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Thoughts on Research Fraud

by Paul J. Friedman, MD
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Just before Christmas, the New York Times ran an article entitled, "Global Trend: More Science, More Fraud," inspired by the revelation that stem cell research results in South Korea had been fabricated. This scandal certainly goes to show that the developing world is catching up with us in more ways than one! But as is often the case, the problem of defining what goes in scientific research is more complex than can be captured in a newspaper headline, or for that matter in a courtroom proceeding.

One aspect of this complexity came to light incidentally when, on January 9, the Times printed the translated text of the summary of Seoul National University's report on **Hwang Woo Suk,** the researcher whose team published the faked findings. The report confirmed Hwang's claim of having cloned a dog successfully, a significant breakthrough for that species, but showed that the more difficult task of extracting human egg nuclei had not been achieved. Just as important as the exposure of Hwang's misdeeds is that none of the other members of his team are named in the report. In fact, the last paragraph contains a real teaser: "Not all the wrongdoing of all the individuals associated with fabricated publications can be revealed by this committee."

The fact is that people committing research fraud can't do it without the silent acquiescence – if not knowing participation – of those around them, colleagues or students alike. There is always someone else in the lab who knows that something hasn't been done correctly, and whether research fraud is exposed or corrected early depends on that person courageously coming forward to challenge the falsehoods. Since papers from 2004 on are implicated in Hwang's case, this is not a one-time fabrication; it never is, by the time it is revealed. I expect more will be revealed by diligent bilingual newspaper reporters before this commentary is in print!

A Global Pandemic?

Once upon a time we were mainly concerned that the competitive publish-or-perish environment of American science was producing a rash of embarrassing cases of plagiarism and falsification. This was documented by one of the Times' authors, **William J. Broad**, in his book with **Nicholas Wade**, *Betrayers of the Truth*, more than two decades ago. Now that foreign scientists' results are of a quality and importance to be published in the most respected U.S. journals, this problem has re-emerged, amid fears that it is becoming a global pandemic.

But the subject is dogged by semantic confusion. Consider the use of the term "research misconduct" instead of "research fraud." The legal establishment has induced government agencies to define "FF&P" – fabrication, falsification, and plagiarism – as research misconduct, not fraud. The reason is that "fraud" has a specific legal meaning, requiring that someone suffer a tangible loss as well as implying deliberate dishonesty on the part of the perpetrator. The lawyers were worried about having to prove that someone had suffered measurable misfortune as a result of the "crime"– a problem similar to the one they face in drug liability trials. So we are stuck with "misconduct," which has a much broader connotation than "fraud," and is much harder to get indignant about and to offer sensible warnings about. The Times' somewhat inflammatory article uses the term "fraud" 16 times (including the title), despite its inappropriateness from a legal point of view.

What is wrong with lumping research fraud with the broader category of

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Inside

Friedman *from p. 1*

research misconduct? A long time ago a committee of the National Academy of Sciences opined that more damage was done to science and the scientific record by all the little things people did to cut corners and expedite publishing research results or getting grants. A recent manuscript that I reviewed said the same thing, perhaps believing it was an original observation. Instances of FF&P are rare, considering the amount of funded science being done, and few misdeeds rise to that level. Efforts to improve "research integrity," the opposite of misconduct, run into the semantic difficulty of separating relatively minor misdeeds from FF&P. But misconduct of the lesser sort is indeed the antecedent of research fraud, for it turns out that serious misdeeds don't occur without an individual's prior deviation from ethical practice, which may be minor misconduct, but is not classified or investigated as "research misconduct" by our semantically crippled university system. We certainly don't snoop into how researchers treat outliers or round off their numbers or use bound notebooks, and whether they cite all relevant prior work or ideas. Unless we are doing an investigation of a charge of serious misconduct, it would be inappropriate to ask about such details of an independent investigator's work. But they have cropped up in the history of more egregious fabrication or falsification.

