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Interaction Between Transcription and Posttranscriptional Regulation of Prolactin Chromatin
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SCIENCE 139

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Postmodernism

An essay in a recent issue of Time magazine begins with, "Scientists, it seems, are becoming the new villains of Western society." It goes on to say, "we read about them in newspapers faking and stealing data, and we see them in front of congressional committees defending billion-dollar research budgets. We hear them in sound bites trampling our sensibilities by comparing the Big Bang or some subatomic particle to God."

Does this reflect a growing anticience attitude? If so, the new movie Jurassic Park is not going to help. According to both the writer and producer, the movie intentionally has anticience undertones. Press accounts say that producer Steven Spielberg believes science is "intrusive" and "dangerous."

It is not only outsiders who are being critical. In recent speeches and publications, George Brown, chairman of the House Space, Science and Technology Committee, has seemed to question the very value of science. Brown has observed that, despite our lead in science and technology, we still have many societal ills such as environmental degradation and unaffordable health care. Science, he says, has "promised more than it can deliver." Freeman Dyson seems to share some of this view. In a recent Princeton speech, he stated, "I will not be surprised if attacks against science become more bitter and more widespread in the next few years, so long as the economic inequities in our society remain sharp and science continues to be predominantly engaged in building toys for the rich."

Are these just isolated events, or is something more going on? Harvard's Gerald Holton recently addressed this question from the historian's perspective in a Sigma Xi speech. Holton says "the discussion about science and values has been shifting in remarkable ways" and in this he sees a trend. The trend even has a name: The Postmodern Movement. It is decidedly anticience. Holton acknowledges that today this movement represents a "minority view." However, he goes on to warn, "but a view held in prominent circles, among persons who can indeed influence the direction of a cultural shift."

What is the appropriate response? One's first reaction to all of this is apt to be indignation: How can science be blamed for all the ills of society, especially when science has contributed so much? But the perception remains that science has promised too much. Time puts it this way: "[Scientists] have silently acquired in the proposition that if we just keep writing checks and leaving them alone, science could solve the problems of the world."

Surely no informed person can doubt the essential importance of science to our future. Just imagine, for example, what society would be like today if it were not possible to test for HIV in blood. The public seems to understand the value of science, if only intuitively, as shown by polls that place science among the most admired professions. That status would be impaired, however, if the public starts perceiving science as yet another entitlement. Scientists need to be more careful to note when, as mathematicians would say, science is necessary but not sufficient.

This tack is taken in a new report from the three U.S. academies (hereafter "the Academy") that acknowledges some of the perception problems mentioned above in what it refers to as the "changing context for science and technology." The Academy proposes a new "covenant" between science and society that is rooted in national goals, two for science and one for technology:

- The United States should be among the world leaders in all major areas of science.
- The United States should be the clear leader in some selected areas of science.
- The federal government should cooperate with the private sector to ensure U.S. leadership in selected technologies that promise major industrial and economic impact.

While not the sole answer, the Academy's report is responsive to some of the current debate. Most importantly, the report should help focus the discussion along constructive, rather than negative lines. The Academy concludes with some very good advice: "This country needs to explore how to ensure the progress of science and how to use new knowledge more effectively to meet human needs. If we succeed in doing so, human well-being will be greatly improved."

Richard S. Nicholson

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Let Me Count the Ways

I was amused and disturbed by a phrase in Gary Taubes’ article “Measure for measure in science” (News & Comment, 14 May, p. 884): “objective measures of quality.” Are we to believe that quality can now be objectified? Will our proclamations now be expected to take the form, “My love is like a single flower of Rosa grandiflora with a peak visible light reflectance of 650 nm”? Shall we expect the quality of mercy to be not only strained, but captured in a graduated cylinder? Will the Institute for Scientific Information (ISI) provide us with an assessment of whether we are disgusted by the emphasis on citation analysis, or just annoyed?

Still, I suppose this is the price of entry to the brave new world. I eagerly await the next great contribution from ISI: the subjective measure of quantity.

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Science Watch’s selection (1) of M. Leid et al.’s paper (2) as the hottest of 1992 reveals a problem with this type of analysis. Science Watch did not inform its readers that Leid et al.’s paper was not the only, and not even the first, of a series of papers reporting on similar discoveries. One was published a month earlier in the same journal (3), three others the same week (2, 4), and three more a few months later (5).

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4. X. K. Zhang, B. Hoffmann, P. B. Tran, G. Graupner, M. Plathl, Naturu 385, 441 (1992); S. A. Kliewer, K. Umesono, D. J. Mangelsdorf, R. M. Evans, ibid., p. 446.

