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Autor: Kaspar Aldrich, Elizabeth
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Quantum Textuality and Its Limits

Elizabeth Kaspar Aldrich

The first limit to "quantum textuality" which I must address is in the area of meaning, and it is such a severe one that, except in the strictest sense of each of its constituent parts taken separately, it might be fair to say that the term has very little meaning or none at all. So I will begin where it does, at the strictest sense of its first constituent part. The formula incorporating what has come to be called Planck's constant will serve at the same time as an exemplary text:

$$E = h\,\nu$$

Max Planck arrived at this formula in 1900 in the course of solving a puzzle – already then a generation old – known as the black body radiation problem. In brief: when you heat up a black or non-reflecting body it emits radiation, and the problem Planck confronted was how to account for its distribution: how much of the total energy of the radiation belonged to each frequency (or how much of each color was there in the "glow" – from blue to red-hot to white-hot, and so forth). The distribution was evidently solely a function of temperature and not affected by whatever material the black body was made of; thus the challenge was to locate and identify that function. Planck's friend Wilhelm Wien had come up with a formula that seemed to work until the measurements were extended, when it turned out to be true only of the higher frequencies and not of the lower. Planck then devised a much more ingenious or elegant mathematical solution that worked for the whole range of measurements. But he called it "an act of desperation," and he was never completely happy with it, despite its continual experimental confirmation (it remains confirmed to this day). To make his mathematical formulation correspond to physical laws, Planck was obliged to discard the continuity of the wave – or in other words, to fly in the face of common sense as well as some fundamental assumptions about how those laws worked. If you imagine a wave as a swing, then in Planck's formulation you imagine a discontinuous

one, a swing that can swing only in arcs of say three feet, six feet, nine feet and so forth, never in arcs of four feet, or two, or half a foot.[1] This is what we have on a microscopic scale: the oscillations do not change frequencies smoothly or continuously, but in minuscule jumps; and the fraction of energy/oscillation frequency in the formula has the same irreducible value for every jump. Planck later termed this the elementary *quantum* of action. In the formula cited above E equals the energy of one quantum of light whose frequency is ν; Planck's constant is represented by h.

Now h is very small, on a scale for which I have no imagination: 6.5×10^{-27} or six and a half nonillionths of an erg second. Nevertheless, it made a scientific revolution, or to borrow a term from the work that professors of the humanities most frequently borrow from, Thomas Kuhn's *The Structure of Scientific Revolutions* (1962), it signaled a paradigm shift. This is the revolution that is confirmed with Einstein's publications on photoelectric effects in 1905; it was extended in the 1920s by Neils Bohr, who was enabled by Planck's breakthrough to formulate the solar system-like structure of the atom with which we are familiar and who developed the principle of complementarity to accommodate the wave/particle duality that is, along with other mutually exclusive phenomena, a part of quantum mechanics; and by Werner Heisenberg, whose principle of indeterminacy – a formulation of the relationship between position and momentum in sub-elementary particles (one relation involving our inability to measure both at once) – was presented the same year, 1927, as Bohr's. By 1927, indeed, we could say that the paradigm shift is complete. *Natura non fecit salta*, Aristotle wrote, and Leibniz could confirm some two millennia later, "Nature makes no jumps." Now Max Planck must contradict, with characteristic modesty of phrase: "Nature certainly seems to move in jerks, indeed of a very definite kind."[2]

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[1] Hence the failure of Wien's solution, which at the lower frequencies required division of what is irreducible. The analogy of the swing appears in Banesh Hoffmann's explanation of Planck's solution to the black body puzzle (*Einstein* 48-49). I am indebted to this among many other accounts of Planck's work, of which the most sympathetic is probably Everdell's (*The First Moderns*, Chapter 11 *et passim*). Cline's account includes a clear explanation of why black body radiation was considered an anomaly or puzzle in the first place (*New Physics* 51-54): it involves empirically unfulfilled theoretical predictions of something called the "ultraviolet catastrophe" on which I do not here elaborate.