Our First Case

I became familiar with this distasteful subject back in the '80s when I was associate dean for academic affairs (half-time) in the School of Medicine and read in Science magazine of the research fraud at Harvard committed by **John Darsee**. This episode tarnished the reputation of the Chair of Medicine who sponsored him, who happened to be our former chief, the redoubtable **Eugene Braunwald**. It turned out that much of the delay and denial by Harvard were due to the fact that the investigation was initially carried out in the department under Braunwald's supervision. I looked at our policies and procedures and saw a gap, so I asked the Dean (**Bob Petersdorf**) if I could form a committee (the UC way!) and draw up some guidance for what to do in case we had to deal with such an issue. We led off by requiring the recipients of fraud charges to pass them on to the Dean's Office, so as to avoid the kind of departmental conflict of interest that had arisen at Harvard. We put together a three-page set of procedures and had it approved by our faculty council. I should mention that we invited the general campus to take a lead in the development of procedures, but the VCAA told us (a little snootily, I thought) that unlike the medical school they had no need of such a policy.

> *"Plagiarize!*
> *Plagiarize,*
> *Let no one else's work*
> *evade your eyes."*
>
> *Tom Lehrer, "Lobachevsky"*

The policy lay dormant until 1985, when **Bob Slutsky**, a bright and aggressive cardiologist, was up for appointment as a faculty member in radiology, having served as a resident and research fellow under the tutelage of another student of Braunwald's, **Charles Higgins**, someone with an extraordinary bibliography and work ethic. One of the referees, the founding chair of the Department of Radiology, **Elliot Lasser**, noticed that values reported in tables in two papers were identical, including the standard deviations, even though the number of dogs studied was not the same. Sensing that this was probably an error of some kind, since it was most unlikely mathematically, he asked that Slutsky look up the experiments and check the data. That was evidently the key challenge, because Slutsky quickly quit UCSD, saying he couldn't find the data, bringing in only a handful of strip charts from the experiments.

For a faculty unaccustomed to dealing with research fraud, this was perhaps the ideal situation. The perpetrator had left; there was no one to protest that his rights were violated; there was very little experimental data to mull over; the proposal of the lawyer hired by Slutsky sounded like a whitewash, and we had time and felt little pressure so we could follow our procedures in a deliberate fashion. The committee of investigation appointed by the Dean, led by **Richard Peters**, with me serving *ex officio* as secretary, got to work. We concluded quickly on the basis of testimony that at least two published papers and another manuscript contained fabrications. We reported this to the Dean, suggesting that another committee be formed to review all of Slutsky's 137 published papers. We were plagued with the question of when reported research stopped being reliable, since we were unable to draw a line.

To make a very long story short, it took fifteen months for the new committee to review all these papers and any objective data, interview available coauthors, and decide on criteria for assessing papers as probably valid, questionable, or fraudulent. As a working member of the committee, I drew the papers on cardiac CT in dogs post ligation of a coronary artery, much helped by having a log book of the research use of the scanner. We wrote to the journals – that's a story in itself – asking them to retract the fraudulent papers, flag the questionable ones, and note the acceptability of the rest. Our chair emphasized that it would be wrong to damage the careers of various trainees whose names were on various papers if there was no reason to suggest the papers were invalid. (You can read more of our philosophy and what else I did in the footnoted references.)

What's Plagiarism Legally?

That was, as I say, an ideal first experience. Later cases were messier and less satisfactory in outcome. The federal government's involvement increased, as did the complexity of the policy and procedures put out by the campus, dictated in large part by federal requirements. The

latest version is still under prolonged revision. The involvement of lawyers has also increased, with results which can only make a scientist sad. In the Slutsky case the University attorney came down from Berkeley, talked to the committee, found out what our game plan was, and went back to Berkeley. We never heard from our lawyers again. It's not like that anymore.

This is not a problem that is well suited to legal procedures. Consider plagiarism. This is still considered the easiest of the forms of major misconduct (fraud) to establish – wrongly, in my opinion. Computer programs have been devised to compare texts to see if the same words and phrases are used in both. But that's only a small aspect of plagiarism. The definition is usually taken as "using the words or ideas of another without proper credit." Quoting someone else's words has formal limits. To quote a paragraph requires setting the extract off with quotation marks. One sentence can be quoted without putting it in quotes. What about longer borrowings? I believe one should lean toward the paragraph model. Note that there can be difficulty when it comes to "Methods," which may be written essentially identically to those in a previous publication, by the same or a different author. But credit or attribution is what is really important. Footnotes or endnotes should identify the source of phrases used with or without quotes. Some righteous individuals describe self-plagiarism as just a form of plagiarism, and want to stigmatize scientists who quote themselves without attribution. This is clearly an over-reaction.

