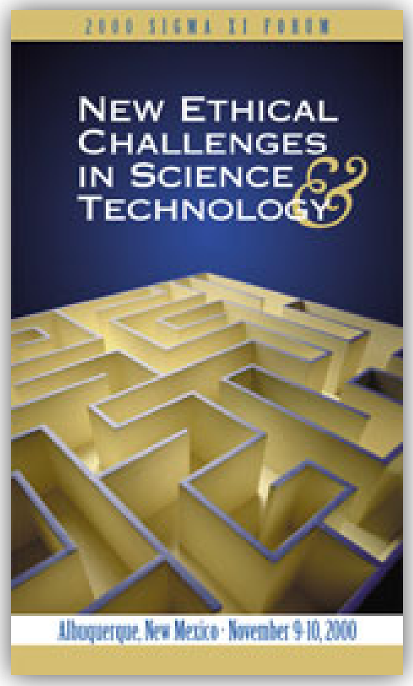
2000 Sigma Xi Forum Proceedings

New Ethical Challenges in Science and Technology

November 9-10, 2000
Hyatt Regency Albuquerque
Albuquerque, New Mexico



Plenary Sessions

Concurrent Breakout Sessions

Teaching the Responsible Conduct of Research: The Why, The What, and The How

An Acadia Institute study, funded by the National Science Foundation, found that faculty, in general, felt that students learned the responsible conduct of research by "osmosis" during their graduate training. But the study also indicated that mentoring was in short supply. Conflicts often arise when research groups lack explicit understandings about forms of credit, the basis for credit and grounds for assigning authorship. Suggestions were offered for explicitly handling issues of research credit, including guidelines from a consortium of biological science editors. Teaching the responsible conduct of research involves making explicit information that is often implicit.

Session Leader: **Vivian Weil**, Director, Center for Study of Ethics in the Professions, Illinois Institute of Technology

Panelists: **Judith P. Swazey**, President, The Acadia Institute PHS Policy on Instruction in the Responsible Conduct of Research (RCR); and **Stephanie Bird**, Special Assistant to the Provost, Massachusetts Institute of Technology

Intellectual Freedom and the National Laboratories

The culture of intellectual freedom, while presenting certain management challenges, has served the national laboratories well, making it possible for employees to pursue professional interests, participate in open debate, disseminate the results of their research, collaborate with external researchers, and contribute to public understanding of science and technology. However, employees must recognize and understand that their intellectual freedom must be tempered by the principal mission of national security.

Session Leader: **John C. Browne**, Director, Los Alamos National Laboratory

Panelists: **Wendell B. Jones**, Laboratory Ombuds, Sandia National Laboratories; and **Jeff Wadsworth**, Lawrence Livemore National Laboratory

Oversight of Research Staff by the Principal Investigator

The Principal Investigator (PI) has a responsibility to impart to each co-investigator, collaborator, employee, and trainee appropriate standards for scientific and fiscal conduct. To what degree is the PI responsible when other of his/her research staff engage in scientific misconduct? Should the PI be considered culpable when a staff person has been found engaged in scientific misconduct, and the corresponding PI has been found to be lax or inadequate in some or all oversight responsibilities?

Session Leader: **David C. Clark**, Director, Research Affairs, Rush-Presbyterian St. Luke's Medical Center

Panelists: **Chris Pascal**, Director, Office of Research Integrity, U.S. Public Health Service; and **Robert Zand**, Professor, Biophysics Research Division, University of Michigan

Responsibilities of Scientists to Society

Do researchers have a responsibility to consider possible implications and applications of their research before they undertake the research? Should researchers become involved in developing restrictions on the use or the boundaries of their research? These questions arise with regard to work on nuclear weapons, biological agents, stem cells and genetic engineering.

Session Leader: **Robert J. Eagan**, Vice President, Energy, Information & Infrastructure Surety Division, Sandia National Laboratories

Panelists: **Robert A. Frosch**, John F. Kennedy School of Government, Harvard University; and **Beverly Hartline**, Acting Deputy Associate Laboratory Director for Strategic and Supporting Research, Los Alamos National Laboratory

The New Federal Research Misconduct Policy

The new Federal research misconduct policy was discussed by representatives of federal research agencies. Panelists talked about the rationale for the new policy, issues raised during its development and challenges to its implementation.

Session Leader: **Holly L. Gwin**, Chief of Staff and General Counsel, Office of Science and Technology Policy

Panelists: **Peggy L. Fischer**, Associate Inspector General for Scientific Integrity, National Science Foundation; **William J. Valdez**, Director, Office of Planning & Analysis DOE-Office of Science; and **Chris Pascal**, Acting Director, Office of Research Integrity

Educational Resources to Increase Ethical Awareness for Scientists and Engineers

The National Science Foundation has been making awards in ethics education for more than 20 years. Some awards have trained faculty to incorporate ethics into their science and engineering classes; other awards have resulted in specific products that educate science and engineering students and professionals in ethical issues. This session featured two very successful products that have resulted from NSF awards in ethics education: a scenario-based video for classroom use and a CD-ROM on computer ethics.

Session Leader: **John P. Perhonis**, Program Officer, National Science Foundation

Panelists: **John L. Fodor**, Executive Director, Educational Media Resources; and **Aarne Vesilind**, R.L. Rooke Professor of Engineering, Bucknell University

Bioethical Challenges on the Horizon

We have reached the point where we face concrete ethical choices that only a decade or two ago would have been considered merely hypothetical. What new bioethical problems will cross the line from science fiction to reality in the next decade or so? This session involved an exercise in "educated prognostication" to try to identify ethical issues on the horizon that are likely to arise given the current trajectory of biological research, as a point of departure for considering the professional duties of scientists with regard to them.

Session Leader: **Robert T. Pennock**, Associate Professor, Lyman Briggs School, Michigan State University

Panelists: **Lawrence J. Prochaska**, Professor, Department of Biochemistry/Molecular Biology, Wright State University School of Medicine; and **Janice Voltzow**, Associate Professor, Department of Biology, University of Scranton

Some New Wrinkles on Faculty Conflicts of Interest in Research
This session explored new aspects of potential conflicts of interest for faculty engaged in sponsored research that arise from fairly recent and substantial changes in the modes and expectations for university research. Temptations and confusions surrounding multiple sponsors of a given research program, ownership and development of intellectual property, involvement of students and university facilities in commercializable aspects of faculty research abound. This session discussed these aspects with a goal of sharing experiences and insights from participants who represent the university, government and industry sectors.

Session Leader: **Paul A. Fleury**, Dean of Engineering, Yale University formerly at University of New Mexico

Panelists: **Kumar Patel**, Professor of Physics and Astronomy, UCLA; and **Patricia L. Oddone**, Executive Assistant to the Director, Lawrence Berkeley National Laboratory

Intergenerational Ethics
As we enter the new millennium we are drawn to focus more on the equitable and thoughtful use of the awesome power of science and technology. Concerns about social equity across geographical regions and economic classes are now clearly on the public agenda, as is concern for environmental quality and security. But the same concern across time (intergenerations) is only slowly taking form and is struggling with deeply embedded paradigms such as economic discount rates. As we begin to impact the whole planet with actions that can affect the biosphere for centuries or longer, what is our responsibility for the future?

Session Leaders: **John H. Gibbons**, Senior Fellow, National Academy of Engineering; and **Thomas Malone**, former Foreign Secretary, National Academy of Sciences

Beyond Adversarial Ethics: Web Resources for Solving Problems About Research Conduct
This session gave a guided tour of online materials to support research ethics education for

group learning within departments and laboratories, and a discussion of methods for doing so. The advantage of the method is that it is engaging, builds the group's competence for handling issues in research ethics, makes very modest demands on faculty time, and provides information when people are ready to learn it. The materials are available in the Web pages of the Online Ethics Center for Engineering and Science.

Session Leader: **Caroline A. Whitbeck**, Elmer G. Beamer-Hubert H. Schneider University Professor in Ethics, and Director, Online Ethics Center for Engineering & Science, Case Western Reserve University

Panelists: **Elysa Koppelman**, Special Consultant for Research Ethics, Case Western Reserve University; and **Michael S. Pritchard**, Willard A. Brown Professor of Philosophy; Director, Center for the Study of Ethics in Society; and Associate Dean, The Graduate College, Western Michigan University

2000 Sigma Xi Forum Proceedings
New Ethical Challenges in Science and Technology

Plenary Sessions

Plenary I - Introduction

by: Peter D. Blair
Sigma Xi, The Scientific Research Society

The following is the proceedings for the 10th Sigma Xi Forum, *New Ethical Challenges in Science and Technology*, which was held in Albuquerque, New Mexico, on November 9-10, 2000, in conjunction with the annual meeting of the Society. As breathtaking scientific progress in the past several years has delivered, for example, the map of the human genome and a host of information security and privacy issues, the 2000 Sigma Xi Forum addressed a complex dimension of the societal implications of such developments, namely an array of new ethical challenges facing the science and technology enterprise. It was one of the most interesting and provocative programs we've offered in our forum series, with a splendid array of plenary talks and panel discussions.

This is the first time Sigma Xi has published a full forum proceedings on the World Wide Web. Plenary talks and breakout session remarks are being posted as they become available, and we invite you to visit the site often for updates. We also plan to produce a printed proceedings volume later this spring, which will be sent to all forum participants and will be available to Sigma Xi members and the general public through the Society's administrative offices.

Also, all of the plenary talks and one breakout session, on "Intellectual Freedom and the National Laboratories," were Web-cast during the forum and are available for viewing in a digital archive through the following link: 2000 Sigma Xi Forum Video Archive.

Let me thank our forum co-sponsors: the Burroughs Wellcome Fund, the U.S. Department of Energy and the Sandia Corporation. Special thanks also go to the University of New Mexico and the New Mexico Highlands University chapters of Sigma Xi and to the steering committee members who helped recruit an outstanding set of speakers. The committee members were John C. Cummings, Peggy L. Fischer, Beverly K. Hartline, Peggie J. Hollingsworth, John P. Perhonis, John W. Prados and Robert W. Vallario. Finally, let me thank the Sigma Xi staff who worked so hard in fashioning this program, and especially John Ahearne, director of ethics programs for Sigma Xi, who was a guiding force in putting this forum together.

Sigma Xi was founded as the honor society for science and engineering, and ethics in research has been a primary focus for more than 100 years, so the 2000 Forum was particularly timely and a point of departure for related activities under the aegis of the developing Sigma Xi Center in Research Triangle Park, North Carolina. We have visited some of these issues before, in one form or another, and no doubt will visit them again in the years ahead. We at Sigma Xi are pleased to add this report of the 2000 Sigma Xi Forum, *New Ethical Challenges in Science and Technology*, to the growing literature in this fast-paced field.

Plenary II - Engineering Achievements in the 20th Century and Challenges for the 21st

by: William Wulf
National Academy of Engineering

For two years the National Academy of Engineering worked with the engineering professional societies to identify the 20 greatest engineering achievements of the 20th century. The criteria was not the technological "Gee-whiz," but rather the greatest impact on our quality of life. The result is a pretty remarkable list. If you remove any single achievement from it, our lives would be dramatically changed, and not in a positive way.

The list covers everything from the vast electric power grid, which was Number 1, to the development of high performance materials, which was Number 20. In between were achievements that fundamentally changed the way people live (safe drinking water, for example, was Number 4), the way people work (computers were Number 8; telephones were Number 9) and the way people travel (automobiles were Number 2; airplanes were Number 3).

The impact of many of these achievements was immediate, and so it's not surprising to see automobiles and airplanes on the list. The impact of other achievements, on the other hand, was less obvious. For example, together with sanitary sewers, the availability of safe drinking water fundamentally changed the way people live and die in the United States. In the early 1900s, water-borne diseases, like typhoid fever and cholera, killed tens of thousands of people annually. Dysentery and diarrhea, the most common among those diseases, constituted the third largest cause of death in the United States. By the 1940s, water treatment and distribution systems almost totally eliminated those diseases in America and other developed countries. As a result of these and other advances, life expectancy in the U.S. rose from 46 years in 1900 to 76 today, an increase of 30 years. Two-thirds of that increase is due to clean drinking water and sanitary sewers.

Engineering is all around us. I'm not going to read you the whole list of 20 achievements, but let me note a couple that I haven't mentioned. One of them is agricultural mechanization. In

1900, 50 percent of the U.S. population lived on farms, and it took that half of the population to feed the other half. Today, two percent of the population live on farms and feed not only the other 98 percent of us, but the much of the rest of the world as well.

Number 10 on the list was air conditioning and refrigeration. You probably couldn't have had the breakfast you had this morning without refrigeration. Someone in your family couldn't have had the medicines they need if not for the refrigerator.

Number 15 was household appliances, which radically and dramatically changed lives, especially within the first half of the century.

But let's move on from congratulating ourselves and look to the future.

I'm an optimist. I believe that quality of life in 2100 will be significantly better than it is today, and as different from today's life as today's is from that in 1900. I also believe that we will achieve a much broader distribution of that quality of life around the world. But neither of those beliefs is guaranteed, and there are challenges between here and there.

I would want to take a little side track for a moment and talk about a particular program element that the Academy is undertaking; it's called Earth Systems Engineering. First, I want you to realize that in a very real sense the Earth has already become an engineered artifact. Whether you consider very large projects, like the damming of the Mississippi, or whether you consider small projects such as paving over a parking lot and consequently interfering with the aquifer that is used several hundred miles away, the fact is we are engineering the planet. The trouble is, we're not doing it holistically. We're not doing it ethically. We don't understand the global impacts of our local actions. So, part of what I mean by Earth Systems Engineering is simply being holistic and ethical about what we are already doing.

But, secondly, I think we need to at least contemplate the possibility of intentional intervention in large-scale macroscopic ecological systems. An experiment was just conducted recently off Antarctica to seed the ocean with iron, the purpose of which is to encourage algae formation and consequently sequestering carbon. That's an example of a very large scale intervention in our ecosystems. Frankly, I think it's pretty scary. What I'm going to come back to later is, perhaps, a quantification of why I think it is scary and why I think it presents a special challenge to the engineering profession.

I could go into a great deal more depth on other such challenges, but I am not going to do that. Instead, I'm going to try to talk about just one of the challenges–one that I believe may be the greatest engineering challenge for the 21st century. That challenge is engineering ethics.

Let me start out by trying to be very clear. I believe engineers are, on the whole, extremely ethical. There are ethics courses in virtually every engineering school. I knew there were myriad books on the subject, but I didn't know how many until I started to prepare this talk. Every professional engineering society has a code of ethics, and they almost invariably start with something like "…uphold as paramount the health and welfare of the public." Those particular words come from the National Society of Professional Engineers (NSPE), but they have been copied by virtually every other professional society. The codes of ethics are quite elaborate and go on to detail each engineer's responsibility to clients, employers, each engineer's responsibility to report illegal or dangerous acts, an engineer's responsibility to respect the public interest, and on and on. The NSPE Code of Ethics has some 170 specific points.

I have vivid memories of talking with my father and my uncle, who were engineers, and with my professors and with colleagues over the years, about the ethics of everything from safety margins in engineering products, to dealing with inappropriate pressure from management to try to cut corners. All of that is still in place. Frankly, one of the reasons I feel very proud to be an engineer is because of the strong ethical orientation.

So, why do I want to talk about ethics? Why do I believe that ethics will be the greatest engineering challenge in the 21st century? Why do I think the NAE needs to start a new program? There are two reasons that are closely intertwined.

First, the practice of engineering is changing; and, in particular, it's changing in ways that raise a different kind of ethical issue. Second, the issues that are arising from this particular nature of engineering practice are macro-ethical issues that the profession has not dealt with before.

In preparing this talk, I ran across a wonderful quote by John Ladd, an emeritus professor of philosophy at Brown University. He said, "Perhaps the most mischievous side effect of ethical codes is that they tend to divert attention from the macro-ethical problems in our profession to its micro-ethical problems." The literature on engineering ethics, the myriad books, the codes of all of the professional societies, the courses I have been able to review, all focus on individual behavior. The behavior of the individual is micro-ethics. When I say "micro," I don't mean they're small and unimportant, but simply that they are individual.

The changes in engineering practice are ones that I believe pose ethical questions for the profession rather than the individual–these are called "macro" ethical issues. I have yet to convince you what I just said is true, but it's the reason I believe the NAE should develop programmatic activity. Engineering has not squarely faced these macro-ethical issues before.

Macro-ethical questions are more common in some other fields--medicine, for example. The micro-ethical questions in medicine are almost identical to those in engineering--they map almost exactly word for word. But in medicine, the macro-ethical questions have been dealt with. The most common and easily explained example is allocation. Who gets the scarce organ for transplant? Who gets the physician's attention if there are more patients than can be treated? Who gets the medicine if there's not enough to go around? The individual physician cannot make that decision. The profession, or better yet, society guided by the profession, has to set up guidelines, and then it becomes a micro-ethical question for the physician to implement those guidelines.

So, I assert that engineering hasn't had to deal with macro-ethical questions, and now I'm going to talk about what is changing in engineering that has given rise to macro-ethical issues. I'm going to focus on just one change, complexity. In particular, I'm concerned with the complexity arising from information technology and biotechnology, both of which are going to show up in virtually every engineering product.

I will elaborate on this in a minute, but let me say it in a bald-faced way first. Increasingly, we're building engineering systems whose complexity is such that it is impossible to predict all of their behaviors. Let me say that again just to make the point. I am not saying it's hard to predict. I am not saying that you somehow have to think about it differently. I am saying that it is impossible to predict all of their behaviors.

There is extensive literature on engineering failures. I haven't read all of that literature, but I have a stack of it on my shelf, and I happened to pull off two volumes when I was preparing this talk. One is a 1984 text by Charles Perrow called *Normal Accidents*. The other one is a 1997 book by Ed Tenner called *Why Things Bite Back*. I found these two interesting because in the 13 years that separate those two books there had been a clear progression of thought about why failures happen and what engineers ought to do about it.

For Perrow, in 1984, the problem was simply that we weren't thinking about the possibility of multiple simultaneous failures in highly interconnected systems, and the clear implication was—think about it! In fact, the systems engineering community and risk analysis community have been very good about doing just that. The probability that two or three simultaneous failures will take down even the most complicated system is much lower than it was 15 or 20 years ago.

In Tenner, the more recent book, we begin to see a glimmer of a notion that for very complicated systems it might be *really, really* hard to predict all of the possible failures. But I still get the feeling that he thinks that if we just thought a bit harder, we would be okay--we would figure out all of the possible failures, we would anticipate the problems. That's not what I am talking about.

By the way, neither Perrow nor Tenner are engineers. One's a sociologist and the other is a historian. One of the things I find interesting is that they clearly bring their disciplinary tools to bear in looking at these problems, and their tools don't include the kinds of very sophisticated mathematics that engineers and scientists employ. So, in particular, they don't think about the fact that there might actually be a technical explanation for why systems fail. And, of course, they are probably partly right in looking back at failures and systems that weren't as complicated as the ones we are engineering now.

Over the past several decades, at places like the Santa Fe Institute, increasingly sophisticated mathematical models of complex systems have been developed. In some ways, those mathematical models are—what shall I say—"squishy." They're not as finely honed as the mathematics that we use in other parts of engineering. But one thing is very solid, and that is, a sufficiently complex system will exhibit properties that cannot be predicted a priori.

I said that they are squishy, that's partly deserved and partly not. The deserved part simply has to do with the fact they are not all that mature yet. The part that's not deserved is that it is associated with a couple of other things that are questionable. The term used to describe behaviors that are not anticipated is "emergent properties," a phrase that first arose in the late 1930s in the context of sociologists trying to explain group behaviors. Those theories have pretty much been discredited. The second thing is that there's been some effort by post-modern, anti-science types to use the phrase to discredit reductionist scientific approaches.

I don't want to get too technical, but I want to give you a flavor of what I mean when I say it's *impossible* to predict behaviors. Let me work from my field, software, and ask the question, "Why is software so flaky?" There are lots of reasons. But one of them is not "errors" in the conventional sense of the term. It's not that the software does something it wasn't intended to do. In fact, these "errors" often happen in the course of the software's doing exactly what it was specified to do. It's just that the consequences of those specifications were not understood. The number of circumstances under which that behavior would be appropriate or inappropriate was simply impossible to contemplate.

Let me try and indicate this to you by some numbers. There are probably scientists in the audience who know the right number here, but my recollection is that there's something on the order of 10 to the 100th atoms in the universe. The number of states in my laptop is 10 raised to 10 to the 20th power. The exponent has a 1 followed by 20 zeros. If every atom in the universe were in the computer, and if every one of those computers could analyze 10 to the 100th states per second, there isn't enough time from the Big Bang until now to analyze all of the states.

That's what I mean by impossible. It's not that there isn't a process that, given enough time, could analyze the situation. The situation is so complicated, there simply isn't enough time. That's why it's impossible, or "intractable," which is the technical term.

That's what has changed. We can now build systems all of whose properties we cannot predict. We can, however, with some certainty, predict that any system we build will have behaviors that we can't predict, that some of those behaviors will be negative; some of them may even be catastrophic. We just don't know what they will be.

This wouldn't be an ethical question if we didn't know there would be negative behaviors. Legal system ethicists have long agreed that if the engineer or scientist doesn't know what the consequences will be, they are not responsible. But, here, we know. We know that there will be behavior we can't predict, and there's a high probability it will be negative, maybe even catastrophic. So how do we behave? How do we engineer in situations like that?

Harking back to the NAE program in Earth Systems Engineering that I mentioned earlier --it's clear that the ecosystem of the earth is a very complex system. It is exactly the kind of system we've been talking about here. It's a system that, even if we scientifically understood the behavior of every part, and even if we understood all of the potential interactions, we could not predict all the consequences of our behavior. If we change it, there will be consequences that we cannot predict. It's a good example of a system where everything has a consequence and the effect of our behavior may show up in totally unexpected places. We certainly have examples of that.

And, yet, in many cases, we must act. Not acting is also an action. We can't just not act and say, "I'm being ethical." That may be the most disastrous act of all. So how do we do that ethically? It's clearly a deep question, clearly an ethical question, clearly a macro-ethical question. Our codes of ethics don't tell us how to behave in such circumstances.

Let me switch to another example. Last spring, Bill Joy, somebody I've known for a long time, who co-founded Sun Microsystems and is a leading Silicon Valley technologist, raised a somewhat related but different issue in what I frankly thought was an irresponsibly alarmist article. Joy mused about whether individually or collectively information technology, biotechnology and nano-technology would develop self-replicating systems that would replace humans. He then went on to raise the question of whether, given the specter of the possibility of that, we should stop research in all of these areas.

Frankly, I abhor the way Bill raised the question, but I think we have to deal with the fact that something like this is really at the root of public concern about cloning, about genetically modified organisms and so on. I think they are rightfully concerned. Do we, the science and engineering community, know all of the consequences of our actions?

Joy's question, should we stop doing research, is something that personally repels me. The notion that there is truth that we should not know is absolutely abhorrent to me. I can embrace the notion that there are ways by which we should not learn the truth, that some research practices are unethical. The obvious example is the way the Nazis conducted some medical experiments in World War II. I don't happen to agree with it, but I can understand the arguments of people who object to fetal-tissue research. So, I can agree with the notion that there are ways of gaining knowledge that we should prohibit. I can also embrace the notion that there are ways that we should not use knowledge. But the notion that there is truth, that there is knowledge that should not be known is something that I find impossible to accept.

It's somewhat ironic that the first academies in western Europe, academies of science, were created because science, this empirical way of knowing, this new way to search for truth, was not accepted by the scholastic university establishment of the 17th century. Thomas Jefferson was making a radical assertion, even more than a hundred years later, when, in founding the University of Virginia, the first secular university in the United States, he said,

"This institution will be based upon illimitable freedom of the human mind. For here we are not afraid to follow the truth wherever it may lead."

That's the notion of truth that I teethed on, and, yet there I was, two weeks ago, in the Academy, asking the question, is there truth we should not know? I have to admit that we don't have a stellar record on the misuse of freedom of knowledge, but that, I think, is where controls have to go.

One could rightly ask, "Why is that we engineers should ponder this as our ethical question?" Well, because science is about discovery of truth–so scientists have to deal with the question of, "What are the appropriate ways to discover truth?" But engineering is about using knowledge to solve human problems. So, we have to deal with the ethical question of the misuse of knowledge.

While I can't bring myself to agree with implied answer in Bill Joy's question, I do believe it raises a very deep macro-ethical question about the use of knowledge. How do we ethically, as engineers, ensure the proper use of knowledge? It is not a question the Code of Ethics tells us anything about. It is something that society, guided by the profession, informed by the profession, needs to deal with.

The first part of my talk emphasized that engineering—really, engineers—have made tremendous contributions to our quality of life, particularly so far as the developed world is concerned. There is much to be done to bring that quality of life to the rest of the planet. But, I am, basically, an unabashed optimist about the prospects in the 21st century for further improving that quality of life and spreading it around the globe. There are many challenges to achieving that, and I have only touched on one--the question of engineering ethics.

Projects like the further reclamation of the Everglades will be done with imperfect knowledge of the consequences of the actions being taken. They should be done with an awareness that some of the consequences might be disastrous. We've got to figure out how ethically to cope with that, how to rethink the process of engineering so that we can backtrack if we need to, so that we can adapt, so that we can work within a very complicated system.

My dad's engineering consisted of a specification from his boss. I don't think that works any more. You can't write the specification that will function, in all cases, the way we want it to. What we need is, somehow, to adapt ethically as we go along. Again, I want to point out that we don't have the option of choosing not to act. That is also an action. So we've got to face the question. We don't have a choice.

Plenary III - Managing Conflicts of Interest? The UCSD Experience

by: Robert C. Dynes
Chancellor, University of California, San Diego

Chancellor Dynes' talk elaborated on the ideas and issues outlined in the following slides. Digital video of his forum presentation is available.

Scenario 1

--Researcher has consulting agreement with a company and performs accordingly.
--During a consulting visit, the company mentions an interesting question; researcher performs related experiment in university lab, using university students.
--Should researcher share the results of the lab experiment with his client?
Researcher's consulting agreement conforms to university policy.

Researcher discloses consulting agreement.
Students are free to publish results of research.

Scenario 2

--Researcher makes an invention as a result of federally funded grant.
--Researcher starts his own company in his/her garage and secures license for his invention.
--Researcher continues basic research at the university and works at his company.
--Should researcher's continuation federal grant be funded?

Researcher is a full-time faculty member.
Invention was disclosed to the university TT office.
Researcher's family members staff the company; researcher is VP-Research at the company.
In lieu of license fees, researcher offers the university an equity stake in company.

Scenario 3

--Researcher is about to conduct clinical trials at the university; trials are related to a product under development in a company in which researcher has substantial stake.

--Researcher's work is being funded by a federal grant.
--Researcher holds position of Chief Scientist at the company and receives consulting income.
--Should researcher be allowed to conduct trials?

Researcher's relationship with the company is public.
Product promises significant, long-awaited benefits to the public.

What Conflict?

--Ethics in the Research Environment
--Individual versus Institutional Conflicts of Interest
--Actual versus Perceived Conflicts of Interest

An Overview of COI Activity

--An Historical Perspective
--Why Have Things Changed?
--Public Perceptions

UCSD Model

--Philosophy & Principles

UCSD Practice

--Independent Review Committee (IRC)

Faculty Involvement
Administrative Oversight

Management Strategies
--Negotiated strategies include:
Consulting agreement appropriately structured
PI required to resign from management or scientific advisory board
PI required to divest equity holdings
Find another PI to manage the project
--Charge ad hoc panels for ongoing monitoring

Ad Hoc Panels
--Charge
--Number
--Monitoring Activities

A Look at the Numbers

	FY90		**FY97**		**FY99**	
	No. of Dollars	Awards	No. of Dollars	Awards	No. of Dollars	Awards
Total	1,721	240.6	2,125	351.4	2,493	446.1
DHHS	540	102.6	525	134.0	556	166.0
NSF	279	29.7	307	48.1	378	89.8
Industrial	69	8.2	320	29.7	455	40.3

	FY90	**FY97**	**FY99**
Disclosures	333*	1,669	2,090
Positives	35	123	148
% Positive	10.5%	7.4%	7.1%

*Represents industrial only; federal disclosure requirement was not implemented until October 1995

The Future
--Increased complexity & uncertainty
--Redoubled efforts to refine policies and practices
--Ongoing scrutiny from the public

Entrepreneurial scientists overcome conflict issues
Final slide was an article by this title from the *San Diego Union-Tribune* (October 29, 2000).

Plenary IV - The Overselling of Computers in Science

Cliff Stoll, Author
Abstract not available

Plenary V - Ethics in Medical Research

David C. Clark, Director, Research Affairs, Rush-Presbyterian St. Luke's Medical Center
Abstract not available

Plenary VI - On Being a Scientist: The Year 2000

by: Francisco J. Ayala
University of California at Irvine

Science and Science Education in the United States
The United States invests, annually, more than $200 billion in scientific research and technological development (R&D). Between 1995 and 1999, the outlays for R&D grew at a rate greater than 9 percent per year, in constant dollars, and the rate of the yearly increase itself has been increasing. This expenditure is widely perceived as a sound and high-return investment. President Clinton's Council of Economic Advisors has estimated that 50 percent of all economic growth in the United States over the past 50 years can be directly credited to scientific discoveries and technological developments performed over the same period.

The returns on this investment are indeed splendid. The country invests 3 percent of its yearly gross domestic product (GDP), currently valued at more than $7 trillion, on R&D and gets in return for this investment 50 percent of the GDP annual growth. Astonishingly, the favorable impact of R&D on the U.S. economy seems to be accelerating over time. The large expansion of the U.S. economy over the past decade (and its unanticipated association with virtually full employment without inflation) has been attributed by Federal Reserve Chairman Alan Greenspan to the investment on R&D. He said in the summer of 1999, "The evidence … for a technology-driven rise in the prospective rate of return on new capital, and an associated acceleration in labor productivity, is compelling." (President's Committee of Advisors on Science and Technology, *Wellspring of Prosperity*, Office of the President, 2000, p. 29.)

Scientific discoveries and technological achievements pervade the gamut of human activities and concerns, including health care, agriculture, industrial development, transportation, information technologies and more.

Moreover, the United States enjoys a formidable assortment of universities and research institutes, where millions of scientists and engineers receive superb training, as college, graduate, and postdoctoral students and researchers, and where wonderful scientific discoveries and engineering feats are accomplished on a daily basis. Students come from all over the world to benefit from the superb training provided by these institutions of higher learning.

Anybody aware of these accomplishments might conclude that, underlying the great research institutions and the endless scientific discoveries, and subjacent to the country's enormous investment on R&D, there must be in the U.S. an excellent school system, engaged in the education of the young and preparing them for productive careers in scientific research and technological development. Alas, the observers would be wrong. Surely, there are in the U.S. many excellent elementary and secondary schools, where superior science education is imparted as part of the curriculum. But there are many others, perhaps a majority, where science courses are degraded or, in extreme cases, virtually absent from the curriculum.

One reason for the deficiency of science education in many of our schools is the decentralization of education—there is no nationally prescribed program of studies, course requirements, or assessment standards for either elementary or secondary education. There are 16,000 school districts in the United States, and each school district largely independently is entitled to set up much of the school curriculum, the subjects to be studied and assessed and the textbooks to be used. These matters are, to some extent, determined at the state level, and each of the 50 states of the Union zealously protects its right to self-determination in educational goals, as in many other matters. The dispersion of responsibilities accounts to a large extent for the great heterogeneity in performances and standards of quality in education, particularly with regard to science.

Science and Religious Fundamentalism

You are well aware that just a little more than a year ago, in August of 1999, the Kansas Board of Education decided to eliminate any reference to evolution or cosmology from all examination requirements, or as subject matter required to be covered in the public schools in Kansas. The schools are not forbidden to teach cosmology and evolution, but these would not be subjects for assessing scholastic achievement. Subject matters not subject to examination are unlikely to be taught in the schools, at least at any length and depth, which is precisely the objective sought by the Board.

The Governor of Kansas, the moderate Republican Bill Graves, called the school board’s decision "a terrible, tragic, embarrassing solution to a problem that did not have to exist," and announced that he would seek to erase the decision through legislation or otherwise. This may now be unnecessary, since the new Kansas Board of Education elected on November 7, 2000, consists of a majority of members, Republicans as well as Democrats, who have announced they will restore the teaching of cosmology and evolution to the curriculum.

The August 1999 decision of the Kansas Board of Education does not represent an uncommon attitude in the United States. It rather reflects a conviction, common among biblical literalists and other Christian fundamentalists, that the teachings of science concerning the origin of the universe, the living world and, most importantly, humans are contrary to the Biblical texts and the Christian faith. This conviction was not, however, the reason alleged by the Kansas Board members who endorsed the majority's decision. The United States Constitution's separation of church and state, as forcefully set in its First Amendment and numerous decisions by federal courts, would have made the religious grounds obviously unconstitutional and subject to judicial challenge. Rather, members of the Kansas school board ostensibly constituted themselves into fly-by-night scientists and epistemologists who rejected the recommendations of their own panel of experts and declared that the theory of evolution is nothing but theory, rather than fact; and that science relies on observation, replication and experimentation, but nobody has seen the origin of the universe or the evolution of species, nor have these events been replicated in the laboratory or by experiment

Opposition to the teaching of evolution and other scientific theories in U.S. schools has a long history that can be traced to the middle of the 19th century and, starting with the 20th century, has mainly involved two Christian groups, the Pentecostal Church, on the one hand, derived from Methodism, and the Seventh-Day Adventists, derived from Southern Baptists.

The Pentecostal (originally known as Holiness) movement emerged in the U.S. in the late 19th century among Methodist followers of John Wesley. On the first day of the 20th century, Charles Fox Parham, an itinerant Holiness healer, and a small group of followers began speaking in tongues in Topeka, Kansas, a practice that motivated the name of "Pentecostalism," by reference to the gifts of the spirit received by the early Christians during the Day of Pentecost that allowed them to speak in unknown languages. In the second half of the 20th century, the flamboyant televangelists Oral Roberts, Jimmy Swaggart and others propagated the movement, converting masses throughout the world, promoting "charismatic practices" among Christians. "By the mid-1990s roughly one-fourth of the 2 billion Christians in the world had embraced the Pentecostal-Charismatic faith" (Ronald L. Numbers,*Darwinism Comes to America*, Harvard University Press, 1998, p. 112).

In the past several decades, many Pentecostals have largely adopted and endorsed the tenets of so-called "creation science," including the recent origin of the earth and Noah's flood geology. But the Pentecostals differ from Seventh-Day Adventists and other creationists in their tolerance of diverse views and the limited import they attribute to the evolution-creation controversy.

Seventh-Day Adventism arose out of the ashes of the Millerite disaster. The New York State Baptist William Miller acquired an enormous following with his prediction that Christ would return to earth in 1843 or 1844. When the date pinned down by many Millerites, October 22, 1843 went by, the ensuing disappointment led to the disintegration of the movement. The teenage visionary Ellen G. White regrouped some of the followers into a movement that in the early 1860s became the Seventh-Day Adventist Church, holding to the belief of an imminent Second Coming of Christ. Because of the belief in the seventh-day Sabbath as a memorial of the creation, the Adventists insist on the recent creation of life and the universality of the Noachian flood, which deposited the fossil-bearing rocks.

The opposition of Christian fundamentalists and others to the teaching of evolution in the schools has led to a number of court cases, perhaps the most famous being the Scopes trial of 1925 in Dayton, Tennessee. It was lionized in the movie "Inherit the Wind" and involved a young high-school teacher by the name of John Thomas Scopes who was convicted and fined for having violated a recently passed bill that made it unlawful for state-supported schools "to teach any theory that denies the story of the divine creation of man as taught in the Bible and to teach instead that man has descended from a lower order of animals."

The case was championed by the American Civil Liberties Union, which persuaded Scopes to declare publicly that he had violated the statute. The ACLU wanted the State of Tennessee to initiate a lawsuit against Scopes for the purpose of having a decision that eventually could lead to the Supreme Court of the United States, with the expectation that the prohibition against teaching evolution in schools would be declared unconstitutional.

Scopes was fined $100. Unfortunately, at least from a certain point of view, owing to a technical error, the conviction was abrogated and the fine removed, so that the case could not be appealed. Eventually, however, many years later, a similar prohibition in the State of Arkansas was appealed to the U.S. Supreme Court, which led in 1968 to a decision (Epperson v. Arkansas) that achieved what the ACLU had hoped to accomplish with the Scopes case. The court declared it unconstitutional to prohibit the teaching of evolution in the public schools.