[2] This often quoted series on jumps and jerks can be found in Everdell (176). Heisenberg's Indeterminacy – or as I first read of it the Uncertainty Principle – has probably been the most popular element of quantum mechanics for (mis)appropriation to other domains. This is so in part because Heisenberg himself was so given to philosophical extensions of it, and also, I suspect, because it can serve as a kind of scientific imprimatur for all varieties of modern and postmodern *angst*. But it is Bohr's Complementariety, with its allowance for mutually exclusive concepts in the formation of a general picture or conclusion, that can lend itself most read-

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Quanta, Amy said, on the train, in that blizzard, in answer to my question, Quanta.

Not here, Diana said, to her lasting regret, to her own daughter, who approached her, crying, in front of all those people. Not here.

Just ax for José, the young man said, on the ride out to Newark Airport. You need a ambulance, or a driver, for any reason, you call the same number. Just ax for José. Also, when the People Express terminal loomed nearby, just beyond the small maze of side streets and overpasses; you can see it from here, but just try to get to it.

Quanta. Not here. Just ax for José. (41)

Quanta, Amy said on the train, in that blizzard, in answer to my question. Hello, this is Medea. Wasn't always. Well, he asked for me, you inquired after me, at the conference in the Motel on the Mountain. The motel has since become, what does it matter what it has since become. I don't think they hold conferences there. It is where we began. (54)

Quanta, Amy said, on the train, in that blizzard, in answer to my question. Not here, Diana said, to her lasting regret, to her own daughter, who approached her, crying, in front of all those people. (79)

Quanta, Amy said to me, on the train, in that blizzard, in answer to my question. Not here, Diana said, to her lasting regret, to her own daughter, who approached her, crying, in front of all those people. Not here. But in London, don't you see, the phone rang. In London, the phone calls began. Well, I waited. I told no one. For the next few days, in my case, my voice was gone; it might have been a fever. I waited for them to find the car. I waited for them to find the ticket, me. But it was not until long afterward, when it was explained to me, that I understood that there was, after all, something else quite wrong in the course of these events, and that there really was something they were trying to frame me for, in the matter of the car. But I didn't understand it then. Quanta. Not here.

You can see it from here, but just try to get to it. (91)

These passages are from Renata Adler's 1983 novel *Pitch Dark,* an exemplary text of the sort with which we are professionally more familiar. The first of them opens Part Two of this three-part work, and the fourth and last closes it. The novel's own resistance to narrative coherence or meaning may

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ily to the theories and practices of deconstruction. See Mara Beller for a fascinating discussion of the philosophical pronouncements of Heisenberg, Bohr, and other founding fathers of quantum theory and their share in "the blame for the excesses of the postmodernist critique of science" (29).

be exaggerated by such passages' being taken from context, but they are characteristic of the text as a whole, and the resistance is very much a part of that whole which – to repeat a phrase I used of Planck – discards continuity with a vengeance. Adler offers us an example of what I will call informed evocation of a quantum universe: the prose is enigmatic, but the term as used is easy enough to decode.

Amy, who has no other role in the book, is clearly an educated woman – she gets the plural form right – who is deeply in touch with the *Zeitgeist.* Although we never learn precisely what was "my question" (in Part Three there is a hint of its having something to do with unhappiness in Amy's life and how it has come to her) we sense immediately that her reply is negative in tone, even ominous. "Quanta" connotes a kind of postmodern, post-Eliotic "I can connect / Nothing with nothing"; these fragments cannot be shored against anything, ruins or otherwise. The term also serves as a *mise en abyme* for the apparently random and discontinuous (but also well-educated) work before our eyes, the narrative that moves in jerks. An unsympathetic critic might remark that the fashionable term from science thus serves as a preemptive excuse for this highly mannered narrative style; but even here one must admit the appropriateness of "random and discontinuous" to the universe it reflects. By the second or third repetition of "quanta" with "blizzard" we can hardly avoid seeing the latter as an image of colliding atoms; a suggestion of Heisenberg seems to follow in José's location / momentum problem ("you can see it, but just try to get to it"); and so forth. Attached to no single or definite referent, the term "quanta" is a kind of cultural / linguistic icon: a self-reflexive symptom, if you like, a symptom *of* reflexivity.