I would like to clarify a reference to my comments by Taubes in his 14 May article “Measure for measure in science.” In fact, at the CHUL Research Centre, we do not use the numbers of citations of individual researchers to account for 40% of their performance; rather, we use the impact factor of the journals in which they publish. Moreover, to avoid bias toward specific research fields, we are putting in place a correction factor that takes into consideration the different levels of citations in various research disciplines. Another criterion of performance, researchers’ grants, as Taubes correctly indicates, counts for 40%. High-quality publications and grants are both well-recognized criteria of performance in research.

To complete the assessment, an additional parameter, the number of graduate students and postdoctoral fellows, accounts for the remaining 20%, and not students’ performance, as indicated by Taubes. This last parameter endorses the academic involvement of researchers.

We are convinced that the use of these objective criteria has played a key role in the dynamic development of our institution, which now ranks among the top biomedical research centers in Canada.

Fernand Labrie
Director of Research,
Centre de Recherche du CHUL,
Centre Hospitalier de l’Université Laval,
2705, boulevard Laurier,
Québec G1V 4G2, Canada

AAAS’s Red Scar

Unlike Wernher von Braun, I was not a celebrity being handled with kid gloves by the military in the tumultuous 1950s (News & Comment, 23 Apr., p. 486). No one “warned” me not to join the AAAS in 1950 or to not accept election as a AAAS Fellow in 1956. But my experience was consistent with von Braun’s. In order to participate in a project under an anticipated contract from the Air Force, I needed “secret” clearance. The Personnel Security Questionnaire I filled out in 1956 requested much detail about every organization in which I had ever held membership. I filled it out very carefully, because, although I didn’t have handlers, I did recognize the problem that I would face when it was observed that my parents, who had been naturalized citizens since 1913, were born in Russia. So I was meticulous as I listed 24 organizations (including even three alumni associations, five honorary societies, the American Automobile Association, and the Museum of Modern Art).
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win's book. The other was Eugene M. Shoemaker, a young geologist with dreams of going to the moon himself. In 1963 Baldwin published a second book, The Measure of the Moon, which presented an abundance of new observations and ideas, most of which were confirmed during the Apollo missions. Thus Wilhelms's own book is "dedicated to the amazing Ralph Baldwin, who got so much right so early." Perhaps Baldwin's accomplishments might not seem so amazing if he had been a full professor advising a succession of eager graduate students. But Ralph Baldwin never entered academia; his primary activity was running the family machinery company in Michigan. He pursued his lunar studies alone, as time permitted. (Now retired from the family business, he continues to study the moon.) Such achievements by this route must be well-nigh unique in the annals of contemporary science.

Wilhelms credits Eugene Shoemaker with being primarily responsible for persuading the National Aeronautics and Space Administration to incorporate geology and geophysics into the Apollo missions. Not only was Shoemaker himself an enormously persuasive advocate with a fine-tuned political sense, his cause received timely aid now and then from the successes of unmanned Soviet missions to the moon. One more scientist whom Wilhelms lists as a leader in the development of lunar science is Gerard P. Kuiper (1905–1973), who founded the Lunar and Planetary Laboratory at the University of Arizona. In addition to such key figures Wilhelms discusses the contributions of all the men and women who worked on lunar geology, down to the youngest recruits. In a final chapter, entitled "Debriefing," he reports where each of them is now. Twenty-one years after the flight of Apollo 17, all but a few are still living and active as lunar or planetary scientists; practically none have retired from scientific pursuits altogether.

Once it became apparent that the manned spacecraft would have the capacity to carry scientific apparatus and sample boxes, there were strong clashes over who should go to the moon—test pilots or scientists. NASA favored test pilots. Scientists favored scientists. Wilhelms's own comments are
refreshing. He says that although many scientists regarded the test pilots as dumb fighter jocks incapable of learning science, that opinion definitely was not shared by those geologists, including himself, who led the pilots through their rigorous training for geological reconnaissance and sampling. In the end, only one scientist-astronaut, geologist Harrison (Jack) Schmitt, flew to the moon, but Wilhelms reports that all of the pilot-astronauts performed well and some of them provided outstanding geological observations.

About half of the book is devoted to descriptions of the planning for each mission and reports of what the astronauts observed on the ground, what decisions they made in real time, what rocks and soils they collected, and what these samples added to our geological knowledge of the moon. Some sites of key geological importance could not be visited, much to Wilhelms’s disappointment. Nevertheless, in an account of the geological evolution of the moon keyed to a radiometric time scale he shows that the missions yielded answers to most of our long-standing questions. For example, with regard to the fundamental questions of how the moon originated and why Earth is the only planet between the sun and Pluto possessing a single large satellite, he supports the widely accepted scenario promulgated at a conference at Kona, Hawaii, in 1984. After the separation of an iron core, the protoearth was struck a glancing blow by a passing body about the size of Mars; most of the iron from the impactor’s core fell to Earth while a disk of vapor and debris from both the impactor and Earth’s mantle spun out into space. Some of this hybrid material, which lost all of its water and most of its volatile elements in the event, coalesced to form the moon. Thus the odd pairing of Earth and moon arose by sheer happenstance.