It doesn't sound too difficult to avoid plagiarizing words, but there's another part of the definition which clearly causes problems: the attribution of ideas. "I thought of that first" is the outcry from the injured party. Or, "I thought we were doing this together." Or "this was really the idea of the whole laboratory group." And when do these complaints surface? When there is competition or disagreement of some kind between scientists or supervisors and supervisees, not as a result of administrative checks on the integrity of researchers. Can we really call this plagiarism? Can we bring the full weight of inquiries and investigations to bear on a faculty member accused of failing to share? I believe one may take charges in this context as evidence of conflict rather than fraud or misconduct. Note that the Office of Research Integrity has officially advised that it will not investigate charges that can be interpreted as disagreements over authorship or ownership of data. It's up to the Universities in their wisdom!

What about the graduate student or post-doc who goes forth to do his or her own thing? How close to the mentor's ideas can one go without seeming to be dependent on another's work? Charges of plagiarism rarely arise in this context, but disputes and disagreements are common. (Usually the student loses.) Ironically, the most conspicuous failure of foreign post-docs to become acculturated is in the treatment of attribution. More than one has quoted from a respected author out of "admiration" – perhaps mixed with difficulty putting things into good English – but inexplicably omitting the usual forms of citation. One must take the "admiration" excuse with a grain of salt.

We Have a Role to Play

What is the bottom line? I'm sure everyone is relieved that U.S. researchers don't exercise a monopoly of research misconduct or fraud. However, some major research journals are chagrined by what has emerged about some of the international articles they have published. How shall we deal with this as an ongoing problem? Of course, don't believe everything you read. But also work to elevate the standard of research integrity where you can. Keep preaching to trainees about ethical research, and the importance of sticking to the norms. Get them to understand the motivations for cutting corners or making things up; they are in a competitive profession so they have a pretty good idea of the pressures on the individual. Tell them about the psychology of breaking rules, the slippery slope, the pride in putting one over on others, in short, the theory of deviance as described by UCSD sociologist **Jack Douglas**.

Teach trainees and junior faculty to seek the advice of a trusted individual in science, for some suspicions are just the result of incomplete understanding of what is going on. You, fellow emeriti, may turn out to be the people who will be consulted about such issues. Are you prepared to act as an unofficial ombudsman? You have to be able to advise a potential whistleblower of the kind of facts needed to bring a case to the attention of the authorities – and in detail what will happen if the suspicions are right or wrong. I have suggested that trainees can start by openly challenging as "mistakes" various deviations from good practice – since correcting them may prevent far worse deviations in the future; remember that fixing science is more important than "catching" and punishing errant scientists who have exercised bad judgment. This is a vote for the informal, free-wheeling laboratory meeting. Finally, be willing to reassure your friends outside the university – despite headlines such as that in the Times – that by far most science is still reliable, but that anything really important should be verified or refuted by other researchers. Standards are not the same everywhere, and the proper scientific test of reproducibility of results cannot be ignored. That the press may magnify a result should not affect how you think about reported findings. Trust, but with a dose of skepticism.

Bibliography: Misrepresentation and responsibility in medical research. NEJM 1987; 317:1383-1389 (R.L. Engler, J.W. Covell, PJ Friedman, P.S. Kitcher, R.M. Peters); Fraud in radiologic research: a perspective. AJR 1988; 150:27-30, Correcting the literature following fraudulent publication. JAMA 1990; 263: 1416-1419; Integrity in Biomedical Research. Academic Medicine 1993; 68: S1-S102. (edited and partially authored by P.J. Friedman).

UCSD

Remembering César Graña

By Joseph Gusfield
Professor Emeritus of Sociology

To have a conversation with **César Graña** was to experience the sheer joy of imagination and knowledge at play. A classicist I once knew expressed his view of the goal of education as enabling us to engage in "good talk." César Graña was "the Master of the Good Talk," a man whose grace, erudition, and poetic style made the analytical play of the human intellect itself a work of art.

