance, Hayles and

[2] Interested readers may want to refer to a discussion of *Higher Superstition* by Deborah Brown-Kazazis, which appeared in the previous issue of this journal (Kazazis, 1997).

[3] I should, for the readers' benefit, present my own background: my first degree was in electrical engineering, and my Ph.D. was in Applied Mathematics. Currently I am teaching mathematics to college freshmen and doing research in neural network theory, a branch of artificial intelligence. Clearly, my point of view is influenced to a great degree by these facts.

Argyros would go in this category, as well as more widely known scholars such as Derrida, Lacan, Kristeva and Foucault. However, I find it difficult to give a precise definition of postmodernism. I hope no confusion results from this admittedly vague usage.

II. Thesis: *Chaos Bound*

Chaos Bound is rich in examples of mathematically peculiar (to say the least!) pronouncements. I started looking for such examples after reading the following passage in Gross and Levitt's commentary.

> … All the strange pronouncements upon which we have focused occur, as we note, on one page. There is nothing particularly special about that page. This book is stuffed with similar solecisms, which make reading it a painful experience (Gross and Levitt, 1994, p.103).

Indeed Gross and Levitt present a fairly long list of alleged solecisms; and, following their example, I have compiled my own list, which is meant to be representative, rather than exhaustive. I have limited myself to mathematical curiosities, since these fall within my expertise; perhaps another reader will catalog the physical ones. Consider then the following passages from *Chaos Bound*

1. The first passage involves a rather long discussion of the Cantor set and concludes "… the symmetry of the resulting small part mirrors the larger part of the step before, and each time we can obtain the larger part by multiplying the smaller part by 3. Sets that possess this kind of symmetry are said to posses fixed points … " (p. 156).
2. "… Similarly, time in fractal geometry is not treated as the advancement of points along a number line. Rather, it is conceptualized as small changes in the iterative formulae that are used to generate fractal shapes. … " (p. 290).
3. "… This extreme sensitivity to initial conditions is characteristic of chaotic systems. … When the function is strongly nonlinear, small fluctuations in the data are not smoothed out as iteration proceeds. … " (p. 14).
4. " … algorithmic complexity theory restricts the concept of naming by saying that an integer has been named when it has

been calculated by a computer program. Such a program then becomes the number's name. …" (p. 162).

5. "… A theoretical model for conceptual ecologies has been proposed by Stanislaw Lem (1981), who suggests that they can be modeled as closed topological spaces." (p. 185)
6. "… It is precisely this "unfolding" that iteration accomplishes. In Derrida's hands, repeating Rousseau's language with incremental differences becomes a way to unfold and make visible the inherent contradictions upon which the text's dialectic is based. This iterative procedure produces the undecidables that radically destabilize meaning … ". (p. 181).

The above *mathematically offensive* passages can be separated into three groups. The first group includes 1, 2 and one half of 3; these are outright false. The second group includes 4 and 5; these are written in terrible mathematical style, but they can possibly be salvaged. Finally, the third group includes one half of 3 and 6; while strictly speaking these passages are not false, they can be quite misleading: they involve metaphors that do not appear to be wrong, but are so general that they could be substituted by other metaphors that would appear equally correct. Here is a brief commentary for each of the above passages.

1. "Sets that possess this kind of symmetry are said to posses fixed points … ". This is not said by mathematicians. Instead, mathematicians say that a set X possesses a fixed point x_0, *with respect to a particular operator* P, if (and only if) $P(x_0)=x_0$. If this is the kind of fixed point that Hayles has in mind (and she provides no alternative definition) then the connection to the Cantor set makes no sense. The operator P, which is never defined, is the really important object; while the underlying set plays no particular role, except as the domain and range of P. Hence the fixed point is really a property of the operator rather than the set.
2. "… Similarly, time in fractal geometry is not treated as the advancement of points along a number line. Rather, it is conceptualized as small changes in the iterative formulae that are used to generate fractal shapes. … ". As far as I know, in fractal geometry time is understood as a *well-ordered index set* T (Nicodemi, 1987, p.204), where T is usually either {0, 1, 2, … } or the real numbers **R**; in simpler words, time is *exactly* the set of

points along a number line. Even to write down the iterative formulae (which supposedly afford a conceptualization of time) presupposes that time (i.e. the set T) has already been defined. In addition, the iterative formulae that define a chaotic system generally remain unchanged as the system is iterated.