In 1981, the State of Arkansas passed a law that sought to circumvent this ruling. It was a so-called "balanced treatment" statute that required equal time be given to the teaching of evolution and of "creation science" in school curriculums. The statute held that there were two theories of origins, one is "creation science," which held six tenets made up of statements taken literally from the book of *Genesis*. Nobody had heard of creation *science*before this. There is another theory of origins, the statute said, called evolution. The statute required that any teacher teaching one of the theories had to dedicate equal time to teaching the other theory.

There are many theories of origins, not only these two. But there is only one that professional scientists, experts on the subject, consider scientifically well corroborated. Be that as it may, there was a trial in federal court in Little Rock, for which I was one of the expert witnesses. The decision (McLean v. Arkansas Board of Education, 1982) was forcefully argued and worded by the district court judge, who stated that the balance-treatment statute violated the First Amendment of the U.S. Constitution. The court declared that creation science is not in fact science. Underlying the balanced treatment law was its proponents' expectation that many teachers would not teach evolution because they would have to dedicate an equal amount of time teaching creation science, which is, of course, not science, and, therefore, to avoid teaching the second, they would avoid teaching the first. The judge's tightly argued decision extended for some 10,000 words. It was published in Science because it was a wonderful essay on the scientific method and how to distinguish science from non-science. The State of Arkansas decided not to appeal. In the meantime, the State of Louisiana had passed a similar balance-treatment law. The Louisiana law was appealed all the way to the U.S. Supreme Court (Edwards v. Aguillard, 1987), which held that Louisiana's "Creationism Act" was unconstitutional because by advancing the religious belief that a supernatural being created humankind, which is embraced by the phrase "creation science," the act impermissibly endorses religion.

Those are the most recent court cases at the national level. At the local level there have been many, and continue to be, in various school districts and in various states, developments like the one I referred to in Kansas (see Voices for Evolution, edited by Molleen Matsumura, National Center for Science Education, Berkeley, CA, 2nd ed., 1995).

Evolution and Christianity

Why has this opposition against the teaching of evolution arisen in the United States? And, mind you, it's very often opposition to the teaching of science; it's not only against evolution. This opposition is a distinctly American problem that can largely be traced, as I said, to two movements, the Seventh-Day Adventists, deriving from Southern Baptists, and the Pentecostal movement, deriving from the Methodists. However, other Christian denominations, even though they may not take an official position against the teaching of evolution or cosmology, believe that evolution and theories about the origin of the universe, the Big Bang theory, for example, are contrary to Christian beliefs. It seems to many religious people that evolution runs contrary to the notion that the universe and everything in it, humans in particular, was created by God, and that evolution, the Big Bang and many other theories of science, are in literal contradiction with the Bible.

Let me start by pointing out that it is curious that the Judeo-Christian tradition should be the major source of opposition among the world religions against the theory of evolution. It is curious, I say, because, at least among cultural western traditions, the Bible is the one that is implicitly evolutionistic. What I mean is that if we look at classical Greece and Rome, the other roots of Western culture, they had a concept of time that was circular, so to speak. It has been labeled "the myth of the eternal return" by the philosopher Mircea Eliade. Greek philosophers held that everything repeats itself; nothing significantly happens in history; there is no progression. On the contrary, the Biblical notion of time is clearly directional and progressive. Things happen. First, the world is created, then there is sin, the prophets come, later redemption is accomplished by the Messiah, and finally, the Kingdom of God will arrive.

There are, nevertheless, Christian believers who see scientific cosmology and the theory of evolution as contrary to the creation narrative of the book of *Genesis*.1 These believers depart from a powerful Christian tradition that starts in the second century AD with the first commentators of the Bible, who say that Biblical descriptions of the physical world and many historical details should not be taken literally. I will use, in referring to early Christianity, the authority of perhaps the greatest Christian theologian of all time, St. Augustine. He wrote a "literal" commentary about the book of *Genesis*. This is about the year 400. Augustine is aware of discussions going on at the time about the configuration of the universe and whether the earth is placed at the center of it. He writes, "It is also frequently asked what our belief must be about the form and shape of heaven, according to Sacred Scripture. Many scribes engage in lengthy discussions on these matters, but such subjects are of no profit for those who seek beatitude. And what is worse, they take up very precious time that ought to be given to what is spiritually beneficial. What concern is it of mine whether heaven is like a sphere and Earth is enclosed by it and suspended in the middle of the universe, or whether heaven is like a disk and the Earth is above it and hovering to one side" (*The Literal Meaning of Genesis*, Book 2, ch. 9). St. Augustine adds a little later in the same chapter: "In the matter of the shape of heaven, the sacred writers did not wish to teach men facts that could be of no avail for their salvation." What St. Augustine is saying is that the book of *Genesis* is not an elementary book of astronomy. It's something else. It's a book about religion, and it is not the purpose of the religious authors to settle questions about the shape of the universe that are of no relevance whatsoever to how to seek salvation.

Later on, there's a remarkable statement. He asks the question whether everything that now exists in the world was created by God from the beginning, including all plants and animals. Augustine says that God created some creatures and "in these existing beings God created the reason-principles of other beings to come in the future, but not the beings themselves" (*The Literal Meaning of Genesis*, Book 7, ch. 5). And further, "God, therefore, stored away in creatures the causal reasons of the plants and trees that were to be, and, as if these plants and trees already existed" (Book 8, ch. 3.).

Do these texts imply that Augustine was a cryptic evolutionist? Well, obviously he wasn't. He was not concerned with scientific issues; these were of no interest to him. He was concerned with religious issues, including the narrative of the Noachian Flood and Noah's Ark, and whether every animal species could have been put there, and he was smart enough to know that a boat could not have been built large enough to include every animal in existence at the time of Augustine, and moreover that other problems would arise, like who eats who and the like over the several months they survived in the Ark. His opinion is that many species did not exist at the time of Noah's Ark. They have been created by God only in their seeds, in their potentiality, and they have come about later. It's not as if he was trying to be an evolutionist. The significant point for us, however, is that he saw no problem with the notion that not everything, including all animal and plant species, may have existed from the beginning.

But let us now jump 850 years ahead, to the middle of the 13th century, to St. Thomas Aquinas, perhaps the other greatest theologian after St. Augustine in the Christian tradition. In his treatise on theology, *Summa Theologica*, a several-volume work, Aquinas explicitly raises the question of the origin of life, whether life could arise from inorganic matter by natural processes. I bring this issue up because it is one of great concern to the anti-evolutionists, who defend that life must have been created by God, that it could not come up by natural processes without God's special intervention. Aquinas has a full chapter dedicated to the question. Why would he concern himself with that issue? In a way, for the same reason that Christians might be concerned now, because "scientists" and other people were saying that life may have come about from nonliving matter by natural processes, and this would seem contrary to Christian theology.

The evidence for the spontaneous origin of life in Thomas Aquinas' time came from the observation that in decaying matter, such as in the excrement of cattle or in rotten meat, maggots appear, apparently spontaneously. He asks, "Is that possible?" He proceeds, just as in every chapter of the *Summa Theologica*, by reviewing evidence from the Bible, then turning to evidence from the early commentators, the so-called Fathers of the Church, and then he looks for rational arguments derived from theology and philosophy. He concludes that there is nothing in any of these sources that would contradict the notion that life may spontaneously arise from nonliving matter. Nevertheless, he was not quite ready to accept the "evidence." He asks, does my conclusion mean that living beings come out from nonliving matter? He answers, I don't know. This is for scientists to decide, but the possibility cannot be excluded on the grounds that it would contradict the Christian faith. This seems to me a very important point, because this is one of the issues that is so problematic for those Christians who oppose evolution, namely the question of how could life possibly arise by natural processes from nonliving things.

More recently, within the Catholic tradition, the present Pope John Paul II has written, "The Bible speaks to us of the origins of the universe and its makeup, not in order to provide us with a scientific treatise, but in order to state the correct relationship of man with God and the universe. Sacred Scripture wishes simply to declare that the world was created by God, and in order to teach these truths, it expresses itself in the terms of the cosmology in use at the time of the writer. The Sacred Book, likewise, wishes to tell men that the world was … created for the service of man and the glory of God. Any other teaching about the origin and makeup of the universe is alien to the intentions of the Bible, which does not wish to teach how heaven was made, but how one goes to heaven." (Address to the Pontifical Academy of Sciences on October 3, 1981.) Addressing the same Academy on October 22, 1996, the Pope again deplores interpreting the Bible's teachings as scientific rather than religious, and says: "[N]ew knowledge has led us to realize that the theory of evolution is no longer a mere hypothesis. It is indeed remarkable that this theory has been progressively accepted by researchers, following a series of discoveries in various fields of knowledge. The convergence, neither sought nor fabricated, of the results of work that was conducted independently is in itself a significant argument in favor of this theory."

The point made by the Pope is the same as St. Augustine's, namely, that it is a blunder to mistake the Bible for an elementary textbook of astronomy, geology and biology. Instead, it is possible to believe that the world has been created by God while also accepting that the planets, the mountains, the plants and the animals came about, after the initial creation, by natural processes. One can believe to be God's creature without denying that the individual develops from a single cell in the mother's womb by natural processes.

Intelligent Design?
A surprising recent development against the theory of evolution is the argument known as "intelligent design." A number of books have been published in the past five or six years arguing that living beings give obvious evidence that they have been intelligently designed, and therefore there has to be an intelligent designer, which, of course, implies that all living beings have been originally created by God. I say that it is surprising because it is an old argument, well-developed in the 19th century, and answered by Darwin in *The Origin of Species*. For example, William Paley in his book*Natural Theology* (1802), read by Darwin as part of the canonical curriculum when he was a student at the University of Cambridge, had developed the argument-from-design as a demonstration of the existence of the Creator. It would be absurd to suppose, he wrote, that the exquisite functional complexity of the human eye would have come about by mere chance. It was Darwin's genius that he discovered natural selection, the process that accounts for the adaptive organization, or design, of organisms and their parts. Evolutionists down to the present invest much time, resources and imagination designing observations and experiments to investigate how natural selection contributes to the evolution of particular adaptations. It seems, therefore, unbecoming that several authors would have recently revived Paley's argument claiming that organisms and living processes give evidence of "intelligent design" unaccountable by natural selection.

There is hardly any need to refute, once again, the argument, but I would like to say that, in my view, attributing the "design" of organisms to God's special action amounts to blasphemy. Consider the human jaw. We have too many teeth for the jaw's size, so that wisdom teeth need to be removed and orthodontists make a decent living straightening the others. Would we want to blame God for such defective design? A human engineer could have done better. Evolution gives a good account of this imperfection. Brain size increased over time in our ancestors, and the remodeling of the skull to fit the larger brain entailed a reduction of the jaw. Evolution responds to the organism's needs through natural selection, not by optimal design but by "tinkering," as it were, by slowly modifying existing structures. Consider now the birth canal of women, much too narrow for easy passage of the infant's head, so that thousands upon thousands of babies die during delivery. Surely we don't want to blame God for this defective design or for the children's deaths. Science makes it understandable, a consequence of the evolutionary enlargement of our brain. Females of other animals do not experience this difficulty.

One more example: Why are our arms and our legs, which are used for such different functions, made of the same materials, the same bones, muscles and nerves, all arranged in the same overall pattern? Evolution makes sense of the anomaly. Our remote ancestors' forelimbs were legs. After our ancestors became bipedal and started using their forelimbs for functions other than walking, these became gradually modified, but retaining their original composition and arrangement. Engineers start with raw materials and a design suited for a particular purpose; evolution can only modify what is already there. An engineer who would design cars and airplanes, or wings and wheels, using the same materials arranged in a similar pattern, would surely be fired.

Recently, some authors have used biochemical examples to argue for intelligent design, partly because some biochemical processes are quite complicated. One example used is the blood-clotting mechanism in humans and other mammals. The claim is that this is a very complex process, which does not work unless all the components are present. But this argument is fundamentally no different from the one used by Paley and others nearly two centuries ago. And the blood clotting mechanism is a worse example than the eye because it's so unnecessarily complicated that only Rube Goldberg could have designed it. If we see it as a result of evolution, the blood-clotting mechanism is understandable; evolution makes sense of its convoluted complexity, why it takes so many steps to eventually coagulate the blood. But seeing it as the result of a special design, I will say again: A human engineer would have done better.

The defective design of organisms could be attributed to the gods of the ancient Greeks, Romans and Egyptians, who fought with one another, made blunders and were clumsy in their endeavors. But, in my view, it is not compatible with special action by the omniscient and omnipotent God of Judaism, Christianity and Islam.

So what about the religious notion that we have been created by God, and, therefore, we cannot accept that we are products of evolution? One possible answer that many Christians hold is to accept that the world has been created by God, without necessarily believing that God is intervening at every point in the workings of the universe. One can accept the creation of the world, and that the laws of the universe come from God, and then accept that natural processes account for the origin of species, including our own. A believer can accept the notion that he or she has been created by God, and that every human being has been created by God, without denying the natural process, namely that there was an egg in the mother's womb that was fertilized by sperm, and that the resulting cell divided into two cells, then four cells, and eventually a baby was born. For many Christians and other people of faith, the notion of "created by God" doesn't necessarily mean special creation, everything created especially by God. It can imply that everything was originally created by God, and also that everything falls under God's providence.

Teaching Evolution in the Schools

The opposition to the teaching of evolution in the schools is often buttressed, as I noted earlier, with the argument that the theory of evolution is just that, a "theory," not a fact. This argument ignores that when scientists talk about the "theory of evolution," they use the word "theory" differently than in ordinary language. In everyday English, a theory is an imperfect fact, as in "I have a theory as to what caused the explosion of TWA flight 800." In science, however, a theory is based on and incorporates a body of knowledge. According to the theory of evolution, organisms are related by common descent. There is a multiplicity of species because organisms change from generation to generation, and different lineages change in different ways. Species that share a recent ancestor are therefore more similar than those with more remote ancestors. Thus, humans and chimpanzees are, in configuration and genetic make-up, more similar to each other than they are to baboons or to elephants.

Scientists agree that the evolutionary origin of animals and plants is a scientific conclusion beyond reasonable doubt. They place it beside such established concepts as the roundness of the earth, its revolution around the sun and the molecular composition of matter. That evolution has occurred is, in ordinary language, a fact.

How is this factual claim compatible with the accepted view that science relies on observation, replication and experimentation, since nobody has observed the evolution of species, much less replicated it by experiment? What scientists observe are not the concepts or general conclusions of theories, but their consequences. Copernicus's heliocentric theory affirms that the earth revolves around the sun. Nobody has observed this phenomenon, but we accept it because of numerous confirmations of its predicted consequences. We accept that matter is made of atoms, even though nobody has seen them, because of corroborating observations and experiments in physics and chemistry. The same with the theory of evolution. For example, the claim that humans and chimpanzees are more closely related to each other than they are to baboons leads to the prediction that the DNA is more similar between humans and chimps than between chimps and baboons. To test this prediction, scientists select a particular gene, examine its DNA structure in each species, and thus corroborate the inference. Experiments of this kind are replicated in a variety of ways to gain further confidence in the conclusion. And so it is for myriad predictions and inferences between all sorts of organisms.

Not everything in the theory of evolution is equally certain. Many aspects remain subject for research, discussion and discovery. But uncertainty about these aspects does not cast doubt on the fact of evolution. Similarly, we do not know all the details about the configuration of the Rocky Mountains and how they came about, but this is not reason to doubt that the Rockies exist.

The theory of evolution needs to be taught in the schools because nothing in biology makes sense without it. Modern biology has broken the genetic code, developed highly productive crops and provided knowledge for improved health care. Students need to be properly trained in biology in order to improve their education and their chances for gainful employment, and to enjoy a meaningful life in a technological world.

One final comment. Science seeks material explanations for material processes, but it has nothing definitive to say about realities beyond its scope. Science is a way of acquiring knowledge about ourselves and the world around us, but it is not the only way. We acquire knowledge in many other ways, such as literature, the arts, philosophical reflection and religious experience. Scientific knowledge may enrich aesthetic and moral perceptions, but these subjects transcend science's realm. Successful as science is, and universally encompassing as its subject is, a scientific view of the world is hopelessly incomplete. Once science has had its say, there remains much about reality that is of interest, matters that may well be thought by many to be of equal or greater import than scientific questions--questions of value, meaning and purpose that are forever beyond science's scope.2

Footnotes

[1] An example of the biblical literalist position is the Statement of Belief of the Creation Research Society: "The Bible is the Written Word of God, and because we believe it to be inspired thruout (sic), all of its assertions are historically and scientifically true in all of the original autographs. To the student of nature, this means that the account of origins in *Genesis* is a factual presentation of simple historical truths." (Creation Research Society Quarterly, any issue.) I am puzzled by

the insistence of the *Creation Research Society* and other biblical fundamentalists on the strict literalist interpretation holding that all historical and geographical narratives of the Bible are precisely correct. My puzzle emerges from my own extensive reading of the Bible and the discovery of many literal inconsistencies and outright contradictions between various Biblical texts, in addition to their contradiction with received knowledge. Already in the book of *Genesis* there are two different narratives of God's creation of the universe, animals and humans. The first chapter and the first three verses of chapter two convey the familiar narrative of the successive creation of the earth, the plants, the sun and the moon and the animals over five days, culminating with the simultaneous creation of man and woman on the sixth day: "So God created man in his own image, in the image of God created he him; male and female created he them"(*Genesis*, 1, 27; King James version). "And on the seventh day God ended his work which he had made; and he rested on the seventh day from all his work which he had made" (*Genesis*, 2, 2). A different creation narrative starts in chapter two, verse four, and fills the rest of the chapter. The creation of *man* is given in 2, 7: "And the Lord formed man of the dust of the ground, and breathed into his nostrils the breadth of life; and became a living soul." It is after the creation of man that "God planted a garden" (2, 8); "And out of the ground made the Lord God to grow every tree" (2, 9). "And the Lord God took the man, and put him into the Garden of Eden" (2, 15). The creation of the animals comes later: "And out of the ground the Lord God formed every beast of the field, and every fowl of the air; and brought them unto Adam to see what he would call them" (2, 19). Only afterwards, does God proceed to create woman: "And the Lord God caused a deep sleep to fall upon Adam, and he slept: and he took one of his ribs, … and the rib, which the Lord God had taken from man, made he a woman, and brought her unto the man" (*Genesis* 2, 21-22).

[2] This lecture incorporates portions of my "Arguing for Evolution," *The Science Teacher* 67(2):30-32, 2000; and "An American Malaise: The Debate between Darwin and Christian Fundamentalism," *History and Philosophy of the Life Sciences* (in press).

Plenary VII - In the Case of Robert Andrews Millikan

by: David Goodstein
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I am grateful to Sigma Xi for honoring me with the John P. McGovern award, and for the opportunity it gives me to speak to you today, at this forum on ethical challenges in science. I have decided to use my McGovern award lecture to tell the story of the case against Robert A. Millikan. Millikan was the founder, first leader, first Nobel Prize-winner and all-around patron saint of the California Institute of Technology, an institution that has given me employment for more years than I care to remember. We at Caltech feel a solemn duty to defend our hero. He has been accused of male chauvinism, anti-Semitism, mistreating his graduate students and, worst of all, scientific fraud. My purpose today is to tell his story, look into these various accusations and, to the extent that I can, mount a defense for Professor Millikan.

Millikan was born in 1868, the son of a Midwestern minister. He attended Oberlin College, got his Ph.D. in physics from Columbia University, did some postdoctoral work in Germany and, in the last decade of the 19th century, took a position at the brand-new University of Chicago in a physics department headed by his idol, A. A. Michelson.

During the next decade, Millikan wrote some very successful textbooks, but he made little progress as a research scientist. This was a period of crucial change in the history of physics. J. J. Thomson discovered the electron, Max Planck kicked off the quantum revolution, Albert Einstein produced his theories of relativity and the photo-electric effect, and Einstein’s theory and Perrin’s experiments on Brownian motion established forever that matter was made of atoms. Professor Millikan made no contribution to these events. Nearing 40 years of age, he became very anxious indeed to make his mark in the world of physics. He chose to try to measure the charge of the electron.

Cathode-ray tubes had been around for decades when, in 1896, J. J. Thomson in England succeeded in showing that all cathode rays are electrically charged and have the same ratio of electric charge to mass. This was the discovery of the electron. It was the first demonstration that atoms had internal parts. The challenge then was to measure separately the electric charge of the electron. Thomson and his colleagues tried to do that by observing the effect of an applied electric field on the rate of gravitational fall of clouds of water droplets that had nucleated on ions in a cloud chamber. The upper edge of the cloud, which had the smallest droplets, could be assumed to contain single charges. In this way, a crude but correct estimate of the unit of electric charge could be obtained. These cloud chamber experiments were the starting point of Millikan’s efforts.

Working with a graduate student named Louis Begeman, Millikan had the idea of applying a much stronger electric field than had previously been used in the hope of stopping completely the descent of the cloud. To Millikan's surprise, what happened instead was that nearly all of the droplets with their different positive and negative charges dispersed, leaving in view just a few individual droplets that had just the right charge to permit the electric force to come close to balancing the effect of gravity. Millikan quickly realized that measuring the charge on individual ionized droplets was a method far superior to finding the average charge on droplets in a cloud.

It may have been during this period that Millikan's wife, Greta, attending a social event while Millikan spent one of his many long evenings in the lab, was asked where Robert was. "Oh," she answered, "He's probably gone to watch an ion." "Well," one of the faculty wives was later overheard to say, "I know we don't pay our Assistant Professors very much, but I didn't think they had to wash and iron!"

Unfortunately the single-droplet method had a serious flaw. The water evaporated too rapidly to allow accurate measurements. Millikan, Begeman and a new graduate student named Harvey Fletcher discussed the situation and decided to try to do the experiment with some substance that evaporated less rapidly than water. Millikan assigned to Fletcher the job of devising a way to do the experiment using mercury or glycerin or oil.

Fletcher immediately got a crude apparatus working, using tiny droplets of watch oil made by means of a perfume atomizer he bought in a drugstore. When he focused his telescope on the suspended oil droplets, he could see them dancing around in what is called Brownian motion, caused by impacts of unseen air molecules. This itself was a phenomenon of considerable current scientific interest. When Fletcher got the busy Millikan to look through his telescope at the dancing suspended droplets of oil, Millikan immediately dropped all work on water and turned his attention to refining the oil-drop method.

A couple of years later (around 1910) Fletcher and Millikan had produced two results. One was an accurate determination of the unit electric charge (called e) from observing the rate of fall or rise of oil drops in gravitational and electric fields, and the other was a determination of the product *Ne*, where *N*is a separate constant called Avagadro's number. The product *Ne* came out of observations of Brownian motion. Millikan approached his student Fletcher with a deal. Fletcher could use a published paper as his Ph.D. thesis, but only if he was sole author. Millikan proposed that Fletcher be sole author on the Brownian motion work and that he, Millikan, be sole author on the unit electric charge work. This is the source of the assertion that Millikan mistreated his graduate students. No doubt Millikan understood that the measurement of *e* would establish his reputation, and he wanted the credit for himself. Fletcher understood this too, and he was somewhat disappointed, but

Millikan had been his protector and champion throughout his graduate career, and so he had little choice but to accept the deal. The two men remained good friends throughout their lives, and Fletcher saw to it that this version of the story was not published until after Millikan's death, and after his own death.

Let us turn now to the question of scientific fraud. In 1984, Sigma Xi published a booklet called *Honor in Science*. More than 50,000 copies have been distributed over the years. *Honor in Science* includes a brief discussion of the Millikan case that begins, "One of the best-known cases of cooking is that of Physicist Robert A. Millikan." Cooking, meaning "retaining only those results that fit the theory and discarding others," is one of the classic forms of scientific misconduct, first described in an article by Charles Babbage written in 1830. According to *Honor in Science* it is a well-established fact that Millikan has been accused, tried and convicted of cooking his data. What is going on here? There are really two stories. One is the question of what actually happened back in the period 1910-1917, and the other is how, much more recently, he came to be accused, tried and convicted. It's time to tell both of these stories.

The accusation against Millikan, very briefly, is this. After the 1910 paper (with Millikan alone, not Fletcher, as author) presenting his measurement of the unit of electric charge, Millikan found himself embroiled in controversy with a Viennese physicist named Felix Ehrenhaft. Ehrenhaft, using an apparatus rather similar to Millikan's, found cases of electric charges much smaller than Millikan's value of e (Millikan refers to these as "subelectrons"). In order to refute Ehrenhaft's assertion of the existence of subelectrons, Millikan (now working alone; Fletcher had gotten his doctorate and left) made a new series of measurements, published in 1913, in which the charge on every single droplet studied was, within a very narrow range of error, an integer multiple of a single value of e. The 1913 paper succeeded in dispatching Ehrenhaft and contributed significantly to Millikan's 1923 Nobel Prize.

However, an examination of Millikan's private laboratory notebooks (housed in the Caltech Archives) reveals that he did not in fact report every droplet on which he recorded data. He reports the results of measurements on 58 drops, whereas the notebooks reveal data on approximately 175 drops in the period between November 11, 1911 and April 16, 1912. In a classic case of cooking, the accusation goes, he reported results that supported his own hypothesis of a smallest unit of charge, and discarded those contrary results that would have supported Ehrenhaft's position. And, to make matters very much worse, he lied about it. The 1913 paper presenting Millikan's results contains this explicit assertion: **"It is to be remarked, too, that this is not a selected group of drops, but represents all the drops experimented upon during 60 consecutive days**, during which time the apparatus was taken down several times and set up anew." (Emphasis in the original). Thus, Millikan is accused of cheating and covering up his cheating by lying about it in one of the most important scientific papers of the 20th century. There couldn't be a clearer case of scientific misconduct.

Let us look at some of the pages in Millikan's private laboratory notebooks. The first figure shows a page, dated at the top, November 18, 1911. At the top right the temperature, t18.0ºC (obviously, Millikan's lab was not well heated for the bitter Chicago weather), and the pressure, 73.45 cm (possibly a stormy day). On the left, a column of figures under G, for gravity. These were the times taken for a tiny droplet, a pinpoint of light too small to focus in his telescope, to fall between scratch marks in his telescope's focal plane. These measurements gave the terminal velocity of the drop when the force of gravity was balanced by the viscosity of air. From this measurement alone, he could determine the size of the tiny, spherical drop. Then there is another column under F for "field." These were the times taken for the drop to rise between the scratch marks under the combined influence of gravity, viscosity and the applied electric field, which had been turned off during the "G" measurements. The combined "F" and "G" measurements made it possible to determine the charge on the drop.

We can see that the "F" measurements change from time to time. The first series give an average of 8.83, then 10.06, then 16.4 and so on. That happens because the charge on the drop changes from time to time, when the drop captures an ion from the air. Millikan made use of the changes to help deduce the number of units of charge on the drop. To the right of these columns, a series of laborious hand calculations (not necessarily done on the same day as the data were taken) using logarithms to do multiplication and square roots, then finally, bottom right, the comment, "**very low** something wrong" with arrows to "not sure of distance…." Needless to say, this was not one of the 58 drops Millikan published.

The next figure shows observations on two drops, taken November 20 and 22, 1911, with similar columns of figures. To the right at the bottom of the first observation we see again "**very low something wrong**"and below that, "found meas[uremen]t of distance to the hole did not…." Once again, not up to snuff. But, on the third slide, a page dated "Wednes. Dec. 20, 1911" (the temperature now a comfortable 22.2ºC – did the university turn the heat on in December?), we find the remark, "This is almost exactly **right**, the best one I ever had!!!"

Millikan, in his crucial 1913 paper, did not publish any of the drops for which the raw data are shown in these first three slides, not even "the best one I ever had." This was all part of a warm-up period during which Millikan gradually refined his apparatus and technique, in order to make the best measurements anyone had ever made of the unit of electric charge. The first observation that passed muster and made it into print was taken on February 13, 1912, and all of the published data were taken between then and April 16, 1912, actually a period of 63 days (1912 was a leap year). Raw data taken during this period are shown in the fourth slide, dated March 14, 1912. Our eye is immediately drawn to the comment, on the top center part of the page, "Beauty Publish." Note also the pressure, 16.75 cm, too low for even the stormiest day in Chicago.

During the period February 13 to April 16, 1912, Millikan recorded in his notebooks data for about 100 separate drops. Of these, about 25 are obviously aborted during the run and so cannot be counted as complete data sets. Of the remaining 75 or so, he chose 58 for publication. Millikan's standards for acceptability were exacting. If a drop were too small, it was excessively affected by Brownian motion, or at least by inaccuracy in Stoke's law for the viscous force of air (more about this later). If it was too large, it would fall too rapidly for accurate measurement. He also preferred to have a drop change its charge a number of times in the course of an observation, so that he could have changes in charge, as well as a total charge, that had to be integer multiples of a single unit of charge. None of this could be determined without actually taking and recording data on a candidate drop. Thus, it should not be surprising that Millikan chose to use the data on only 58 of the drops he observed during the period when he and his apparatus had reached near perfection. Furthermore, he had no special bias in choosing which drops to discard. A modern reanalysis of Millikan's raw data by Allan Franklin reveals that his result for the unit of charge and for the limits of uncertainty in the result would barely have changed at all had he made use of all the data he had, rather than just the 58 drops he used.

I don't think that any scientist, having studied Millikan's techniques and procedures for conducting this most demanding and difficult experiment, would fault him in any way for picking out what he considered to be his most dependable measurements in order to arrive at the most accurate possible result. In the 1913 paper, he cites his result with an uncertainty of 0.2 percent, some 15 times better than the best previous measurement (which reported an error of 3 percent). Furthermore, the modern value of the charge of the electron agrees with Millikan's result within his cited uncertainty of 0.2 percent. The experiment was nothing less than a masterpiece, and the 1913 paper reporting it is a classic of scientific exposition. Nevertheless, it contains the phrase "…this is not a select group of drops but represents all of the drops experimented upon during 60 consecutive days…." which is manifestly untrue. The question is, why did Millikan mar his masterpiece with what is unquestionably an outright lie?

Many years after the fact, Millikan's work was studied by historian Gerald Holton, who told the story of the Millikan-Ehrenhaft dispute and contrasted Millikan's published results with what he found in Millikan's laboratory notebooks. Holton did not accuse Millikan of misconduct of any kind, but instead found in the unpublished laboratory notebooks an opportunity to contrast a scientist's public, published behavior with what went on in the privacy of the laboratory. Holton's work was seized upon by two journalists, William Broad and Nicholas Wade, who in 1982 published a book about misconduct in science called *Betrayers of the Truth*. Broad and Wade, both of whom were then reporters for *Science* magazine, and both of whom now write for the *New York Times*, are the ones who tried and convicted Robert Millikan of scientific misconduct. Others, like the writer of Sigma Xi's *Honor in Science*, simply bought their argument at face value.

In *Betrayers of the Truth*, Broad and Wade want to make the point that scientists cheat. Chapter 2, *Deceit in History* starts out with a list of culprits: Claudius Ptolemy, Galileo Galilei, Isaac Newton, John Dalton, Gregor Mendel and Robert Millikan. At the very least, Millikan is in good company. Of Millikan they say he "…extensively misrepresented his work in order to make his experimental results seem more convincing than was in fact the case."

I would argue that this statement is profoundly incorrect. Incidentally, although I have no time to make the case today, the accusations against most of the other scientists on the list are equally spurious.

For the statement by Broad and Wade to make sense, Millikan's principal experimental result would have to be that there exists a smallest unit of electric charge. We would have to imagine that the existence of electrons, and by implication the existence of atoms, was an issue of burning controversy in 1913, with Millikan on one side and Ehrenhaft on the other, and that the whole point of Millikan's exercise was to prove that "subelectrons" did not exist. In fact, there were, in 1913, a small number of respectable scientists who still insisted that the existence of unseen atoms was an unnecessary and unscientific hypothesis, but they had by then been left far behind by the mainstream of science, and besides, even they would not have chosen Ehrenhaft as their champion.

To Millikan, who had seen Brownian motion with his own eyes, the existence of atoms and electrons was beyond question. Every revision of his technique, every improvement of his apparatus, every word he wrote, public or private, was directed to one goal only: the most accurate possible measurement of the charge of the electron. Ehrenhaft and the supposed controversy are never so much as mentioned. And it is worth remembering that history has vindicated Millikan in that his result is still regarded as correct. Nevertheless, we are still stuck with the blatantly false statement, "… all the drops experimented upon during 60 consecutive days."

To understand the significance of that statement, I must make a small digression. Millikan's oil drops rose and fell under the influence of three countervailing forces: gravity, electricity and viscosity. The first two of these were very well understood. For the third, the 19th-century hydrodynamicist George Stokes had produced an exact formula applicable to a sphere moving slowly through an infinite, continuous viscous medium. The conditions that would make Stokes' law exact were well-satisfied by Millikan's oil drops in all respects except one: The drops were so small that the air through which they moved could not safely be considered a continuous medium. In fact (as Millikan firmly believed) the air was made up of molecules, and the average distance between molecules was not completely negligible compared to the size of an oil drop. For this reason, Stokes' law could not be depended on as absolutely correct.

To deal with this problem, Millikan assumed, entirely without theoretical basis, as he stressed in his paper, that Stokes' law could be adequately corrected by an unknown term that was strictly proportional to the ratio of the distance between air molecules to the size of the drop, so long as that ratio was reasonably small. To test this idea, he purposely made that damaging ratio larger than it had to be by pumping some of the air out of his experimental chamber. That is the reason he recorded such low pressure on the page we looked at from his notebook dated March 14, 1912.

Then, when he had assembled all of his data, he used a trick that would be appreciated by any experimentalist. He plotted a graph of all his data in such a way that, if his supposition was correct, all the data points would fall on a single straight line, and the position of the line on the graph would give the magnitude of the unknown correction term. Thus, if it were successful, this procedure would all at once prove that the proposed method of correcting Stokes' law was justified and give the magnitude of the necessary correction. In other words, this procedure, like everything else in this experiment, was designed not to question whether charge came in units, but rather to measure the unit of charge with the greatest possible accuracy.

Now let us turn to Millikan's actual published paper. It begins on page 109 of Volume II, No. 2 of the *Physical Review*. He explains how the experiment is done and, using specific drops as examples, how he analyzes his data, using changes in the charge on a drop to help determine the total number of units of charge on the drop. Then, on page 133, he writes:

"Table XX contains a complete summary of the results obtained on all of the 58 different drops upon which complete series of observations were made during a period of 60 consecutive days." As we have already seen, his published results came from measurements made over a period of 63, not 60 days, but I think we can forgive him that lapse. The clear implication of the sentence is that there were only 58 drops for which the data were complete enough to be included in the analysis. Page 133 is followed by two pages of Table XX and an additional two pages of the graph of the straight line test of the correction to Stokes' law described above. On page 138, Millikan discusses his test of his presumed correction to Stokes' law. He points out that all of the points do indeed fall on the line, and in fact "… there is but one drop in the 58 whose departure from the line amounts to as much as 0.5 percent." And then, the very next sentence is, "It is to be remarked, too, that this is not a selected group of drops but represents all of the drops experimented on during 60 consecutive days…".

The damning remark is made, not in regard to whether charge comes in units, but in regard to getting the correction to Stokes' law right. What he meant to say is, every one of those 58 drops I told you about confirms my presumed formula for correcting Stokes' law. And, although in *Physical Review* it comes five pages after the remark that qualified the choice of those 58 drops, the intervening pages are tables and graphs. In the typescript submitted by Millikan (which does not survive, to my knowledge), it would have followed almost immediately after the qualifying statement. Thus a careful reading of the context of Millikan's words greatly diminishes their apparent significance as evidence of misconduct.

In fairness, it should be pointed out that when, in 1917, Millikan published his book *The Electron*, he did take the trouble to confront Ehrenhaft explicitly and, very effectively, demolish Ehrenhaft's arguments. He also used verbatim the section of his 1913 paper on Stokes' law, thus repeating the offending assertion of having used every drop, without the earlier qualifying statement. Most probably by 1917, he had forgotten the very existence of the other drops he had observed, however incompletely, between February and April of 1912. I believe, after reading *The Electron* that Millikan's real rival was never the hapless Ehrenhaft. Millikan's real rival was J. J. Thomson, not because they disagreed scientifically, but because both wanted to be remembered in history as the father of the electron.

In recent times, Millikan has become a juicy target for certain historians because he was white, male and very much a part of the establishment, and, of course, he is no longer here to defend himself (I'm trying to fill in on that last point). For example, there is a letter, noted in feminist circles, in which Millikan advised the president of Duke University not to hire a woman professor of physics. This occurs much later, in 1936, and Millikan is now famous and powerful, head of the California Institute of Technology (as chairman of the executive committee; he never accepted the title president). W. P. Few, Duke's president, had written to Millikan in confidence, asking his advice on this delicate issue.