As it is in my next exemplary text, non-scientific and non-literary, the advertisement reproduced opposite. In this case and by this point, however, and by this point I mean the winter of 1998 when a blizzard of such posters blanketed an affluent region of Westchester County, NY, we see that "quantum" as modifier has been emptied of even indefinite reference and points only to, or we might say speaks only for itself. "I am of Modern Science," its message runs; "I am highly specialized: within your universe of discourse in that you recognize me, but outside your technical understanding." Mel Schwartz, who would seem to offer a kind of homeopathic cure for whatever ails Amy, understands and will put at our service the benefits of modern science. In the dynamic of this post-Barthean *mythologie*, quantum is Something Good. This is the silliest example I could find of what I will call, to double the process and borrow a term from the music biz, "crossover" borrowing and the downward direction it can take to misapprehension and empty pretension.

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More serious instances of such borrowing from the terms and concepts of science for other fields such as philosophy and literary theory provide Alan Sokal, of recent hoax notoriety, with material for an entire book; and they do represent a principal issue in today's debates on textuality.[3] But as a text exemplary of frivolous borrowing Mel Schwartz will serve for the moment: it is to be understood that the title "quantum textuality" acknowledges this decadent end point with its own form of postmodern irony.

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There were a great many ironies for Planck in the quantum revolution he began. As I said, he never liked his own contribution to it, and in many ways he resisted its implications – although he always faced up to them when he had to. Planck had begun his own career in physics as an *anti*-atomist, and he was led to the black body puzzle as part of his attempt to arrive at an absolute value for entropy; later on he resisted the statistical (that is, non-absolute) model that he had to adopt and that has also since prevailed. As he tells us in his *Scientific Autobiography* (1949), Planck thought that the search for absolutes was "the most sublime scientific pursuit in life" (13), and he is a most unlikely father to indeterminacy. Of course the great embracing irony is that this quintessentially conservative and law-abiding German who taught for years in the same school as did his father and grandfather before him seems to "break," or by breaking change the laws of classical physics that had prevailed since Newton.

I say "seems to," but semantics or the conventions of narrative discourse are always a potential danger in this regard. Indeed the very incompatibility of narrative and scientific discourse, a strong thread in the argument of Jean-François Lyotard's influential work *The Postmodern Condition: A Report on Knowledge* (1979), will demand our attention. What does it mean to say that Planck broke or changed the laws of physics? If you believe that such laws are in operation in the cosmos, objectively "out there" and independent of, while ultimately accessible to, human observation and formulation, then of course Planck cannot change or even break them: he can only discover and

[3] With colleague Jean Bricmont: first published in France as *Impostures Intellectuelles* (1997), and the following year in the U.S. under the title *Fashionable Nonsense*, the book claims to expose the misapprehensions and empty pretensions of mostly French "intellectual icons," to quote its jacket publicity, such as Jacques Lacan, Julia Kristeva, Luce Irigaray, Jean Beaudrillard, et al.; but the implicit targets of exposure are those American academics who have adopted and are busy disseminating their work.

(approximately) correct a mistake or misconception in the formulated human version. If you do not believe in such a reality independent of observation then you are on the "social constructivist" side of the Sokal debate (its extreme or "strong" end, perhaps: see below). But no matter your position on *what* Planck does, *how* he does it remains the same. It is the how that I wish to emphasize here, although it is precisely the how that resists narrative. This is because whereas Planck observes an anomaly in the natural world that is clearly enough described in the language or discourse of that world, his solution to the puzzle it presents must be made in the realm of pure mathematics, an emphatically unnatural world of constants and absolutes which eludes the very language that must be overturned in its service.[4]

A correct mathematical proof is complete and perfect, not subject to change; or as Simon Singh explains in *Fermat's Enigma* (1997), "Mathematical theorems . . . once proven are true until the end of time. Mathematical proofs are absolute" (21). The science of physics is grounded in – we can even say impelled by – pure mathematics, which imposes one all-important constraint or limit on its written texts; but *it is separate.* (The other sciences – chemistry, biology, and so forth – are grounded in physics.) What is the relation of a pure mathematical proof to a scientific one? Singh refers to the latter as its "poor relation" (21). The scientific proof depends on the testing by experiment of an hypothesis which has been proposed to explain a physical phenomenon. Since such hypotheses must themselves not only explain but predict the results of other such phenomena, experiments must also test this predictive power. Cumulative experimental success amounts to evidence in favor of a given hypothesis, and when evidence seems overwhelming it is accepted as scientific theory.