3. "… This extreme sensitivity to initial conditions is characteristic of chaotic systems. … When the function is strongly nonlinear, small fluctuations in the data are not smoothed out as iteration proceeds. … ". The second half of this statement is absolutely wrong. Take a strongly nonlinear system, such as $x_n = \sqrt{x_{n-1}}$; as n goes to infinity, x_n goes to one, *no matter what the initial conditions*. So nonlinear systems can be insensitive to initial conditions; conversely, very simple linear systems, such as the one described by $\frac{dx}{dt} = x$ can be sensitive to initial conditions, as Norman and Levitt have argued in their book. As for *chaotic* systems (which are not identical to nonlinear ones!!!) sensitivity to initial conditions, is one of their characteristics; they must also satisfy additional conditions (Devaney, 1989, p.50). In this sense, Hayles' statement is misleading: contrary to her description, sensitivity to initial conditions is a characteristic of certain linear systems as well as chaotic ones, but not necessarily of all nonlinear systems.
4. " … algorithmic complexity theory restricts the concept of naming by saying that an integer has been named when it has been calculated by a computer program. Such a program then becomes the number's name. …". Mathematically this is terrible. Ironically, the main problem is definitions, which appears to be exactly the point Hayles is talking about. First of all, *naming* and *defining* are not the same thing. Consider the following statements: (a) "x_0 is the number that solves the equation $x+1=0$ (i.e. $x_0+1=0$ is true)" and (b) "y_0 is the number that solves the equation $x+1=0$ (i.e. $y_0+1=0$ is true)". These two statements define the same number (namely -1), but two different names are used for it (x_0 and y_0). Note also the use of the name x, which is left undefined. What Hayles should have said is "… an integer has been *defined* when … Such a program then becomes the number's *definition*. …" But this is not enough: the particular definition used by Hayles is defective. Note the switch from *integer* to *number* (3/2 is a number but not an

integer!!!) and also that, according to the above, a number may have more than one definition, since two different programs can be used to compute the number on the same computer, or two different computers may be in use. Hence we cannot use this definition to *define* a number; and we do not need to use it to *name* a number.

5. "… A theoretical model for conceptual ecologies has been proposed by Stanislaw Lem (1981), who suggests that they can be modeled as closed topological spaces." This statement is puzzling because, by definition, *every* topological space is closed *with respect to the operations of union and intersection* (Royden, 1968), so there is no reason to add "closed" to topological. If Hayles has in mind topological spaces closed with respect to a different operation, which would be nonstandard usage, she should define this before she discusses a theoretical model that involves topological spaces.
6. "…This iterative procedure produces the undecidables that radically destabilize meaning …". It appears from this passage that iteration produces destabilizing entities. From the mathematical point of view this is true in some cases and false in others. For instance, the iteration $x_n=2x_{n-1}$ describes an unstable[4] system , while $x_n= \frac{1}{2} x_{n-1}$ a stable one. Once again, there is nothing strictly wrong in Hayles' statement but it can be quite misleading in presenting iteration as a sufficient condition for instability.

I have limited myself to mathematical issues and offered only a small sample of objectionable passages; the list can be expanded to great length with examples from all three categories. It is clear that errors of the first category are unacceptable: a scholar should not make false statements. Errors of the second category, while quite offending to a mathematician, are not fatal.[5] Regarding errors of the third category, the use of sweeping metaphors may be mathematically inexpedient, but useful in other contexts; in fact several mathematicians and physicists have employed

[4] And linear, with sensitive dependence on initial conditions. For a more complete discussion see (Devaney, 1989).