Millikan's reply shows his unease: "I scarcely know how to reply to your letter…." he begins. "Women have done altogether outstanding work and are now in the front rank of scientists in the fields of biology and somewhat in the fields of chemistry and even astronomy," Millikan writes later, "but we have developed in this country as yet no outstanding women physicists." He points out that "Fraulein Meitner in Berlin and Madame Curie in Paris" are among the world's best physicists, but that's Europe, not the U.S. "I should therefore," he concludes his confidential advice, "expect to go farther in influence and get more for my expenditure if in introducing young blood into the department of physics I picked one or two of the most outstanding younger men, rather than if I filled one of my openings with a woman."

In his private correspondence, Millikan also reveals an attitude toward Jews that would not be acceptable today. For example, writing from Europe to his wife, Greta, he describes physicist Paul Ehrenfest (not to be confused with Felix Ehrenhaft) as "… a Polish or Hungarian Jew [Ehrenfest was, in fact, Austrian] with a very short, stocky figure, broad shoulders and absolutely no neck. His suavity and ingratiating manner are a bit Hebraic (unfortunately) and to be fair, perhaps I ought to say too that his genial open-mindedness, extraordinarily quick perception and air of universal interest are also characteristic of his race."

What are we to make of these lapses? They are certainly not the rantings of a mindless bigot. Undoubtedly Millikan's biases were typical at the time of a man of his upbringing and background. It should be said that, regardless of whatever prejudices he harbored, they never interfered with his judgment of scientists. His hero A. A. Michelson was Jewish, as were many of the stars Millikan personally recruited to Caltech: Paul Epstein, Albert Einstein, Theodore von Karman and Beno Gutenberg among others. On the other hand, Caltech was an all-male school in Millikan's time and remained so until long after his death.

That, as best as I can tell it, is the story of Robert Millikan. Ladies and gentlemen of this Sigma Xi forum on ethics in science, the defense rests.

Concurrent Sessions

Teaching the Responsible Conduct of Research:
The Why, the What and the How

Panel
Research Credit and Authorship: Graduate Student and Postdoc Case Studies

Vivian Weil, Director, Center for the Study of Ethics in the Professions
Illinois Institute of Technology

Teaching and Learning Research Ethics: Needs and Opportunities Revisited

Judith P. Swazey, President
The Acadia Institute

Stephanie J. Bird, Special Assistant to the Provost
Massachusetts Institute of Technology

Introduction

by: Vivian Weil
Center for the Study of Ethics in the Professions, Illinois Institute of Technology

Here we are, seven years after the last Sigma Xi forum on ethics, values and the promise of science. I was pleased to see the proceedings volume from that forum distributed here, because it provides a kind of benchmark. Since that volume came out, the National Institutes of Health has made it a requirement that grantees who hold training grants have to include ethics in their training, and very recently that requirement has been extended to NIH grants more broadly. So responding to these promptings and to other promptings, as well, the programs, graduate study and graduate schools have undertaken some kind of research ethics training.

We hear reports about lecture series, about graduate student seminars, about survival courses and other kinds of courses that suggest some educators have awakened to the need to incorporate ethics in graduate training. At the same time, we continue to get reports that attention to ethics in graduate programs is thin and sporadic in many places; that the responsibility is handed off to less powerful members of research groups and departments; and that well-meaning people often hesitate to push because they believe they do not know enough about how to proceed. Some may even think that there isn't enough knowledge about how to proceed.

Now, Judith Swazey on our panel points out that we need more and more current knowledge about the nature and effectiveness of ethics training. And of course, the evidence I mentioned is hardly systematic. She holds that in particular we need to get the views and the perceptions and experience of faculty who train graduate students in science and in engineering. They should be the ones primarily responsible for the training of their students in ethics. And Swazey speaks out of empirical knowledge.

She and colleagues conducted a study of faculty and students in graduate education that was completed in the early 1990s and funded at least in part by the National Science Foundation. Her important finding was that many faculty held the view that the best way to transmit ethical standards was by example, by a process of osmosis. The point she makes is that if many, or most, still hold some form of that view, then we have an urgent need to train the trainers. In explaining how she makes that inference, she is going to address the "why" of teaching research ethics and focus on faculty responsibility.

While we lack empirical studies of the perceptions and experience of faculty during the 1990s, we have access to the views and the experience of some graduate students and postdocs with regard to training and research ethics. In fact, the 1993 Sigma Xi Forum proceedings volume that you found on your chairs at the earlier session contains the reports of some postdocs at that meeting, and one of them introduced a set of ethical obligations for faculty of graduate students and postdocs. She modestly concluded that she wasn't in the best position to say what the duties of students should be.

In the meantime, we've gotten published findings of a number of surveys of graduate students and postdocs of biomedical trainees, and postdoctoral researchers in biomedical programs of research, and also of physics students, and, from my point of view, most relevant this morning, four volumes of research ethics cases that came out of a project funded by the National Science Foundation and carried out by the Association for Practical and Professional Ethics.

That program initiated, in 1996, workshops for graduate students and postdocs in sciences and engineering. The requirement for each attendee and participant was to produce a case study. The case studies of all of the students who participated in the workshops between 1996 and 1999 have been published. There are four volumes, one for each of those years, and the volume for the summer workshop of 2000 is in press right now. Following the workshop, the participants refined their cases and produced commentaries, and the faculty at the workshops also produced commentary on the cases. So those volumes contain the commentaries, as well.

My remarks, which will follow Judith Swazey's, are drawn from a study of those volumes. I went back and re-read them all recently. There are 63 cases, and of them, 27 deal with conflicts in research groups or departments over data, over ideas, over credit, over the assigning of authorship and over intellectual property. The 2000 volume, which is in press, has its share of such cases.

These are precisely the topics that Judith Swazey picked out in her abstract as in great need of attention. Interestingly, they are the topics that over 40 percent of the participants in our workshops wrote their cases about. The cases include narratives and dialogues, and they point up the need for faculty to assume responsibility for articulating policies for making them explicit, that address the issues in those cases. And the students comment on the need to create opportunities for discussing the policies for considering the rationales.

Stephanie Bird unites three important perspectives in approaching the teaching of research ethics. One is the perspective of a scientist in the neurosciences. The second is the perspective of an administrator in a major research university who is charged with dealing with research ethics, and particularly the teaching of research ethics. And the third is her position as co-editor of a journal *Science and Engineering Ethics*. That journal, in fact, has published some of the empirical research that I mentioned representing the perceptions of graduate students and postdocs.

Stephanie Bird has extensive experience in teaching research ethics in a variety of settings. We know that part of the challenge is fitting the teaching to research groups, seminars, courses, even company situations. She observes that teaching the responsible conduct of science is primarily a matter of conveying the professional values and ethical standards of the discipline, and she emphasizes how this effort requires making explicit information that is often implicit.

Notice the implication that it is a mistake to rest on unspoken standards. Again, we have a counter to the idea that we can rely on teaching by example or osmosis alone. Stephanie Bird goes on to explain the methods of teaching in a variety of settings within research groups and in courses. This is the "how." So the "why," the "what," and the "how." And I'm now going to turn to Judith Swazey.

Teaching and Learning Research Ethics: Needs and Opportunities Revisited

by: Judith P. Swazey
The Acadia Institute

My comments this morning focus on why teaching and learning research ethics, now termed the responsible conduct of research, is important, and why I continue to believe that the first order task is to train the trainers—that is, the faculty responsible for teaching undergraduate and graduate students in fields of science and technology. In February 1993 I did a background paper for Sigma Xi's Forum on *Ethics, Values, and the Promise of Science*, for the session on Teaching Ethics. That paper drew on some of the findings from the Acadia Institute's study of professional values and ethical issues in the graduate education of scientists and engineers. The study included a survey of graduate school deans and surveys of national samples of 2000 doctoral students and 2000 faculty in 98 of the largest graduate departments in four fields: chemistry, civil engineering, microbiology and sociology. Reviewing our study and my paper for the 1993 Sigma Xi Forum, and some of the subsequent literature on the responsible conduct of research, I must confess I found myself wondering what I have to say that is new on the occasion of this year's forum. And the answer, to myself, was "not much, in terms of a research-based presentation." There has been no comparable large scale study in the decade since Acadia's work, which my colleagues and I believe there should be. We would like to know whether the experiences and views of students and faculty have changed, and if so, how and why?

There have been a number of smaller studies of individual departments and groups, such as medical residents; individuals have been assessing their research ethics programs; and the Office of Research Integrity is launching a much needed initiative for research on research integrity. On a larger scale, agencies such as the National Institutes of Health have, I think, missed a major opportunity to learn more about both the content and effects of research ethics education by not having built an evaluation component into their requirements for training grant recipients. This is a defect that I wish they would remedy as those requirements currently are being extended to all investigators supported by NIH funds, and as a linkage is being developed by the Department of Health and Human Services between education and training in the protection of human subjects and in the responsible conduct of research.

Let me next briefly recap a few of the Acadia study's findings that bear on the focus of this session—authorship/recognition/ownership of ideas—and on research ethics training more generally. In our surveys, we asked graduate students and faculty if they had ever observed or had other "direct evidence of" various types of research misconduct, such as plagiarism, and what a decade ago was termed "questionable research practices." With respect to plagiarism, a cardinal type of misconduct with respect to professional recognition and advancement and ownership of ideas, almost one-third of the faculty reported knowledge of plagiarism by their graduate students, with over 40 percent of faculty in civil engineering and sociology reporting such knowledge. Between 6 percent and 9 percent of both students and faculty reported knowledge of faculty who had plagiarized; in civil engineering 18 percent of faculty said they knew of plagiarism by their colleagues.

Another survey item relevant to today's session, under the "questionable research practices" category, was inappropriate assignment of authorship credit. About one-third of both faculty and students reported knowledge of such behavior by faculty. Not surprisingly, since students have less control over authorship, a much smaller percentage reported this behavior by students (9 percent of faculty, 12 percent of students). Given both the actual and symbolic importance of authorship credit, issues around authorship can be some of the most contentious and stressful ones in academia. As we heard repeatedly in testimony before the Commission on Research Integrity, and as faculty and students recurrently report, disputes over authorship can be a prime cause of high stress—or worse—for students, ruptures in research groups and the filing of misconduct allegations.

We also asked students about what they judged to be the most important influences in shaping their professional values and preparing them to deal with ethical issues in their field. Of the 10 sources rated by the student respondents, the three top rankings were supportive faculty, other graduate students and family. The bottom three were discussions in other courses, labs and seminars; professional organizations in their field; and, at the bottom, courses dealing with ethical issues. Faculty, in turn, were asked to rate the effectiveness of seven ways that students can learn about professional values and ethical issues and standards in their field. "Interaction with faculty in research work" and "informal discussion of ethical problems when they occur" received "very effective" ratings from 65 percent and 61 percent, respectively. There was then a dramatic plunge in "very effective" ratings for the other items: for example, only 19 percent judged discussion in regular courses and 14 percent special courses as very effective, and only 7 percent viewed codes of ethics and standards of professional organizations as very effective methods.

Three points about these data concerning teaching and learning research ethics seem especially salient for today's session. First, as other data in our study indicated, most students and their faculty had not been exposed to or engaged in formal instruction in the responsible conduct of research, which poses the question of the bases on which they were judging the effectiveness of these educational methods. It would be informative to have large-scale comparative data now, a decade later, to know whether more students and faculty have been involved in formal teaching and learning, and, if so, how they assess its effectiveness. Secondly, while the students held that supportive faculty members were the most important influence in shaping their professional values and in their "ethical preparedness" training, other data in our study and a profusion of other research and reports show that students receive far too little "mentoring" by their advisors or other faculty with respect to many important aspects of their training.

Third, our faculty respondents clearly believed—and I would bet that most faculty today continue to believe—that informal means are the most effective way to transmit professional values and ethical standards-what we call "the osmosis factor." Many of us who have been involved in research ethics believe that informal means, including latent role modeling or "teaching by example"—assuming, which is not always the case, that the examples are positive ones—are important and necessary, but not sufficient for a number of reasons. For example, as Dr. Bird will discuss, authorship credit involves complex issues and a wide range of variations in disciplinary and sub-disciplinary conventions and practices that are difficult for "novices" to decipher unless they are explicated and discussed. In many fields, furthermore, the large size of laboratory or other types of research groups, and often absentee senior figures, makes reliance on informal transmission processes even more dubious.

Finally, one might ask, "So, so what? Why all the fuss about training in the responsible conduct of research?" Attention to research ethics has been catalyzed by many highly publicized cases of research misconduct, such as data falsification. But to me, misconduct is almost the least important facet of learning about and engaging in the responsible conduct of research. There are more, and more complex, issues in the everyday conduct of research and life of a lab group that arise in developing research proposals; in performing and analyzing research; in reporting findings; and in the professional interpersonal relationships between members of a research group.

Among the many answers to "so what?", one is that the both the public and the academy believe that research integrity is important. When we surveyed graduate school deans, faculty, and students, very large majorities (99, 88, and 82 percent respectively) felt that "ethical preparedness training" *should be* an important activity of their universities and departments. All three groups, however, reported a substantial difference between "should" and "is" for research ethics training. Remembering that our data were collected a "long time" ago, between 1988-91, 51 percent of the deans reported that their institution was not very or not at all active in this realm, and 25 percent of students and 13 percent of faculty said their departments were not at all active. Hopefully, these percentages would be much lower today—but we just don't know.

Finally, why do I believe that "first, train the trainers" is a priority for increasing and enhancing teaching and learning research ethics? Since both students and faculty, in our study and in the experience of most of us, hold that faculty are the most important source of students' acquiring their professional values and learning about ethical standards and issues, it doesn't take a rocket scientist to reason that the teachers of graduate students can, in principle, do the most effective job of *explicitly* teaching them about various aspects of the responsible conduct of research. And by "effective" I mean, in part, that the students' own faculty will have the greatest credibility and impact, compared to those of us who are parachuted in from the outside. If faculty are to accept and assume this task, however, they need to learn both the subject matter and methods of teaching research ethics—and on that note, let me turn to my colleagues, Vivian Weil and Stephanie Bird.

Research Credit and Authorship: Graduate Student and Postdoc Case Studies

by: Vivian Weil
Center for the Study of Ethics in the Professions, Illinois Institute of Technology

My data are the cases produced by the graduate students and postdocs in the summer workshops I mentioned earlier. In many of the 27 cases dealing with conflicts over data, ideas, credit, authorship, intellectual property, the writers speak of ownership of ideas. Now, that's not, strictly speaking, what intellectual property is. Our patent law doesn't cover ideas. It is restricted to material embodiment of ideas, and research groups, in fact, have a strong interest in generating ideas and in circulating ideas. I came to the conclusion that the conflicts expressed in terms of ownership of ideas were misleadingly expressed in those terms, and that they have, more importantly, to do with recognition for work, with credit and authorship, rather than property.

The research groups in question almost always lack explicit understanding about form of credit, the bases of credit, and the grounds for assigning authorship. Discussion about such understandings and the rationales for those understandings seem to be rare occurrences, and the cases, of course, show the damaging misunderstandings that occur when criteria for recognition and standards for authorship are unspoken.

Because self-esteem and career advancement are at stake, evaluations within research groups that determine credit and authorship can be uncomfortable to make and uncomfortable to confront and to accept. These very natural reactions, I want to argue, make it all the more necessary for research groups to have policies and to state those policies and explain them, the policies governing credit and authorship.

Some of the cases even yield suggestions for policies. For example, there was one case in the most recent group, which is not yet published, that indicates a need to articulate whether credit can be gained from data that is produced when students rotate through labs and produce data in a lab as a rotating student, rather than a member of that lab or research group.

What I want to do now is go through one of the cases produced in our workshops, and it comes, of course, from a subgroup of the 27, dealing with the topics that we've mentioned. As in many of the cases, this one shows the absence of local policies and standards, and failure to use any channel of communication to articulate in advance standards or criteria for authorship. After we go through that case, I want to show what can be accomplished when people give attention to the standards. We had a marvelous demonstration in the lecture that preceded this group of what you can achieve if you really give attention to making explicit the principle that should govern conduct.

I’m going to present some authorship standards that were arrived at by a consortium of journal editors and offer it as a model that groups can consider. And then, in conclusion, I’m going to have some more to say about why research groups and departments should—a strong sense of "should," ethically "should"—undertake the responsibility for defining standards and criteria, and making them explicit, and bringing them forward for discussion, with senior members taking the lead.

The first example is from our first volume, and it’s called "Informal Discussions, Formal Authority." It’s a situation in which a graduate student and his professor have two discussions, the first one quite lengthy, over an hour. Through those discussions, the professor and the graduate student together come up with a hypothesis that seems to give punch and impact to a paper by a postdoc in the same group, which the professor is revising. The graduate student is really very instrumental in putting a robust interpretation into the paper.

When the graduate student sees the revised draft, he recognizes the conclusion. It is the premise of his thesis and something that he considers to be a seminal element of his work. When he mentions this to the professor, the professor replies that if the student thinks he deserves authorship, well, then he can be co-author. The student answers that he is uncomfortable arbitrating his own case, especially when it involves the postdoc in the group. The professor then decides to deal with the situation, which he calls unusual, at a group meeting the following week, and they will let the lab group decide whether the graduate student should be co-author.

The discussions between the graduate student and the professor seem to exemplify the kind of mutual teaching and learning that should go on between competent graduate students and their professors. But the situation leaves it unclear whether the student has made a contribution that is significant enough to merit co-authorship. Don’t know what the standards are and how the professor regards the student’s contribution in criteria. It might well be that the student merits co-authorship if the professor is using a model that was developed by the student. Notice that the professor treats the matter carelessly, leaving it up to the student, who justifiably feels uncomfortable.

I think we can say that the professor acts wrongly in two respects. He says, in effect, that the standards for authorship and credit are not his concern; thereby, he’s refusing to accept responsibility for his own conduct and for deciding this issue. Notice, recalling Judith’s point, that by saying that we don't want to rely solely on modeling, we don't want to deny that modeling is important and takes place in any case. In this case, the professor offers a model of treating credit and authorship in a cavalier, you might even say, negligent way.

The case shows how a lack of clarity or of conventions for credit and authorship in a research group can produce damaging misunderstandings. In opting to leave the matter for the lab group to decide collectively at a group meeting, the professor continues to refuse to take the responsibility himself, and he sets up a situation for discussing credit and authorship after the fact, having lost the opportunity to do it in advance, and in a situation that is likely to heighten the discomfort associated with assigning credit and authorship.

What I want to do now is turn to a model for authorship standards, so that we have an example of what we can get if people do give attention to articulating standards. This one, as I mentioned, comes from a consortium of journal editors, and you see it there. It rejects guest, ghost and gift authorship. It also rejects what one editor described as this murky business of deciphering what being first or last means. It's also attuned to the circumstances of collaborative research and to situations in which there are many contributors to the research.

The idea is that the byline includes only a few names of investigators, who take responsibility for the contents of the article. Think of them as guarantors who vouch for what the report contains. At the end of the article is the listing of contributors, with the contribution of each, as each contributor has described it. The editors provide a checklist of the kinds of contributions that contributors might indicate. Finally, there may be acknowledgments, and the acknowledgments are limited to sources of funding or sponsorship of the research. So you see a move from authorship toward contributorship, which corresponds better to the circumstances under which research is carried out and reported.

My suggestion is that this kind of approach is one that a research group can pick up in looking for, defining and explaining their own standards. What is the function of the names in the byline; and why do we need to know what each of the contributors did; and why do we relegate to acknowledgments the funding sources and include nothing else in the acknowledgments?

I want to conclude with the question of collective responsibility of the research group or department. I think through our questions so far we've already brought out good reasons for the responsibility falling onto the research groups, themselves. That's where the problems arise. That's where credibility of those who pronounce on standards resides. In addition, I think we can make an ethical argument that research groups and departments have a duty to develop collective action strategies.

There's a philosopher at the University of Arizona, Alan Buchanan, who has observed that there's a fundamental ethical principle of common sense that says, "Acting responsibly requires doing what we can to improve the chances of acting responsibly." So if you put this principle together with the perception of laxity in many research settings, laxity which may be understandable in people when you don't have some mechanism to coordinate the behavior, individuals waiting for others to step forward, or have a sense that they don't want to be putting forth when others aren't going to put forth, a lot of reasons why—not evil intent or anything of that sort—laxity persists.

When you combine this principle with the existing laxity, we have a strong basis for saying that departments and research groups have an ethical duty to assume collective responsibility for defining and making explicit and explaining their policies for managing data, for circulating ideas, for allocating research projects to students and postdocs, and for assigning authorship. I think it's a kind of neat and interesting point that the practice within local research groups complies with the content of ethics teaching, and they also make up the environment for the ethics teaching, or the climate. And investigators and research groups must attend to both.

Intellectual Freedom and the National Laboratories

Panel
Introduction
John C. Browne, Director
Los Alamos National Laboratory

The Impact of Recent Constraints on Intellectual Freedom on Science and Technology at the Lawrence Livermore National Laboratory
Jeffrey Wadsworth, Deputy Director for Science and Technology
Lawrence Livermore National Laboratory

Reflections on Intellectual Freedom and Laboratory Culture
Wendell B. Jones, Laboratory Ombudsman
Sandia National Laboratories

Introduction

by: John C. Browne
Los Alamos National Laboratory

As we move toward the 21st century, I believe the importance of the ethical system on which the scientific establishment, including the national laboratories, can build its contributions to society is becoming increasingly more important. Issues include the impact of the research we do, the trust we have between ourselves and the general public and the federal government, and the complexity of the problems that we work on.

One of the most important roles that I see for research management in large institutions, like the national laboratories, is to create the appropriate environment for ethical behavior for all of its employees. Ethics and modern science demands that we create and live a set of shared values. As Bob Dynes pointed out this morning, we're not just talking about rules. We really must have values upon which we build and create the kind of behaviors we want to see. The major issue that I see in developing these shared values is that management and employees must jointly develop, socialize and live those shared values.

In this session today, as I said, we want to explore the issues of intellectual freedom and ethical environment in government and the contracts under which the national laboratories operate. One of the laboratories is run by a nonprofit, the University of California, and the other is a paid-for-profit corporation. I don't know if there are any differences, depending on who the overseeing contractor is. I don't think there are, but it would be interesting to explore any differences we might see between the two.

We have chosen the title "Intellectual Freedom." It's not academic freedom. Although, clearly, there are a lot of shared attributes between academic freedom and intellectual freedom. In our case, intellectual freedom allows our researchers to challenge technical decisions that are made by the laboratory, by the government or by their peers in their area of expertise, not in policy making, but in the scientific realm. It really does not permit them the freedom to roam at will outside of their areas of technical expertise into the realm of policy without clearly stating that their remarks are those of a private citizen and must be handled, therefore, outside of the laboratory business. This is a major issue within our laboratories, and I think one that we try to nurture very carefully, because without it, we think that there would be a closing up of laboratories because of the type of classified work that we do.

The issues that I hope we might explore in this session include, (1) how do you give technical advice to a policymaker? Where do you draw the line with respect to your judgment, your advice versus your opinion, which can change how government attacks very significant societal problems; (2) The issue of security and classification. How does that affect the intellectual freedom of our staff?

(3) Dealing with the public on matters of risk. I am going to add to that, also, dealing with our employees on matters of risk, because one of the things that we're finding as we get better with our detection technology and our screening technologies, we now find out about how employees might be susceptible to illnesses, such as berylliosis, problems with beryllium sensitivity. What ethical issues arise when you now have the ability to learn more about impacts on, not only the public, but your employees as well?

We share similar types of problems that Bob Dynes mentioned: conflict of interest, intellectual property questions. And we also have the conflict of interest as an institution. And I'll just mention one—perhaps it could come up in the discussion—is in the past year we had an issue with respect to how the Congress and the Administration handled the Comprehensive Test Ban Treaty. What kind of ethical issues arise in testing on our technical judgment regarding the CTBT versus institutional conflict of interest? Questions were raised. Were the laboratory directors simply protecting budgets or were they speaking out on technical matters in which they believe very strongly? So, I think we have many of these issues that come into play in our jobs every day.

The security issues raised in the past year regarding Los Alamos were complicated, in my opinion, because of stories in the media that were not necessarily complete or accurate. They were also complicated by the actions of the federal government in not allowing certain information to be made public because of ongoing litigation.

So, it raised a lot of questions, and I think, in the next five to 10 years, the events of the past year or two will actually raise a lot of ethical questions about how people handle classified information, not just at the national laboratories, but in general. Not only the legal issues, but the ethical issues, the values associated with handling classified information. And it's complicated in today's world because the ability to move large amounts of classified information through the Internet has greatly changed the challenge of protecting classified information.

So, let me close out by just saying that I think this is a very rich subject. I made a list of the number of issues that I faced in the past couple of years and I surpassed two pages. But rather than going into any of those issues, since they might come up during your questions, I'd like to turn it over, first, to Jeff Wadsworth.

The Impact of Recent Constraints on Intellectual Freedom on Science and Technology at the Lawrence Livermore National Laboratory

by: Jeffrey Wadsworth
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U.S. Department of Energy/Lawrence Livermore National Laboratory *
Preprint UCRL-JC-141422

Introduction

The Lawrence Livermore National Laboratory (LLNL) was created in 1952 to meet the nation's need for an expanded nuclear weapons research and development (R&D) capability. LLNL quickly grew to become a full-fledged nuclear weapons design laboratory with a broad range of technical capabilities similar to those of our sister laboratory—Los Alamos—with which we shared mission responsibilities. By its very nature, nuclear weapons R&D requires some of the most advanced science and technology (S&T). Accordingly, there is an obvious need for careful attention to ensure that appropriate security measures exist to deal with the sensitive aspects of nuclear weapons development. The trade-off between advancing S&T at the laboratory and the need for security is a complex issue that has always been with us. As Edward Teller noted in a May, 1999 editorial in the *New York Times*:

The reaction of President Harry Truman to the leaking of information is well-known. He imposed no additional measures for security. Instead, we have clear knowledge that the disclosures by (Klaus) Fuchs caused Truman to call for accelerated work on all aspects of nuclear weapons.

…The right prescription for safety is not reaction to dangers that are arising, but rather action leading to more knowledge and, one hopes, toward positive interaction between nations.

To explore the issue of intellectual freedom at a national security laboratory such as LLNL, one must understand the type of activities we pursue and how our research portfolio has evolved since the Laboratory was established. Our mission affects the workforce skills, capabilities and security measures that the laboratory requires. The national security needs of the U.S. have evolved, along with the S&T community in which the laboratory resides and to which it contributes. These factors give rise to a greater need for the laboratory to interact with universities, industry and other national laboratories. Intellectual freedom at the laboratory and constraints on it can be understood only within the context of our mission, our necessary interactions with other entities and our need for an exceptional multidisciplinary workforce.

Issues of Intellectual Freedom at LLNL

The significance of intellectual freedom to a scientist or engineer is similar to that of freedom of speech. Their freedom is constrained only by intellectual honesty and the rigors of the scientific method; scientists and engineers have the right and responsibility to publish the results of their research and comment on the public policy implications of their work. For national security research, classification is a further constraint, but one with which those doing classified work have learned to live through long-practiced classification procedures established by the Atomic Energy Act in 1954. Like freedom of speech, intellectual freedom has generally well-understood boundaries of acceptable behavior. Just as one's freedom of speech is limited by responsibility for the consequences (e. g., shouting "Fire!" in a crowded theater), laboratory employees, in general, intentionally do not divulge classified information.

As conceived by most laboratory researchers, intellectual freedom has two other key components: (1) the latitude to follow their scientific instincts to pursue exploratory research that supports mission goals and (2) unrestricted (except for classified) communication with other researchers with common interests. It is in these two areas that laboratory employees can feel most constrained in their intellectual freedom.

Historically, employees have felt limitations on their flexibility to pursue exploratory research most strongly at times when budgets were very tight (e.g., post-Vietnam War and after the Cold War before the inception of the Stockpile Stewardship Program). Another factor affecting research flexibility is the growing tendency of sponsors to take a piecemeal, specific-task-oriented approach to funding research.

Unrestricted communication with other researchers who have common interests arises particularly for laboratory employees working on unclassified projects; this work nowadays includes a sizeable fraction of our national security research. In many cases the very success of our R&D endeavors depends on extensive collaboration and communication with scientists and engineers in academia, industry and other national laboratories.

In regard to intellectual freedom versus security issues, our cooperative efforts with others are important from at least two perspectives. First of all, we work fairly routinely with non-national-security laboratories as well as with universities and industry. Clearly, the security requirements at those sites are quite different from ours. Secondly, interactions with all those outside the national security laboratories raise the complex issue of interactions with foreign nationals, from both "sensitive" and "non-sensitive" countries. These issues are not only relevant to our interactions with others but are also relevant to our own workforce.

The Laboratory's Mission

Along with Los Alamos and Sandia national laboratories, Lawrence Livermore is a premier applied-science national security laboratory—not just a weapons laboratory. In the most succinct terms, the mission of LLNL is: To ensure national security and apply S&T to the important problems of our time. A more comprehensive mission statement is:

- Lawrence Livermore National Laboratory is a premier applied-science national security laboratory.
- Our primary mission is to ensure that the nation's nuclear weapons remain safe, secure and reliable and to prevent the spread and use of nuclear weapons worldwide.
- This mission enables our programs in advanced defense technologies, energy, environment, biosciences and basic science to apply Livermore's unique capabilities, and to enhance the competencies needed for our national security mission.
- The laboratory serves as a resource to U.S. government and a partner with industry and academia.

Clearly from our mission statement the laboratory engages in diverse S&T areas that may appear to be outside the national security aegis. This approach to research is the legacy of Ernest O. Lawrence, for whom LLNL is named. Lawrence's model was one of "team science"—large projects of national importance that require a multidisciplinary approach. That is our heritage— of which we are most proud. Major consequences of Lawrence's approach were the development of unique capabilities at the Laboratory, our use of multidisciplinary teams to tackle challenging problems, and a deep-seated partnership with the University of California (UC).

At its inception, LLNL focused almost exclusively on nuclear weapons. Our primary mission remains national security, which accounts for about 80 percent of our budget. However, our national security activities have not only changed significantly since 1952, but have also broadened, particularly since the end of the Cold War. The original nuclear weapons mission— designing and engineering new weapons for the stockpile that are more militarily effective and safer—has evolved to the Stockpile Stewardship Program, which accounts for about 50 percent of our budget. It is a science-based effort to maintain the stockpile in the absence of nuclear testing. With an emphasis on developing a fundamental scientific understanding of weapons-performance issues, such as the aging of materials, we are interacting with the academic community even more than we have in the past. Furthermore, the national security challenges are now broader, having evolved to a level of about 30 percent of our budget that includes areas such as nonproliferation, arms control and work for the Department of Defense (DoD).

About 20 percent of our research portfolio is in other mission areas that build on the core capabilities and unique facilities needed for our national security mission. These include efforts to meet important national needs in energy, environment and the biosciences. A few examples illustrate how Lawrence's basic model—use of multidisciplinary teams of scientists and engineers to tackle significant problems—has led to the laboratory's current programmatic base and diverse scientific accomplishments.

- **Energy:** Our interest in thermonuclear weapons led to our interest in fusion science, with the ultimate goal of fusion for civilian energy. In addition to our work on magnetic confinement fusion, LLNL took the lead in pursuing inertial confinement fusion and large glass lasers for that purpose. We hope to achieve fusion ignition and burn in the National Ignition Facility (NIF), which is currently under construction at LLNL.
- **Environmental Sciences***:* Through the Cold War, the laboratory conducted nuclear tests, at first in the atmosphere and then underground. Accordingly, we developed expertise in atmospheric and earth sciences to understand and to limit the effects of these tests. Our atmospheric science expertise led to the establishment of the National Atmospheric Release Advisory Center, which provides real-time information for emergency response in the event of an atmospheric release of radioactive or toxic materials (such as the Chernobyl event in 1986 and the Mt. Pinatubo explosion in 1991). We are also a major contributor to international efforts to model climate change and are home to the Program for Climate Model Diagnosis and Intercomparison. Our geoscience expertise is contributing to the Yucca Mountain Project to dispose of nuclear wastes and to efforts to improve technical capabilities to monitor an international ban on nuclear testing.
- **Bioscience**: Our studies of the biological effects of ionizing radiation resulted in the development of fast-flow cytometry and other technologies that led to DOE's Human Genome Initiative in 1987 and LLNL's participation in the Human Genome Project. Our expertise in genomics and biotechnology is now enabling us to pursue functional

genomics and to develop fast, portable, biological-agent detectors for nonproliferation applications.

- **Other areas of science such as astrophysics**: The laboratory's interests in astrophysics stem from expertise in high-energy-density physics and capabilities to develop advanced instrumentation. In the 1990s, LLNL researchers discovered Massive Compact Halo Objects (MACHOs) in the search for "missing mass" in the universe, developed the sensor suite for Clementine (which collected over 1.7 million images while orbiting the moon), created metallic hydrogen in a laboratory setting, and developed laser guide-star adaptive optics to improve images from terrestrial telescopes.

These examples, and many others not mentioned, illustrate that even with a primary focus of national security, LLNL scientists and engineers have special expertise that enables them to make scientific discoveries and develop technologies in fields not directly tied to nuclear weapons. Our mission is broader than nuclear weapons, and we cannot accomplish our mission in isolation from the broader scientific and technical community.

Interactions with Universities, Industry, and Other Laboratories

To execute the nuclear weapons program, along with our broader national security mission and other research activities, LLNL has always worked with other laboratories, industry and universities. Through these interactions, the laboratory contributes its special expertise to advance S&T, and we draw upon the best others have to offer to ensure that our national security efforts stay on the cutting edge of what is possible.

With the University of California and Other Universities

LLNL has been part of the UC since the Laboratory's inception. This special relationship is deeply ingrained in our culture. An almost inevitable finding of every review of UC's management of its DOE laboratories has been the importance of the UC connection for maintaining intellectual freedom:

It is of the utmost importance that the U.S. retain, in the crucial and controversial area affecting nuclear deterrence, people who are at once technically outstanding and as independent as possible from bureaucratic and political restraints on the expression of unpopular views.

(Buchsbaum Report to the DOE, 1979)

[The Council] believes that it is critical that the laboratories continue to be defined by the highest standards of scientific quality and by other more elusive, but no less important, characteristics, such as openness, scientific freedom and independence.

(UC President's Council on the National Laboratories, Report, 1996)

Preservation of the academic atmosphere at the laboratories is a cornerstone to the UC/DOE contract.

(UC President's Report to the UC Regents, 1997)

LLNL's ties to UC go beyond the UC President's Office management and oversight. Since our beginning, our relationship with UC has evolved steadily—from a series of informal, individual contacts between our employees and UC faculty to extensive research collaborations with virtually every UC campus. In particular, five LLNL-UC research institutes are on site at Livermore that focus on areas where expertise is needed to execute laboratory programs. They provide a hospitable working environment for visiting students, postdoctoral fellows and faculty as they work with laboratory researchers on collaborative projects. In addition, the Department of Applied Science of UC Davis has facilities at Livermore, and recently the laboratory has signed a memorandum of understanding with the new UC Merced, the 10th UC campus and the first new research university of the 21st Century. We expect that UC Merced will become an important partner in joint research activities and a future source of high-caliber employees.

The laboratory also maintains extensive collaborative relationships with many other universities. As in the case with UC, these collaborations strengthen the research programs at LLNL and serve as a vehicle for recruiting new talent. One prominent example of our academic collaborations is the Academic Strategic Alliances Program (ASAP), a $250 million initiative that forms part of the Accelerated Strategic Computing Initiative (ASCI) to help meet the computing goals of the Stockpile Stewardship Program. ASAP is engaging the best minds in the U.S. academic community—which includes foreign nationals—to accelerate the emergence of new unclassified simulation science and methodology and associated supporting technology.

Our many partnerships with universities have also yielded important scientific benefits to our programs. An excellent example is the Massive Compact Halo Objects (MACHO) Project, an experimental search for the dark matter that makes up at least 95 percent of the mass of our galaxy. In addition to the University of Washington, Notre Dame and UC San Diego, our partners include the Mt. Stromlo Observatory in Australia, McMaster University in Canada, Oxford University in England and the European Southern Observatory in Chile.

With Industry

We have always partnered with U.S. industry to obtain capabilities we need for our weapons program. The most notable example is in the area of computers—from the laboratory's acquisition of a Univac in 1953 to our current participation in DOE's ASCI program and the delivery this year of a 12 teraops (12 trillion operations per second) supercomputer from IBM. ASCI relies on the computer industry not as a mere supplier but as a true partner in developing what will ultimately be a series of 100 teraops computers, with the associated software and memory requirements. Similarly, construction of the NIF, the largest laser in the world, has a vital reliance on industry partners, as have our past efforts designing and building successively larger laser systems from Shiva to Nova.