César was known as a sociologist of culture, especially literature and art. He had published a book in 1964 that is still a classic study of what he called "the literary mind." It was entitled *Bohemian versus Bourgeois: French Society and the French Man of Letters in the Nineteenth Century.* Two years later it was given a new title which more clearly expresses the scope of his interests, *Modernity and Its Discontents.* It is a study of the literary mind and its alienation from modern culture. It has become a classic of cultural analysis whose importance transcends the history of a specific time and place. In 1971 he published *Fact and Symbol: Essays in the Sociology of Art and Literature,* a book nominated for the National Book Award.

To reread Graña is to hear him speaking. He wrote as he spoke, with a style of elegance, of insight, and of complexity expressed with both succinctness and illumination. It was poetic in the richness of the metaphors used. The subjects had been redirected into new channels with a form that was as much art as analysis. As the philosopher of art **Arthur Danto** has described art, so was César's language "the transfiguration of the commonplace."

César was born in 1919 and raised in Peru. He was educated in Lima at the University of San Marcos and in the United States at Brown and Duke Universities. He received his Ph.D. in Sociology from

UC Berkeley in 1957. Throughout his lifetime he frequently visited and lived in Spain. His Hispanic affiliation was important; he was one of the founders of UCSD's Iberian Studies program. But his love of the United States was deep. Unlike so many 1960s intellectuals and academics, he held a great admiration for America. Its egalitarian and democratic values and its appropriation of aristocratic virtues were for him a unique accomplishment. Some thought him a conservative, perhaps because he had a high respect for ritual and tradition. He was both Catholic and catholic; at home in French, Spanish and English literature and the art of Europe and America.

Irma and I had the good fortune to spend a Holy Week in Seville with César along with **Marc Swartz**, our colleague in Anthropology, and **Booker** and **Susan Kelly**, friends from Santa Fe, a city he loved. He knew Seville and was gathering material for a book on the city. He was a magnificent guide as we toured Andalusia and Seville. His knowledge and descriptions were enriching. He was a keen observer of the Moorish influences in architecture and language and the Hebraic tradition in the liturgy and song of Spain. It was a marvelous week. The entire city was suffused with the joy of living and the rituals of spirituality, with food and drink; and the color and sense of sacrifice that the floats exhibited as they made their way from each of the 55 churches to Seville's Cathedral and back. The city and César were as one with each other. Without his knowledge and his colorful, scholarly observations it would have been just another parade.

He had a great gift, in talk and in writing, for expressing in pithy words and elegant style, cultural matters of considerable depth. Consider how he described the image of the bourgeois world in France against which the literary and artistic Bohemia of the time sought to pattern their own *vie de Boheme*: "The bourgeoisie represented ambition without passion, possessiveness without depth of desire, power without grandeur, everything that was spiritually paltry and antivital, everything that was inadequate and pettily self-protective, in a psychological and even a biological way. Greed was bourgeois, but so were carpet slippers and colds." (*Bohemian versus Bourgeois*, pp.68-69.)

He shared much of the French Bohemians' alienation from the modernity of the modern rational, organized world, yet his own thought was too complex to subscribe to an unadulterated love of the past and the romantic. He looked backward but did not forget to turn forward as well. Nor did he engage in simplistic indictment of the modern and the scientific. He valued material progress yet he valued also the qualities of analytic intellect and the play of imagination that were shunted aside in the triumph of rationality and organization.

Literature and Art were more than the object of his scholarship. They represented ways of experience and understanding given too short a shrift in the modern world of material advance. He expressed these differences in the sense of intellectual freedom and forms of knowledge: "To the scientist, unfreedom and constraint are represented by the inability to solve specific problems . . .

Literature . . . is simply a different kind of knowledge . . . In literary art the opportunity for intellectual exertion, curiosity, struggle – in a word, freedom – depends on the possibility of returning to the human predicament, on the assumption that human problems are in some sense insoluble, just as the moral and aesthetic imagination are in some sense inexhaustible." (*Bohemian and Bourgeois*, p.199.)

It was this sense of the aesthetic in life as well as in academia that for me was so valuable in knowing César. It was tragic that the qualities which are so memorable were, in his later years, less appreciated by students and by university organization. He published little in the last fifteen years of his life though he continued to write. (Some of his essays were published posthumously as *Meaning and Authenticity*.) He could never play the "career game" most of his colleagues had mastered.

He died in 1986 at the age of 67 in an automobile accident en route from Seville to Cadiz. With his death this campus lost not only a fine scholar but one of uncommon elegance, style, and creative imagination.