[5] It is fair to say that, among all scholarly disciplines, mathematics has the highest standard of proof; this is only possible because the area of mathematical discourse is extremely restricted.

quite lax (by mathematical standards) arguments in their more speculative writing. Mandelbrot's work (Mandelbrot, 1983) is a good example of this.

At any rate, I do not think that the *mathematical* objections described above affect the validity of Hayles' main thesis, namely: in the twentieth century a world view that stresses uncertainty, fragmentation of knowledge and subjectivity has emerged in both the natural sciences and the humanities. Perhaps a philosopher or historian of science may raise objections which affect Hayles' argument more seriously. The main question, as far as I am concerned, is: what exactly is the use of all the highfalutin mathematical abuses? Hayles' argument would sound much more attractive if it were written in plainer English. As it is, her poor mathematical performance makes me suspicious of the overall quality of her scholarship; I believe many mathematicians and natural scientists who read the book had the same reaction [6].

When I raised this point in discussions with humanists, the answer I usually got was that Hayles' mathematical usage is *metaphorical* and hence mathematical criteria of exactness should not be applied to it (at least this is my paraphrased average of the responses that I got). Prima facie this sounds reasonable.[7] By definition, a metaphor is not a proof[8]; literary critics deal in metaphors and they play their game using different rules and different standards of reasoning than the ones used in mathematics. What does this imply about the epistemological status of their conclusions? A more exact[9] line of reasoning does not guarantee a more certain conclusion, since it may start from false premises. Hence, in the final analysis I cannot say much positive or negative about Hayles' conclusions. But I can certainly make an esthetic judgement: her writing style seems to me convoluted, inexact and ponderous. Of course, this is a subjective judgement, influenced to a great degree by my own academic training.

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[6] A secondary, but important, question that comes to mind is why did Hayles not ask a mathematician to proofread the book after it was written.

[7] I wonder whether Hayles herself would accept this kind of defense.

[8] Although a particularly powerful metaphor may point to a logical road to proof (remark by D. Brown- Kazazis).

[9] By the usual rules of Aristotelian logic, that is.

III. Antithesis: *Higher Superstition*

Gross and Levitt, on the other hand, criticize Hayles on what they claim to be much more objective grounds: "... These supposed insights rest, as we have seen, on a technical competence so shallow and incomplete as to be analytically worthless...". It is rather obvious that "analytically" here refers to the only method of analysis which Gross and Levitt consider acceptable; namely the one employed primarily by mathematics and, to an approximate degree, by the natural sciences. This analysis consists of starting with exact definitions of the terms to be used (and, when appropriate, with experimentally verified facts) and proceed to the final conclusion by a clear sequence of Aristotelian syllogisms. In this sense, because Hayles does not understand precisely the concepts of mathematics and physics, Gross and Levitt conclude that she is not capable of providing a solid theory of their conceptual evolution. This is Gross and Levitt's *primary* argument against Hayles' cultural-relativistic conclusion.

Gross and Levitt present an additional, *secondary* argument against Hayles' analysis. This argument emphasizes the high reliability of mathematical and physical theories; it follows that Hayles' cultural-relativistic assessment of these theories cannot be correct.

I find both arguments problematic. It is perhaps easier to point out the weakness of the secondary argument. Everybody who has a passing acquaintance with real science knows well that much of what is published in the literature is pedantic, trivial, wrong or even dishonest. Data fudging is a well-known phenomenon, both in the flagrant form which occasionally makes newspaper headlines and in the more discrete version which is quite widespread in the scientific literature. Regarding reasoning standards, it is clear that analytical rigor is not practiced by natural scientists at a uniformly high level. Mathematicians consider "physicists' mathematics" substandard as a matter of course, and engineers rank even lower on the analytical totem pole. Biologists, generally speaking, live somewhere in the mathematical twilight zone. In addition, it cannot be assumed that all work originating in the humanities is by definition fuzzier and less certain than "hard" science. An example: the decipherment of Linear B (Chadwick, 1961) probably rests on a much sounder foundation than, say, neural network theory (a branch of artificial intelligence).