Our interactions with industry have evolved, particularly since the end of the Cold War, to include other elements, for example direct support to the laboratory by industrial consortia (e.g., the Extreme Ultraviolet Lithography program) and transfer of technology by commercialization in the private sector. Areas such as environmental remediation and health care provide examples of LLNL-developed technologies that we "spin off" for public benefit through mechanisms such as cooperative research and development agreements (CRADAs) and licensing. The laboratory has been particularly successful in the arena of industrial partnering, although success at times creates controversy. Issues that arise center around competition with the private sector as well as export control and foreign company involvement.

Naturally, LLNL benefits from interacting with industry to access new S&T. Industry funds more R&D than the combination of the federal government, universities and colleges, federally-funded research and development centers and nonprofits. Industrial globalization means that foreign involvement is inevitable. The very concept of what constitutes a "U.S. company" is reflected in the fact that over 50 percent of Ford and IBM employees are located outside the U.S. Furthermore, the current U.S. spending on R&D is less than the total R&D spending in the other G7 countries (Japan, Germany, France, UK, Italy and Canada). These data imply that for the laboratory to isolate itself from industry and ignore foreign R&D is not a viable option. But we must deal with the security implications.

With Other Laboratories

Work with other laboratories is vital to the execution of LLNL's portfolio. Indeed the history of such interactions has its roots in the early competition and collaboration with Los Alamos. Through competition we improved the performance and safety of weapons in the U.S. nuclear weapons stockpile throughout the Cold War; and through collaboration we advanced the S&T base for nuclear weapons, which is especially important now that we no longer conduct nuclear tests. The Stockpile Stewardship Program is a highly collaborative effort that makes use of the unique capabilities at each of the DOE national security laboratories, the Nevada Test Site and the production sites within the DOE nuclear weapons complex. The program also draws on many sources of external expertise.

LLNL has joint programs with nearly all of the major laboratories in the U.S. as well as with most prominent foreign laboratories such as Atomic Weapons Establishment in the UK and Commissariat à l'Énergie Atomique (CEA) research centers in France. Through a variety of lab-to-lab programs, we also work with scientists at the nuclear weapons research laboratories in the former Soviet Union. Examples of partnerships include our work with Lawrence Berkeley National Laboratory and Stanford Linear Accelerator Center on the B-Factory and the Next Linear Collider; the Joint Genome Institute, which involves Berkeley and Los Alamos national laboratories; and our work with CEA in France and others on the NIF. Many other collaborative research efforts in energy, environment and bioscience could be cited as well.

Our Exceptional Workforce—Current Challenges
To achieve the challenging goals of our mission areas, LLNL and the other national-security laboratories have always sought the best possible scientists and engineers, and they have historically been able to attract a workforce of exceptional quality. This high-quality staff has kept us at the forefront of R&D within the nation.

Several key factors have contributed greatly to attracting exceptional people to these national laboratories:

1. **A mission and a vision**: Historically the laboratories have enjoyed a national commitment to, and appreciation of, our national security mission, as well as a clear vision of our role in making the world a safer place through S&T.
2. **Work excitement**: R&D conducted is of national importance, with the flexibility to focus efforts from exploratory research to advanced development according to project needs.
3. **Work environment**: The labs provide an environment for conducting world-renowned research, a reputation for excellence and a competitive compensation and benefit package for employees.

Adverse trends in each of these areas were accentuated by recent security-related events and actions in response to those events, which resulted in a difficult environment for the laboratories in 1999. Data indicate that our ability to attract and retain a quality workforce has suffered as a result—we hope not irreparably.

Last year was a particularly difficult year for the laboratory in terms of recruitment and retention. The nominal annual attrition rate at LLNL has been extremely low, at about 2 percent for recent years. However, over the last year, it has risen to about 7 percent, more than three times the usual rate, though this rate would be considered low in some industry sectors. Of even greater concern than the abnormally high average attrition rate is the extremely high attrition rate—up to 20 percent—in key areas such as computing and selected engineering fields. Concurrently, the overall acceptance rate for job offers has dropped from 85 percent to about 65 percent.

The negative impact is not the result of a single issue, such as compensation or a decline in intellectual freedom, but a collection of factors. To reverse these adverse trends, we are taking a number of tactical actions.

Recruiting

We have established changes in hiring practices, e.g., targeted salary areas, cash awards, sign-on bonuses, on-the-spot hires, etc. We have also instituted the prestigious Lawrence Postdoctoral Fellowship Program and other postdoctoral programs. It is worth noting that between 50 percent to 75 percent of applicants for these Lawrence Fellowships are foreign nationals.

Retention

In the area of retention, we created a number of new programs at the laboratory to provide additional incentives for our scientific leaders and future managers. For example, in 2000 we began the Edward Teller Fellowship Program that is comprised of MacArthur-type awards presented to individual scientists who have made significant accomplishments in their field. The award allows them to continue to pursue research unconstrained by their normal programmatic responsibilities. In addition, the Long Range Strategy Project group was formed with 22 of our mid-career scientists and engineers who spent 18 months exploring what the laboratory will look like in the year 2020.

In addition to the above tactical areas, strategic areas where the DOE national security laboratories need help to reverse the attrition trend. In many cases, these areas relate to specific events and changes at the laboratories that happened last year, and tie directly or indirectly to the issue of intellectual freedom versus security.

Recent Security Measures and Changes and Their Effect on Intellectual Freedom

In 1999, a number of reactive responses to security events and other actions were taken that affected the workplace at LLNL. While it seems apparent that these factors have had an impact on recruiting and retaining employees, at least in the short term, it is difficult to discern their impact on intellectual freedom. These security measures and changes include:

- **The threat of wide-spread use of polygraphs**: It is unclear how polygraph testing of LLNL personnel will ensure security. However, it is clear that the reaction of employees within the laboratory has been very negative. While the scope and extent of the testing remains uncertain, the threat of polygraph testing has led in a few cases to scientists and engineers requesting reassignment.

- **Increased attention to managing "export sensitive" information**: Laboratory employees, many of whom are engaged in efforts to stem the proliferation of weapons of mass destruction, are diligent in protecting information that could be helpful to potential adversaries. However, when the definition of what is sensitive and what is not remains ambiguous, bureaucracies tend to act conservatively, resulting in excessive restrictions on information dissemination and unnecessary paperwork. Additionally, the standards for handling sensitive information often differ, for example, between DOE's national security laboratories and its science laboratories. These issues, which have a broader impact than just DOE, are beginning to sort themselves out. The long-term effect will be additional paperwork and costs, and likely additional restrictions on information dissemination, with a possible loss of intellectual freedom.
- **Restrictions on interactions with foreign nationals within and outside the laboratory**: Within the laboratory, cyber security concerns are limiting the access foreign nationals have to our most powerful computers. In addition, the past year has witnessed a moratorium on visits of sensitive-country foreign nationals to the DOE national security laboratories (unless permission was granted by exception). That moratorium has been lifted, but foreign trips by LLNL personnel and visits by foreign nationals to the laboratory still undergo very careful scrutiny. Unfortunately, this results in foreign visitors often feeling unwelcome, even in unclassified areas of the laboratory, due to the cumbersome steps that must be followed to arrange the visit and the restrictions to which visitors are subjected while they are here.
- **Reductions in Laboratory-Directed Research and Development (LDRD)**: For FY 2000, LDRD at the DOE laboratories was reduced from 6 percent to 4 percent of the total budget. While this reduction did not directly restrain intellectual freedom, the large cut reduced LLNL's ability to conduct exploratory research, which is very important to our scientific and technological vitality. LDRD is also an important source of funding collaborative research efforts. In FY 2001, LDRD was restored by Congress to the 6 percent level. It is noted, however, that the time to restore lost capabilities, resulting from cuts of these types, greatly exceeds the time it took to create the lost capabilities.
- **Uncertainty in our continuing relationship with the University of California**: Our continuing relationship with UC, which is extremely important to laboratory employees, appeared to be at grave risk last year. We are pleased that DOE Secretary Richardson recently announced that DOE will enter into negotiations with UC to extend the contract for three years.
- **Budget and program concerns**: In FY 2000, LLNL employees were especially concerned about the future of major programs at the laboratory, including the NIF,

the future of ASCI at LLNL beyond the 12 teraops machine just delivered, and funding for and our role in the Stockpile Stewardship Program. At least for the time being, these issues now seem to be behind us. A re-base-lined program for the construction of NIF has been approved by DOE and funded by Congress, and we continue construction of the Terascale Simulation Facility at Livermore, which will house a next-generation ASCI supercomputer (60 to 100 teraops).

Although 1999 was a difficult year, improvements have been steady. Though there is cause for optimism, not all issues will be cleanly resolved and the laboratory will continue to feel the impact from these issues on intellectual freedom and the latitude to pursue cutting-edge research within the laboratory and with a wide range of external partners.

In addition, the laboratory would benefit greatly from a reaffirmation of our mission and vision. The National Nuclear Security Administration (NNSA) within DOE and the national security laboratories have an important mission and also require adequate funding to pursue fundamental science to get the job done. By strengthening the basic laboratory tenets of intellectual freedom—the latitude to undertake research activities that support laboratory missions and the continuing ability to interact with the international science community—we will ensure the health of the laboratory and the continued excellence of its workforce.

Summary

The DOE national security laboratories have effectively managed the pursuit of S&T in a secure environment for half a century. We are an integral part of the international S&T community, and we depend on interactions with others to sustain the quality of our programs by ensuring that our work is at the cutting edge of what is possible. For laboratory employees, intellectual freedom means having the latitude to pursue exploratory research, open communication with other researchers and the right to publish their research results.

As White House Science Advisor Neal Lane said in his address entitled "The New Security Environment" to the National Academy's forum on "Scientific Communication and National Security" (October 6, 2000):

… History clearly shows that we rely on science to ensure our security, not to mention our economy and our whole way of life. But at the same time, we certainly cannot reap the benefits of that science unless our national security is secured. Let me first make three assertions:

- National security requires scientific excellence
- Scientific excellence requires openness and
- Openness is inherently international.

Today we are facing real challenges. Compared to the past, our mission requires us to engage in ever closer and more extensive interactions with universities, other laboratories and industry. And S&T—as well as the laboratory's workforce—has grown more international. Unfortunately, recent events have triggered actions and some over-reactions to tighten security. The result has been a difficult year with attendant challenges in recruiting and retaining personnel and possibly some limitations on intellectual freedom. As we find less onerous ways to implement enhanced security at the laboratories, we continue our efforts to reduce some of these limitations on intellectual freedom and to foster a work environment that is conducive to leading-edge research.

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Reflections on Intellectual Freedom and Laboratory Culture
by: Wendell B. Jones, Laboratory Ombudsman, Sandia National Laboratories

"You should never wear your best trousers when you go out to fight for freedom and truth."

Henrik Ibsen 1828 – 1906
Enemy of the People [1882], act V

When I was first invited to be part of this panel, I looked at a number of quotes. Here is one from the playwright Ibsen. What you are going to hear from me is a discussion about the ethos, the culture within, certainly Sandia, and in many ways applicable to Los Alamos and Lawrence Livermore Labs, that says: "This is a reality, that working towards freedom and truth is not always a clean, crisp business, and if you want a clean, crisp life that doesn't have any messiness in it, get out your best trousers and retreat somewhere." What you are going to hear from me is a challenge that says, we, as a staff that carry a national responsibility, international responsibility, in these national laboratories, should always leave our best trousers in the closet and get to work to honor this.

"Thus in the highest position there is the least freedom of action."

Sallust [Gaius Sallustius Crispus] 86 – 34 B.C.
The War with Catiline [c. 40 B.C.], sec. 51

So, I'm going to come at it clearly from the point of view of—it's not a pretty picture when you are out there trying to sort through the realities of freedom and truth. Another one from a Roman—I feel a bit of a burden. Let me tell you a little bit about my role as an ombudsman. But it certainly means I probably have a little bit more freedom of action today than either John Browne or Jeff Wadsworth has, and want to exercise it in that space. Now, you do need to know, as recently as Tuesday, I was cautioned by the Sandia Laboratory director, Paul Robinson, to consider myself on par with him when it came to freedom of action. So, I have some interior tension about how far to go. Those in the Sandia culture will know that part of what works for me as a laboratory ombudsman is both having and exercising the freedom to be outspoken. So it's always an internal tension to me.

I spent six years on the research staff in Sandia and another 10 as a research manager, and it's now been eight years since I became the laboratory ombudsman. By way of background, let me say that as an ombudsman, I report to the lab director and a designated, neutral and a highly confidential resource to resolve conflicts and differences, service difficult issues. That gives me a much wider reach into the issues of the laboratory than I sometimes would like to have; nonetheless, it does give me that breadth of reach into what the concerns in the institution are. And I'm going to share what I've learned.

"Intellectual freedom is the only guarantee of a scientific-democratic approach to politics, economic development, and culture."

Andrei Dmitrievich Sakharov 1921 – 1989
Progress, Coexistence, and Intellectual Freedom [1968]

Let's look at another colleague, in addition to Edward Teller, in the nuclear weapons design business, Sakharov. An interesting comment made in 1968, and the truth of their system, being the experience in the Soviet Union, bears out all of the truth in Sakharov's comment.

I hold this out because it's a standard against which all of us in these institutions need to hold ourselves. I love the frame that talks about macro and micro freedom, macro and micro ethics. In conservative institutions with conflict-averse people, we like to define our freedom of action inside a nice, little micro box that assures that we will have very robust discussions around whether we should use the finite difference method in solving this problem, or the finite element problem. We just love to get into really robust discussions. But we all collude that no one's going to ask whether we should be solving this problem. A gasp goes around the room when someone says that. No, let's get back in the micro box and have really robust, wide-reaching discussions inside of our definition. I think we all have to think of ourselves in the larger context. Does every discussion have to be wide-reaching? Of course, not. But should we define the range of our discussion so narrowly that we never go outside of clearly defined micro space?

"But what is Freedom? Rightly understood, A universal license to be good."

David Hartley Coleridge 1796 – 1849
Liberty [1833]

Let's talk a little bit about the culture that developed within the context that Jeff just discussed. What happened over time is highly synergistic. The population base of staff evolves according to the situation in which they came to work with these laboratories. That, in turn, influences how the institution evolves, and so the whole thing is highly cooperative and collaborative in how it evolves. I want to talk first about some institutional overlay and then about how we, as a set of people over the decades, have responded.

Within the Sandia culture, there is a very important letter that was written by Harry Truman in 1949 asking the president of AT&T to manage a new laboratory to be called Sandia Laboratory. Deep in our culture, as a footer in our letterhead, is the phrase from Truman's letter: "...render an exceptional service in the national interest." AT&T was asked, in 1949, to manage Sandia as an engineering laboratory. Now, that makes Sandia a little distinctive from Los Alamos, and subsequent to this, Lawrence Livermore, that we were charted as an engineering laboratory to do the engineering work for all of the non-nuclear components for nuclear weapons, firing, fusing, safeguarding functions, all of the engineering. We were established as a highly conservative engineering company to make very conservative engineering choices in which we were to assure that functioning and safeguards, all occurred appropriately within very conservative margins of error. I don't think anybody in this room would want us to have approached that mission any differently. Let's stop and think a minute about the staff you recruit to accomplish that type of mission, done in total security. Do you get wildly crazy thinkers that are out-of-the-box and all over the map, wondering about social context and right and wrong in the world?

"Freedom of speech and freedom of action are meaningless without freedom to think. And there is no freedom of thought without doubt."

Bergen Evans 1904 – 1978
The Natural History of Nonsense [1946], ch. 19

When I came to work at Sandia in 1976, as a culture, we were fiercely proud that no one knew we existed. Most everybody had heard of Los Alamos, but no one had heard of Sandia, and we were proud of that. It meant that the people on the Hill tended to be a little flashy, a little self-centered. They tended to indulge in these fruitless, cataclysmic debates with Lawrence Livermore in California. We, at Sandia, would come in and show wisdom and good judgment in the midst of overly emotional and highly polarized debates. I want you to know that was a self-concept that became reinforced in the people we have present in the laboratory. What arose was an incredibly conflict-averse culture, and it was a culture in which we wanted engineering solutions to everything, and we wanted everything to reduce to engineering rules, to institutional rules, and then if people violated the rules, they could be taken out "to the woodshed," have the rules enforced, and they would come back. Very conflict-averse.

"If there is any principle of the Constitution that more imperatively calls for attachment than any other it is the principle of free thought — not free thought for those who agree with us but freedom for the thought that we hate."

Oliver Wendell Holmes, Jr. 1841 – 1935
United States v. Schwimmer, 279 U.S. 644, 653 [1928]

Through those early years of the 1950s and 1960s and into the 1970s, that homogenous work force was white, it was male, it was overwhelmingly conservative Christian. It was patriots. These were cold war patriots doing a secret mission for God and for country who sought no publicity for it. What that produced—because there was lack of really open debate—was a kind of overconfidence in which doubt was not highly valued, to pose doubt, put question on yourself. Well, as Jeff noted, what happened was that in the 1970s and the 1980s and 1990s, the world has turned over. I apologize for a long quote, but Oliver Wendell Holmes is an uncle of mine, so I had to indulge myself in a quote here. Our work force became more diverse, as Jeff pointed out, the work we did became more diverse. We had differing opinions arriving, all in the midst of this dominant culture.

"I believe there are more instances of the abridgment of the freedom of the people by gradual and silent encroachments of those in power than by violent and sudden usurpations."

James Madison 1751 – 1836
Speech in the Virginia Convention [June 16, 1788]

As Jeff said, from a business point of view, we have been struggling with new potential ventures; but, internally, in our culture, we have also been struggling with defining the norms. As our new diverse work force starts to speak out, some of those norms are under constant attack. So, internally, there's a lot of attack going on. And, internally, we are struggling with the notion of how to exercise responsibly into this space of freedom that is about ideas, about tolerance, about differences. It's going on actively all of the time. Here is my concern. That among us, on the staff, in these laboratories, it's our conflict aversion that limits our freedom of expression. It's our own aversion to conflict that really puts a more effective cap on how far out we can move.

I had a long discussion last week with a member of the Sandia staff who gave me full permission to quote him, Bill Sullivan, you may have heard his name quoted in the press. He was chair of the Wen Ho Lee committee; oftentimes, quoted in any press reports of the ongoing court actions. So, I sat down with Bill to talk about how he has experienced that public expression of his freedom within the context of Sandia. He shook his head and said, "Nothing has happened." In a year of being a highly public, visible person, no one has said anything to him about it. Over the whole time, he's gotten five e-mails from Sandians that were simple statements of, "Appreciate your courage." Nothing more than that. That speaks to me that there is a great space in which people can exercise their freedoms that is unexplored, that is about us putting a damper on our own freedom.

The polygraph discussion that Jeff brought up was one such discussion. Let me tell you what's disturbing about the polygraph discussion. When nearly all employees faced the possibility of a polygraph test, what was the volume of the discourse within the labs? When it became hundreds who might have a polygraph test imposed, what did the volume do? It went to nearly zero. Now that the legislation calls for several more thousands, the volume goes up. That's not a discussion about the principle of polygraph tests. It's not a discussion about the veracity of polygraphs in national security. That's a discussion that says, "I don't want to have to take a polygraph test." It's a much narrower, much more self-serving discussion than the issue of national security and polygraphs. So, as I've watched the volume of discussion track the number of people who are going to have to do it, I said, "This does not have intellectual integrity." Do I want to take a polygraph test? No. Okay, I'm not going pretend that I do. If my concern drops to zero when I find out I don't have to take one, then the discussion about the issue of intellectual freedom in the role of polygraph is disingenuous. In so doing, I would have clearly marked it for what it means to me.

Well, it's a tension, and there will always be tension between the security and intellectual freedom. For those of you who come out of a cultural anthropology or social science background, as my second career, it's interesting, you will find that in every culture through all time there is a type of story that's deep at the core of virtually every culture. And that story has three main players in it. It has a villain. It has a victim. And it has a hero. It might be the princess, the knight and the witch. It has many variations and forms. What we know in terms of human formation is that as long as I can internalize what I am experiencing, have me representing one of those three parties, nothing will change.

So, I call to my colleague in the labs, if I can internalize where I am the victim, and then seek to find out who the villains are, and seek out a hero, nobody will change. What psychology and social science tell us is that change occurs when I realize that I, myself, am the victim, the villain and the hero in my own story, and I need to choose which role I am going to play. We have a long way to go in the interior culture, to live into the space of freedom that I think is out there for intellectual curiosity, intellectual searching. Are there other parties that produce concerns that deserve addressing? Yes. But I care a lot more about the challenge to us as free-thinking people.

The last quote I will put up is a Soren Kierkegaard quote. I refuse to let the discussion be cheapened to the point that it is only about freedom of speech. It needs to be about freedom of thought and the exercise of freedom of thought. And we have a long way to go inside the laboratories to fully exercise the kind of freedom of thought to which I think we're all called, based on our mission.

"People hardly ever make use of the freedom they have, for example, freedom of thought; instead they demand freedom of speech as a compensation."

Soren Kierkegaard

Oversight of Research Staff by Principal Investigator

Panel
David C. Clark, Director, Research Affairs
Rush-Presbyterian St. Luke's Medical Center

Research Integrity Issues Relevant to the Principal Investigator
Chris Pascal, Director, Office of Research Integrity
U.S. Public Health Service

Ethical Responsibilities of the Principal Investigator
Robert Zand, Professor, Biophysics Research Division
University of Michigan

Research Integrity Issues Relevant to the Principal Investigator
by: Chris Pascal
U.S. Public Health Service

ORI sponsored, with the University of Arizona, a principal investigators' conference on "How to Manage a Biomedical Research Laboratory," because we recognized that the PIs or the laboratory directors weren't always really in tune with all of the issues, that from our perspective, overlap with the responsible conduct of research. In order to be effective lab directors, and in order to compete for grants and be well published, one has to be able to deal with things like authorship issues and authorship disputes and do a good job of mentoring new students coming in as postdocs and young investigators. So we weaved that into the agenda on lab management.

I understand that Howard Hughes is considering doing a regular course in the area. They have been in touch with our office about it. Also, the University of Arizona has indicated an interest in building into its curriculum for new investigators a regular course on lab management. I don't know if that's happened yet, but I agree there is a need.

I think a small group, like we have here, provides an opportunity for a dialogue. I'd like to make a couple of comments, and then move right into a case study so we can have some interaction about the issues. As Dr. Clark mentioned, we're not just focusing on research misconduct in this session. In addition to the questionable research practices that one should be concerned about, obviously, there are the affirmative activities that the PI and the laboratory can take to promote research integrity, such as providing education on the responsible conduct of research, or establishing procedures in the laboratory such as regular laboratory meetings or policies for reviewing the raw data prior to publication or submission of a grant application.

Those sorts of things, I think, are fairly important. So we're not just talking about negative things that could go wrong that the PI has to be aware of. We're talking about what the PI can affirmatively do. I'm sure many of you have your own thoughts and experiences on those issues.

I would like to discuss a research misconduct case, adapted from an ORI case, without any identifiers. I would like for you to think about these questions as we go through the case study. As a PI, how would you have handled the situation and the issues? What other options would there be for responding to a particular situation and how could the institution, such as through its research integrity officer, handle the allegation of scientific misconduct? How could your institution have handled it differently from what actually happened?
This is the allegation. A postdoctoral fellow alleged that a technician falsified data in a grant application. The data looked to be impossibly good and unbelievable. When the lab chief, her mentor, took no action, the individual came directly to ORI (which is not that unusual, but most of our cases do arise at the institutional level, and we only hear about it after the fact, after the institution has decided to conduct an investigation). Then, the mentor "fired" the fellow.
Now, I put the word "fired" in quotes because this person was on a training grant, and actually, the PI had no authority to fire the individual, who ended up being unfired after ORI contacted the institution. What has already happened that went wrong, and how could you respond differently if you were in this situation? Put yourself in the situation of the PI. Or you could be the lab technician or the institutional official. What are some other ways to respond to this?
[Transcription of discussion not usable.]

I'm just going to go through a few more of these slides, then I'll go to the end and tell you what happened just so you know. It was a very interesting case.
When the institution got around to reviewing the allegation, it looked at the data sets and said, "Fine." The mentor also defended the technician's findings. The institutional inquiry concluded the allegation was not supported, so it was going to stop at this point. We didn't let them. ORI looked at the data and found problems. Ultimately, there was a finding of research misconduct on this first allegation. Later, the mentor confronted the technician about a second experiment and the technician admitted falsification and was fired. By the way, this happened in the hallway. There were no witnesses. When we asked, there was no supporting documentation and data that we could use to confirm the admission. Now what do you think is happening?
Is it a matter of the mentor, who is scared now and thinks, "I'm getting in trouble. Should I get rid of the problem?" We don't know. This second case went on for a while, but we sometimes get admissions from institutions that we find deficient and we just can't use. In a different case, we recently had a major institution that found research misconduct, said there was an admission, and when we looked into it, we couldn't sustain it. The individual denied that there was ever an admission. Then the institution went back and reopened the case and found out,

"Oops, we don't think there was research misconduct." So I would just say we can't take things at face value all the time. I think that's true for the institutions as well.

To summarize, on the second allegation, we could not find anything to support it, and so we did not pursue that second allegation. But on the original allegation, after the institution couldn't find anything, we actually sent somebody up on site to help them download the data files, and get the "erased files" from the computer, and we did an analysis of it which we then fed back to the institution. They pursued it and still didn't find research misconduct. But we had found that the technician had used some formula to back-calculate from the desired results to the original "data," and we did find research misconduct in this case.

Furthermore, even though the institution did respond affirmatively to the whistle-blower protection issue when we raised it, we were concerned that their policy was not clear on this point. We asked them to make it explicit in their policy that retaliation was not acceptable, and that individual attempts at retaliation could be sanctioned, and they did that.

Let's review some lessons learned from this case.

One important lesson in this case is it's important to get the evidence up front. The evidence that we got from the technician could have disappeared. Fortunately, it was still available, but sometimes it does disappear. The accused scientist sometimes gets scared and makes up data. If you can collect all the data up front, you might stop them from doing something worse. We just had a case that settled on a 10-year debarment when the major charge, the major proof against the individual, was fabrication of data after the fact in order to give it to the university committee which was investigating the allegation.

In-house expert analysis is often critical. Unfortunately, these cases are not easy. While I have a lot of sympathy with the institutions that try to handle them, if you just take a superficial look at the allegation, sometimes you won't have a clue as to what really happened.

A quick response is important to a whistle-blower. We've had maybe 20 whistle-blower cases in the last seven or eight years, and it makes a big difference if the institution can come in and stop the retaliation and protect the whistle-blower, right away. It can prevent lawsuits. It can prevent tremendous misunderstandings and lots of pain and agony. That's not to say that every whistle-blower deserves protection. There are whistle-blowers who are being laid off because the grant or the contract is ready to expire and then they conveniently come up with an allegation of misconduct or wrongdoing after the decision to let the individual go has already been made. We don't pursue those cases. But there are a lot of legitimate issues raised by whistle-blowers. Quick response by the institution is the best way to resolve it when it is a legitimate whistle-blower protection issue. Once again, the admission is only as good as the proof, witnesses, documents, etc. A bald statement that an admission occurred, without more, is not very helpful.

Case: Falsified Data in Application

Allegation:

A postdoctoral fellow alleged a technician falsified data in a grant application and manuscript because the data was "too good to be true." When the lab chief, her mentor, took no action, the fellow notified ORI. The mentor "fired" the fellow.
Source: Adapted from ORI Case

The Institutional Process
As an institutional official, what do you do now?

- Review institutional policies and Public Health Service regulations.
- Take steps to protect the whistle-blower.
- Assess the allegation: determine PHS jurisdiction; review PHS definition; review evidence.
- Notify counsel, initiate inquiry, sequester evidence, notify respondent.

Facts from the Inquiry

- Reviewed data sets provided by the technician.
- Mentor defended technician's findings.
- Inquiry concluded allegation was not supported.
- Mentor confronted the technician about a second experiment and the technician admitted falsification and was fired.

What do you, as the institutional official, do next?

- Initiate investigation and notify ORI (was done after ORI recommended investigation).
- Collect and analyze evidence supporting the allegation.
- Verify that the admission occurred, is supportable, and in writing.
- If misconduct, take appropriate institutional actions.

Facts after Investigation

- On original allegation, found data sets supported results and, thus, no misconduct.
- On second allegation, technician stated he entered data on an existing spreadsheet and it was an honest mistake.
- The committee concluded that data was entered into research record without a legitimate basis and, thus, constituted falsification.
- The institution agreed that dismissal was an appropriate sanction on second allegation.

Criteria for ORI Review

- Review both affirmative and exculpatory evidence.
- Weight of documentary evidence and testimony.
- Significance of alleged misconduct for funding, publication, etc.
- Seniority of investigator, seriousness of misconduct, repetition or pattern of misconduct and impact on public health or others (e.g. clinical trials).
- Other actions needed, such as correction of literature, data bases, etc.

ORI Oversight

- On the second allegation, admission of falsification, ORI found:
- No witness to the admission.

- Admission not in writing.
- Technician denied the admission and allegation.
- Neither the mentor nor institution could provide evidence or data to confirm the allegation.

On the original allegation, ORI:

- Obtained data files from technician's computer.
- Identified files which showed false final values had been entered into a spreadsheet and a formula used to back calculate desired "data."
- Found physical evidence of data falsification/fabrication based on examination of successive spreadsheet files.

Public Health Service Adjudication

Voluntary agreement, where technician was for two years:

- Debarred from federal grants and contracts.
- Excluded from PHS advisory committees.

Institutional Compliance

- Because of attempted whistle-blower retaliation, ORI requested revision of institutional policies.
- Policies revised to state explicitly that whistle-blowers would be protected and violators would be sanctioned.

Lessons Learned

- Importance of gathering all evidence at outset. Respondent's data files needed to prove falsification.
- In depth analysis often critical to outcome.
- A "quick response" crucial to protecting whistle-blower.
- An "admission" of misconduct is only as strong as the supporting evidence. The best evidence is a signed statement of research misconduct and supporting facts.

Ethical Responsibilities of the Principal Investigator

by: Robert Zand
University of Michigan

The question of what are the responsibilities of the principal investigator to the people that they supervise has become an increasingly important concern in both academe, government labs and industry. The days of the hallowed halls of ivy changed with the arrival of Sputnik to the hallowed labs of research grants and contracts. In the academic community, the change in the mission of many universities from one of education and training to one of contract research has imposed many more pressures on the faculty and research staff. I make this distinction for several reasons.

First, in recent years many universities have created a two tier or two track system of the academic professoriat and the research track appointment. Second, the traditional concept of scholarship for the sake of new knowledge and understanding has had constraints imposed on what is "worthwhile" research. Both groups have experienced increased pressures and are subject to the consequences of the pressures. Some of these pressures are driven by ego and the need for peer recognition both by the individual and by the institution.

I would suggest that a more serious driving force creating pressure in the academic and research establishment is the pressure to "show me the money." There is a hierarchy in the present day, research intensive universities that depends upon the input of grant and contract funds to support the research as well as the operation of the institution. The PI is well aware of the consequences of not providing an appropriate level of research and contract funds with their associated overhead contribution.

What are some of these consequences for the PI?

1. Loss of laboratory space
2. Loss of access to graduate students
3. Loss of annual salary increases
4. Increased teaching loads to compensate for loss of grants
5. Denial of tenure (for whatever that term means)
6. Reduced level of publications with concomitant loss of professional recognition

These are a few of the more visible factors that motivate unprofessional and unethical actions of the kind that are not outright fraud and felonies, but fall clearly in the domain of unethical actions or as defined by the National Academy of Sciences as "questionable research practices." The greater the pressure, the greater the temptation to indulge in such action. The penalty for indulging in questionable research practices is generally insignificant. The rewards are quite high.

Scenario 1

A not uncommon situation is the one in which the PI has a theory as to the correct answer to the problem that a student, postdoc or technician is working on. When the experiments do not yield results that agree with the PI's expectations the researcher is told that something is wrong and to go back and repeat the experiments. When the results continue to fail the PI's expectations and the individual doing the experiments feels threatened that they will bear the consequences of this failure to meet the PI's sought for answer, eventually that answer will be provided.

Very often it is the brightest individuals who resort to such fabrication of data believing that they will be long gone with good recommendations before the fabrication is discovered. Here, the PI and the individual who fabricates the data are both responsible for the fraud. Invariably the onus falls upon the student, or postdoc and not the PI. These are situations in which there can be no ambiguity that wrongdoing was committed. These situations usually have a well laid out sequence of steps that result in the punishment of the miscreants.

Scenario 2
The PI runs a large laboratory with some 10 graduate students, six postdocs and one or two technicians. The PI is a very busy individual, serving on committees, study sections, editorial boards etc., and when presented with data obtained by a member of the research team does not have the time to carefully scrutinize the data in order to assess whether it is reasonable or just too good to be true.
What about situations in which there is an abuse of power and where in legal terms no crime was committed but the actions are clearly open to ethical considerations. These fall under the category of questionable research practices. Some institutions have established guidelines and procedures for handling ethical abuses.

Some institutions use the National Institutes of Health or National Science Foundation guidelines and procedures. However, the existence of such guidelines and definitions of rights does not always mean that they are enforced or that redress is possible. Generally these guidelines and procedures are meant for instances where it can be clearly demonstrated that a major legally defined offense has been committed. Some of these areas of misconduct are:

1. Fabrication of data
2. Plagiarism
3. Falsification of research
4. Publication of misleading material
5. Abuse of confidentiality

There are other areas where the question of the practice is less well defined. For example, the following scenario was considered by Professor Carl Berger in his presentation at the University of Michigan Sigma Xi Chapter forum on ethics.
A graduate student leaves the university. A set of data still exists in the student's computer account that is paid for by the grant under whose auspices the data was obtained. This data requires a password to be accessed. The data is needed to complete the research for which the grant was awarded. We are faced with the following questions:

1. What should the professor do?
2. What are the professor's responsibilities to the university?
3. What are the professor's responsibilities to the file "owner"?

In other instances the questionable practice is less well defined. Some situations in this category are:
At a faculty lunch table there is a discussion about how to proceed with a particular research problem. A colleague recognizes that this approach can effectively be applied to a different problem and uses it, without informing the other colleague, to apply for a grant. Does he owe his colleague any acknowledgment for his contribution to the research?

A junior faculty member agrees to collaborate with the chair of the department, with each individual providing their own unique expertise. The research problem is based on ideas initially contributed by the junior faculty member. At a particular stage of the research there is sufficient data to warrant a publication. This is prepared and the chair decides to place all authors in alphabetical order. Since the chair's name begins with "A" and the junior faculty member's name begins with "S," that individual's name appears last. When the manuscript appears in print the chair informs the junior faculty member that the collaboration is ended since he no longer requires the expertise contributed by the junior faculty having learned what was needed in the initial effort. The continuation of the research can proceed without further collaborative efforts. Does the junior faculty have any recourse in this situation?

These are questions and problems that arise every day in the offices and laboratories in academia and industry. The floors in the Halls of Ivy are often full of ethical sink holes. Are these simply matters of political concern, or do they fall under the umbrella of ethical considerations?

Beating the System

A faculty member publishes the same research result in three different journals in order to enhance his list of publications.

1. Is this practice ethical?
2. Suppose the research is germane to three different research areas. The PI then argues that in order to give his work the distribution that it warrants he needs to publish it in diverse journals that are read by practitioners of those disciplines and not by the practitioners in other research disciplines.

Does this justify the practice? See K. McDonald article in the Chronicle of Higher Education, June 5, 1985.

The last scenario I propose is the following: In a given department, a major consideration for merit pay increases and for promotion depends on the number of citations the PI receives in the Citation Index. In order to increase this number, the PI, who has done an excellent study with excellent data, decides to deliberately interpret the data in a way that, although not immediately obvious, is incorrect. The research of the study is in a very important and highly visible area, and soon a multitude of investigators have written papers that do not challenge the data. However, the interpretations is challenged and the paper is cited a multitude of times. Is the PI ethical in doing this? If not, how should the matter be handled?

As I bring my presentation to an end, I want to raise the question of the difference between unethical actions and bullying-like behavior. I believe that the distinction between these terms is primarily semantic rather than demonstrating a difference in behavior. The ultimate consequence of such actions is intimidation. The principal investigator functions as a team or group manager. Department chairpersons function as managers of the principal investigators. Deans function as managers of the department chairpersons. Thus, there is a hierarchy of responsibility to ensuring ethical behavior. This responsibility is frequently neglected.