Emeriti Website

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http://emeriti.ucsd.edu

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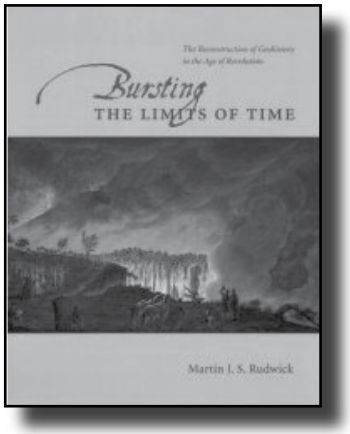
Bursting the Limits of Time



By Martin J. S. Rudwick
Professor Emeritus of History

As a title, this may suggest yet another book of pop science, telling a heroic story of how "Science" in shining armor slew the dragon of "Religion" and released us all from the illusion of living in a world created only a few thousand years ago in one week flat. But any such story is just as much a myth as the fundamentalist one it rightly opposes. The historical reality is more complex, but also much more interesting than the stereotype of intrinsic conflict between science and religion.

I borrowed my title from a famous text by **Georges Cuvier**, the French zoologist who is a key figure in my story. Almost two centuries ago, Cuvier suggested that those whom we now call earth scientists could properly aspire to emulate another more prestigious group. Astronomers had already "burst the limits of space," by making the solar system and the stars beyond it accurately *knowable* to humans confined to one small planet. In the same way, Cuvier claimed, geologists could learn how to "burst the limits of time," by making the vast pre-human history of the earth reliably knowable to humans confined to the present moment. (By this time, everyone working in the natural sciences – though not the general public – realized that the earth's timescale must be inconceivably vast in relation to human history, although it could not be quantified; those who were also religious believers recognized that it was misleading, even perverse, to interpret biblical texts with inappropriate literalism, not least because it obscured their religious meaning.)

The way to "burst the limits of time," according to Cuvier and his contemporaries, was to apply the insights and methods of ordinary human history to the stuff of the natural world. Clues such as rocks and fossils, mountains and volcanoes, were in effect nature's documents and archives, nature's monuments and chronicles. If interpreted correctly, they could yield *nature's own history*, a history no less reliable for not having had any human witnesses to record it at the time. This was the idea that lay behind the most creative period in the entire history of the earth sciences (not even excepting the much more recent period that saw the establishment of plate tectonic theory, with distinguished contributions from scientists at SIO).

Cuvier's parallel between the then newly named science of "geology" (covering all the modern earth sciences) and the well established science of astronomy, combined with his transposition of a historical perspective from the human to the natural sciences, gave the geologists of the early nineteenth century the template on which they could develop their main research program. They learned how to reconstruct the history of the earth, including its living organisms, in all its unexpected and surprising complexity. Within about half a century – or in a single scientific lifetime – they worked out the course of "geohistory" in a way that endures in its main outlines to the present day. Modern earth scientists use a geohistorical perspective as a matter of course, without needing to think about what they are doing. But this is something that their forerunners knowingly and deliberately borrowed from human historians, from those on the other side of what often now seems to be a gulf between "two cultures" (but which wasn't treated that way two centuries ago).

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Rudwick *from p.5*

Earth scientists are not only ones who have profited from this great intellectual transfer. It's not for nothing that the young **Charles Darwin** regarded himself primarily as a geologist. Only gradually did he find himself diverted into a problem that the geology of his time had made acute, namely the mode of origin of new species. So he then transposed the historical perspective from geology into biology, by showing that organisms – their anatomy, their physiology, their ecology, and so on – can only be fully understood by taking their past *history* into account. And it's at least arguable that the same historical perspective, first worked out in the earth sciences, has now also permeated other natural sciences such as cosmology.

So the story I tell in my book (and in its sequel volume, which I'm now busy completing) is the story of the gradual adoption of a historical perspective in what became the earth sciences, during the period of the late eighteenth and early nineteenth centuries that historians sometimes call "the age of revolution" (the revolt by transatlantic colonials against His Britannic Majesty **George III** being the first of several such upheavals). I've tried to emulate the "savants" of the time in being as international and multilingual in my coverage as they were: serious scientific debate, then as now, knew no political boundaries. But having worked as a scientist before reinventing myself as a historian in mid-career, I've tried hard to make the story accessible to both groups, and therefore also to readers who don't belong to either.