It must be said, in Gross and Levitt's defense, that the time dimension plays an important role in their discussion. They admit that a lot of what

gets published in the natural sciences literature has very little value; however they posit that "bad science" is, in good time, filtered out by a "natural selection" process. But if this process is taken into account, it seems unfair to compare physical theories which have been around for nearly a century (such as relativity theory and quantum mechanics) with postmodern literary criticism, which is a much younger theory, still in its formative stages. If, instead, literary criticism were compared with currently developing theories in physics, computer science or mathematics, it would become evident that a lot of this material is also analytically questionable.

I suppose that Gross and Levitt would accept all of the above but would still maintain that even poor work in, say, computer science, conforms to a much higher standard of rigor than postmodern literary criticism. This is where their primary argument, regarding postmodern methodology, becomes important. To return to the specific example of Hayles, it certainly sounds reasonable that she should not expound on concepts which she clearly does not understand sufficiently; the fact that she does is enough to make one skeptical about the validity of her final conclusions. On the other hand, it seems equally clear that literary criticism is a separate discipline, with its own rules and standards, and it is not fair to judge it by the standards used for mathematics and physics work. It is clear, for instance, that literary critics attach less importance to analytical proof than to metaphorical thinking; with mathematicians the situation is exactly the opposite. In short, Gross and Levitt's analysis of Hayles' work can be seen as a mirror-symmetric image of Hayles' analysis of chaos theory, in the sense that in both cases an outsider is analyzing an academic discipline, using a methodology outside this discipline. Now, if what is required is an unbiased evaluation of Hayles' work (or, for that matter, of chaos theory) this analysis must rest on commonly accepted ground. I think this is where Gross and Levitt's criticism went wrong.[10]

[10] On the other hand, it is quite fair to point out to literary critics that their standard of proof is not the highest available and their conclusions are not facts but speculations. One is often tempted to do this, given the especially ponderous style that characterizes much recent writing in the discipline.

IV. A Physicist's Experiment

I suppose then that an unbiased evaluation of postmodernism is possible if either of the following alternatives is realized: (a) a referee is found who is expert in both natural and human sciences, or (b) a commonsense approach, which does not depend on discipline-specific preconceptions, is used.

Unfortunately, the first alternative is not practical: very few scholars are active researchers in both the natural and human sciences.[11] Besides, why limit our requirements to knowledge of natural and human science? The imaginary referee should be able to appreciate all facets of human understanding; why not ask that he or she be also an accomplished philosopher *and* artist *and* statesman ... the list of requirements is as endless as the human experience.

Given that universal humans do not abound in our time, does it follow that an unbiased evaluation of postmodernism must be postponed until the advent of the next Leonardo? Not necessarily. Perhaps we can fall back on the second, "commonsense" alternative. This is where Sokal enters the scene.

My interpretation of Sokal's experiment is the following.[12] Since he does not know postmodernism well enough he cannot directly evaluate "genuine" postmodern works. However, he knows enough to write a paper which is nonsensical but looks like a serious postmodern work. If this "forgery" were accepted by postmodern experts as legitimate work, if, in other words, the experts cannot distinguish nonsense from profundity, then perhaps the profundity is not so profound after all.

Notice the difference between Sokal's and Gross and Levitt's approaches. While Gross and Levitt attacked postmodernism frontally, Sokal used an indirect line of reasoning, which certainly sounds plausible and commonsensical; in fact, blind tests of this type are so commonsensical that they are also used in TV detergent commercials.

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[11] I know of one example: the physicist R. Feynmann did work on deciphering Aztec mathematical work. There probably are some more examples of this type, but even in this case we really have a physicist who entered philology as a dilettante, rather than as a conventional researcher. The fact that R. Feynmann may fairly be called a genius is also important.

[12] Probably this is only part of his motivation; it is interesting to read his own explanation in (Sokal, 1996b).