The effective mentoring of students, lab assistants, junior faculty and researchers by managers and administrators depends on the development of mutual respect between all parties. To give managers and administrators, who lack people skills, supervisory power without built-in controls leads to the abuse of power and the erosion of respect for the institution. The antagonism that can develop can result in a loss of research credibility.
Data can be manipulated and modified to fit preconceived concepts or expectations of the PI or granting sources. The occurrence of abuses of power by faculty and administrators in academic institutions cannot be denied. What controls can the institution provide to ensure that abuses are kept to a minimum? What redress can be pursued without incurring additional penalties to the aggrieved? I have no simple solution to the problem. But, given the current rewards for those who successfully get away with unethical, but not illegal, actions, this kind of behavior will increase.

Not dealing with the problem in a meaningful way will surely promote an increase in such behavior to the detriment of the education, scholarship and research components of the university's mission. Willie Sutton, the bank robber, when asked why he robbed banks rather than small enterprises that were less dangerous replied "because that is where the big money is." Unethical behavior at universities occurs because the rewards are sufficiently great to warrant the risk. In banks there are alarms and guards to deter most thieves. No such meaningful barrier is normally present in most academic settings to impede unethical, but not illegal, activities.

Responsibilities of Scientists to Society

Panel
Robert J. Eagan, Vice President Energy, Information & Infrastructure Surety Division Sandia National Laboratories

Scientific Ethics for Policy Participants
Robert A. Frosch, John F. Kennedy School of Government
Harvard University

Considering the Implications and Applications of Research
Beverly K. Hartline, Acting Deputy Associate Laboratory Director
Los Alamos National Laboratory

Scientific Ethics for Policy Participants

by: Robert A. Frosch
John F. Kennedy School of Government, Harvard University

As you can see from my biography, I have spent a peculiar life, partly as a scientist, partly as an engineer, and largely as a government and industry manager of R&D. I have dealt with ethical questions on one scale or another, including macro-scale and micro-scale, every day of my professional career.

A scientist in public life is not just a scientist, in the usual sense, but may be scientist and engineer and technologist, as well as a policy official, and a personal and professional decision-maker. It is necessary to keep clear in your mind what hat you're wearing at a particular moment. When am I being a scientist as a scientist? When am I making decisions and being a policy official; what else am I doing and being?

Hence, I use 'science' in this context in a broad way, not only to mean scientific research as the creation of knowledge. I also include technology (knowing how to do something, which is different than knowing how things work), and engineering development and implementation (how to complete the job into practice). These aspects of what is sometimes called innovation all have somewhat different ethical and professional dimensions. (Note that it is not always the case that the scientific knowledge leads to technology leads to development. Sometimes it works backwards, from technology or product to science, and there are frequently feedback and feed-forward loops.)

Since scientists acting in the R&D world, the policy world and the industrial world see complications of the various aspects of 'science' from a personal ethical point of view, they are always setting ethical limits or asking ethical questions, or having the questions asked of, or set for, them. These questions are frequently of the following kinds: Will I or won't I pursue this question, whether it's science, technology or development? Will I or won't I argue with people about whether this question should be pursued? If the question is pursued in the way I have argued against, will I or won't I quit.

If you are in such a job, you have to think about those kinds of ethical questions and limits, and the answers arrived at may be different for each of us. You have the right to decide what you, yourself, will do. You do not have the right to decide what anybody else will do, except in a social, consultative sense. You always have the right to argue. I will return to these questions later.

The role of the scientist, now used in the narrower sense of a seeker of truth, a seeker of knowledge, is to be an advocate of scientific method, by which I mean: hypothesis tested against reality by experiment or observation. The scientist should be an advocate of scientific method not only in the areas which are normally delineated as science and technology, namely, in physics, chemistry, biology, etc. The scientist should be an advocate of scientific method applied to all aspects of decision making and policy. When people talk about finance, or when they talk about assertions concerning the results of a public policy, a scientist has an obligation to say, 'How would you know? How could you find out if you were right? What body of facts can this idea be tested against? Given the results, how sure are we? What are the errors and potential errors?' This is an ethical imperative for the scientist. In public life that's part of the obligation of the scientist: to be a scientifically oriented critic.

That critical role of the scientist is different than being a scientist purely in an academic sense. The scientist ought to be the organized skeptic, the representative of skepticism and of critical questioning. It is very important, not only to state 'the state of the science,' but to be careful to say: 'This is what we know.' 'This is what we know well'. 'This is what we kind of think we know, etc.', all the way to: 'We don't know anything about that.' Stating that 'we don't know' something can be a crucially important public role. Going beyond such statements, (in the sense of 'trans-scientific,' using Alvin Weinberg's term), we must say, when applicable: 'It is very unlikely we could ever find out about that.' Further, we may be obligated to say, as Bill Wulf has pointed out: 'We may want to know that for this decision, but it's going to be tough to find anything out. In any case, we're not going to have the knowledge in time for the decision time you have scheduled.' That's an important ethical role for the scientist to play. It also applies to the role of scientist as technologist, developer and innovator.

Scientists ought to spend more time and effort insisting they be included at the policy table where the questions are defined, particularly if the questions have scientific dimensions in the strict sense, or in the expanded senses defined above. As I just said, we have something to say there, not just with our 'I am a citizen' hat on, but with the hat: "I am a scientist and I have ways of criticizing and being skeptical about things that will be useful in better defining the policy questions.' If the problem is badly set out, the answer may be terrible. Social and political scientists call such question asking 'framing.' I just call it asking what the key questions are.

When we consider technology, engineering and development, the purpose is to create useful 'how-to' capabilities. That is, if I follow certain procedures, I get a strong, ductile metal; I build a strong bridge. However, in technology, engineering and development we are always impaled on what we don't know. The idea that I won't develop and apply a technology or use a machine until I know it is completely safe is a delusion about an uncapturable will-o'-the-wisp. There will always be some unknown risk. In the Pentagon, we talked about what were the knowns in a development project, and the unknowns (really the known unknowns), and the unknown unknowns. (Unknown unknowns were called 'unk-unks'.)

Knowns: We know the bridge can fall down; therefore, we do certain things that we know how to do to make sure it's strong enough to carry the expected loads.

Unknowns: We know that we don't actually know what all the loads will be. For example, we don't know what winds will blow on this bridge. We have some historical knowledge of probable winds, but we know we don't know what winds may really blow. We do know how to be reasonably careful in the face of this ignorance. (Sometimes, as in the case of "Galloping Gertie," the Tacoma Narrows Bridge, it turns out we are wrong; we didn't know what the forces, and the bridge's response to them, would really be. We learned.

Unknown Unknowns: Well, we've never yet seen a steel bridge designed with this new suspension and this new kind of steel that actually lasted 150 years, because we've never built one before. In spite of all our theory and experiments and tests, there may be something lurking in the properties of this new suspension, or this new steel that we haven't tested, because nobody was wise enough to think of the new possibility. Or, in a case that I know well, when you're developing an automobile with new technology in it, you only have a few months, or at most a year or so to test the technology realistically in an actual test automobile. (And the test automobile cannot be a sample from the production line after it's been running for a while.) If there's something that's going to happen once in 100 million miles, you aren't going to have driven 100 million miles on the test track before you put the car out. Thus you don't even know what you don't know. That's an unknown unknown.

The knowledge that there are 'known unknowns' (which you can try to design for) and 'unknown unknowns' (which you cannot design for because you don't even suspect what they might be) must be considered in the ethical balancing of decisions. The ethical issues are part of the engineering question. There are ethical decisions about what you say about the project, and what cannot clearly be said because it is unknown that it is unknown. It is not always clear how to deal with this problem. It is another reason for the scientist/technologist/engineer to be at the policy table.

When you're developing something, that's where 'I will' or 'I won't' arises. Do I think what's being developed is worth developing; is this a good thing or a bad thing; should it be developed, or not be developed? Will I or won't I argue about what is being developed, and what risks it poses? Will I or won't I quit if I don't like what I'm being asked to do? Is it really all right to ask someone to work on a particular idea, given the consequences I can envision?

Even there, there are unknown unknowns, especially with regard to future possible uses of a technology. Even if I think the proposed use is ok, do I know what else might be done with it? I know of no civilian technology that I couldn't figure out how to use for some military purpose, and I have never seen a military technology that I didn't know how to use for a civilian purpose, frequently more valuable than the military purpose. That's a function of the imagination, not a function just of science and technology knowledge.

When Maxwell and Marconi and Graham Bell started what they were doing, they certainly didn't have in mind what we're doing now. They certainly didn't have in mind the telephone becoming what the telephone is, or communication beyond the wired telephone to cellular, etc. They could not plan for the unknown future 100 years away. When Thomas Midgely invented the chlorofluorocarbons, the CFCs, he was solving a terrible problem. Refrigerators were being run with poison gasses, sulfur dioxide and ammonia, chosen because they had the right thermodynamic properties. Gases leaking from defective refrigerators were killing people. Refrigerators were occasionally exploding. He was finding a refrigerant to solve those problems. He had no way of knowing, because nobody knew, that there was a stratospheric ozone layer, and that it blocked the sun's ultraviolet radiation. (At the time I don't think anyone knew much, if anything, about ultra-violet radiation.) He couldn't know that the CFCs would deliver chlorine to the stratosphere, and that chlorine chemistry in the stratosphere was going to interfere with the processes that shield the earth from the sun's ultraviolet radiation. How could he anticipate the unknown unknowns of the long future of chemistry and geochemistry?

All of these problems produce ethical dilemmas that I do not believe can be solved with simple rules, or simple principles (e.g.: the 'precautionary principle,' in any of its versions). One learns to solve them (sort of) with simple basic ethical principles, and with your logic and your gut feelings. I learned much at my father's knee (he was a physician). Even with guidance it's hard to learn and hard to teach to students. It certainly isn't going to do be completely done with a course and a textbook, but that might expand minds, so that continued experience in practice will lead to continued ethical learning.

(In addressing an orientation class of freshmen, in my father's day, or perhaps before, a dean of Columbia College said: 'You have come to Columbia College, among other things, to open your minds. Open your minds, but, for God's sake, don't open your minds so much that your brains fall out.')

The principles of disclosure and transparency are very important. I make the assumption that anybody who has a real connection with a subject and an intellectual interest in it almost certainly has some bias or conflict of interest. You are obligated to do your best to understand your own biases and conflicts and to try to explain what they are. If it appears that there is a financial or business connection, then oversight by third parties is very useful. However, I would not like to disqualify the best possible person to do a piece of work from doing it because they may have a conflict of interest. I'd much rather have a third party help by watching over the process, keeping track of it and calling a halt if there's a problem.

(An anecdote about perceived biases and conflicts. I talked the other day with a postdoc who was working on a very interesting problem. She was trying to understand the political, psychological and social background and motivation of some global warming contrarians. She said, roughly, 'I went into this with the standard assumption that these guys are in the pockets of industry, and that they have those conflicts of interest. As I met and talked to them, I realized they are all sufficiently old and distinguished so that that is probably not a relevant issue. There must be something deeper than that in their contrarianism.' As it happens, I know some of her interviewees. They are not in anybody's pocket; they are contrarians on many subjects, perhaps most subjects, and perhaps a little conservative in their politics, so their contrarianism on climate is not unusual for them, and does not imply that they have been 'bought.' Deeper explanations for their views must be sought.)

I am more concerned with clear explanations and honesty than with looking under the bed for conflicts. It is the ethical responsibility of the scientist to be as clear as possible about what he/she thinks they know, to what degree they are sure, and why. One should be as clear as possible about uncertainties, what might or might not reduce them, and what is and is not known about consequences. Try to be as clear and explicit as possible about your own biases and conflicts.

On the policy scene, it is the additional obligation of the scientist to be critical (in the scientific sense) about intellectual rigor, quality of data and logic, but not to claim too much.

I have chosen to talk about this general aspect of the scientist as ethicist on the policy scene because most discussion of scientific ethics focuses on small issues and suspicions, and I wanted to discuss the general problem and its characteristics from various angles. In discussions of science and ethics there has been far too little emphasis on the large positive ethical responsibilities of the scientist in public and policy life.

Considering the Implications and Applications of Research

by: Beverly K. Hartline
Los Alamos National Laboratory

I don’t have nearly the experience with these dicey issues that Bob has, but I’m perfectly capable of being nearly as controversial. One thing I can say now: It’s always the people who don’t need to come to meetings like this who come to meetings like this. We would get a lot farther a lot faster if people who aren’t in this room were here, and one challenge is to make that happen.

We are definitely very fortunate members of society as scientists. We have specialized expertise, and use it to explore and understand the unknown. The responsibilities of scientists to society are larger than, of course, the questions we were given today, but what I’m going to do is propose some thoughts and answers and then ask some other questions that relate considering the implications and applications of research before one undertakes the research and what we do.

I believe very strongly that one does have a responsibility to consider possible implications and applications of research. It’s not only socially responsible, but it is scientifically enriching. It’s the type of thinking that can prepare the researcher to be alert to developments and connections--details, that might be missed by an investigator whose planning was restricted narrowly to the technical specifics of the study. Moreover, possible implications and applications often dominate the justification presented in a typical grant proposal for why the research is important and worth funding.

In addition, an awareness of possible applications can help the researcher communicate to peers and to the public the context and potential value of the research, as well as implement measures to prevent the worst, if the worst is potentially knowable or imaginable. A follow-on question that was not asked is whether the researcher should then eschew a line of inquiry if its result might have adverse implications or applications. If so, at what probability level of adverse implications or applications should this self-prohibition kick in?

I agree with Bob that it’s basically up to every individual to make the choice. Each person should make it conscientiously, I think, and I would be interested in your thoughts on a process that could help investigators anticipate possible outcomes and thus be in a position to make a wiser choice than if they were missing something. These are the obvious or unobvious, the known and the unknown. Different people could easily make different choices. People could have agreements or disagreements about this.

In most cases, I think my choice would be to proceed with the study and simultaneously do everything I could to minimize or mitigate possible damaging uses of the result. My reasoning is based on the fact that much of research is the investigation of the unknown. Anticipating consequences could be erroneous. I could make mistakes when I'm guessing what dire consequences could happen, and I would hate to see the creation of new scientific knowledge systematically blocked by preconceived concerns about a possible adverse impact.

Moreover, individuals or organizations, such as terrorists, could choose not to abandon the research, if it occurred to them. Defense against the abuse of new technologies and knowledge is much easier when they're understood by the good guys. I like to think I'm a good guy. Instead of limiting the horizons of research, I feel we should work deliberately to create a social and political environment that somehow neutralizes the potentially damaging uses of discoveries and inventions and knowledge.

The second question we were posed was should we become involved in developing restrictions on the use or boundaries of our research. If researchers are not involved in the development of restrictions or boundaries in areas where there's a lot of public concern--cloning, genetic engineering, nuclear power--then the likelihood of less knowledgeable people developing uninformed and unwise restrictions is extremely high.

We live in a litigious and regulation-rich nation, where the public expects the government to protect it totally from harm. I was at the White House Office of Science and Technology when Dolly, the cloned sheep, was announced. Congress rapidly introduced draft legislation to outlaw human cloning, and most of the draft bills included features that would have been devastating to valuable biomedical research. At OSTP, we would have had no success opposing any restrictive legislation outright. Our challenge was to allow the acute public anxiety to abate—time does help in these cases—to help more information emerge, and to work with the system to develop alternative approaches and legislation that would provide the protection the public wanted without handicapping the research enterprise.

I'm not in biomedicine. I wasn't really in the biomedical policy. It was mostly my colleagues that were engaged with this issue, but, of course, there's a lot of dialogue that goes on when you have acute questions like this. What the President did was ask his already empanelled National Bioethics Advisory Panel—it had been empanelled a few months before that—to consider the development of Dolly, the sheep, and to provide advice, which it did after working on the issue for several months.

The participation of experts is the only defense against the imposition of many nonsensical and unreasonable boundaries in research. In many cases, the damage of ill-conceived restrictions to research, education, health, quality of life and the economy would far exceed the dreamed-up consequences of possible misuse of the research being regulated. As Thomas Jefferson noted back when this great nation was created: "Reason and free inquiry are the only effective agents against error. They are the natural enemy of error and error only."

Self-regulation has, in fact, been chosen in cases where there was a serious concern about a grave consequence to society. Perhaps the most noteworthy example is the secrecy associated with early fission research. Owen Chamberlain gave a talk at the University of California at Berkeley in 1969 on the social responsibility of scientists and how the physics community handled this situation in the early 1940s. The following is all in Chamberlain's words:

"The early work, the work that really started in this country sometime in the middle of 1939, was kept secret by a completely voluntary process. There was no government regulation of this. It was kept secret by a decision among scientists to have a committee of scientists who would act as secondary referees on papers that were to be published. Thus, any articles that the editors of the physics journals thought should not be published for reasons of secrecy were sent to the committee, members of which would consult with the author. As far as I know, in every case they obtained the author's cooperation in simply not publishing material that might have a direct bearing on the possible military application."

"There was a general feeling that it was to everyone's interest in this country to see that any military project in Germany did not accidentally get help from the people here. Although the secrecy was remarkably well maintained on a voluntary basis, as time went on and the project moved to Los Alamos, we began to encounter stricter government regulations."

The voluntary secrecy was in effect before there was any sizable government commitment to support physics or nuclear weapons, and by the middle of 1942 it turns out only $40,000 of government funding had been spent on the Manhattan Project and its precursors. All the rest was done by university faculty harnessing graduate students and the like.

Now, you have no doubt heard about the impact of increasing government regulations and restrictions on Los Alamos and other places as time goes on, but I wasn't going to spend much time talking about that. Currently, we're in a state where scientific knowledge and the development of new technologies are opening whole new fields of inquiry, and the interface of science and society is growing at an enormous rate and becoming ever more complex. Researchers are the only people who have a hope of understanding some of the implications of the research before it is published or before it is pursued.

In my view, our active engagement as scientists and research managers is essential for defining effective mechanisms for managing this interface. Our involvement in creating only appropriate boundaries and restrictions can help society benefit, rather than suffer, from our discoveries and results, and will be essential for a promising future for both science and society.

The New Federal Research Misconduct Policy

Panel
The New Federal Research Misconduct Policy
Holly L. Gwin, Chief of Staff and General Counsel
Office of Science and Technology Policy

Peggy L. Fischer, Associate Inspector General for Investigations
National Science Foundation

Managing the Complexities of Implementing OSTP's Research Misconduct Policy at the U.S. Department of Energy
William J. Valdez, Director, Office of Planning and Analysis
U.S. Department of Energy, Office of Science

Federal Research Misconduct Policy from the Public Health Service Perspective
Chris Pascal, Director, Office of Research Integrity
U.S. Public Health Service

The New Federal Research Misconduct Policy

by: Holly L. Gwin
Office of Science and Technology Policy

Our panelists will discuss what the implementation of the new federal research misconduct policy will mean for their agencies and for research institutions. First, I would like to give you a status report on the policy. Some of you may recall, and others may be surprised to learn, that the National Science and Technology Council (NSTC) established a research integrity panel in 1996 to initiate work on this policy.

The original panel included Frances Cordova of NASA, who originally served as the panel chair; Ruth Kirschstein of NIH later assumed that job; Helen Kerch with DOE; Ann Peterson with NSF; Asha Varma, DOE; and Cathie Woteki at USDA. Some of these folks remain in government, and others have gone back to the research community. We are grateful to them for getting us started and look to them for help during the implementation phase.

The panel's report proposed a definition of "research misconduct" and some guiding principles for investigations and agency actions. After the panel completed its report, Sybil Francis continued working with the agencies to bring the policy to the point where we are today. The public part of this policy-making process started last fall. We now have incorporated the public comments on the proposed policy into a draft that was submitted to the NSTC for clearance. We received comments from more than 200 people, some writing as individuals, but most writing as representatives of universities or other research institutions. We even got some of the federal agency comments that way, and some research associations. The comments are a matter of public record, and I'm going to summarize some of the major ones for you in just a minute.

I would like to say that initiation of the final agency clearance process engaged the interest of some agencies that hadn't really paid much attention before now. They have some legitimate concerns about implementation of the policy, which is going to pose some difficulties. I wish they had raised those concerns earlier in the process, but I'm still confident that we're going to get this out this fall.* I'm going to go through the major elements of the policy. I will talk to you about the revisions I think we're going to make, or in some cases will not be making.

Maybe the most important point I'd like to make today is the nearly unanimous support for the policy that we got during the public comment period. Of course, that doesn't mean there weren't a lot of questions and recommendations for improvements, starting with the definition for "research." There were a lot of questions about whether the definition was meant to encompass particular fields. Does it include medicine? Did it include the social sciences? The policy is not intended to exclude any field of research, and we're going to try to make that clear in the final version of the policy.

Our attempt to define each of the main elements of the "research misconduct" definition also elicited a lot of comments. Several comments noted that "fabrication" certainly includes more than making up results. It also includes making up data, and we agree. Several commenters feared that the definition of "falsification" could lead to penalties for researchers who make completely appropriate omissions of data. We think it's important for the policy to cover instances where omission of data misleads the consumers of research. We're probably going to use language in a preamble that will accompany the final publication of the policy to make our intentions clear on that point.

Several commenters also expressed concern that our definition of "plagiarism" tacitly approved the use of information obtained through the confidential peer review process so long as it was properly cited. And we are going to try to fix that. After the definition, the policy describes the requirements for a finding of research misconduct. Several commenters wanted to make sure that practices don't have to be written down to meet the accepted practices threshold. Others noted differences in practices among various fields of research. It is not the intent of this policy to call accepted practices into question or to define what the practices are. We'll continue to depend on the relevant research communities to do that job.

We'll also try to address, in the final policy, the concerns of readers who weren't sure whether we meant to require proof of all three levels of intent or just one. "Intentionally," "knowingly," and "reckless disregard" are mutually exclusive terms. They are already a hierarchy, starting at the worst and going to the least worst, and that also comes into play when the implementers are thinking about the sanctions that they want to apply.

There is a significant difference of opinion on the appropriate burden of proof. Some reviewers find "preponderance of the evidence" acceptable. I'd say more reviewers urged us to go with "clear and convincing evidence," but I think we will be sticking with the standard of "preponderance of the evidence."

We did receive comments on the three remaining elements of the policy, but most of those are going to have to be addressed during the implementation phase rather than through modification of the policy. Time lines, for instance, will be established on an agency-by-agency basis. That's also true for measures that ensure confidentiality or that protect whistle-blowers from retaliation.

I would like to note that there was, again, near unanimous support for agency referral to a home institution whenever possible. We'll be keeping that in the final policy.

We're going to try to clarify some issues about the finality of certain decisions. For instance, we need to make it clearer than it was in the proposal that agencies will be making the final decisions on whether there will be an agency finding of research misconduct; and they'll consider that after the institution is through; and an agency decision cannot be appealed back to the research institutions.

We'll also present a more comprehensive, but still non-inclusive, list of corrective actions based on the reviewer's comments, and we're going to recognize a role for agency inspectors general in research misconduct cases. We worked very hard to get inter-agency consensus on this policy, but when it's final, as we have been discussing, it's going to be up to each agency to implement the policy through mechanisms that best suit its needs. Some agencies will need to revise existing policies and regulations. That's true for the National Science Foundation and for Health and Human Services. Some agencies will need to implement the new policy from scratch. That includes the Department of Energy, and they're just waiting for us to get our work done before they go forward. Some agencies will go through formal rule-making, and others will implement through administrative mechanisms.

But in all cases, the implementation process is going to be a very public process. Regardless of whether they use rule-making or some other mechanisms, they will be publishing their proposed policies and rules for public review before they are finalized. And the Office of

Science and Technology Policy will stay involved in this issue throughout the implementation phase. We established an implementation working group that had its first meeting last July. I think it will meet again in conjunction with publication of the final policy.

The agenda will include a discussion of areas where there can be consistency among the agencies in implementation of the policy. We will also ask for a preliminary reading on whether agencies will be using rule-making or some other mechanism. And we're going to probably ask agencies to go ahead and identify their investigations offices and their decision-making officials. That's a quick summary of where we are on the policy.

- The final policy was published in the *Federal Register* December 6, 2000.

News Release Announcing New Federal Research Misconduct Policy

Research Misconduct: A New Definition and Guidelines for Federal Research Agencies

Managing the Complexities of Implementing OSTP's Research Misconduct Policy at the U.S. Department of Energy

by: William J. Valdez
U.S. Department of Energy, Office of Science

What I want to do today is give you an idea of the complexity of the situation that we're facing and some of the issues we are grappling with at the Department of Energy. I am struck by the fact that all of the questions that I have heard today are the questions we are asking ourselves. The kinds of situations you bring up are the kinds of situations that we are thinking about as we begin to implement OSTP's research misconduct policy. And we're much farther along than a lot of the other agencies, because we have been involved in this from the beginning.

We're involved in a lot of different sciences. We're very big. We have a lot of money. And so by nature, the complexity of the questions that have come before us are large and many. Within the department, we have four major business lines. Science is one of them. But we also deal with environmental quality and national security and energy resources. And, again, the numbers are big, and the numbers of projects that are funded are really big.

In addition to various research organizations, which are many, we have many other offices, ranging from the Office of Inspector General, to the Office of Hearings and Appeals, to Environmental Safety and Health, to our chief financial officer, down the line of folks who all have a stake in the implementation of this prolicy. All of them are going to be involved in this process. Adding to the complexity, as you may be aware, Congress recently imposed upon us a new National Nuclear Security Administration (NNSA), which has a separate structure.

By law, you can't have what they call dual hats. So there is a sovereignty issue currently being debated within the department about whether the Office of Science can even take the lead on research misconduct, because we have to respect the Congressional wishes on sovereignty issues regarding NNSA.

If you're familiar with the labs that we have—one of the questions is: "What falls under this policy?" We have many, many scientific user facilities. We have many, many national laboratories. The National Synchrotron Light Source is just one example of our major facilities, where there are about 5,000 users. Those 5,000 users don't actually do the majority of their research on behalf of DOE. That research funding and direction comes from NIH, it comes from universities, it comes in from private sector corporations. And we have to decide, when they use our facility, doing research that is peer reviewed by us—because we peer review every project that goes in there—will that fall under our research misconduct policy? The answer is probably, "Yes." And because we are the home institution, we're the ones who are going to have to handle the investigation, or the lab where the allegation was made. So it gets very complex.

The Office of Science alone deals with more than 200 universities. Forty percent of our research dollars, about $400 million per year, goes to universities. There are a couple thousand grants per year that goes to different universities.

So we're all over the place. We have all sorts of conflicting and diverse issues that we have to deal with. So what have we done so far? We were an original representative on the task force that led to the development of the Office of Science and Technology Policy's research misconduct policy. We developed a strawman proposal for DOE in December of 1999, which required a wink/nod by the Undersecretary, who is Ernie Moniz. It took us until May 2000 to get Ernie to say, "Okay, I've winked and nodded on it." So this is going to be a process that takes time and will evolve. Ernie was the originator of the process that led to this. He cares about it, and it took us five months to get his attention on it.

So now we have a new gang of people coming in, a whole new administration to educate, to work with, getting their winks and their nods. It's going to be very, very difficult. We may have the OSTP policy before we have a President. You know, it's going to be close. We have an R&D councill that brings together all the major offices that are involved in research. That already has taken place, but we might need to get them to approve our approach once again. Now we go into the next stages, buy-in, rule-making, and implementation.

The actual strawman proposal itself was developed around the principles and guidelines of the OSTP policy you've heard about. A number of other options were considered. We thought about setting up a separate office. We went to the idea of farming this out to NSF, NIH. And a number of issues remain unresolved, even today.

For example, we are deciding who within DOE should handle further investigations. We had a meeting between our Office of Inspector General and our Office of Hearings and Appeals. Office of Inspector General really likes to do criminal complaints; that's what they're in business to do. The Office of Hearings and Appeals handles the civil kinds of complaints and whistle-blower kinds of things. Office of Hearings and Appeals was interested in doing this. But the Office of Inspector General said, "We think that there are some classes of research misconduct that we want to be involved in." And we said, "Well, can you give us an idea of what those classes may be?" And they said, "No. We just need to see them." And we said, "Well, we have this problem with timeliness, and we want to make it a fair and open process." And they said, "Well, we can't help you." And we said, "Let's take a step back. Can you define the classes?" And they said, "No."

"Okay," we said. "So what if we gave you a 30-day time period to say that these are the ones that would be something we would like to investigate?" They said, "Well, we can live with that." So they're going to have a 30-day time period, and when we were walking out the door, they said, "But we are going to want the opportunity to intervene in any case." So we're still debating that issue.

Another issue is who should be the deciding and debarring officials? The deciding official is the one who ultimately decides if there has been a finding of misconduct. The debarring official is the one who says, "You have been bad, and here's the penalty." Because of the NNSA issue, we have a question about who actually will do that. And then, of course, there are a number of other issues involved with NNSA. One of the issues we are grappling with is the long lead time.

Given the diversity of the program offices and other associated performers, we have no DOE policy in place. But the research performers are not operating in a vacuum. For example, we fund a lot of the same people that you guys fund. So what have they done in the absence of a policy from the Department of Energy? They've adopted the National Science Foundation policy. For example, Pacific Northwest National Labs, up in Washington State, adopted the National Science Foundation policy just wholesale. Well, it's completely inappropriate for a national lab, the way they adopted it. But that's the way they are currently handling their policies.

Implementation is raising a lot of issues, and I'll briefly go through them. Amnesty is a real interesting issue. When you come out with a notice to your performers that you are going to have a research misconduct policy, you want to give them time to come clean, right? So our labs and our university performers have not had a requirement, to this point, to tell us that there are instances of misconduct at their institutions. So we expect there is going to be a bubble, that people are going to come up and say, "Well, we're not quite sure if this qualifies as misconduct. We think we actually are in the middle of a misconduct investigation, and we want to tell you about that."

The reason it's a question of amnesty is that there is going to be an absolute requirement that they notify us if they have an allegation of misconduct, using our dollars. So in the absence of that absolute requirement in the paat, we need to give them a time period when they can come clean.

The appeals process is complicated, and we haven't been able to come to a good answer on that. We have an appeals process with our Office of Hearings and Appeals, but whether it's appropriate to research misconduct is another question. Due process. You know, I mentioned the Office of Inspector General clouds the issue. What is the threshold that we're going to investigate? We haven't come to a decision on that. Then there is this whole issue of assurances. We're going to ask our research performers to assure us, like they do NSF and NIH, that they have a research policy in place that's consistent with what we need as an agency; the form that that assurance will take; and what happens if they don't actually have a policy in place after they have assured us that they have one are open questions.

Then, finally, the infrastructure considerations are not inconsiderable. Training was mentioned. We agreed that there needs to be training. But look at the number of performers we have. This is going to require extra staff, and currently we don't have a lot of money for extra staffing, enforcement, investigation, hearings, etc. So, real quickly, that's what we're dealing with, and if you're one of our research performers, get ready for a wild ride.

I would just note that during the past five years, at least, there has only been one serious allegation of research misconduct that the Department of Energy has been involved in, and that was out at Berkeley National Laboratory. We did an informal survey of our national laboratories and said, "If there was a research misconduct policy in place and you had to tell us about cases, how many would you bring to us each year?" And the total out of our labs was about five to 10 cases where they would just notify us that something was happening. But most of them we think wouldn't really rise to the level of research misconduct. They would just give us an advisory that this is happening. I'm not saying that we don't have research misconduct, but I don't think we have had the drivers that NSF and NIH have had in the past to develop such a formal system. We agree it's a good thing to have, but we don't think we're going to have the volume of business that NSF and NIH have had.

One last comment. We're committed to completing rule-making within the next year, max two years, depending on the diversity of the comments that we get. But you can look at the OSTP process as an example. They began this four years ago, and this is not something you do lightly. This is not something you do quickly, because you can make a lot of mistakes. So we are not going to rush into it, and we're going to do it right the first time.

Federal Research Misconduct Policy from the Public Health Service Perspective
by: Chris Pascal
U.S. Public Health Service

The federal agencies should try to have common implementation of the policy where they can, and the Office of Science and Technology Policy (OSTP) is going to help us with that by having an implementation group. Just quickly, I'd like to go through ORI's view of the impact of these changes.

The policy establishes a common definition that should help the institutions and the scientists, because the definition will be applied across disciplines and federal agencies, so there will be one standard to implement at the institutional level. It should increase confidence in the scientific community, because the policy has gone through a public comment process, getting the community's input on the definition. It should also simplify administration by research institutions. If you have six grants from different federal agencies, you will now have a common standard to apply. I think it will increase awareness of misconduct and, possibly, the number of allegations handled by the institutions because the definition and policy will apply to a greater number of research projects across a range of disciplines. The institutions may not see that as positive; but nevertheless, I think that's an impact, and it could be a positive for the agencies, in the sense that it should increase expertise by the institutions in handling allegations.

One of the things that ORI has noticed is, except for a few institutions that have a lot of cases, most institutions don't have enough volume to develop a level of expertise or maintain that level of expertise at the institution, and these are difficult cases to investigate. By broadening the definition and policy to all federal research, it could improve the quality of institutional response. The proposed policies and procedures retain the existing framework for institutions, as the National Science Foundation mentioned. It's basically the same approach as taken previously.

A big point that is not really commonly understood is that the institutions have the authority to have a broader definition of "research misconduct," and to have other policies on other types of misconduct. Frankly, some institutions, even when they don't find federal research misconduct, impose sanctions or discipline the researcher. As the employer of scientists within their institution, they have that authority. That flexibility for institutions is retained under the new structure, and certainly it highlights the need for agencies to work together in interpreting and implementing the federal policy, as was mentioned.

In Health and Human Services (HHS), we have anticipated these changes because we were involved with the OSTP work group and knew what was being proposed. Therefore, we have already made some changes to be consistent with the new policies. The intramural programs in the Public Health Service will now be doing their own investigations. That's new. That's consistent with the structure in the new federal policy - that the intramural program is treated like an extramural institution in the sense that it will have authority for its own investigations.

HHS has now assigned the authority for investigations to the Office of Inspector General. However, ORI is going to rely heavily on the institutions, as we have in the past. Over 95 percent of investigations since 1995 have been done at the institution. So far, ORI has not referred a single case to OIG. ORI will continue to do oversight in order to separate the investigative process from the decision-making process. The Assistant Secretary for Health, for the past several months, has been making decisions on research misconduct. ORI makes a recommendation. He makes the final decision. We have had maybe four or five cases like that since we've instituted this new process. The Departmental Appeals Board will handle the appeals process. That is consistent with the way we have done it previously.

HHS will have new regulations to implement the new federal definition of "misconduct" and the federal policies and procedures that are generally described as guidelines in the federal policy. It will have a separate whistle-blower regulation, which is currently before the Office of Management and Budget for approval. (Subsequently published as an NPRM at 65 Fed. Reg. 70830; November 28, 2000). That's a statutory requirement, so it's being issued as a separate regulation, but it will be part of our overall regulation when that is finalized. ORI plans to put the HHS appeals process into a regulation and update our current regulations and cover the intramural programs. That's all I have on the federal policy.

Authors Note: Below is a quiz presented by ORI on the application of the new Federal Policy and Definition of Misconduct. An answer key is provided following the quiz.

Quiz on Federal Policy and Definition

1. How are "questionable research practices," such as conflicts of interest and authorship disputes, handled under the policy?
2. Is misconduct in human subjects research covered?
3. Is failure to obtain "informed consent" research misconduct? (Multiple Choice)
    a. No, under federal policy
    b. Yes, under some institutional policies
    c. Falsified signature might be misconduct under federal policy
    d. Violations of informed consent are covered under human subjects regulations (OHRP)
    e. All of the above
4. Is omission of data research misconduct?
5. Is use of author's manuscript obtained during peer review of a journal article misconduct?
6. True or False? If an allegation of falsification of data involves federal funding, you must conduct a formal investigation.
7. True or False? Due to retaliation, most whistle-blowers regret having made the allegation.
8. True or False? A substantial number of "bad faith" allegations are made against innocent scientists.
9. When should you take possession of the research data? (Multiple Choice)
    a. When the respondent finishes cleaning out his office.
    b. After the investigation is completed.
    c. When the research integrity officer finishes her golf game.
    d. Immediately after it is determined the allegation deserves an inquiry.
10. Who finds the misconduct investigation extremely difficult? (Multiple Choice)
    a. The whistle-blower.
    b. The respondent.
    c. Witnesses who are asked to testify.
    d. The research integrity officer who manages the investigation.
    e. The support staff who work for the RIO.
    f. Scientists who work on the investigation committee.
    g. Members of the laboratory where the accused is located who worry about their reputation and funding.
    h. All of the above.

Answer Key

1. Not covered; handled by institution or separate process.
2. Yes.
3. E. All of the above.

4. It depends. Need to determine whether a significant departure from the community standard (acceptable scientific practice). Deliberate omission to deceive would be falsification.
5. Yes, it may constitute plagiarism during the "review" of research.
6. False. An inquiry is required, but if there is insufficient evidence that misconduct could have occurred, the institution can close the case after the inquiry is completed and not report it to the federal agency.
7. False. Whistle-blowers report they would make the allegation again.
8. False. ORI receives very few reports of bad faith allegations. An allegation that is not proven or is incorrect is not considered in bad faith.
9. D.
10. H. All of the above.