> However, the scientific theory can never be proved to the same absolute level of a mathematical theorem: it is merely considered highly likely based on the evidence available. So-called scientific proof relies on observation and perception, both of which are fallible and provide only approximations to the truth. . . . Even the most widely accepted scientific "proofs" always have a small element of doubt in them. Sometimes this doubt diminishes, although it never disappears completely, while on other occasions the proof is ultimately shown to be wrong.

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[4] We probably get the best sense of the relation of this mad world to language and logic from the works of Lewis Carroll, that is of mathematics don C.L. Dodgson. For superb commentary on this subject and on the role of science within it see Martin Gardiner's edition of the *Alice* books (on their anticipation of Einstein's thought experiments 29 n. 2; on theories of the expanding / diminishing universe 39 n. 5; on the relation of science to ethics 69 n. 6; on the paradoxes of pure mathematics, quoting Bertrand Russell, 91 n. 9; on the theory of Relativity 99 n. 8, and so forth).

> This weakness in scientific proof leads to scientific revolutions, in which one theory that was assumed to be correct is replaced with another theory, which may be merely a refinement of the original theory, or which may be a complete contradiction. (21-22)

This is a simplified and what we might call "classic" version of Kuhn's account of scientific revolutions, but it can serve well enough to clarify the how of Planck's breakthrough.[5] When his solution to the black body puzzle has been proved mathematically, then and only then is it brought into conformity with known physical laws. "On the foundation of infallible mathematical logic science lay inaccurate measurements and imperfect observations" (Singh 26). When the logical demands of Planck's mathematical solution conflict with those laws, it is the laws that must be revised accordingly.

Or to inject a crossover remark, somethin's gotta give. This is the (textual) point Jeremy Bernstein makes in his memoir of becoming a physicist, *The Life It Brings* (1987), when he recalls himself-as-callow-youth embarking on the project of reading Einstein. Having counted up the pages in the gratifyingly slim book *The Meaning of Relativity*, Bernstein calculates that if he reads one a day he will have mastered the whole in a little over three months. His plan is to apply the kind of close reading he devoted to the study of poetry in high school, for "[s]urely," he concludes in what might pass today for parody of the English professor as science critic, "I could understand anything written in English if only I read it slowly" (38). The account is a kind of ecphrasis as *mise en abyme*: with its own subtly parodic echoes (Bernstein sees his young self rather as mock Talmudic scholar), it takes us to a moment of anti-revelation:

> The book begins straightforwardly enough, "The theory of relativity is intimately connected with the theory of space and time," Einstein writes. So far so good. He then describes the experience of time and what is understood by a clock. Thus

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[5] Classic because it assumes the existence of an independent reality of which science approximates with progressively increasing accuracy ("truth") a theoretical account or version. On the radicalism of Kuhn's rejection of such scientific truth and his appeal to deconstructionists of the "strong" (Edinburgh) school of science studies see Weinberg, "The Revolution That Didn't Happen" (1998), discussed below. I include what some might consider an insultingly basic survey of the nature of proof in part because it was in this area – one with which all educated people are presumed to have some acquaintance, as opposed to, say, the areas of higher math or quantum theory – that Alan Sokal "caught" the editors who accepted his hoax article as serious. In a climactic take-off of social construction, it announces that its argument has allowed us to see the π of Euclid – a mathematical constant or absolute – in all its "ineluctable historicity" – thus leading me to conclude that we can all use reminders from time to time of what we learned in our distracted adolescence. See below, note 8.