Sokal ran his experiment and the *Social Text* editors proved to be unable to distinguish his forgery from the real thing. Of course it does not necessarily follow that postmodernism (or postmodernist use of mathematics and physics) is bogus. Various objections can be and have been raised. Sokal himself admits that the experiment was not controlled. While genuine ("control") papers were submitted to the same authors, only one forgery was submitted, so the outcome may be statistically insignificant (because the particular editors were incompetent, or because the particular forgery was very good, or for purely random reasons). Another important point is that forgeries, both as frauds and pranks, have a long and honored history in the natural sciences; few people would accept this as reason to question the validity of, say, quantum mechanics or the theory of natural selection. However, Sokal's experiment *does* furnish an additional increment of evidence that perhaps not all is well in the postmodern house. It can be seen as a complementary approach to that of Gross and Levitt, as an additional test that can be applied by an independent assessor attempting to evaluate the academic merits of postmodernism.

I use the term "test" having a particular precedent in mind. In a way Sokal's experiment reminds us of Turing's test of artificial intelligence. For a description of this test, see for example (Hofstadter, 1984). In the very early days of computer science, there was much speculation as to whether computers can display intelligent behavior. To resolve the question, Alan Turing, who was one of the founders of computer science, proposed the following test. An interrogator sits in a completely isolated room; the only contact with the outside world is through two computer terminals, call them A and B. Terminal A is connected to another terminal, manipulated by a human being (situated in a different room); terminal B is connected to a computer. The computer (more precisely, the program run by the computer) attempts to fool the interrogator into believing it is a human being, while the actual human being attempts to dissuade the interrogator. If the computer program succeeds, it can be termed intelligent.

Replace "interrogator" with "editor", "human being" with "postmodern scholar" and "computer" with "human being without academic training in postmodernism"; Turing's test is then transformed into "Sokal's test". Turing's test is used for determining whether a computer can generate intelligent behavior. Sokal's test is used for determining whether a person without academic training in postmodernism can generate postmodern scholarship. In both cases we have a blind test, which seems to be a pretty

good way to reduce bias. In both cases the final result depends on the interrogator's competence, which implies that a more valid version of the test would require multiple runs, with several interrogators (correspondingly, editors). Finally, and perhaps most significantly, in case the interrogators (corresp., editors) are consistently fooled, two conclusions are possible, and they are worth spelling out in detail. The first conclusion is that intelligent behavior is not a property of human beings exclusively (corresp., postmodern scholarship is not a property of postmodern scholars exclusively). The second conclusion is that intelligent behavior does not really require what we usually understand as intelligence, i.e. thinking, insight, creativity; instead it is a collection of formal manipulations which, if complex enough, recreate an illusion of intelligence (corresp., postmodern scholarship does not really require what we usually understand as scholarship, i.e. research, reasoning, analysis; instead it is a collection of formal manipulations which, if complex enough, recreate an illusion of scholarship).

In case the second conclusion, especially the part which refers to postmodernism, sounds exceedingly harsh, I should hasten to add that there is nothing special about applying Sokal's test to postmodernism or literary criticism; it could be applied to any other academic discipline. In fact it *is* applied to every discipline which employs a peer review system for publication. For instance, in my own discipline, artificial intelligence, while it is unusual to submit a deliberate forgery, quite a few substandard papers are submitted (and a significant percentage of these is accepted) for publication. I suspect that a similar situation holds in literary criticism, quantum mechanics, mathematics, archeology, or any other scholarly discipline you may care to name. In this sense, Sokal's test is not unusual; it is simply a highly publicized instance of a constantly recurring routine.

I leave the readers to draw their own conclusions (from Sokal's original experiment or its everyday variations) regarding the validity of postmodern scholarship. Personally, I would evaluate with caution and I would keep in mind the biblical injunction: "He that is without sin let him first cast a stone.". However, in conclusion, I would like to stress two particularly attractive points about the experiment. First, much like Turing's test, it can be conducted by a nonexpert experimenter. To be sure, the experimenter must be a good forger; but he or she need not be an expert in either postmodernism, physics, mathematics or any other scholarly discipline. Second, we should not overlook its humorous aspect, which is especially welcome in contradistinction to the ponderous

and essentially humorless style which seems to prevail in, among other places, postmodernist work. Perhaps after all the new emperor really has no clothes.

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