2000 Sigma Xi Forum Proceedings

New Ethical Challenges in Science and Technology

Educational Resources to Increase Ethical Awareness for Scientists and Engineers

Panel
Introduction
John P. Perhonis, Program Officer
National Science Foundation

John L. Fodor, Executive Director
Educational Media Resources, Inc.

P. Aarne Vesilind, R.L. Rooke Professor of Engineering
Bucknell University

Introduction
by: John P. Perhonis
National Science Foundation

The National Science Foundation (NSF) has been making awards in ethics education for more than 20 years through its Societal Dimensions of Engineering, Science and Technology Program in the Directorate of Social, Behavioral and Economic Sciences. The Ethics and Value Studies component of this program funds research on ethical and value issues that arise in the practice of science in its social context. This link also provides a profile of current NSF Research Experience for Undergraduate awards that have ethics components.

Some NSF awards have trained faculty to incorporate ethics into their science and engineering classes; other awards have resulted in specific products that educate science and engineering students and professionals in ethical issues. This forum breakout session featured two very successful products that have resulted from NSF awards in ethics education. The first is a scenario-based video for classroom use aimed at providing the tools for starting conversations that lead to the development of ethical reasoning skills. The video uses academic integrity as a bridge to understanding professional ethics.

The second is a CD-ROM titled Understanding Computer Ethics produced by Educational Media Resources, a non-profit corporation specializing in educational programs. The CD serves as an effective stand-alone intervention for self-education, and as a pedagogical tool for classroom teaching. The video and CD can be used in both formal and informal educational settings. Both demonstrate the potential of media tools for scientists and engineers in professional development and teaching.

Bioethical Challenges on the Horizon

Panel

The Virtuous Scientist Meets the Human Clone

Robert T. Pennock, Associate Professor, Lyman Briggs School
Michigan State University

Bioethical Challenges on the Horizon in Biomedical Sciences

Lawrence J. Prochaska, Professor, Department of Biochemistry/Molecular Biology
Wright State University School of Medicine

Bioethical Challenges on the Horizon: Environmental Issues

Janice Voltzow, Associate Professor, Department of Biology
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The Virtuous Scientist Meets the Human Clone

by: Robert T. Pennock
Michigan State University

The topic of our session deals with bioethical challenges on the horizon, and we have heard mention of a dozen or more significant ethical issues already. Given that we have just an hour or so remaining, we're only going to have time to solve about six of them, I'm afraid. Well, perhaps that is a bit optimistic, but what I want to do is at least suggest that ethical questions are not something about which we should just throw our hands up in defeat or exasperation. Too often people believe that ethical problems can never be solved. However, while it is not easy, we can make progress. I'm going to discuss one example of something that's on the edge of genetic technological research now that I think most people think of still as an extremely problematic ethical issue but which I think is solvable and that will be solved. I'll argue that it can be done with a little bit of cooperation. But before introducing and discussing the issue, let me make a few preliminary points.

* * *

One common assumption that many people make when they think of moral issues, is that morality just involves telling us what we may not do. Typically, when you begin ethical discussions, people think you are going to be talking in terms of "thou-shalt-nots." If you are thinking of your own research, the reason many of you laughed nervously during the previous talk at questions about animal rights probably is because you all have to worry about what you can't do because of the sorts of regulations that have been imposed because of ethical concerns. But, in fact, ethics as much or more tells you about the things that you *should* do and also that you *may* do. Probably the reason people focus on the thou-shalt-nots is, in part, that common cultural biases lead many to think of morality only in narrow religious terms, but also because there are times that we do need to constrain ourselves.

Sometimes we philosophers do take that to be part of our job—to play the tough cop and point out some of these boundaries. However, if you look in the philosophical literature, you will find the whole range of ethical recommendations. The case I want to discuss here is one where the initial reaction from the public has been highly negative. It is a case about which many intellectual leaders have concluded that morality demands that science go no further, but where I think that if you look at how the arguments work out philosophically, one finds that isn't necessarily so. This is but one case from among many, but I offer it as one that shows how progress can be made on ethical problems if scientists and ethicists cooperate. The case I have in mind is human cloning. What is the virtuous scientist to do about this issue?

When Ian Wilmut and his associates announced the cloning of the sheep Dolly in *Nature* , their dramatic achievement made headlines everywhere. Let's take a look at the way in which the public reacted to the news. It was quick, it was forceful, and it had little to do with sheep. If science could clone a sheep from an adult somatic cell, then what about us?

The initial reaction to this idea came as an almost visceral feeling of repugnance. Even Wilmut himself said of the idea of human cloning, that although there was no reason in principle it couldn't be done, "All of us would find that offensive." Much of the negative reaction involved religious objections. Cloning threatens the "sanctity" of life and "traditional family values," some claimed. Isn't this a case of scientists "playing God," stepping in and usurping powers that don't belong to us? A number of international religious bodies quickly issued statements, saying in the strongest language that there should be no cloning of human beings, that this was an outrage and could never be acceptable. The political reaction was also mostly negative. Just one week after Wilmut's announcement, President Bill Clinton issued an executive order for a moratorium on government-funded research on human cloning, saying "Each human life is unique, born of a miracle that reaches beyond laboratory science," and that "[W]e must respect this profound gift and resist the temptation to replicate ourselves." Such early reactions, and these are typical, expressed the feeling that somehow this would be going to be beyond the pale, that cloning of humans simply could not be countenanced ethically.

There were, however, a few voices that spoke in favor of the idea. They pointed out its possible indirect benefits for medical research and its direct benefit as a new form of assisted reproduction for those who could otherwise not have children genetically related to themselves. Senator Tom Harkin was a lone politician who spoke in favor of human cloning, saying "I think it is right and proper…. It holds untold benefits for humankind in the future." There were even a few religious voices that spoke of the theological tradition that holds that human beings are "co-creators" with God, and pointing out that developing reproductive technology was just another aspect of that creative spark.

The question now is, can science provide the solution to what seem to be insurmountable ethical disagreements? The cover of our program for this conference on *New Ethical Challenges in Science and Technology* depicts a maze—presumably the ethical maze. In the context of our present discussion, this image brings to mind a recent *New Yorker* cartoon that deals with the maze that many feel we are in with regard to genetic engineering generally. The cartoon depicts two scientists in their white lab coats who are lost in a maze. One is holding a leash, which is being tugged by a white lab mouse. Looking resolutely at his colleague, the scientist says, "Genetic engineering got us into this mess, and genetic engineering will get us out of it."

This is the question I want to pose. Is science really capable, by itself, of providing the solution? As scientists, our natural reaction when confronted with a problem is to try to gather data, update or redesign our techniques, and so on. But is there going to be a technical solution to these sorts of ethical issues?

* * *

Let us talk briefly about what science can and can't do. This conference is about scientists confronting ethical challenges, but what you first need to ask is the following: Is there something specifically in your expertise as scientists that gives you the ability to answer those sorts of ethical questions? I want to suggest that there is not. The expertise that you have, qua scientist, is rather specific. Science is the knowledge of scientific method. It gives you techniques—extremely powerful techniques—to go out and answer certain types of empirical questions. Here is a question about the world that we want to investigate. Here is a way to do it. Here is a way to do it carefully so we can actually confirm an answer.

But can scientific method tell you when we have crossed beyond the moral pale? Can scientific method tell you what moral rights and responsibilities there are? You can do a study to tell whether something is an invertebrate, but you can't do an experiment to tell whether invertebrates have rights. What you can do is conduct a survey, using scientific methods to find what people think about that issue. You make sure your survey technique is right so you ask focused questions to find out that certain people have certain views, and you check whether the results are statistically significant, and you can draw a histogram to show what those different moral views are. But that is not the same thing as answering the question, "All right, what should we do?" You have now gotten an expression of a set of opinions, but that is not how you answer normative questions.

There is a logical distinction that ethicists take to be basic. It is the difference between descriptive ethics and normative ethics. In descriptive ethics, you simply report what people think about moral questions; you describe people's moral views. But that is not the main content or even a very large part of the standard ethics course. The content of an ethics course and of ethics as a discipline has to be normative ethics. How do you give good arguments, and how do you rule out bad arguments to make progress towards normative conclusions, prescriptive conclusions, that is, that we should do this, or we shouldn't do that.

The distinction is that between is's and oughts. So, if we are talking about these sorts of issues—questions about knowledge of good and evil, and what we should or shouldn't do—this is a basic point that we have to always remember. When one is making moral judgments, there are always going to be two sorts of components to it. What should I do in this particular case? To answer that question, I am going to have to gather a bunch of facts. And once I have the facts, I will have to think how those relate to moral values. Thus, any sort of ethical decision is going to require both of those components. Where does science fit into this schema? Science can't deal directly with the oughts. Science deals with the is's. It investigates the facts of the physical world. For value inquiry, you have to look elsewhere. Some would say you have to look to religion, but, speaking more generally, the answer is that you have to look to philosophical ethics.

* * *

Now we are ready to confront the human cloning case directly. Let us begin by looking at some of the relevant factual issues that one has to take into account to try to figure out whether using cloning techniques is a morally acceptable act or not with respect to human reproduction.

Some of these factual questions involve technical risk assessment. Scientists will be able to say something about that. When Dolly was successfully cloned, it was not a very safe procedure; Wilmut and his colleagues tried 277 fusions before they got one that actually worked. If you have a technique with that rate of failure applied to human beings, obviously, it is not going to in any sense be morally acceptable. Indeed, when the National Bioethics Advisory Commission recommended a ban on human cloning, the primary reason for their conclusion was the safety issue. So, one of the things scientists contribute to the ethical assessment is the empirical assessment of risks. Scientists can also investigate what might be done to minimize such risks. (It is significant that the Bioethics Commission also recommended that any legal ban should expire after a few years, so that the question would have to be reconsidered, given the expectation that techniques might improve and obviate the ethical objection based on safety concerns.) And scientists can actually do the empirical work to improve techniques. In all of these ways, scientific expertise will apply.

There are other areas, as well, where scientific knowledge is relevant. For instance, the initial negative reaction people had and many of the arguments that they gave against cloning were based upon misconceptions about how the process works. Let me mention one representative example.

In 1996, I was part of a group of scientists and philosophers in a summer institute jointly sponsored by National Science Foundation and National Endowment for the Humanities on the social and ethical implications of the human genome project. During the institute, the movie *Multiplicity* was to be released, and we were all invited to an advance screening because the film-makers thought we would be interested, since it dealt with cloning.

The promotional tag-line for the movie was "Better Living through Cloning" and the setup involves a fellow who is overworked and thinks that it would be great if he could clone himself so he would have more free time. The premise of the scenario is that cloning works rather like photocopying, and it spins this out into a clever plot. Now the protagonist can send his clone to the job site and take off sailing, but then his cloned self tries the same trick. Of course, as with photocopying, with each subsequent cloning, the copy quality decreases, which leads to many humorous problems.

It's a great concept for a movie, but the premise is based upon a complete misunderstanding. When you clone an organism, whether it be a sheep or a person, you do not get another adult version of it. Cloning is not like photocopying. Neither is it resurrection. It would not bring back Hitler or Jesus if one could find a cell from them to clone. Obviously, one should not expect scientific accuracy from Hollywood, but, unfortunately, the movies do often provide the public with their ideas about science. What scientists can do from their expertise to help resolve these issues is explain such facts. How does cloning really work?in fact, not in fiction? Getting rid of some of these factual misconceptions is a critical prerequisite to our moral deliberation.

Science can also explain cloning in familiar terms, such as by pointing out that clones are rather like twins. When you can explain a new technology to people in terms of something else that they already know, that will allay many of the fears they have associated with it. Once the connection between cloning and twining is made clear, people can see that idea that a clone would be a "soulless zombie"—a religious worry that was regularly expressed—is just silly. Once they understand that clones are like twins, most of these irrational fears will disappear.

Science can do more. In the previous talk we heard how when you do cloning and insert nuclear DNA, this does not affect the mitochondrial DNA. There are also other causal factors involved, such as intrauterine factors, and all of these things play a role in embryonic development. So, even setting aside the significant subsequent effects of environment and nurture, it is not even the case that cloning result in an exact genetic replica. Investigating and explaining what actually takes places biologically is one of the things you can do from the point of view of your expertise as scientists.

* * *

Those are examples of what science can contribute to intelligent discourse on the subject at hand. How about contributions from ethics?

Ethicists looked at many of the early arguments made against human cloning and immediately saw they were fallacious. Many kinds of arguments have been discussed and dismissed in other contexts decades ago, and even millennia ago in some cases. For instance, arguments against "playing God" or purportedly "slippery slopes" are still very common, but the vast majority of those can be shown to be ill-conceived, irrational or irrelevant. One of the things you can do on the ethics side is to say, "Wait a minute. Your reasoning about this is poor." Ethicists can help us get rid of some bad ethical arguments.

On the positive side, ethicists can offer a variety of general ethical principles that are relevant to the case at hand. For instance, there is a very well worked out ethical framework for what it is to be an autonomous agent and about the respect we owe to autonomous agents. This goes all the way back to Immanuel Kant's work. One version of his categorical imperative deals with the basic principle of treating others never as means only but as ends in themselves. The fact that someone was born with the help of cloning doesn't mean he or she can be used as just a means. It will be another person. It will have its own autonomy. Thus, the suggestion that people may clone a "spare" copy of themselves to use as a histo-compatible organ bank, should they need a transplant, is absurd.

Also relevant to this argument is the large literature in philosophy on personal identity—what it is to be a person. If you look at this, it is quite obvious that the idea people have that somehow clones would have no individuality or that they could just be used as one wished and wouldn't have rights doesn't make sense. The notion of what it is to be a person and the individual rights that go along with that apples equally to people conceived with the help of cloning technology as it does to twins and to anyone else.

Principles of proportionality also come into play when you are considering innovations that will have costs as well as benefits, which our last speaker mentioned. Again, there is a well developed theoretical framework for thinking about those sorts of issues.

A lot of these questions, in fact, are not really new. They are old questions that have taken on a new form, so one can often readily apply ethical principles and considerations to these sorts of cases that have been discussed and have been very well worked out. When you do that for the case of human cloning, given the sorts of things I've just pointed to, the kinds of arguments that people brought up against human cloning may be seen not to hold. In the end there doesn't seem to be any good moral objection to human cloning once one gets the technical problem solved.

From a moral point of view, the debate over human cloning has much in common with the earlier debate about *in vitro* fertilization. When it first was introduced, IVF was also thought to be immoral, for mostly the same sorts of reasons we have been considering, and very quickly philosophers showed that those arguments were not very good, either. Even the social fears were similar; in both cases, for instance, the argument was made that children born with the help of genetic technology would suffer some social stigma. Today this is put in terms of how "clones" might be regarded, whereas before it was "test tube babies." Such fears faded as IVF became more common. There is no stigma attached to being conceived with the help of IVF; if anything, it demonstrates the loving determination of parents to bear a child. Family values were not undermined by IVF; if anything, they were strengthened. And now we accept and use this technology very broadly. There are some holdouts who still reject IVF, of course, but they do so for the most part because of specific religious beliefs. In general, the moral permissibility of IVF is no longer seriously in question.

I predict that the same shift in attitude will happen, and is now happening, for cloning. If you take a room full of people who have not thought through the question, you will find a large majority who will say that human cloning is morally illegitimate. However, after you take them through the arguments, explaining to them the facts of the matter scientifically, as well as the philosophical arguments and ethical principles, most will very quickly realize that the ethical objections they had are really not sound.

* * *

Let us review what we have learned. We have seen that finding our way out of the ethical maze of new genetic technologies will necessarily require the expertise of both scientists and ethicists. Answering these kinds of questions involves both factual and value considerations, and there will have to be collaboration. It is notoriously difficult for those in the humanities and those in the sciences to talk to one another, but collaborating on these issues is just the place to work to bridge the two cultures. Untangling the ethics of human cloning is a perfect case where you need to have the expertise of both, and as we have seen, by bringing to bear the expertise of each, we have already made considerable progress, finding that there is no compelling ethical reason why cloning should be banned.

We do need to be clear about the scope of our conclusion here, of course. We have seen that, provided the safety problems are solved, ethics does not rule out using cloning as a method of assisted reproduction for human beings. However, this does not imply that scientists should now make this a priority and divert significant financial and intellectual resources to it. One member of the Bioethics Commission, Retaugh Graves Dumas, who was vice provost for health affairs at the University of Michigan, did argue that "It is immoral not to have access to the best technology we could muster" , but this is too broad a principle. Given limited resources, we may not be able to have the best of everything. It is by no means clear that achieving the ability to clone human beings is an important goal relative to other research pursuits. With the problems already caused by over-population, should we pour scarce research funds into a new method of assisted human reproduction before we pursue projects that will help those human beings already living have their basic needs met? And if we do go ahead with cloning, how can we do so in a way that does not exacerbate social injustices? These and other ethical questions will have to be addressed elsewhere.

In conclusion, we may simply affirm that the virtuous scientist takes such ethical issues seriously. The virtuous scientist respects the limits of scientific expertise and collaborates with those who have other relevant sorts of expertise. And it is by virtue of this that we may continue to make small but steady steps towards the resolution of whatever bioethical challenges are yet to come over the horizon.

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Bioethical Challenges on the Horizon in Biomedical Sciences

by: Lawrence J. Prochaska
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Introduction

In today's presentation, I will discuss recent advances in molecular genetics and the effect of these new discoveries on bioethical issues that will present us with new moral challenges both as scientists and laypersons in the near future. I will focus on three topics of research, first giving some scientific background in each area, and then discussing ethical issues that will be created by these new avenues of research.

I have identified three different areas as state-of-the-art technologies that are currently being developed in biomedical sciences and have assessed what ethical issues might be raised in each area. The first area is DNA chip/array technology, which when used on a person's DNA will raise the issue of the individual's right to privacy. The second technology is human and animal cloning, which will create ethical problems of individuality and immortality in humans, and in animals, the morality of harvesting organs for transplantation into humans. And finally, I will discuss the modification of the genome of gametes, which could change the human genome and cause serious bioethical concerns.

DNA Array and Gene Chip Technology

The Human Genome Project, at least in my opinion, has really focused using the large array of genetic information to investigate the states of genes in human diseases. In an overview of how DNA chip technology works, cells from a tissue of interest are grown and the RNA is isolated. The RNA content in the cells at the time of isolation reflects the expression levels of different proteins in these cells. The enzyme, reverse transcriptase, then transcribes the isolated RNA into copy DNA (cDNA). The reaction is carried out so that the cDNA is labeled with a specific marker molecule. The cDNA is then hybridized to known gene sequences on a microchip, and the information from the hybridization is collected by a DNA array machine. You may have read about this process in a recent issue of ***American Scientist*** (1), where this technology was fully discussed.

Essentially, the labeled cDNA fragments are hybridized onto a chip in the DNA array which contains DNA sequences from 10,000 or 20,000 different genes. The fact that the cDNA transcripts made from the original cellular RNA were labeled with a marker molecule allows their detection in the DNA array, so that proteins expressed in the cell can be quantified by the ability of the labeled cDNA to hybridize to the chip. For example, the intensity of the signals from specific, expressed genes can be monitored in a normal patient versus a patient that has a malignant breast cancer in epithelial cells. The data show a dramatic difference in the types of genes that are being expressed in the two patients. In the normal patient, there are uniquely expressed genes and an entirely different set of genes that are up-regulated. But focusing on the malignant cell line, there are many new genes that are being induced by the malignancy. These data show the tremendous impact that the DNA array technology will have on disease diagnosis and treatment.

The human genome is now known and there is intensive, ongoing work that will describe molecular events in the cancer/heart disease process. So one can imagine that an individual might have this kind of scan done on different tissues or, in fact, on any tissue for diagnosis of disease.

One use for the DNA array technology is to identify where specific mutations are occurring within each individual person. And if one thinks about that, the type of information that this technology is going to provide scientists and clinicians is immense. This technique of knowing the exact position of a mutation is called genotyping. Genotyping will allow physicians to diagnose and design treatment of disease. It will also allow gene therapy and facilitate additional discovery and research on disease processes.

Thus, we will have information about the kind of disease processes that will occur in each individual. One ethical concern that I can easily identify is who will have access to the knowledge of the genotype of each individual. This will become a major individual privacy issue. If I go into a clinic and I have a gene array scan on my genome, I'm going to be concerned about who gets that information. Who has access to that gene array data on my genome, and what can be done with that information? As one can imagine, individual privacy is a very important civil rights issue.

On top of that, there's a chance that there could be discrimination against a person who carries the gene for a disease that was identified in the gene array. For the individual, there may also be personal psychosocial concerns upon learning the results of the DNA array test. If a patient finds out that his/her genome dictates that they will be hypercholesterolemic, how will they react? A patient that has a genetic defect may develop some self-stigma. For example, perhaps it is in the patient's genome that he/she might develop Alzheimer's Disease in the future. Does the physician tell the patient that he/she will probably develop Alzheimer's disease? What does that do to his or her self-image?

There are other bioethical concerns that need to be addressed when it comes to the information gathered by the DNA array technique. What type of legal protection against the misuse of test results will be enacted? Can insurance companies or employers use that information against the individual? So, for DNA chip technology, these are some significant examples of future ethical concerns.

Human and Animal Cloning

The second research area that will impact future bioethical concerns is human and animal cloning. The experimental approach for cloning from somatic cells is to isolate the nucleus from a cell of the donor DNA and remove the nucleus from a recipient egg germ cell. This technique was used for Dolly, the sheep. The donor nucleus can be injected into an enucleated recipient egg to restore the DNA in the cell and form a clone of the donor. The clone can pass the transferred gene and other accumulated mutations or alterations of the donor genome to its progeny. Only one organism can be a nuclear donor. For example, a desirable trait in sheep is a heavy coat. The animal that expresses this trait would be a candidate to donate a nucleus from a somatic cell. The donor nucleus could then be injected into a recipient egg from another animal where the nucleus has been removed. Any animal that develops will be a clone of the sheep that produced a heavy coat and can pass that trait onto its progeny. The same approach can be used for organs for future transplantation into humans.

The scientific problem with cloning is that it retains the mitochondrial genome of the recipient individual and, thus, an exact duplicate cannot be made using this experimental approach. The mitochondria of the cell regulate energy metabolism and the health of the individual. Therefore, the clone is not an extract copy of the donor due to the difference in mitochondrial genomes and, thus, different efficiencies of cellular energy metabolism.

So one ethical concern for cloning in humans includes how will this affect individuality of the human species?. We should be concerned about the maintenance of individuality when we discuss cloning. What is the morality of the whole issue of whether humans should be cloned? I think our session chair, Robert Pennock, will address this later. What is the ramification of creation of genetically engineered life on our society? These unresolved issues must be addressed both in the scientific and lay communities.

With animal cloning, one can envision a future where we will harvest organs from animals for human transplantation. Genetic modification of animals will be necessary, so that their organs will be compatible with humans. Will these genetically modified organisms be patented by individuals or corporations? Are patents going to be issued for modified animal genomes and any new genomes that are created? These bioethical issues are just now beginning to be addressed by laws and will need significant legal scrutiny.

Inheritable Genetic (Gamete) Modification

The last new technology that will raise bioethical issues is the use of gamete genetic modification to change the human genome. Chapman and Frankel have recently chaired a group of bioethicists for the American Society for the Advancement of Science discussing this major issue (2). By changing the DNA within a sperm or an egg, an individual's unique genetic characteristics could be modified in any progeny. As it stands now, there is no real mechanism for gene transfer in gametes, but there are laboratories intensively investigating experimental approaches to this problem. Most are using traditional gene transfer techniques; that is, trying to use DNA targeting vectors that recognize certain sequences of DNA and then using the vectors to incorporate a trait into the genome of that individual's gametes. An additional approach to gamete modification is using DNA repair enzymes to correct mutations within the gamete. The goal of both approaches is to treat the genetic basis for diseases such as sickle cell hemoglobin, so that an individual can pass on to its progeny a disease-free genome. The limitation of both approaches is that the mutant nuclear genomic DNA sequences will be corrected, but the mitochondrial genome is unaffected by the techniques, so that any disease induced by changes in the mitochondrial genome cannot be repaired.

The bioethical implications of gamete modification are profound. Are we, as a human race, going to end up commercializing designer traits for our children? Do we want our children to have blue eyes or straight hair or specific physical features? Will anyone who discovers gamete modification be able to commercialize it and profit from it? Once this treatment is implemented, how will this affect the human gene pool?

Other ethical questions arise from the use of gamete modification. What is the impact of being able to change DNA sequences on our long-term survivability as a species? Furthermore, two or three generations down the line after a genetic modification has occurred, how are our grandchildren or great grandchildren going to feel about their family members who actually modified their genome? Future generations lacked consent in the decision-making process of genome modification.

Another ethical question raised is that there may be inequities of access to the therapy, and as such, not everyone is going to be able to receive it. The therapy may not be available to all individuals across the board. Only a limited number of people may have access to this therapy. Is that something we should be worried about? Also, will gene gamete modification reinforce or increase existing discrimination within our society? And then, finally, by doing this kind of modification, what kind of challenges to equality in our society will result?

Concluding Remarks

The new molecular biology techniques discussed today are going to dramatically affect society and medicine in the next five to 10 years. New molecular biological methods will challenge our current bioethical values. Sequencing the human genome will lead to dramatic changes in the treatment of disease, which will include gene therapy. DNA array chip technology, cloning of humans and animals and modification of gametes will raise serious ethical issues. Society will need to address these ethical questions (morally and legally) in depth in the short term future. Dr. Pennock will provide us with a better view about what the status of the ethical and legal debate is at this point.

Acknowledgement
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Bioethical Challenges on the Horizon: Environmental Issues

by: Janice Voltzow
University of Scranton

The organizational hierarchy of nature gives biologists a framework for their research. Beginning at the lowest levels of atoms, molecules and organelles, the hierarchy extends through cells, tissues, organs, organ systems, individual organisms, populations, communities, ecosystems and the biosphere. Many researchers focus on a particular level or levels of organization. But it is the interactions between levels, including positive and negative feedback, that are especially important in environmental issues. For example, there are top-down effects. Spraying a pesticide that kills insects by interfering with molting may also kill shrimp, crabs and other crustaceans because the biochemistry for building the exoskeleton is similar throughout arthropods. Similarly, there are also bottom-up effects. A gene introduced for pest resistance may affect other insects that are not the target pest.

As technology becomes more sophisticated, the interactions between levels will become more complex, and so will the ethical issues arising from that technology. So complex, in fact, that their long-term consequences may well be, as William Wulf pointed out in his plenary address to this forum, impossible to measure. Because we will be capable of (almost) anything, the things we do will potentially have even greater, far-reaching implications. Thus it will be increasingly important to have scientists that are trained across disciplines so that they can integrate across levels of the organizational hierarchy.

One of the most significant issues on the horizon (both figuratively and literally) is global climate change. According to some calculations, we have just had the warmest year on record (Spotts 2000). Hansen et al (1998) estimate that the global temperature is rising by 0.11° Celsius per decade. Other estimates predict an increase of 6 to 11°C over the next 100 years. This warming is due especially to greenhouse gases, produced primarily by burning fossil fuels. The Intergovernmental Panel on Climate Change (IPCC) has produced a strong statement calling for action. At the meeting at The Hague in November 1999 they tried to negotiate the details of the Kyoto Protocol, a treaty to reduce CO2 and other greenhouse gases. As Hansen et al (1998) state "The issue should no longer be whether global warming is occurring, but what is the rate of warming, what is its practical significance, and what should be done about it."

Some of the effects of global warming are easy to predict, at least qualitatively—rising sea level, changes in distributions of organisms that will greatly affect the natural landscape, effects on crop production due to increasing levels of CO2, the spread of "tropical" diseases such as malaria and dengue associated today with developing regions to temperate, developed regions.

One example of the complexity of understanding these effects involves coral bleaching. Over the past 10 to 15 years, researchers have recorded an increasing frequency of patches of white, dead coral on reefs. Most coral polyps contain unicellular dinoflagellates, called zooxanthellae, that live symbiotically in the coral tissue. The bleached corals have lost their zooxanthellae by discharging them, and usually die shortly thereafter. Initially, global warming was blamed as a cause of coral bleaching. It was believed that the bleaching was a response to increased levels of ultra-violet radiation and/or elevated water temperatures. But the situation is not that simple. Bleached polyps of one species of coral that has been studied extensively, *Oculina patagonica*, contain large numbers of a rod-shaped bacterium, *Vibrio shiloi* AK1 (Kushmaro et al. 1996). These bacteria are commonly present in the host coral tissue, but in low numbers. Bleached corals have high numbers, and inoculating healthy coral with the bacterium can cause them to become bleached. Normally, cool winter temperatures, which inhibit adhesion of the bacterium to the coral tissue (Kushmaro et al. 1997), hold the bacterial population in check. Thus, global warming is contributing to bleaching, but not simply because of increasingly higher temperatures; rather, because of lack of cool weather.

A second issue that is rapidly moving to the forefront is the problem of invasive species. Approximately 50,000 introduced and invasive non-native species have entered the United States to date, including purple loosestrife, zebra mussels, Formosan termites, Asian swamp eels and an unknown number of microorganisms in ballast water (Robichaux 2000, Ruiz et al. 2000). Invasive species are expensive for the environment and for the economy. Non-native species are blamed for almost half of the species listed as endangered or threatened. Documenting and controlling these invaders cost the U.S. an estimated $138 billion annually (Robichaux 2000). A new National Invasive Species Council was charged with developing a National Invasive Species Plan to deal with these organisms. A major question on the horizon will be: Which is worse, the spread of the invader or the cost of eliminating it?

A third bioethical challenge on the horizon is space. With the arrival of Russian and American astronauts on the International Space Station, we have entered an era of permanent human occupancy of space. NASA official Jim Van Laak hopes this marks the beginning of "at least 15 years of continuous human presence in space" (Leary 2000). Issues such as mining other planets, waste disposal and international jurisdiction have barely been addressed, much less resolved. As evidence rises that microbes might survive interplanetary travel (McFarling 2000), we run the risk of ballast-borne interplanetary invasive species. The consequences may lead to *Silent Spring*meets *Silent Running*. What will happen when (and if) we discover other life forms or they discover us?

What's on the horizon? A new level in the organizational hierarchy of nature—space.

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Some New Wrinkles on Faculty Conflicts of Interest in Research

Panel
New Wrinkles on Faculty Conflicts of Interest in Research
Paul A. Fleury, Dean of Engineering
Yale University, formerly at University of New Mexico

National Laboratory Perspective
Patricia L. Oddone, Executive Assistant to the Director
Lawrence Berkeley National Laboratory

Kumar Patel, Professor of Physics and Astronomy
University of California at Los Angeles

New Wrinkles on Faculty Conflicts of Interest in Research

by: Paul Fleury
Yale University, formerly at University of New Mexico

To set the stage for this panel discussion on faculty conflict of interest in research, I would like to emphasize the importance of this topic and why it is providing new challenges to universities. These issues will only increase in their complexity as the research engine that has fueled our technology since the Second World War will be based increasingly in research universities. The research university is a relatively new phenomenon dating back only to the early 1950s. It has proven a very wise investment intellectually, socially and economically for our country, and it is up to us to make sure that such complications arising from conflicts of interest do not jeopardize it in the future.

To begin with we must broaden the definition and understanding of issues related to conflict of interest in the conduct and support of research. The traditional theme of research ethics has centered around issues of falsification, fabrication and plagiarism, which tends to focus on individual execution and honesty of the researcher. The changing landscape for university research over the past several years has brought new complexities to the way that researchers must address ethical questions. Generally it appears that the tools required are often lacking within the university system for faculty to properly address these issues.

Much of the new challenge arises from the growing pressure for outcomes-oriented research both from funding agencies and universities themselves. This involves a thrust toward commercialization of inventions or discoveries, which involves the university, both institutionally and through individual faculty members, in the technology transfer process. Questions of intellectual property ownership, methods of rewards and incentives, etc. give rise to new aspects of faculty conflict of interest . For example, it has led to a focus on the expanded notion of "conflict of commitment" that in many instances has highlighted the difference between policies on consulting as stated in the faculty handbook on the one hand and the university's intellectual property policy on the other.

The Bayh-Dole Act of 1980 gave federally funded research inventors and their institutions rights to commercialize their advances. Together with the increasing congressional demand for accountability in research, this has expanded the focus on what I call "outcomes driven research" as an ever-larger component in the university research portfolio. Indeed, today calls for proposals not only from the mission agencies, but even from the National Science Foundation itself, often require identifying areas of likely application, industrial partners in the research and either in-kind or actual dollar matches to the federal funds supporting these grants. Consequently, the weight in the selection process to determine which projects are funded has been shifting away from scientific impact and potential for breakthrough discovery, increasingly toward potential for eventual commercial application.

As industry supported in-house research has decreased, industry has turned increasingly to universities to meet at least the front end of their R&D needs. As the United States lags behind other developed countries in its investment in civilian R&D, several studies [including the 1997 "Endless Frontiers -Limited Resources" Report by the Council on Competitiveness] have pointed out the need to do ‘more with less' in the research and development arena. The key idea that emerges from these deliberations is that partnering is the way to go. Partnerships increasingly involve players not only from different disciplines, but from different institutions and even different types of institutions. In general, it may be a very good idea to use industry investment to help leverage taxpayer funded research, but at the same time this approach complicates the conflict of interest landscape for both faculty and students. These partnerships often involve imposition of deliverables or requirements that influence the direction of and areas in which the research is done.

Many universities are seeking additional revenues through such partnerships and through the revenues that might arise eventually from commercialization of the resulting intellectual property. Given the increased complexities of this research environment, even those faculty whose hearts are pure and whose minds are clear still find that there are too many pitfalls to navigate successfully. Consequently they face the possibility that within several years they may themselves be subjects of audits or possibly even litigation. University technology transfer offices have been formed throughout the country to manage and develop intellectual property generated by the faculty. But economics alone does not justify the magnitude of this effort. There are scarcely a dozen universities nationwide whose revenues from such activities exceed $10 million annually. Indeed, over the top 100 research universities the direct results of technology commercialization amount to only about 2 percent of their total research funding.

There are, however, other advantages such as providing avenues for faculty and students to exercise their entrepreneurial capabilities, building bridges to industry and contributing to the economic development of the region in which the university is located. While these are worthy goals of technology commercialization, unclear guidelines that presently exist in most universities for faculty behavior are causing substantial present and potential future problems. The mix of private and public funding in support of faculty research activity causes problems not only of ownership, but of potential exploitation of students and diversion of research directions into avenues which might be influenced by the prospect for financial gain on the part of the university or the faculty.

In this panel, experts from a major research university and a fundamentally oriented national laboratory will discuss these and related issues facing the university research community. We hope that the audience will participate in the discussion of the points that they raise. Let me end this preamble with a couple of suggestions for what I believe universities need to do. First they must develop a clear and fully disseminated intellectual property policy. This must contain an exploration and understanding of the consequences? Many potential unintended consequences? That the policy might lead to. Second, in the case of a university affiliated technology transfer organization, the mission objective must be clearly understood by both the technology transfer group and the faculty. Whether the goal is to enhance local economic development, to maximize revenue stream for the university or to provide entrepreneurial opportunities for faculty and students, it must be made clear at the outset. Following from these general principles there should then be generated more specific rules governing the participation of faculty and even students in consulting for companies in which they may have a financial interest and for reporting potential conflicts of interest in both the case of government funded and industry funded research.

Let us now hear from our experts on the panel.

National Laboratory Perspective
by: Patricia L. Oddone
Lawrence Berkeley National Laboratory

We've heard a lot about the pressures and the rules facing researchers. I would like to focus on the responsibility of researchers funded by taxpayer dollars to spend money wisely and ethically, and what has happened in a place like the Lawrence Berkeley National Laboratory when this has not been the case. Yesterday, you heard from representatives of the Department of Energy nuclear weapons laboratories, Sandia, Livermore and Los Alamos.

There are 10 other DOE national laboratories that do not work on weapons, and Berkeley is one, located on University of California land next to the Berkeley campus. We have a lot of rules and regulations, being managed by the University of California for the Department of Energy. Nevertheless, the view of our director is that right and wrong are still pretty clear. He comes from Bell Labs, as do my other two panel members, and he is troubled by what he describes as an "entitlement mentality" that comes with federal funding.