> endeth page one. For five days I proceeded with my plan, if not understanding everything, at least understanding enough to feel encouraged. On the sixth day the whole thing collapsed. In the middle of the page there stood a formula of which I could understand nothing. The symbols were completely meaningless to me. It was not going to be like reading *The Lady of the Lake* after all. (38)

The literature of scientific memoirs is full of such collisions of discourses and sign systems, and the temptation to amass and simply cite examples is hard to resist.[6] But I will confine myself to one additional instance – it amounts to a rather curious passing remark within an all-important context – from one of the best-known of these works, J.D. Watson's *The Double Helix* (1968). Like Bernstein, Watson tells us early in his memoir of an encounter with a "parent" text, Linus Pauling's report of his discovery of the structure of the α-helix protein. The postdoctoral Watson is wiser than the freshman Bernstein in his knowledge of what he doesn't know ("Most of his language was above me . . . " and so forth, 25) but infinitely more brash in his competitive determination to "beat" Pauling to the next logical discovery, of the structure of DNA, and the Nobel Prize that must follow on it. Soon, apropos of the α-helix report, Watson is taught by more expert colleagues that "Pauling's accomplishment was a product of common sense, not the result of complicated mathematical reasoning. Equations occasionally crept into his argument, but in most cases words would have sufficed" (34).

There are two aspects of this remark on the sufficiency of words that make it worth our pausing over. First, of course, in its reference to "most cases" it acknowledges the few cases where words would *not* suffice, and hence that area of Pauling's argument, or of any scientific text, that is outside narrative discourse. We may think of this as the area grounded in pure mathematics, what we might (be tempted to) call the "objective." But the second aspect of the remark that we notice complicates the very notion of the objective; for Watson's treatment of Pauling – this entire section of the memoir, if not the memoir in its entirety – is dominated and directed by his concern with what he calls "style," a matter frequently of words and always, one would assume, of subjectivity. Watson describes the lecture in which Pauling has announced the same discovery, given with "his usual dramatic flair" and, "like all of his dazzling performances," such that "even if he were to say nonsense" his "mesmerized" listeners would never notice or care. (It is

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[6] In the case of Richard Feynman, impossible. I can only recommend the memoir of 1985 in general, and in particular the accounts of Feynman and the professor of poetry (66); Feynman and the philosophers reading Whitehead (69-70); Feynman, the mathematicians, and the theorem of immeasurable measure (84-87).

with something of a shock that the careful reader realizes that Watson describes this particular performance at second-hand; the dazzle and its effects are in his imagining of the scene.) Pauling's written texts are also performances, equally "dazzling" and imaginatively inspiring, equally independent, it seems, of one's understanding their content. Watson's reading of the α-helix article has in fact been a matter of just such appreciation:

> Most of the language was above me, and so I could only get a general impression of his argument. I had no way of judging whether it made sense. The only thing I was sure of was that it was written with style. A few days later the next issue of the journal arrived, this time containing seven more Pauling articles. Again the language was dazzling and full of rhetorical tricks. One article started with the phrase, "Collagen is a very interesting protein." It inspired me to compose opening lines of the paper I would write about DNA, if I solved its structure. A sentence like "Genes are interesting to geneticists" would distinguish my way of thought from Pauling's. (26-27)

The imagined sentence is a marvelous clunker: this is the perspective or manner of age-deprecating-youth assumed by Bernstein but, of course, to be distinguished from his no less than from Pauling's. The younger self recalled by Watson imagines the figure he will cut in the world come a not too distant future: a figure of fame equal to Pauling's, and possessed (the present memoir is our proof) of an equally distinctive style.

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We might seem to have wandered rather far from our earlier question about the relationship between mathematical and scientific texts, or from the more general one about the relation between scientific texts and the narrative or more specifically literary. But the whole issue of style, not surprisingly, raises matters not only of individual, personal distinction, but also of distinctions between and among individual subjects, fields, "territories." With his own unmistakable style Richard Feynman takes up this matter of distinctions in his famous *Lectures on Physics*, introducing the subject of the relation of physics to other sciences and to the world outside science itself. He regrets that limitations of space render it "impossible . . . really to deal with the complex, subtle, beautiful matters in these other fields" – of chemistry, biology, astronomy, geology, and psychology – that he will briefly discuss; and he goes on to a still briefer and final glance at the rest:

> Lack of space also prevents our discussing the relation of physics to engineering, industry, society, and war, or even the most remarkable relationship between mathematics and physics. (Mathematics is not a science from our point of view in the sense that it is not a *natural* science. The test of its validity is not experiment.) We must, incidentally, make it clear from the beginning that if a thing is not a science, it is not necessarily bad. For example, love is not a science. So if something is said not to be a science, it does not mean that there is something wrong with it; it just means that it is not a science. (vol. 1, sect. 3, p. 1)

It is worth remarking, in the light of recent debates on social construction, that Feynman acknowledges the connection of physics to social, economic, and political forces without in any way considering – not even to deny, so impossible is the idea – that that connection might be substantive to the scientific content of the field. (Note the difference of emphasis given to physics over mathematics, as opposed to Singh's math over physics). It is amusing as well to note that as early as 1961, when this lecture was first delivered, Feynman is aware of and quick to forestall the odd, territorial sensitivity or touchiness that can be aroused in people over the question of what is and is not science and of who gets to say so in the first place.[7]

This matter of touchiness brings us, unavoidably, to the Sokal affair – a quite explosive manifestation of touchiness for which the term itself seems absurdly tame. I have written on this subject (along with some thousands of others) in another context, and I will not repeat my views and review of it here. The controversy surrounding the hoax has indeed been staggering in its intensity, range, and duration, and it continues to generate heat on the Internet and elsewhere.[8] Some contributions to the debate, as I suggested above,

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[7] Feynman is careful to distinguish the science of psychology (his description suggests a field we might specify as neuropsychology) from psychoanalysis, which is "not a science [but] at best a kind of medical process, and perhaps even more like witch-doctoring" (*Lectures* vol. 1, sect. 3, p. 8) Freud's career was of course dogged by his failure to establish for psychoanalysis the status of science (and for himself, of scientist) on which he was so bitterly insistent. For an excellent treatment of this strain in Freud's life and of the status of medicine and other emerging sciences at the end of the nineteenth century, see Everdell Chapter 9, 127-32 especially. David Stannard's *Shrinking History* is a bracingly hostile treatment of the scientific pretensions of psychoanalysis then and now (see especially 3-50). Touchiness and territoriality are prominent themes in both.

[8] I trust I am correct in assuming general knowledge of the incident. In brief: in the summer of 1996 the editors of *Social Text*, a journal of cultural studies emanating from New York University, published a special issue on the "Science Wars" which contained an article by Alan Sokal, a theoretical physicist at the same university, entitled "Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity." At virtually the same time Sokal announced in *Lingua Franca* that he had passed off a parodic hoax studded with scientific errors or "nonsense" and outlandish violations of logic and the rules of evidence and argument. Front page coverage of the story by the *New York Times* led to a kind of academic firestorm that

have spoken directly and even excitingly to the more general issues of textuality and its limits that concern us today, notably those of scientists more distinguished than Sokal, like Steven Weinberg, as they have engaged with literary critics and theorists more distinguished than the editors of *Social Text* – as well as with the legacy of Thomas Kuhn, and the more problematic legacy of the founding fathers of quantum mechanics who actually began the crossover applications of scientific theory for which the postmodernists are held to account. But the most curious aspect of the mass of text generated is how ultimately flat and unprofitable most of it turns out to be. Some of this is a matter of the touchiness just mentioned. Name-calling and finger-pointing are not edifying (however much fun), and whereas anger might occasionally have inspired or ennobled a debater, embarrassment never has. Institutional and professional politics have played their role as well, with charges and counter-charges of careerism versus jealousy, ignorant relativism versus naive positivism, and so on and on. But the effect of endless repetition and circularity that long reading in these materials invariably calls forth and that I was once inclined to ascribe to some intrinsic properties of the hoax form itself stems rather, I now think, from a more fundamental problem. The "debate" is circular and repetitious because it is not for the most part a debate at all – not according to the rules laid down in an old-fashioned text such as M.J. Adler's *Dialectic* (1927), one of my favorites of this genre; not according to the "moves in the language game" as Lyotard, following Wittgenstein and failing to convince Sokal and friends, sets them forth for postmodernism.