The research that lies behind my book has taken more years of my life than I like to recall, but an important phase was the period I spent teaching at UCSD. In several seminars in the Science Studies Program and the History Department, I had the stimulus of some very smart and likeable graduate students, whose lively input improved my argument far more than they can have imagined. In a more focused way, giving the Faculty Research Lecture in 1996 was an invaluable opportunity to try out that argument condensed into less than an hour. And throughout my ten years at UCSD I enjoyed the calming effect of working in the library at SIO and looking out on an incomparable view of the ocean. Under the leaden skies of an English winter, and even with the aesthetic and intellectual pleasures of the English Cambridge, I do sometimes pine for sun-drenched La Jolla.

Bursting the Limits of Time: The Reconstruction of Geohistory in the Age of Revolution, University of Chicago Press, 2005 (ISBN: 0-226-73111-1), $45.

Anecdotage

by Sandy Lakoff

All Things Bright and British

Ralph Lewin recalls that when he was a schoolboy in Northwest London, the United Dairies of Swiss Cottage delivered milk daily by horse-drawn cart. The rule was: "Leave your shilling in an empty bottle, and he leaves your pinta." Which gave rise to two untrue but amusing tales:

• *A housewife called to the milkman: "Do you have the time?" He replied "Yes, lady, but who'll hold my horse?"*

• *Then there was* **George**. *Friday night, closing time at the pub, he put his last shilling into the machine, wheels whirled and whirred and stopped, and out showered 120 shillings. Happily he filled his trouser pockets, his jacket pockets and his overcoat pockets, and staggered home. To get at the house key he sat on the doorstep and carefully took out all the coins, piling them neatly in 12 stacks of 10. Then, quietly, he opened the door, went upstairs, and retired for the night. Next morning his wife commented: "You came home late last night; I didn't even hear you. Guess what I found on the doorstep this morning." "120 shillings" said George. "No, 120 pints of milk."*

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They may not brag about having sex as much as the French do, but the Brits are unrivalled at talking about it. In the Times of London recently, columnist **Richard Morrison** discussed a survey of modern British attitudes on sex, courtship, adultery and the like. He was heartened that men "are still intent on living up to, or down to, our eternal stereotype." The results go to show the truth of women's view of men, he noted, as summed up by the "protofeminist" **Helen Rowland** more than eighty years ago: "The follies which a man regrets most in his life are those which he didn't commit when he had the opportunity." **Dorothy Parker,** he adds, said it in verse: "Love is woman's moon and sun; man has other forms of fun…"

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Sabbatical Souvenirs

While a Fellow at the Woodrow Wilson International Center for Scholars in Washington in 1974, I attended a reception for Judge **Robert Bork**, then newly named Attorney General. Making conversation with his wife, I mentioned a short bio of Bork that had just appeared in the New York Times, which noted that on becoming Attorney General he insisted on driving himself to work in the Justice Department in his beloved old Volvo rather than being driven there by chauffered limo. It was accompanied by a photo of him in the driver's seat. "Well," she said, "they got it all wrong. Actually, he hates the Volvo, but he doesn't think it would be morally right to sell it to anyone because it's such a lemon, so he intends to drive it until it falls apart."

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At the National Humanities Center in North Carolina in 1980, the Fellows were asked to submit entries in a limerick contest. The results were reported as "Terse Verse, Could Be Worse." The winner was **Judith Ferster**'s entry about our distinguished colleague, the literary critic **Cleanth Brooks**:

An eminent scholar named Brooks,
Was called a "New Critic" in books.
He said "I'm as handsome
As John Crowe Ransom,
But don't judge our works by our looks."

Please clip and fill out this form and send it to me c/o the Academic Senate Office Campus Mail 0002. We will use the info to update our records and report to the administration on the important contributions UCSD emeriti are making to our professions and community. Please also notify the office if your address should change.

- Mary Corrigan, President

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Chronicles
February 2006

Mark Your Calendar!

Sol Penner

Distinguished Professor of Engineering
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will speak to the Emeriti Association on

Nuclear Energy for the Future

Wednesday, March 8, 4:00 pm
The Green Faculty Club

Chronicles
Newsletter of the UCSD Emeriti Association

EA

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