I want to describe three cases, all occurring in the past two years, to illustrate the kinds of conflicts that we deal with at Berkeley Lab. The first concerns a violation of copyright policies, the second a violation of outside business rules, and the third an example of scientific misconduct. In the first case, the Department of Energy became suspicious about a particularly successful book containing data, funded by the Department of Energy, at our lab. A researcher, who for several years had been responsible for compiling the database, had also contracted with a book publisher on the same data. Under lab policy, the copyright in such a case would be owned by the laboratory; the researcher would be entitled to 35 percent of the royalties and the laboratory would keep the rest. This is the same as for University of California policy on patents, as well as copyrights. In this case, the researcher, who was a long-time lab employee, claimed that he had created the data for the book on his own time, even though it's exactly the same thing. And, in any event, that he had been authorized by the lab to sign the contract. This was, in fact, the case for earlier editions when the book was not successful; but later, he didn't follow through.

From the start of the investigation, it was definitely adversarial. The researcher was uncooperative, refusing to supply information and speaking through an attorney. Ultimately, the laboratory did not take legal action against him because the amount of disputed royalties was smaller than we thought. There was also a statute of limitations, which meant we couldn't get royalties from back years, and there were some lab policy ambiguities that needed to be cleared up. The reaction of the researcher was one of righteous indignation and triumph. Since he got away with it, what he did was right. The lab director, nevertheless, sees it as a clear case of someone taking the government's money and profiting from it.

Okay, second example. A principal investigator hired someone to work four days at the lab and the fifth day on his personal business, which was being run out of the lab on lab equipment. Obviously, we didn't know this at the time of hiring. The subordinate barely spoke English and feared that he would lose his immigration status if he complained. Some years went by, but eventually, he did make a complaint, both because he wasn't getting proper credit for his published work and because the fifth day was really taking all weekend, a really terrible situation. It was found that the principal investigator had violated the lab's outside business policy relating to conflict of interest. He had failed to separate lab and private interests, competed with current and proposed lab projects and engaged in the outside business using lab resources and equipment. He admitted only violating the lab's patent agreement. In the end, the principal investigator was disciplined, but not fired, because of, again, various ambiguities in the rules, which made the lab's legal team cautious.

The most interesting case is the final one. The particulars may sound familiar, since the *New York Times* covered this story extensively. This is the case of a researcher who eventually left the lab following a finding of scientific misconduct. The hook for the press was the research area, which was out on the fringe, indicating that power lines may cause breast cancer. Originally, the scientific misconduct charge was brought by a demoralized graduate student, who thought that the data were being manipulated. It took years, literally, during which the lab carefully reviewed the charges, and found that the data were indeed being falsified. Both the Department of Energy and the National Cancer Institute had funded the research. I should add here that while the review was under way, the lab director received protests on behalf of the researcher, one from a prominent UC faculty member and another from a program manager in the Department of Energy, who controls the lab's funding, asking that we not stop funding the work.

After scientific misconduct was found, the lab reported this finding to the federal Office of Research Integrity, which conducted its own investigation and confirmed the finding. Both the lab and the ORI disciplined the researcher. The researcher entered into a "voluntary exclusion agreement" as part of his punishment, in which he neither admitted nor denied misconduct, but agreed not to seek government funds for a three-year period. This fact was released publicly and attracted press attention, particularly when the researcher himself began giving interviews on the subject because, of course, he felt vindicated. He didn't have to admit or deny to the change of misconduct; he just had to give up funding for three years.

After the *New York Times* reported this story, it wasn't too long before the drumbeat began about the government getting its money back. William Safire devoted a column to the subject and erroneously said that a whistle-blower had alerted the Office of Research Integrity, when, as I mentioned earlier, the lab had initiated its own investigation and forwarded the result to the ORI. In due course, the National Institutes of Health told the lab that it wanted its money back. We had to go to our contractor, the University of California, to cover legal costs, since one government agency can't sue another. The University's response? "You just ought to pay up." The amount was somewhere under one million dollars. Fortunately, the NIH dropped its request a few weeks later.

Clearly multiple conflicts of interest were present here. The researcher has rights. We had to do our scientific misconduct investigation very, very carefully. The DOE and the NIH funded the research. The press and the public want to know about research related to power lines and breast cancer, and members of Congress, along with the Secretary of Energy, want to know what's going on, too. The IG investigation affirmed everything that Berkeley Lab had done, and even criticized its own agency's lack of policies on scientific misconduct. Of course, what makes news is the fact that an investigation has been launched, not the later findings that confirm everything you have done.

A chilling recommendation from our point of view, however, was that all current management contracts, such as those between the Department of Energy and the University of California for Berkeley, Los Alamos, and Livermore, require the DOE to recover research funds when scientific misconduct is found. Think about the incentive here for proceeding with a lengthy inquiry into such charges. In short, the laboratory was rebuked--by the public, the press, the DOE, the NIH, and even the University of California I am embarrassed to say--certainly not praised, for persevering in this very complex case.

In all three cases, individuals flouted authority and got away with it. The primary recourse we have is to tighten the rules and the policies. In each of these, the director intended that our laboratory would find the truth and do the right thing, no matter what the pressures were against this. He believes that stronger punishment should have been given in all three cases.

The perception of the researcher, in these cases, was that he was entitled to use federal funds as he wished, and to profit personally or to enhance his program and reputation if he could. We have employment protections that can make it hard to establish right and wrong, which leads to the sense, in the view of people who are not ethical, that "it's ethical if you can get away with it." One certainly does not want to abrogate federally-funded individuals' rights, but these outcomes are not right either, and at Berkeley Lab, we're continuing to struggle with such issues. I look forward to the discussion to see how others might suggest that we handle them.

Intergenerational Ethics

Panel
A Look at Nature's Numbers
John H. Gibbons, Senior Fellow
National Academy of Engineering

Intergenerational Ethics in the Knowledge Age
Thomas Malone, Former Foreign Secretary
National Academy of Sciences

A Look at Nature's Numbers

by: John Gibbons
National Academy of Engineering

To start off, I'd like to draw a couple of verbal images. "Eat, drink and be merry, for tomorrow you may die." That's from the *Rubaiyat of Omar Khayyam*. Now, here's one I heard during the Reagan administration when I asked a person about some future issues, and he said, "Why should I worry about the future? What's the future done for me?" Sort of a modern *Rubaiyat*, I guess.

Another image is the true story of some Russian scientists who were in charge of a seed collection in an institute in St. Petersburg during World War II. St. Petersburg, as you know, was under an extraordinary siege. People were starving everywhere. They even tore up the floorboards of the local mill in order to try to get some of the flour from between the floorboards so they could eat. But these were custodians of a rather extensive seed collection from all the genomes across Russia. They guarded those seeds with their lives. In fact, several of them starved to death. But they saved the seeds, which survived the war and are part of the precious heritage of Russia today. That gives a little different feeling about why should we care about tomorrow.

We live in an age of discount rates. I think we all pretty much know how to calculate the net present value of future things. We depreciate buildings and other things that decay and in a sense go to zero value at some point, but we also seem to be willing to use some of those same principles for evaluations of things like biological species for which it's very difficult to conceptualize how you can depreciate them to zero over a period of time. We live in a time in which a typical corporate manager has to worry about, not next year's profit, but next quarter's! So we operate in a time of extraordinarily high discount rates in terms of the present value of future conditions.

Here's another vignette. When archeologists excavated in Russell Cave, Alabama, and found some of the earliest artifacts of human presence in North America, they purposefully left untouched a major portion of that cave in which surely lie some very important artifacts. They left it alone for future generations because they knew that technology would likely advance over the years and that a much better excavation could be done 50 or 100 years down the road. A different kind of sense of discount rates and preparation for the future.

We have, within the last 10 or 20 years, begun to think very seriously about such things as natural capital. There's a recent Academy publication called*Nature's Numbers* (authored by some distinguished economists) in which we're now beginning to wrestle with the fact that there are goods and services in our economy, the value of which have never been incorporated into our national economic accounts (our way of accounting goods and services delivered to people). These are so-called "natural capital" accounts, such as the natural environment that cleans water, that provides fertilization of crops, all the other so-called services provided by natural ecosystems. We're now in the process of trying to figure out how we can link them into our economic reporting rather than leaving them outside the systems of national accounts,. Even rough measures tell us that a very substantial portion of our wealth comes from outside our economic system as traditionally calculated.

So we're in the middle of a very interesting transition of realizing the sources of wealth and our responsibilities to the future for not destroying that wealth without at least putting something in place of it. It has been brought to the forefront by a man/biosphere crisis that has emerged in the 20th century as a result of rapid population growth and rapid industrialization, and it is on a collision course in the 21st century. There are clear mandates, it seems to me, for us to understand this business and think again about what the stewardship responsibilities are for humans.

These issues are presently being ignored by the public in general, by business, by politicians—where political lifetimes are very short. You know, when a congressman gets elected, he must immediately start campaigning for the next election. President Clinton told me once when we were working on a climate protection protocol, and I argued for a 20- to 30-year time horizon, "You're absolutely right about the need for a long time horizon, but no number greater than 10 years has any consequence in politics. The discount rate wipes you out."

Let me talk just for a few minutes about things that exemplify the dynamics of stress on the biosphere, all of which relate to science and technology and all of which have the common property of moving slowly but very ponderously, and which require us to think ahead or otherwise we're too late. …A demographic profile of the United States: male, female, age groups and numbers of people, is more or less a rectangle. There's a bulge due to baby boomers, but we're on almost a stable rectangle of population distribution by age in our country. That's typical of a mature industrial society, which is about 5 percent of the world's population. The same demographic profile for Mexico shows an enormous number of very young people in the society. It sort of looks like the U.S. a century ago.

Now, the good news here is, if you have a profile like Mexico and you wonder who is going to take care of you in your old age, you can be pretty sure that some of your children can take care of you. At the same time, when you look at the enormous bulge of youth, these people are going to move into the labor market, and there's an enormous requirement, then, to provide for an economy that can support such a population. A 3 percent growth rate in our economy seems to be a small number these days, doesn't it? But what does a 3 percent growth mean in population? How soon does population double if it's growing at 3 percent? The number is about 23 years. And when you begin to go through a few doubling times, you understand the consequence of that kind of rapid growth.

Human demographic profiles only change very slowly. If we suddenly went to balanced birth rates and death rates in a given country, it would take 70 years for population to equilibrate because there's so much momentum in the system. Example, Africa. Three scenarios: 1980 to about 2100. The two scenarios are that the birth rate decreases from its present high number down to replacement level in either 25 years from now or about 55 years from now. I think that's the number. In other words, a delay of the decrease of birth rates down to replacement level of not 30 years, but up to about 60 years means a difference in the ultimate population of that part of the world of between about 1 billion people and about 4 billion people. In demographics delay can be devastating.

For people who are not familiar with numbers, these statistics don't carry much weight. I think Lord Bertrand Russell once said, "Mankind would rather commit suicide than learn arithmetic." And it seems to me our research community must not only know arithmetic, but also try to get it across to other people. If you take these differential numbers and go to the integral, namely world population, you get a figure like this. The dynamics are such that it's just in the 20th and 21st centuries that we kind of have a moment of truth.

What happens as we go from the year 2000 to, say, the year 2100 is that almost all the population growth, about 90 percent, is going to occur in the Third World. One of the many requirements of managing such a population growth is that each and every year we will have to build the equivalent of about eight cities of 10 million people each to accommodate the increased numbers of people, all of them in the Third World. So the implications of these long-term events, which happen so slowly in terms of most of our thinking that it just doesn't seem to matter—the implications are extraordinary. And, again, that's why I call the 21st century a century-long moment of truth.

I'm going to go to my second example. I think you have all seen this in one form or another, but long-term trends in carbon dioxide and average global temperature over the last 150,000 years show very similar downturns in both CO2 concentration and average temperature until the end of the Pleistocene period, about 20,000 years ago. At the end of the Pleistocene, there's a sudden, erratic rise in temperature and CO2. And, now, after nearly 20 millennia of stable levels of carbon dioxide, which is the most important of the greenhouse gasses, we are moving to levels of carbon dioxide that are above levels seen in the past half million years. What's more, the momentum carries us on toward a domain that the world has never seen before. Concerned? Why should I worry about tomorrow? It does give someone pause if you are sensitive to numbers.

If we look at that projection in a shorter time frame, that is over the past century we see, again, carbon dioxide global and global average temperature riding along in an erratic out rising pattern. Weather is inherently a variable phenomenon. But as we project toward the future using various scenarios of how we use energy, we find a plausible range of CO2 concentration over the next 50 or 100 years here and a plausible range of temperature response like this.

Uncertainties about future climate are due to the uncertainty about how the models work, how well we understand the net result of changes in the atmosphere. The best we can do at this point is something like this: If we don't do anything, our carbon emissions to the atmosphere will probably rise in nearly exponential form.

Let's just talk about the next century, the year 2100. If we come to a conclusion that, for humanity's sake, in the long term we will be in big trouble if we more than double the pre-industrial CO2 concentration in the atmosphere and we want to hold the concentration to that level in the long term but we want to get there in a way that doesn't break us in terms of the economy, then we have to choose a so-called "least cost" strategy to get there. To do that, we have to begin early in the 21st century to depart significantly from our recent trajectory, and ultimately, within perhaps 30, 40 years, 50 years, begin to decrease absolute quantities of carbon emitted per year around the globe.

The bottom line for the U.S. is, because we are one of the major contributors to this business, and at the same time are wealthy and technologically sophisticated, that we need to move our energy system from a carbon intensive fuel to a different system that, if it uses fossil fuels, somehow sequesters carbon, or we have to move to other energy systems that don't release carbon. We have only decades, perhaps a hundred years, to effect that transition. If you want to end up with about 550 parts per million CO2, which is twice the pre-industrial age concentration, you have to follow a trajectory like this if you want to do it in a reasonable way.

Looking at history, we've been there before. This is the sequence of energy systems that we've seen ever since the middle 1800s. The cycles are about 50 years apart. They're about a hundred years full width at half maximum. We're pretty beyond the age of coal donimance; although, it's still significant. We peaked out in U.S. oil production well over a decade ago. Gas is still on the upswing. And what will be the next major source ? There has to be a succession; and what would the succession be? We're not sure yet, but it's a challenge to our community in this part of this century to devise a diverse energy system because energy drives the world. And, again, it takes time to do it.

In fact, intentional or not, we've been reducing carbon emissions from energy for a long time. We've worked from wood to coal to oil. This is a semilog plot of the ratio of hydrogen to carbon per unit of energy, and we've moved on up to oil. We're on our way to a methane economy, and we might be able to get there by 2030 or so, within the lifetime of some of us. But at that point in time, we have to do better than methane. We have to move to hydrogen-to-carbon ratios that carry us into another domain. It's going to require very innovative science to take us to the so-called hydrogen economy. That's a wonderful challenge, but it certainly is a challenge.

Let me touch on a few more numbers: global nitrogen fixation. Even up to 1960, anthropogenic fixation of nitrogen, that is from fertilizer production and combustion, was perhaps a third or less of the global amount of nitrogen fixing from natural sources such as lightning storms and other things that happen, microbes. In that short 40 years, we've gone from being a minor producer of the total to the dominant player of the total global nitrogen fixation. Result: we now have hypoxic zones not only in the Black Sea, but in the Gulf of Mexico. We have air pollution problems. We have many ill effects related to nitrogen fixation, and there is no sign of that domination turning around. We don't even have a clear notion about what it really means for global ecosystem stabilities.

There are good ideas about how we might go to a much less intensive use of nitrogen fertilizers, but it all comes back home to the science community to have a sense of what is happening and, therefore, an ability to think ahead and create the capabilities to do something about it.

Finally, a time series plot of species extinctions from the 17th century to about 1960 shows that the absolute numbers are relatively small, but extinction is following a very rapid exponential increase. It's getting more and more worrisome that species extinction is going on at a rate that, over a period of several hundred years, will be fully equivalent to, if not greater than, the impact of the asteroid collision 65 million years ago in terms of the impact on global species' survivability. In other words, humanity constitutes a "human bolide" or asteroid colliding with the earth's biosphere over perhaps a couple of centuries, that's equivalent to one of those asteroids in terms of species devastation.

Now, to tease your imagination a little bit, let me say a word or two about what's happening in terms of the so-called dematerialization of our economies. The old idea is we select raw materials and transform them into final products, which finally go to discarded waste. The newer ideas that are taking shape now are to move toward closed systems in our "green" design of products so that, at the end, the materials have valuable follow-on uses, and there is very little net flow of materials. It's a great challenge to the engineering community to think about choice of materials, not just so much in terms of what will make a good product, but what also will enable the whole system to operate in a nearly closed condition. I would also say that it's an ethical imperative for our community to take into consideration.

We are just now coming to grips with the notion of moving from a world in which human activities were once washed out on the sands of the environment—to the point it's now a permanent footprint. There's almost no "natural" world left. It is a human dominated biosphere, and the way we're moving gives me great cause for concern about consequences even 100 years in the future, which is but a moment in human history.

So, if you think back on it, we have lived for generations with several paradigms that may have been okay sometime back but are now anachronistic. One paradigm is: "The exponential is our friend; we can float our way up and out of these problems." Herb Stein, the noted economist, once observed, "That which cannot go on forever must at some point come to an end." Departing from the exponential is easier said than done.

So, comes the argument, for instance, of alternative growth models, which enable you to produce more goods with less externalities over time, but soon the exponential catches up with you. An alternative idea is to move towards an S type curve, which ultimately happens whether it's in a petri dish or on the planet, to move toward some kind of dynamic equilibrium. The evidence shows there is some response to this dilemma. It's not all "woe is me." We are beginning slowdown in population growth; although, you can hardly notice it yet. We have energy-to-GNP ratios that are falling. In the U.S., it has fallen by some 40 percent over the last 40 years in the face of continued economic growth. And, in fact, most of that gain in energy efficiency has turned out to be profitable at the bottom line. We have, with the stratospheric ozone, an international agreement and treaty and protocol. We've devised technological ways to fix that problem, and within about 50 years or less we'll begin to see stratospheric ozone return toward normal.

We have visions of global climate change mitigation in the International Panel on Climate Change, with more than 100 countries agreeing scientifically on the effects and maybe what one can do about it, what the implications are. We're protecting more and more natural areas and historic sites. We do have rising concerns for the long term. We have, for instance, people like John Ahearne, who have spent a lot of time worrying about the next 10,000 years with regard to management and development of high-level radioactive waste. Why would someone worry about 10,000 years? There hopefully will be people then, and we need to think about those people. I only wish we also thought about them in terms of 200 years ahead in regard to other things we're doing.

Ken Bowling—I believe he was a member of Sigma Xi—used to call our economy the Cowboy Economy because the idea is that if you have a problem, you simply pick up and move west. The problem is we've run into the Pacific Ocean. We now have people who are saying to rely on God to intervene and bail us out. Francisco Ayala was talking about that earlier this morning. We have severe resistance to the notion of limits and restraints. The notion that somehow man is set apart from the rest of creation and all the rest of creation is simply meant to pleasure us is coming apart because dualism leads you to the notion that, the more people we have, the more we're fulfilled, and if we overcrowd the earth, we will simply expand to some other planet. Crazy notions, I know, but they still influence a lot of people.

There's also resistance to technological innovation in molecular biology and the use of recombinant DNA to devise ways to enhance our capability for improvement of crop species and the like. That resistance is, in part, due to the fact that the public doesn't trust our community in those areas. It's getting too close to home. There is resistance to nuclear power because of radioactivity, however small, by a lot of people that don't understand that they already have a lot of radioactivity in their bodies, mostly is due to K40, which was around when the earth was formed.

So there are a lot of things going on, and, again, our community has a primary opportunity and, therefore, a responsibility to help set the numbers right, to help raise people's awareness of what our options are and what our options could be. So the sine qua non is, when we figure where do we go from here, it's knowledge. A knowledge century, it seems to me, is in the cards, and the science and technology, engineering and mathematics community bears an extraordinary, inordinate amount of the responsibility here because it's our profession that provides the tools, first, to have the ability to foresee, to monitor, to sense, to analyze, to model, to understand earth's systems and understand population dynamics.

There's a Chinese proverb that says, "If we do not change our direction, we're very likely to end up where we're headed." If you can develop the ability to foresee, to understand trends and monitor where we are, then we have a better capability to understand how and when to act. So to *foresee* is one of our commandments. A second is to *forestall* degradation; to devise ways to mitigate the results of human activities; to devise ways to adapt to change that's inevitably upon us; to move toward a dematerialization of the industrial system while still providing goods and services.

Albert Schweitzer, just before he died, said, "Mankind has lost its ability to foresee and therefore to forestall. He will end up destroying the earth." That's a pretty tough statement from one of the great optimists and scholars of the world, but to *foresee* and also to *forestall* are two of the keys for us. Third is an ability to *restore*. If we are smart enough to understand where something is and where it's going, we need to make an investment in it. I think our present wrestling with restoring the Everglades is a good example where we made some mistakes, and we're backing up and spending a lot time in south Florida to enable people in Florida to have fresh water and other amenities. And finally, to help *nourish*, help provide wealth that goes beyond creature comforts, things that are uniquely human. Rene DuBois once said, "Mankind has unique needs for such things as quietness, open space, solitude, natural places to be." These are uniquely needs of human beings.

What we need is research and education. It's a burden. It's also an opportunity. Mainly, and if not predominantly, that burden and opportunity is on the science, engineering and technology community, not just for today's needs but to enable the future to unfold in a way that we would like to see it happen. Saint-Exupéry once said, "Your task is not so much to predict the future, but to enable it." That requires understanding where we're headed and anticipating that process.

I invited Donella Meadows to join us for this forum today. She said, "That sounds very interesting. I'd like to come. Where is it?" I said, "Well, it's out in Albuquerque." She thought for a moment, and she said, "I can't do it. The required travel would exceed my personal carbon budget." She's committed herself at the personal level regarding net carbon production and how it's used. I told her we'd miss her, but we understood and were proud of her.

There was a man named Harry Caudill, an extraordinary Kentucky lawyer, politician and philosopher, who wrote a book called *Night Comes to the Cumberlands*. He described the devastation caused by deforesting the hills of Kentucky and taking the coal out of the ground and the impact of technology when large machines came in and what used to be a mule and a skid and a man with a shovel transformed into giant drag lines and massive machinery. And he lamented this, but he said this resource extraction is bringing wealth to Kentucky; although, he said, most of it is flowing to Philadelphia where people own the companies. But he pleaded, in turn, that as we deplete these resources, we need to supplement them with other resources. His suggestion was separation fees which would go into education and mandatory restoration of the disturbed lands. A substitution, in other words, of something else for the things we take away in our generation. I think that's the bottom line for us in our generation, and I think that's why this is an ethical imperative to our community.

Intergenerational Ethics in the Knowledge Age
by: Thomas Malone
Former Foreign Secretary, National Academy of Sciences

As my contribution to this interesting topic, I would like to touch briefly on four items. First, the broad context into which intergenerational equity falls, that is, the grand challenge to society on the threshold of the 21st century. This challenge involves the primary forces that drive human development while simultaneously threatening environmental sustainability and economic equability. Second, a closer look at the equability issue itself. Third, an emerging hypothesis on the transformation of society under way towards a knowledge-driven economy. Fourth, a proposal to test that hypothesis.

The central challenge of this century is to achieve reconciliation between exponential and asymmetrical growth in human activity on planet earth and the fixed resources of land, air, water, plant and animal life in the world's ecosystems that support that expansion. This challenge involves developing an understanding of the complex interaction between the global human system and the array of natural systems that support human activity. This reconciliation *is* the grand challenge. The facts are simple: during the 20th century, the population of the world multiplied four times and the average capacity of each individual to generate goods and services from these natural resources increased three-and-a-half times. The global economy, then, grew 14 times — to about $28 trillion.

The world economy is likely to grow another four to five times during the next 50 years (annual rate of three per cent). This growth would be the result of, say, a 50 percent increase in the number of people and a three-fold increase in the average economic productivity of individuals. Another four- to five-fold growth from the present $28 trillion world economy would probably be devastating to the global ecosystems that are already in trouble, according to the recently completed Pilot Assessment of Global Ecosystems (PAGE). These numbers briefly encapsulate the issues of environmental sustainability. When we think about a possible collapse of these ecosystems, we realize that society has some formidable problems to address as well as some attractive opportunities to seize.

Now on to economic equability. This issue is inextricably intertwined with environmental sustainability. David Landes, an economic historian at Harvard, noted in his monumental *The Wealth and Poverty of Nations* that: "The gap in wealth and health that separates rich and poor

. . . is the greatest single problem and danger facing the world of the Third Millennium. The only other worry that comes close to this is environmental deterioration, and the two are intimately connected, indeed are one."

Let us examine a few numbers in *Human Development 2000*, prepared by the United Nations Development Programme. That report shows a widening economic gap between the billion people in the 29 OECD countries and the 582 million individuals in the 43 least-developed countries of the world. That gap is ominous. The per capita production of goods and services each year in the OECD countries is $21,000; in the least-developed countries that number is $270 (less than a dollar a day). In short, the average capacity of individuals in OECD countries to produce goods and services is 78 times greater than it is in the least-developed countries. The gross national product in the OECD countries is $23 trillion; it is less than a trillion dollars in the least-developed countries.

A business-as-usual scenario for 2050 would lead to a population of almost 1.4 billion in OECD countries and 1.8 billion in the least developed countries. The ratio of the per capita production of goods and services between the two groups for this scenario would increase from 78 to 107. With reference to Landes' coupling of the gap in health with the gap in wealth, it is worth noting that the life expectancy in industrial countries is nearly 50 per higher than it is in the least-developed countries. There are eight times more doctors per 100,000 people in industrial countries than there are in the least-developed countries.

One (and only one of many) alternative to a business-as-usual scenario would be to reduce the rate of population growth by one-half everywhere and contain the annual growth of individual economic productivity in the OECD countries to 1.0 percent per year (from its current 1.5 per cent), while increasing it in the least-developed countries from its present 0.9 per cent to 7.1 per cent (the 20-year average figure for Korea, China, Mongolia and the Eastern Asian countries). In this scenario, the present ratio of 78 for individual economic productivity between the two groups of countries would be reduced to three. Living standards in the OECD countries, as measured by the average economic productivity of individuals, would improve by 67 per cent. But in the least-developed countries the standard of living, by the same measure, would improve 40 times. This alternative scenario is simply an example of the kind of transition that is possible. Many other possible variations can be envisioned, depending on societal values and the willingness of the stakeholders to act in concert to pursue agreed-upon goals. These considerations outline the issue of economic equability.

We turn now to an emerging hypothesis. Wealth-creating assets in the past have been land and labor, then energy and capital. Now it is knowledge, broadly construed, that is emerging as an additional wealth-generating asset. In his challenging book *Consilience*, E. O. Wilson remarks in the penultimate page that "A great deal of serious thinking will be needed to navigate the decades immediately ahead. … only unified learning, universally shared, makes accurate foresight and wise choice possible. … we are learning the fundamental principle that ethics is everything." I suspect Wilson would be pleased at the emphasis given to ethics at this Forum.

The knowledge enterprise consists of four activities: *discovery, integration*,*dissemination* and *application.* Discovery involves research. Integration crosses disciplines and sectors of society. Dissemination in a knowledge-based society really calls for life-long learning. Application brings in business and industry because it involves putting knowledge to work in producing and consuming goods and services. This array of activities addresses the nature of–and interaction among–matter, living organisms, energy, information and human behavior. Today, this cascading knowledge enterprise holds promise for remarkable human progress, even as it entailing the threats we have just noted. A little reflection on that promise is in order…

Knowledge in the physical sciences continues to grow impressively. It is literally exploding in the biological and health sciences. Moreover, a revolution is under way in the technologies for handling information and distributing knowledge. *Collaboratories* for joint research at a distance and*distance education* for lifelong learning are among the new tools at our disposal. It is timely to propose the hypothesis that cascading knowledge can now be marshaled to pursue imaginative goals within sight and address the problems outlined above. The goal is an environmentally sustainable, economically prosperous and equitable and socially stable society. This is a society in which harmony exists between human and natural systems. It is a society in which all of the basic human needs and an equitable share of human aspirations can be met while maintaining a healthy, physically attractive and biologically productive environment. In the end, decisions and actions by well-informed individuals in local communities in partnership with inspired leaders will forge a path into the future that renews rather than to degrades the physical and biological environment and enriches rather than to impoverishes the cultural environment.

We propose to test this hypothesis in the Western Hemisphere where the issues we have been discussing are all evident. Canada and United States have a combined population of 305 million. The 34 countries in the Latin America and Caribbean regions have 498 million people. The annual production of goods and services per capita in Canada and the U.S. is $28,000. In the other 34 countries it is $3,830. This is a measure of the inequity among nations in the Americas. A business-as-usual scenario to 2050 leads to per capita production of $71,000 in Canada and the U.S. and $10,300 in the other 34 countries. In this scenario the economic gap would then grow from about $2,400 to $61,000.

One of many alternative scenarios would be to utilize our cascading knowledge to (a) reduce the rate of annual growth of population in all countries of the Western Hemisphere by one-half, (b) reduce the annual rate of growth of individual economic productivity in Canada and the USA from 1.8 per cent to 1.2 per cent, and (c) double the rate of annual growth in individual economic productivity in the 34 countries (from 1.9 to 3.8 per cent). This scenario would double the average living standards in Canada and the U.S. and increase them seven-fold in the other 34 countries. Equality would not have been reached, but inequity between these two groups would have been reduced and average living standards raised everywhere. Reduction of inequities *within* countries could be pursued internally in the light of overall prosperity. The economy of the Western Hemisphere would have expanded four-fold. Modification in modes of production and consumption would be required to avoid unacceptable threats to life-supporting ecosystems in the Western Hemisphere. The resilience of these ecosystems would be studied for an array of scenarios.

Other issues also need to be addressed: (a) expansion of the concept of the gross national product to take into account the environmental impact of economic growth, (b) cultivation of eco-efficiency (environmentally benign production and consumption of goods and services), (c) alternatives to fossil fuels to power economic growth, (d) intellectual property rights in a knowledge-based economy, (e) improvements in the delivery of heath care, and (f) development of electronic or optical communications networks for decision-making in local communities. Finally, in addition to the major task of assessing the resilience of natural ecosystem, there is the overarching imperative in *the knowledge age* to foster life-long learning through the distance education.

An informal consortium of institutions is engaged in initiating Western Hemisphere Knowledge Partnerships to test the hypothesis that knowledge, broadly construed, *does* have the potential power to change society in the Americas and to demonstrate this power to the world. In addition to Sigma Xi, the core group at present includes AAAS, the American Distance Education Consortium (ADEC), American Geophysical Union, Business Council for Sustainable Development in Latin America, Inter-American Institute for Global Change Research, Intrah (health care delivery group at the University of North Carolina), INTRAH (Harvard-affiliated academic exchange program). New York Academy of Sciences, Pacific Northwest Laboratory (collaboratories for alternative energy), Phi Beta Kappa, ICSU's START, and the University of Maryland. Members of the initial core group bring together the disciplines (physical, biological health, social, and engineering sciences, as well as the humanities) and the relevant sectors of society (academia, business and industry, government, and nongovernmental organizations) that must be involved in an endeavor of this scope and magnitude of WHKP.

We have had interesting discussions as we seek to forge a response to the grand challenges of the 21st century in which the issue of intergenerational equity is embedded. The knowledge age challenges society and, I might add, it also challenges the more than 500 chapters of Sigma Xi in the Americas to participate in the Western Hemisphere Knowledge Partnerships!

Beyond Adversarial Ethics: Web Resources for Solving Problems About Research Conduct

Panel
Beyond Adversarial Ethics: Web Resources for Solving Problems About Research Conduct

Caroline A. Whitbeck, Director, Online Ethics Center for Engineering & Science
Case Western Reserve University

Goals and Evaluation for the Responsible Conduct of Research Modules

Elysa Koppelman, Special Consultant for Research Ethics
Case Western Reserve University

Using the Online Ethics Center for Engineering and Science

Michael S. Pritchard, Director, Center for the Study of Ethics in Society
Western Michigan University

Beyond Adversarial Ethics: Web Resources for Solving Problems About Research Conduct

by: Caroline Whitbeck
Case Western Reserve University

The Online Ethics Center (OEC) started in 1995 under a grant (#SBR-9511862) from the National Science Foundation and is currently operating under a renewal grant (#SBR-9976500). In 1997, it moved with me from the Massachusetts Institute of Technology to Case Western Reserve University. The OEC is the primary science and engineering ethics Web site. It now has about 3,000 Web pages. We continually update our links. Our policy is to annotate all our links, so users don't waste their effort going down blind alleys. Any of you who have materials in science and engineering ethics that you would like other people to know about, please send us the URL for the material with an appropriate annotation for it.

Some regular users of the Online Ethics Center download the whole site and use that copy on their computer. I have brought such a copy of the site on my computer to demonstrate the materials in my talk today.

You can see that at the top and left of each page there is graphic that provides certain general information and menus of links to all the major sections (indicated by the colored tabs) and minor sections (listed in the top left-hand corner of each page). At the top of each page is a link labeled "text version" that enables you to go to a version without any of the graphics. The text version is useful for people with limited vision who want to increase the font size of all the text, even the information contained in the graphic. It is also useful for those who are printing out OEC pages. People may freely print out and use our materials for classroom use or other purposes that are both educational and non-commercial.

Notice that among the minor sections are: a bibliography, a glossary of terms, a list of organizations and their acronyms that contains links to the Web sites of the organizations. A list of other science and engineering ethics Web sites. We also link to other Web sites that are relevant to particular topics, as appropriate throughout the Online Ethics Center. A fair number of our pages are also offered in Spanish translation. We have a great many other sections in addition to the research ethics section that I will be discussing today.

The OEC has an Ethics Help-Line co-sponsored by the National Institute for Engineering Ethics to provide experienced peer counseling to those facing ethically significant problems in science and engineering. We have an experienced team of counselors. Some of those on the team had run the Institute of Electrical and Electronics Engineers' Ethics Hotline. Most of the questions to the Help-Line are about research practice or engineering practice.

Some of our pages contain materials that are unique to the Online Ethics Center; other pages we maintain contain materials that are also available elsewhere. For example, we maintain copies of some codes of ethics and guidelines containing ethical standards for research practice and for treatment of human subjects. The OEC also has links to other major sites that have materials on responsible conduct of research, including human subjects protection and animal research subjects.

For example, we have information on the National Town Meeting that was held as part of the public comment activities for the new Uniform Federal Policy on Research Misconduct. We show what parts of the draft policy came up for (favorable or unfavorable) comment at that meeting- . We add an annotation to many things we put up, to help people find their way through the subject.

Let's go to the materials we have that you won't find on-line elsewhere. Let me start with the research ethics cases and commentaries. Those of you who were in Vivian Weil's session yesterday heard her discuss these materials and give you some examples from it. These are cases that were worked up by graduate students who were participating in workshops run by the Association for Practical and Professional Ethics. There are now five volumes. We have three volumes posted in the OEC and will soon receive and post the fourth and fifth volumes.

Many of us who are involved in this project have also taught the responsible conduct of research in formal courses, in informal settings, in seminars and speaker series. Often, as good as those are?and some of them are still being offered at Case Western Reserve University?they address only students. As Judith Swazey said yesterday, the trick really is to teach the faculty. It's not that most faculty members need the basics. Very often, they know the basics. In fact, they often have a very sophisticated understanding of how to behave, but they don't know how to talk about it and transmit their understanding to their students. Students often do not know how to approach their advisors with questions about research conduct.

Part of our purpose is to create the circumstances in which those discussions will take place. We are not just transferring information. The learning situation we offer is far removed from that of individuals memorizing regulations and taking a test on them- the approach that is often used to acquire and demonstrate knowledge of the ethical requirements for research with human subjects. All of these modes of education have value, but we are focusing on education that develops awareness, discretion and judgment of departments, laboratories and other research communities as well as the individuals in them. The goal is not merely to ensure that everyone is following the rules but to strengthen the investigators' ability to address the host of subtle issues of research practice. Strengthening the investigators' ability requires improvement of the group recognition of and support for norms appropriate to particular research contexts, and development and transmission of the ability to devise ways of satisfying many potentially competing demands simultaneously.

I started this mode of education over 10 years ago in the computer science "area" of the electrical engineering/computer science department at the Massachusetts Institute of Technology, with the then "area chairman" (roughly, the head of graduate studies for computer science), Albert Meyer. The micro-systems area of EECS took it up and made it their own. The basic goal is to develop education for all members of a department that will be taken over and become a part of the life of the department. The method may change somewhat in the process. It is more useful for the department to develop both standards of conduct that suit the character of the research, conventions and practices (e.g., journal practices) in their disciplinary area, and the means for group mentoring of their students, than it is for an outsider to have long-term responsibility for education in the responsible conduct of research. Elysa or I run a few a sessions and demonstrate how to replace lectures and case presentations with problem-solving of problems common to the group's research practice. We model how keep the session focused on problem-solving, rather than in disputing over what value trumps what, or taking sides in the conflicts described in the scenarios.