Lyotard, as will be remembered, defines the postmodern as "incredulity towards metanarratives" and sees it as "undoubtedly a product of progress in the sciences [which] in turn presupposes it" (xxiv). He is throughout *The Postmodern Condition* eloquent on the heterogeneity of discourses or "language games" in our present society, institutions, or branches of knowledge, and most especially on the incompatibilities – failures of exchange or translation, we might say – that exist among them.

> It is . . . impossible to judge the existence or validity of narrative knowledge on the basis of scientific knowledge and vice versa: the relevant criteria are different. All we can do is gaze in wonderment at the diversity of discursive species,

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eventually became world wide. Today a cursory search on the Internet will call up over 5000 entries on the affair, and even a select bibliography is beyond the scope of this one, which includes only works directly or indirectly cited. Most of the principals seem to have Web pages, and a fair overview of major publications in the U.S. and Europe can be found at the following Internet address: www.physics@nyu.edu.com//faculty/sokal.

> just as we do at the diversity of plant or animal species. Lamenting the "loss of meaning" in postmodernity boils down to mourning the fact that knowledge is no longer principally narrative. (26)

A situation further complicated by the inescapable nature of narrative itself. Scientists especially are burdened with the necessity of playing "by the rules of the narrative game," which do not, in fact, apply to the scientific one. Or as Lyotard explains: "Scientific knowledge cannot know and make known that it is the true knowledge without resorting to the other, narrative kind of knowledge, which from its point of view is no knowledge at all" – or still more epigrammatically – "Knowledge is thus founded on the narrative of its own martyrdom" (28-29).

Lyotard seems to be out of favor with the science side of this debate (to resort to a shorthand of convenience), although his work, Foucauldian and French though it may be, tends in my reading to support it – or at least to demonstrate with convincing logic the impossibility of the two sides' ever inhabiting the same universe of discourse, a prerequisite condition of any constructively critical engagement. One problem with my reading, however, is that what seems to me most applicable to the Sokal controversy and most persuasively descriptive of the scientists' position, both institutional and epistemological, is according to Lyotard's historical analysis anachronistic.[9] Yet the fact that his final analysis of postmodern science as a wholly different "search for instabilities" (53) strikes me as equally apt is not necessarily a contradiction. It may simply indicate that today's scientists, albeit living in "postindustrial society, postmodern culture" (37), are nevertheless not themselves postmoderns, or rather not entirely so.

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[9] See 31-37. The two versions of the narrative of legitimation which evolve during the modern period – one more political, one more philosophical, both "of great importance in modern history" – correspond, respectively, to ideals of practical and intrinsic value, or knowledge to benefit mankind, knowledge for its own sake. Lyotard's description of the first seems remarkably apt for many science critics, at least in terms of stance or attitude: "The subject of the first of these versions [the political] is humanity as the hero of liberty. All peoples have a right to science. If the social subject is not already the subject of scientific knowledge, it is because that has been forbidden by priests and tyrants. The right to science must be reconquered." (31, cf. Robbins and Ross on the "priestly organization" of science, A28; Holquist and Shulman on its protection of "the innermost sanctum of its temple" 53).

The analysis of the second narrative version as it applies to scientists and their relation to the University and the rest of society (534-35) outlines an ideal to which all scientists I know would readily subscribe. As they would to the remark that "today, with the status of knowledge unbalanced and its speculative unity broken, the first version of legitimacy is gaining new vigor" (37).

This is an only slightly different version of the point made by Weinberg in his recent critique of Kuhn's concept of the paradigm shift – "more like a religious conversion than an exercise of reason" – and the uses to which it has been put ("Revolution" 1). "It is not true," Weinberg asserts of one of Kuhn's claims, "that scientists are unable to 'switch back and forth between ways of seeing,' and that after a scientific revolution they become incapable of understanding the science that went before it." The most prominent example of such a total revolution in Kuhn's *Structure,* the shift from Newtonian mechanics to the relativistic mechanics of Einstein, is for Weinberg the revolution that didn't happen: "in fact in educating new physicists the first thing that we teach them is still good old Newtonian mechanics, and they never forget how to think in Newtonian terms, even after they learn about Einstein's theory of relativity" (3).