Wilhelms points out that the Apollo program, which was launched by politics in the late 1950s and slowed by the Vietnam War in the late 1960s, was curtailed by economics in the 1970s. “By the time of Apollo 17 [in December 1972] a magnificent and sophisticated network of rocketry, flight operations, geologic and geophysical support, and geologic and laboratory analysis was functioning with smooth precision. Now it was time to shut it all down and turn out the lights,” writes Wilhelms. Was it worth it? “Hell yes,” he says, “it was worth it, and to pass up the opportunity to land people on the Moon when the once-in-a-lifetime opportunity arose would have been unconscionable.” As one of the continuing benefits of the missions he points to the concept of Spaceship Earth and the urgency it instills in us to preserve the natural ecosystems of our planet. Unlike the prophets of space colonization, Wilhelms believes that the missions showed that Earth is the only viable abode for our species.

This book, which includes four highly informative appendixes, copious notes, a selected bibliography, and numerous illustrations, documents the history of a specific scientific enterprise. The author defines technical terms and explains geological principles so that the general reader can enjoy it for the grand adventure story that it is. At the same time, the volume is certain to find a place on reading lists of many undergraduate and graduate students in the earth and planetary sciences. With its wealth of anecdotes about personal relationships among those involved in lunar exploration and how policies were hammered out, it should also interest students of the social sciences. In the current controversy about who should write the history of science, scientists or historians,

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from any given person, what remains in fact inalienable to the object and to all those through whose hands it has passed is the fame or reputation that goes with it. In other words, it is not truly the object itself that is inalienable but only the prestige attached to it, which can only be gained through its intrinsic alienability. Reflecting on such points we might conclude that the use of the contrast between alienable and inalienable possessions is perhaps not after all the best way of talking about the phenomena Weiner wishes to identify.

These objections aside, Weiner has given us a very rich reconsideration of ethnographic data from all parts of the Pacific and forces us to rethink the major categories in terms of which Pacific societies have been interpreted. She is particularly strong in the stressing of female roles, female reproductive symbolism, and the role of bonds between cross-sex siblings as against those between spouses in the construction of systems of cosmological authentication. Somewhat paradoxically, her argument is less strong, as we have seen, with regard to its overall conceptualization. There are indeed inalienable possessions in some Pacific cultures, and there is indeed a process of strategic retention and disbursement in

Weiner argues. Nevertheless, her book is a monument to her creative rethinking of the analysis of Pacific societies from a female point of view.

Gabriele Stürzenhofecker
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The Greening of Plants


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**Books Received**


Vignettes: Retrospectives on Development

I have been told that Arthur Arndt, who made the first motion picture of slime mold development, used to argue in his public lectures in Germany in the 1930s that their life history was so amazing it could only be explained by resorting to vitalism! As a beginning assistant professor, I gave a brief lecture at the Marine Biological Laboratory at Woods Hole in Massachusetts, and apparently word had spread to the world of journalists about this queer organism. Some days later I received a letter from J. J. O'Neill, the science reporter of the New York Herald Tribune, saying that he had heard I had done something more important than discovering the atomic bomb: I had created a multicellular organism! I quickly replied that it was not I but God who had managed the remarkable phenomenon and please, for my sake, to restrain his journalistic ardor. My leaning so heavily on support from God may be excused by my anxiety not to be embarrassed by the newspaper. . . . If I had wanted to give the reason that I really believe explains the peculiar life cycle of slime molds I would have said natural selection, the brainchild of Darwin and Wallace.


The insularity of disciplines before the 1970s is reflected by popular slogans—the condensed wisdom of the elders. The biochemists' doctrine was "never waste a clean thought on a dirty enzyme." In the early 1970s, as an editor for the Journal of Biological Chemistry who was considered to be a developmental biologist, I received two kinds of papers to review. The first had to do with purification of an enzyme from an embryonic source. The second was any paper dealing with choratin. In those days, scientists studying choratin considered themselves biochemists, and biochemists, when forced to categorize a paper on choratin, thought it had something to do with developmental biology. After all, you should never waste a clean thought on a dirty enzyme. Developmental biology was viewed as hopelessly complicated and, therefore, to be avoided by a traditional biochemist. Biochemistry, when applied to development, measured the increase or decrease of an enzymatic activity as development proceeded. The sophisticated biochemist distinguished between a genuine change in the amount of a protein and the change simply in enzyme activity. . . .

The embryo elders held the opposite point of view. I remember hearing a famous embryologist say that over the years he had developed a working relationship with the chicken embryo. The embryo promised to occasionally divulge a secret if he, in turn, promised never to homogenize it.


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