Scientific thought and practice are perforce hybrid: the sub-elementary particle physicist who works at the unimaginably microscopic scale of Planck's constant works within a quantum paradigm, but on the macro-level of the visible world Newtonian thinking prevails. The "standard *model*" that is the current version of what is known about the natural world is only that, tentative and provisional; and the Grand Unified Theory remains a distant ideal. (We are most of us thus hybrid, I suspect, and have been since the waning of the Middle Ages.) I sympathize with *Social Text* editors Robbins and Ross in their indignant denial of reports in the *New York Times* that they champion "a disbelief in the existence of the physical universe" ("Scientific Priesthood" A 28), but as Sokal, Weinberg, and many others point out, much of the writing in their journal suggests exactly that. They too are subject to the hybrid nature of language, the kinds of linguistic and epistemological flip-flops which our lives and works entail.

It may well be that one such flip-flop has to do with the professional training that today's generation of English professors has undergone. To put aside for a moment the narratives of legitimation and other terminologies of postmodernism, let me suggest that the armies of the science wars are clashing by night over unacknowledged distinctions that can, to a remarkable degree, be subsumed under the outmoded rubric of style and content. We have been so thoroughly trained in the inseparability or even identity of these categories (extended by social construction to context or "conditions" and content) that assertions of difference – tacit or explicit – may seem suspect, retrograde, symptomatic of the "obsessive dualism" attributed to Weinberg et al. (Holquist and Shulman, 1996). Hence my attention here to the memoirs of scientists, narrative cousins to the texts of science itself. In virtually all of

such works that I have read over the years the issue of style is foregrounded, consciously present and inseparable from the writer's view of the social world that is his readership, the scientist's view of the natural world that is his work. Style is a way of doing science – in Watson, notably but not at all exclusively, it is a qualitative factor, often a matter of scientific success or failure.[10] But it is *never* identified or confused with content, the impersonal "what" of nature. Indeed the often flamboyant emphasis on style seems to me like a kind of protective marking off of the legitimately personal from everything else, everything that is legitimately science – a tacit assertion of the objective, if you like, through an insistence on its opposite. It is for this reason, I believe, that so many scientific or science-related texts can seem to their critics to adhere to a politically retrograde or "delegitimated" narrative of the hero. It is not simply the burden of playing by the narrative rules under which the scientist labors, according to Lyotard. It is that this is the *only* narrative: personal and heroic, leading to and "containing" an impenetrable if approximate conclusion that is yet paradoxically independent or outside of it – a conclusion, say, such as $E = h\nu$.

"It is of paramount importance," Max Planck wrote in a passage to which I have already alluded, "that the outside world is something independent from man, something absolute, and the quest for laws which apply to this absolute appeared to me as the most sublime scientific pursuit in life" (13). It is hard to imagine the extreme epistemological flip-flop into which Planck was forced in order to arrive at the quantum as anything other than the result of this real, outside, and independent world, one which tells him something, so to speak, that he neither expects nor wishes to learn. No founder of modern science knew better than Planck (perhaps only Einstein knew as well) the textual limitations on those "laws which apply" and the evident impossibility of establishing common discourse with opponents of their revision. We may thus give him the final word on the nature of scientific revolutions: as he

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[10] A belated note on pronouns: they are here masculine because the memoirs I know are virtually all by men (mostly white, Western), representatives of the group that dominates the science of our century. This is not the place for an engagement with the explicitly feminist critique of science, which rightly plays a large role in the Sokal debates. But I would like to add apropos of Watson that there can be no more devastating exposure of the misogynist conditions under which a woman like Rosalind Franklin worked than the one that is *inadvertently* offered by *The Double Helix*. It is far more effective than the directly feminist defense by Sayre, for example – a defense which backfires through sheer overstatement and inaccuracy. The patriarchal abuses, institutional and cultural, under which Franklin labored did not affect the content of her work in X-ray crystallography from which Watson and Crick profited so greatly; but they certainly affected her "style," very possibly her ability (confidence, etc.) to interpret her own work, hence the measure of her scientific "success."

comments in the *Autobiography* apropos of frustrations in his own career, "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (33-34). And time, not text, is its medium.

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