The problem-oriented presentation of material is very important; that is, we ask people to solve problems together. We use an active learning approach; that is, we generally start by giving people some experiential materials, some scenarios presenting problem situations they wrestle with. We realize that few people are going to do the reading until they become engaged in solving the problems. On-line readings for each topic are on the Web, along with a selected bibliography on the topic.

A method we usually use is to put together a collection of scenarios that present plausible practical problems in the topic area, such as authorship, reviewing articles and grants, or the supervisor-trainee relationship, and send them out by e-mail. The scenarios do not have to be discipline specific, but the majority of them have to present problems that would be plausible in that discipline. We also invite participants to submit other scenarios for discussion. They would send these to me by e-mail. I then remove identifiers from them and they are included among the scenarios handed out at the session. It means that participants who have an issue they think the department needs to discuss can send it in. (Some students who submit scenarios prefer not to have their names associated with the scenarios.) As valuable as it is to have the new scenarios, it is valuable just to ask people if they would like to add scenarios, so they understand that the session is about addressing the problems they face.

The advance distribution of the scenarios is important to build interest in the sessions. The scenarios present problems that participants might face day-to-day. Some attend because they are curious about the situations, or because they are looking for better answers to those problems, or they want to share their hard-earned experience, or because they wish the department would come to a common understanding, or even because they don't want the department to come to consensus without their input. Ideally the department adds to our store of scenarios and eventually takes over running the discussions.

The department provides the refreshments. The scenarios and invitation usually go out under the signature of the department head with the names the members of a panel who will start the discussion of the scenarios. Respected figures in the department are asked to serve on the panel. (Sometimes a department will want to put a difficult person on the panel to ensure that the person comes and takes part.) If we are considering a topic like the supervisor-trainee topic or authorship, we make sure we have at least one experienced student or other trainee on the panel.

Let's look at a few scenarios. Here is one on the subject of the reviewing and editing. The scenario says that you are asked to review an article that contains a proof and become intrigued by the topic. After a few weeks, you come up with a shorter and better proof. You feel clear about your recommendations about the publishability of this result. What, if anything, do you do with your better proof? (Robert Dynes gave a somewhat similar case in his talk yesterday.)

You can see that our Web page statement of the scenario also has links to interpretive comment from two people in different disciplines that both produce theorems. The first is in computer theory, the second in statistics. The two disciplines each bring different expectations to the scenario. The difference in expectations is due in part to a difference in the two disciplines over the relative significance of having any proof of some theorem as compared with having an elegant proof of a theorem. This difference illustrates the point that we are seeking to strengthen the skills of groups for solving ethical problems, not come up with a new set of rules that apply to all investigators.

Here's a scenario from the responsible authorship module. It was recently given to us by Gerald Saidel, professor in the biomedical engineering department at CWRU. One student is initially to be the first author of an article. The journal to which the article is submitted requires some revisions, however, and another student works on the manuscript. Now who should be included as authors, and what order should the authors be listed?

One of the things that participants learn from one another in the module session where we discuss the scenarios is the variety of things that may be underlying the situation described. That discussion helps both the faculty and trainees to learn:
… What factors are morally and practically relevant?
… What sorts of things one should inquire about in such a situation, and how might one do that?
… When you're faced with prima facie evidence that is somewhat ambiguous, what potential pitfalls do you need to be wary of?

Sometimes participants will want immediately to issue a judgment on the situation or the individuals in it. It may take a bit of time for them to see that we are trying to understand the situation and the uncertainties in it, not jump to one conclusion or another. We try to help the group engage in wise deliberation, and demonstrate how reasonable and responsible people deal with ethically significant problems about the conduct of research. The goal is to increase the group's ability to discern what's going on and to make intelligent and responsible queries in a situation, and to learn from each other.

Often the first time we lead a module with a department, some faculty approach the gathering with the attitude that this is going to be simple. They think the departmental faculty will say how research is supposed to be conducted, and the students will learn. Then the faculty members start to discuss how to respond to the scenario situation, and they may find out that they disagree about at least some of what you would do. That's a major bit of learning. When they discover the scope and limits of their areas of agreement, they begin to decide how much latitude there is for acceptable variation in research conduct, and what is simply out of bounds. This clarification is often very important for the students who are frequently confused by the differences among the research conduct of the faculty members.

I emphasize that we are not seeking to provide an algorithm for coming to a judgment about the rights and wrongs in a particular case, although we often do make reference to some clear ethical standards in exploring the problem. Our purpose is rather to prepare a community to discuss these things with one another so that they can take wiser approaches and prevent many later problems. Serious conflicts or wrongdoing in research is much easier to prevent than to resolve once they've occurred.

To take a completely different module topic, you will see we have several modules on research with special groups of subjects. For example, we have one on research with children and another on research with human biological materials. We chose these topics both because of the need for educational materials on these topics and partly based on what we had special expertise at CWRU to do. We wanted to address topics to which we could make a special contribution.

Here are some scenarios from the module on the relationships of supervisors(or "mentors") and their trainees. This one is about a student who is finishing a dissertation. The professor who is the thesis supervisor has some outside consulting and asks if the student would like to earn some extra money by creating some computer code for the consulting project. The student doesn't feel free to refuse. The scenario is written from the position of another student who is trying to get the first student to speak up. Well, this can raise all sorts of issues. Some are quite subtle. For example, why is it that foreign students are less likely to refuse when those requests are made? Because they are often in a more vulnerable position: if they lose their research assistantship, they can't readily borrow money and stay in the program, as a U.S. student could. I use that just as an illustration of the kinds of things that may come up in these discussions, and that you want to be prepared for, if you are leading sessions with these scenarios.

In the scenario on consulting, we put in an additional piece of information: some universities do not allow faculty to hire their own thesis students in their consulting to prevent situations such as this one. We do add some references to organizational responses and good practices that prevent some of the problems we describe. If your university or department has some good practices, please let us know about them, so we can post them on the Web pages.

Here is one on bias on the part of a supervisor. It is written from the standpoint of a student whose parents come from a country that has centuries-old enmity toward the country of origin of the student's thesis supervisor. The student notices that students of the supervisor's ethnicity get invited more to meet visiting scientists and participate in other career opportunities. The student is getting a good technical education, however. What, if anything, should the student do? How would the student even raise this kind of issue?

Here is one on gender issues. Sometimes when your advisor is talking about research with you and other students (all the rest of whom are male), he walks into the men's room, continuing the conversation. The guys follow him in and you are left out and have to hope that one of the male students will fill you in later. I added to this scenario many things that were happening in the particular university for which I first wrote this scenario. All the scenarios are based on compilations of real incidents, although not all of them happened together, nor did they always happen to one person. A new scenario can sometimes raise consciousness about an issue, and sometimes the raising of consciousness is enough.

Here is one about a student who thinks he is finished with his dissertation but is told by his advisor that he must do a lot more work. This turns out to be the all-time favorite scenario. This was created by an undergraduate student of mine, Todd Riggs. It is relevant in disciplines like history as well as scientific fields. Very many participants recognize the situation. One Nobel Prize winner at Princeton said he knew the people involved.

We provide some variations on our method. For the "endless dissertation" scenario, we provide some additional questions to help raise some consciousness about the supervisor-trainee relationship issues. When we offer the authorship module, sometimes we vary the method from the panel-led discussion. One of the methods is to have student trainees interview one or more potential supervisors. Postdocs already have their supervisor, but it is useful with beginning graduate students. It became a required activity for the new students in computer science within the EECS department at MIT, when I offered the modules there. Albert Meyer and I created sample questions for those interviews. If the supervisors did not like these questions, they could take it up with us, rather than the students. Now, of course, the questions are on the Web, so students at other institutions may be able to use them to start their own conversations with their thesis supervisor or departmental advisor. That may make it easier for students at other institutions to get answers to these questions.

Some faculty may refuse to answer the questions, of course. Indeed, in collaborating on an ethics statement on responsible research conduct for an illustrious scientific group, I recall one collaborator saying that there was no way he would let his students ask him these questions on apportioning credit. He said that if trainees were going to work with him, they would just have to trust him. I think we do need to make senior investigators more articulate. Sometimes a faculty member refuses to discuss things with students because he or she doesn't know how to speak about the issues. Some very smart investigators don't know how to begin to talk about research conduct. They don't like to do something badly.

What they don't know how to do well, they avoid doing at all, unless they are given an opportunity to learn.

We found that if we provide the students with a list of questions, none of our faculty? I should say none of the MIT EECS faculty, who are the first ones with whom we did this? None of them objected. Some even said that after a few years of having the discussion in interviews by the new computer science students, they now had the same discussion with all students, from whatever department, who wanted to work with them. It is very important to give the faculty the questions ahead of time, so they will be prepared when the students come to interview them. When it comes to educating the faculty along with the trainees, I find the groups to have similar needs and interests. The main difference is that the faculty know more than they know how to talk about, and they are often more wary of looking stupid.

Let's now hear from Elysa Koppelman about the participant evaluations of the module sessions that we have done so far.

Goals and Evaluation for the Responsible Conduct of Research Modules
by: Elysa Koppelman
Online Ethics Center in Science and Engineering

Some of you are probably thinking you're going to have to figure out fairly soon how to bring your institutions in compliance with the new U.S. Public Health Service requirements with regard to training in ethics. And of course you want to do something that's not just going to comply, but that's going to work. Some of you might already be doing some kind of education in the responsible conduct of research and are interested in figuring out whether what you are doing is working. So I want to talk a little bit about the way we've started to evaluate our modules, in light of the goals that we have in offering them.

We have essentially three main goals that we hope to accomplish by offering these modules to different departments. One goal is to help the members of a lab, department or research group to become more articulate and reflective about their own practices. Second, we are hoping to help them learn about the practices and standards of other members of the group in which they work, or of the scientific community in general, or of authoritative bodies in their field of research. And, finally, we hope to increase the frequency and effectiveness of discussions about the responsible conduct of research within research communities.

So our evaluation is set up to try to determine if we're meeting these goals. What we do is, we ask students and faculty to fill out evaluation forms at the module sessions, and then we plan to follow up six months later with another evaluation form. As Caroline said, we're only

14 months into our 37-month project, so some of the modules are being developed and presented for the first time, and our evaluation data is pretty preliminary. We've also developed and refined our instrument over time, so that makes it even more preliminary. But as a result of a number of these kinds of factors, most of our data right now comes from students and trainees. We don't have enough yet from faculty members to draw substantial conclusions from the data, but I'll mention a few things that were of interest, anyway.

I have some data from two modules that we did with the chemistry department, using our preliminary evaluation tool, which you don't have in front of you. But most of the data that I'm citing is going to come from two modules that we gave to 42 biomedical engineering students, and one from the research with children module that we gave 16 pediatric residents.

So one goal, as I mentioned, was to expose people to the practices and standards of other members of their group. Questions 7 through 9, if you have the faculty evaluation form, the yellow one, and 7 through 12 on the student or resident form, are meant to measure the success of this goal. Our initial data is showing that the majority of students reported they learned something new about how their supervisors thought about the particular topic of the module session; about the ethics of children or about the supervisor relationship or responsible authorship, for example. An overwhelming majority of students learned something new about how faculty, other than their own supervisors, think about the particular topic of the module session.

Another goal is, of course, to increase the effectiveness and frequency of discussions about the responsible conduct of research. This goal can't be met if the discussion that takes place in the module, itself, isn't perceived to be relevant and realistic. So for the last question on each of these, we asked whether the participants thought the scenarios that they read and discussed during the particular module were relevant or realistic. An overwhelming majority of the pediatric residents thought that at least most of the four scenarios were relevant to their situation.

The supervisor-trainee module has significantly more scenarios that cover a wide range of graduate student experiences in a wide range of fields. On this evaluation, we had asked participants to rate each of the scenarios on a scale from 1 to 5, with 5 being "very relevant" and 1 being "not at all relevant." Of the 32 biomedical engineering students who answered that question, most said that at least two of the scenarios were "very relevant," as indicated with a 5 on a 1 to 5 scale. They indicated that many of the others were "relevant" and "somewhat relevant," assigning, say, a 4 or a 3 to them. Rarely did students rate a scenario with a 1, saying that it wasn't relevant at all. Based on the few responses we have from faculty, the vast majority perceived the scenarios as being very relevant.

Now, the way students rate the scenarios in any given module, and this one in particular, may depend on factors like where they are in their graduate careers or what type of research they're doing. In our initial evaluation form, which has since been modified, we asked the open-ended question: "Did you perceive these scenarios as being relevant, and which ones did you think were most relevant?" We did this for chemistry students, and one student responded, "Relevant, good scenarios. However, more scenarios should be oriented towards second- and third-year students." Another student said, "I thought all of them were very relevant topics, except the first one. In my field of research, we have enough time before a symposium to talk with our advisors." So it may be a reflection of where they are or what field they're in.

A significant number of students in chemistry thought that the scenario called "Oops," which is about a student who keeps breaking expensive lab equipment, was extremely relevant. A significant number of biomedical engineering students indicated that the scenario on switching advisors was very relevant. To determine the frequency of discussions about the responsible conduct of research, whether they're increasing, we of course need to know how often they were taking place before the module was given. So questions 1 through 5 on the faculty evaluation and questions 3 through 7 on the student evaluation were meant to find this out.

According to our data thus far, only half of the students report having had discussions about the responsible conduct of research with their supervisor during the six months prior to the module session. About one-quarter report having discussed the particular topic of the module with their supervisors—for example, the supervisor-supervisee relationship—with their own supervisor. One-quarter said, "I discussed this with my supervisor." Rarely do students report talking about the responsible conduct of research with faculty other than their supervisor. About half of the students report discussing issues in the responsible conduct of research with other students, both in general and with respect to whatever module that we were giving them that particular session.

One thing interesting to note, although our current methodology doesn't permit a test of this question, is that some of our evaluations seem to indicate that faculty thought they were talking more frequently with their students about the responsible conduct of research than students thought they were talking to their faculty. So if this is the case, then it might indicate that an understanding of what even constitutes a moral discussion or a moral problem differs among people, and that sitting down together to have such discussions could prove to be helpful in this area, also.

We also asked participants whether they expect that discussions will increase as a result of participating in this kind of exchange. About half of the students expect that such discussions with faculty will increase, and about 70 percent expect an increase in such discussions with fellow students. So about six months from now we will contact people to find out if their expectations were correct. We're also going to ask them, in our follow-up questionnaire, whether the nature of their discussions about the responsible conduct of research has changed.

The answers to these two questions will hopefully give us an indication about whether we're reaching our third objective, which is whether participants have become more articulate and reflective about their own practices.

If discussions about the responsible conduct of research have increased, then this is a case that people are starting to think more and become more reflective about what they're doing on a day-to-day basis, and the kinds of ethical decisions that they're faced with, and that they want to discuss this with other people. And if the nature of the discussion has changed, then depending on how it has changed, this may indicate that they have become more articulate.

Using the Online Ethics Center for Engineering and Science
by: Michael S. Pritchard
Western Michigan University

I want to say just a few things from the standpoint of a contributor and user of the Online Ethics Center for Science and Engineering. I think this is a very exciting development with the research ethics modules that you're developing, and I think what's really nice about the Web site is that as you're developing your products, so to speak, other people can participate in it. So this helps the evaluation process and the quality of what's there. And I think that, in general, ethics in science and engineering is a very dynamic, rapidly developing and changing area or set of areas. One nice thing about the Web site is that you can put something on, and you can change it. You can add to it. You can modify it. I think that's very important.

I want to give a couple of other examples that I've been involved in. About five years ago, I was invited to join Ted Goldfarb, who is an environmental chemist at Stonybrook, to work on an ethics in science project. Ted was conducting summer institutes for high school and middle school science teachers, with the aim of helping these teachers come up with good ways of integrating ethics into their regular science classrooms. This was at the pre-college level, and a very innovative, exciting endeavor.

I was invited to be a consultant on the project, and so I went to these institutes and participated with 25 teachers each year. I was very excited about what I saw the teachers doing. One of the things they had to do was develop some lesson plans that they would take back to their schools, try them out in the classroom, and come back later in the year and meet as a group and talk about them.

This went on for three years. At the end of that time, Ted and I thought: Well, this is very exciting and very good —75 high school and middle school teachers from Long Island. What about the rest of Long Island? What about the rest of the United States? What about other places that might be interested? So we decided, since we both had sabbatical leaves coming up, that we would create an instructional guide for teachers; and we would try to represent, as best we could, the kinds of things that went on in the institute.

We structure the text around these questions: Why bring ethics into science? Does it belong there? What are some of the teaching problems that one has? What are some good case studies, real cases, that help to emphasize this? What about really good sample lesson plans that teachers can take into their classes and try out? So we put all of this together. And then the question was: How do we get it out to the rest of the world so they can see it?

The standard way is to publish a textbook. We thought about that for a while, but then we consulted with some leading science educators around the country, and their suggestion was: Put it on the Web because teachers will look for things like this. They'll find it, and they can use it immediately. So that's what we did. And we have a little corner on the Online Ethics Web site. If you go to the education tab, you can find the pre-college materials. There you will find the equivalent of 180 pages of hard copy text there and some other materials. Also, we invite teachers, or others who are interested in this area, to make their own contributions.

At this point, I'd say we're just beginning, and Ted and I will have to work pretty hard, I think, at getting teachers, the people who are actually using materials in the classroom, to contribute to this, to help it to grow. I find this to be very exciting, and the turn-around time is very short. If there are bad things there, they can be changed. If there are things that are missing, they can be added. If there are people who would like to join in this effort to try to figure out better ways of bringing ethics into the classroom, they can join.

There is another section on the Web site called "Moral Leaders." I'm particularly interested in this, because for a long time the cases that I used in my engineering ethics classes were basically negative ones, failures, breakdowns, wrongdoing and the sort. And I thought at some point: If things can go badly, can they go well? And what would it be like to have good stories about exemplary practices? There were doubters who thought: You can never come up with anything very interesting; we're only interested in the bad stuff.

But in fact, Fred Cuny, who is featured there, was in the news a few years ago, initially because of work that he and his associates had done in Sarejevo in restoring the water system so that people wouldn't have to retrieve pails of water from the river, which was heavily polluted, and wouldn't have to walk through the sniper zone. That is a very exciting story, and Fred Cuny's entire career was devoted to disaster relief work as an engineer. In Dallas, he established a disaster relief agency that employs engineering skills.

In the second edition of a textbook on engineering ethics that I've written with C. E. Harris and Michael Rabins there are features on Fred Cuny, William LeMessurier and Roger Boisjoly, three of the moral leaders presented in the Moral Leaders section of the Web site. What about if we want to talk about some other people? Well, there are six people in that section right now, and the way in which the stories are presented is a good supplement to what we have

A third connection I've had with the Online Ethics Web site involves a research ethics project that the National Science Foundation has supported, involving graduate students in the sciences. I've had the privilege of being one of the faculty members in this project. In each of the past five summers there has been a summer institute on research ethics at Indiana University for graduate students from around the country. One of the products of each institute has been a volume of case studies and commentaries developed by the students and institute faculty. These volumes are now on the Web site. We could have tried getting a publisher to put these out, so two or three years later they would have been out. But this delay seemed unnecessary and, given the uniqueness of the project, undesirable. Most of the workshops that have been conducted on research ethics have been for faculty. Ours was for graduate students. What we quickly learned is that the perspectives of graduate students add a valuable dimension to the problems of research ethics—a dimension that deserves immediate attention, rather than having to suffer through the delays of the standard publication process

Finally, a few years ago Michael Rabins (Mechanical Engineering, Texas A&M) organized an NSF supported summer institute to develop case studies in engineering ethics that involve numerical analysis. Very few of the published case studies in engineering ethics require numerical analysis. But what about ethical problems that arise when one is in the middle of a technical problem that requires numerical analysis? How can engineering faculty help their students integrate ethical reflection into their technical analyses? The institute brought together more than 20 engineering faculty, along with a few ethics faculty, to develop case studies that could be placed directly into standard engineering courses. More than 40 cases were developed and placed on the Texas A&M engineering ethics Web site. They are now also on the Online Ethics Web site, making them more readily noticeable to a wider audience.

Of course, as useful materials are developed in various places, they can be put on local Web sites. However, making them directly available on the Online Ethics Web site contributes significantly to their becoming known by and made use of by a much greater number of faculty, students, scientists, engineers and others who are interested ethics in science and engineering.

2000 Sigma Xi Forum Speaker/Panelist Biographies

Francisco J. Ayala
A biologist at the University of California at Irvine noted for his contributions to population and evolution genetics, Francisco J. Ayala will receive Sigma Xi's 2000 William Procter Prize for Scientific Achievement. He has made singular contributions not only to his discipline but to education, philosophy, ethics, religion and national science policy. With more than 700 articles and 15 books to his credit, his philosophical writings range from the scientific method to the biological foundations of ethics. Among his books are *Tempo and Mode in Evolution* (1995), *Modern Genetics* (second edition, 1984),*Population and Evolutionary Genetics: A Primer* (1982), and *Evolving: The Theory and Processes of Organic Evolution* (1979). A member of Sigma Xi, Ayala has served on the governing council of the National Academy of Sciences and as president and chairman of the board of the American Association for the Advancement of Science.

Stephanie J. Bird
Stephanie Bird is special assistant to the provost of the Massachusetts Institute of Technology, where she works on the development of educational programs that address ethical issues in science and the professional responsibilities of scientists. Her research interests emphasize the ethical, legal and social policy implications of scientific research, especially in the area of neuroscience. Dr. Bird is a laboratory-trained neuroscientist whose graduate work at Yale and postdoctoral fellowships at Johns Hopkins and Case Western Reserve University dealt with the effects of psychoactive substances on brain function. Co-editor of the journal *Science and Engineering Ethics*, Dr. Bird is a past president and recently became one of the first Fellows of the Association for Women in Science.

Peter D. Blair
Peter Blair is executive director of Sigma Xi and publisher of American Scientist magazine. Previously he was assistant director of the Congressional Office of Technology Assessment (OTA) and director of that agency's Industry, Commerce and International Security Division, where he directed programs on energy, transportation and infrastructure; on international security and space; and on industry, telecommunications and commerce. In the 1980s he co-founded and served as a principal of Technecon Consulting Group, Inc., an engineering-economic consulting firm in Philadelphia specializing in investment decision analysis of energy projects and in developing, financing and managing independent power generation projects. Blair received a B.S. in electrical engineering from Swarthmore College, and an M.S.E. in systems engineering and M.S. and Ph.D. degrees in energy management and policy from the University of Pennsylvania.

John Browne
John C. Browne has been director of Los Alamos National Laboratory since 1997. He came to the laboratory in 1979 as a group leader in the Physics Division and went on to hold numerous administrative positions, including associate director for computational and information sciences, associate director for defense research and applications, associate director for research, and associate director for experimental physics. From 1993 to 1997, Browne was program director for Los Alamos Neutron Science Center (LANSCE) and energy research programs, responsible for overseeing LANSCE research and operations and for coordinating the Department of Energy's Office of Energy Research programs. A member of Sigma Xi and the American Association for the Advancement of Science, he is a Fellow in the American Physical Society as well as a member of Phi Kappa Phi and Sigma Pi Sigma honor societies.

David C. Clark
David C. Clark is a clinical psychologist and the Stanley G. Harris Family Professor of Psychiatry at Rush Medical College. He has been the research integrity officer at Rush-Presbyterian-St. Luke Medical Center for a decade, and director of research affairs there for two years. He has been on the Rush faculty since 1974. Clark's research has been focused in the areas of mood and anxiety disorders, evaluating and treating suicidal persons, and the psychosocial development of medical professionals. He is a member of the International Academy for Suicide Research and recently served as secretary-general of the International Association for Suicide Prevention. A member of Sigma Xi, Clark is editor of a half-dozen national and international scientific journals on suicide prevention.

Robert C. Dynes
Robert C. Dynes is chancellor of the University of California at San Diego. Born in London, Ontario, he received his B.Sc. in mathematics and physics at the University of Western Ontario and his M.Sc. and Ph.D. degrees in physics at McMaster University. From 1968 to 1990, he served in a variety of capacities at AT&T Bell Laboratories, including head of the departments of semiconductors and material physics research, as well as director of chemical physics research. Since 1991, Dynes has been a professor of physics at the University of California at San Diego, where he has chaired the department and also served as senior vice chancellor for academic affairs. He became university chancellor in 1996. A member of the National Academy of Sciences, Dynes is a Fellow of the American Physical Society and the Canadian Institute of Advanced Research and a member of the American Academy of Arts and Sciences. His research has included studies of electron properties and transport in semiconductors and metals, including superconductors.

Robert J. Eagan
Robert Eagan is vice president for the Energy, Information and Infrastructure Surety Division

at Sandia National Laboratories, where he has also served as vice president of the Physical Sciences, Electronics and Components Division. Dr. Eagan received his Ph.D. in ceramic engineering from the University of Illinois. He is a past president of the Federation of Materials Societies, a member of the National Research Council Board on Manufacturing and Engineering Design and serves on several university advisory boards. He is also a member of American Association for the Advancement of Science, the New Mexico Academy of Sciences, the National Institute of Ceramic Engineering and Keramos (an honorary fraternity of ceramic engineers). Dr. Eagan is a distinguished life member of the American Ceramic Society, of which he is a past president, and he recently completed a term on the National Materials Advisory Board.

Peggy Fischer
As associate inspector general for scientific integrity in the National Science Foundation's Office of the Inspector General, Peggy Fischer is responsible for the management and resolution of all allegations of wrongdoing involving NSF activities, including misconduct in science. She works closely with NSF grantees and other government agencies to resolve allegations. She also directs the office's outreach program, which is designed to develop and improve partnerships with institutions, NSF and members of the scientific community. Prior to joining the NSF, Fischer served as a senior program officer for the National Research Council's Board on Biology, where she worked principally on the Funding of Young Investigators project, as well as on biodiversity and conservation issues. A member of Sigma Xi, she did her postdoctoral research at the National Cancer Institute and the University of Connecticut Health Center.

Paul Fleury
Paul Fleury is dean of the School of Engineering and professor of electrical and computer engineering at the University of New Mexico. Prior to that, he was vice president for research and exploratory technology at Sandia National Laboratories and spent 30 years at AT&T Bell Laboratories, serving as director of the Materials and Processing Research Laboratory, among other positions. Fleury holds five patents and has authored more than 120 scientific publications. A Fellow of the American Physical Society and the American Association for the Advancement of Science, he received the Michelson-Morley Award for his experimental research on laser spectroscopy and nonlinear optics in condensed matter and the Frank Isakson Prize for his research on optical phenomena in condensed matter systems. A member of Sigma Xi, he is a member of the National Academy of Engineering and the National Academy of Sciences.

John L. Fodor
John L. Fodor is executive director of Educational Media Resources, Inc., a not-for-profit corporation specializing in educational programming, and he is also the senior researcher at

the Research Center on Computing and Society at Southern Connecticut State University. His work and research focus on computer and information ethics. He has produced and directed more than a dozen video documentaries on computing and human values including:*Teaching Computer Ethics*; *Equity and Access to Computing Resources*; and*Privacy in the Computer Age*. Fodor is co-editor (with Bynum and Maner) of:*Teaching Computing and Human Values*; *Equity and Access to Computing Resources*; *Computing and Privacy, Computing Security, Ownership of Software and Intellectual Property*, and *Human Value Issues in Academic Computing*. As a media producer and director he has won AXIEM, Calop, Cheta, Communicator, Emmy and International Cindy Awards.

Sybil Francis
Sybil Francis is a senior policy analyst at the Office of Science and Technology Policy, where she coordinates special initiatives related to the nation's universities and national laboratories. Prior to her tenure at the White House, she was a research associate at Lawrence Livermore National Laboratory, where she conducted studies on the social and political forces shaping technology development. In the early part of her career she was chief legislative assistant for a senior member of Congress focussing on science and technology policy issues. Her Ph.D. in political science is from the Massachusetts Institute of Technology, and her B.A. in chemistry is from Oberlin College.

Robert A. Frosch
A former vice president of research at General Motors and former head of the National Aeronautics and Space Administration, Robert A. Frosch is senior research fellow at the Center for Science and International Affairs at Harvard University's John F. Kennedy School of Government. In 1966 he was nominated by President Lyndon Johnson as assistant secretary of the Navy for research and development, continuing in that post through the first Nixon administration. Frosch also served as the first assistant executive director of the United Nations Environment Programme and as associate director for applied oceanography at the Woods Hole Oceanographic Institution. He is a past president of Sigma Xi.

John H. (Jack) Gibbons
John H. (Jack) Gibbons is a former assistant to the president for science and technology and director of the Office of Science and Technology Policy. Since leaving the White House, he has been involved in a variety of public and private service activities. These include serving as a senior fellow at the National Academy of Engineering, as a special advisor to the U.S. Department of State and as 2000-2001 president of Sigma Xi. In addition, Gibbons is a member of the International Energy Panel of the President's Committee of Advisors on Science and Technology, a member of the steering committee of the National Climate Assessment and also serves on the Committee of Advisors of the National Renewable Energy

Laboratory. During 1998-99, he was the Karl T. Compton Lecturer at the Massachusetts Institute of Technology.

David Goodstein

David L. Goodstein is vice provost and professor of physics and applied physics at the California Institute of Technology. His book, *States of Matter*, first published in 1975, was hailed as the book that launched a new discipline: condensed-matter physics. Goodstein chairs the national advisory committee to the mathematical and physical sciences directorate of the National Science Foundation. He was also the host and project director of *The Mechanical Universe*, a 52-part college physics telecourse based on his popular lectures at Caltech. The project won the 1987 Japan Prize for television. In recent times, Goodstein has turned his attention to issues related to conduct and misconduct in science, developing an academic subspecialty in this area. Together with his colleague, James Woodward, he developed a course on research ethics that has been taught at Caltech since the early 1990s. A Sigma Xi member, Goodstein has been selected to receive Sigma Xi's 2000 John P. McGovern Science and Society Award.

Holly Gwin

Holly L. Gwin rejoined the Office of Science and Technology Policy (OSTP) as Chief of Staff and General Counsel in May 1997; she first served OSTP as General Counsel from February 1993 through July 1995. During her hiatus from OSTP, she served as Deputy Director and Counsel for the Presidential Advisory Committee on Gulf War Veterans' Illnesses, where she directed the research staff and coordinated production of several reports. Gwin worked at the Congressional Office of Technology Assessment from 1982 through January 1993. She served as General Counsel and also worked as an analyst (*Genetic Witness: Forensic Uses of DNA*) and as a project director (*Identifying and Controlling Immunotoxicants; Identifying and Controlling Pulmonary Toxicants*). Gwin earned her B.A. and her J.D. from the University of Tennessee at Knoxville.

Beverly K. Hartline

Beverly Hartline is the acting deputy associate laboratory director for strategic and supporting research at Los Alamos National Laboratory. This directorate oversees eight of the laboratory's scientific divisions, with more than 1,800 scientists and engineers and more than 500 undergraduate and graduate students in a broad range of fields. She is a former assistant director for physical science and engineering for the White House Office of Science and Technology Policy (OSTP). In this position Hartline was involved with policy, budget, human resources and major facilities associated with physical sciences, engineering and mathematics research. In addition, she was the OSTP lead for federal laboratories, scientific user facilities and the Government Performance and Results Act, and she provided staff support to the Energy R&D Panel of the President's Committee of Advisors on Science and Technology.

Wendell B. Jones
Wendell Jones is the laboratory ombudsman for Sandia National Laboratories and a co-founder of the Ombuds Program there, which supports the development of enhanced conflict management skills for everyone in the Sandia community and provides third-party support for dispute resolution. Jones is involved in about 400 cases annually and conducts some 50 mediations per year. He is also board president and a volunteer mediator for the New Mexico Center for Dispute Resolution and a volunteer mediator for the Alliance for Constructive Communication. Jones served for nine years as the manager of several materials science research departments at Sandia. He earned his M.S. and Ph.D. degrees in materials science.

Elysa Koppelman
Elysa Koppelman received her Ph.D. in philosophy last year from the University of Iowa. Her areas of specialization include ethics, moral psychology, personal identity and contemporary Jewish philosophy. Her work in bioethics has focused on such issues as organ donation and surrogate decision-making for patients with Alzheimer's Disease. Her interests also include the tension between Kantian and Utilitarian principles when making ethical decisions regarding research and the so-called gray area in research ethics. "While there are many incidents and behaviors that almost all agree are unethical and many that most believe are sound and ethical, there are many that seem to fall in the gray area--with vast disagreement about the moral status of them," Koppelman said. "I hope to show through my work that whether or not a given action in this gray area is perceived to be ethical or unethical depends largely on implicit assumptions about the nature of the scientific enterprise itself."

Thomas F. Malone
A past president of Sigma Xi (1988-89), Thomas Malone is distinguished university scholar emeritus at North Carolina State University and former Foreign Secretary of the National Academy of Sciences. He was the first secretary general of the International Council of Scientific Union's Scientific Committee on Problems of the Environment and is a recipient of the Gold Medal of the World Meteorological Organization for his contributions to international scientific organizations. A past president of the American Geophysical Union and the American Meteorological Society, Malone is the recipient of the International St. Francis Prize for the Environment.

Patricia L. Oddone
Patricia Oddone is the executive assistant to the director of the Lawrence Berkeley National Laboratory, which is managed by the University of California for the U.S. Department of

Energy. The oldest of the national laboratories, the Lawrence Berkeley facility is a multi-program lab where research is devoted to advanced materials, life sciences, energy efficiency, detectors and accelerators, serving America's needs in technology and the environment. From 1973 to 1984, Oddone held various positions in the office of the president at the University of California. She received a B.A. in English from the University of California at Berkeley in 1972.

Chris B. Pascal
Chris Pascal is acting director of the Office of Research Integrity within the Office of Public Health and Science at the U.S. Department of Health and Human Services. He began his government career over 20 years ago as chief counsel for the Alcohol, Drug Abuse and Mental Health Administration within the U.S. Department of Health and Human Services. After 15 years, he became chief counsel for the Office of Research Integrity within the U.S. Public Health Service, moving on three years later to become director of the Division of Research Investigations. Shortly thereafter, Mr. Pascal assumed his present position. He took his baccalaureate degree at Auburn University and his J.D. degree at Duke University. He did a postdoctoral fellowship in psychology and law in the psychiatry department at Duke University Medical Center.

C. Kumar N. Patel
A recipient of the National Medal of Science, C. Kumar N. Patel holds multiple professorships in physics and astronomy, chemistry, and electrical engineering at the University of California at Los Angeles. He was vice chancellor of research there from 1993 to 2000. Prior to that, he was executive director of the Research, Materials Science, Engineering and Academic Affairs Division at AT&T Bell Laboratories. A member of the National Academy of Sciences and the National Academy of Engineering, Patel is also a Fellow of the American Academy of Arts and Sciences. For his seminal contributions to lasers and quantum electronics (including his invention of the carbon dioxide laser), he has received many awards, including the highest honors from the Institute of Electrical and Electronics Engineers and the Optical Society of America. Patel is a past president of Sigma Xi and the American Physical Society.

Robert T. Pennock
An associate professor at Michigan State University, Robert T. Pennock received his Ph.D. in the history and philosophy of science from the University of Pittsburgh. His research focuses on epistemic and ethical values in science. The author of *Tower of Babel: The Evidence against the New Creationism*, he is the recipient of the Templeton Prize for the exemplary paper in theology and the natural sciences, and the National Endowment for the Humanities/National Science Foundation fellowship on scientific, ethical and social challengers of contemporary genetic technology. In 1997, he co-directed a National Science

Foundation Chautauqua Workshop on the "Ethical Implications of the Human Genome Project." Pennock has served as president of the University of Texas at Austin Chapter of Sigma Xi and is a member of the American Philosophical Association and the American Association for the Advancement of Science.

John Perhonis

John Perhonis received his B.A. from Amherst College and his M.A. and Ph.D. in American studies from the University of Minnesota. He has pursued a career with the federal government, first in program evaluation and science policy at the U.S. General Accounting Office, and, since 1988, through budget and program positions at the National Science Foundation. At the GAO he specialized in studies on federal funding of university research, and prepared and delivered testimony on science policy before the House Science Policy Committee. At NSF, he has been an associate program director in the Science and Technology Studies Program and the Societal Dimensions in Engineering, Science, Technology Program. He manages the dissertation proposals in both programs as well as ethics education projects in the Ethics and Value Studies Program.

Michael S. Pritchard

Michael Pritchard is a professor of philosophy and chair of the department at Western Michigan University, where he also directs the Center for the Study of Ethics in Society. Co-author (with C.E. Harris and Michael Rabins) of*Engineering Ethics: Concepts and Cases*, Pritchard has also prepared a set of 33 case studies in software form with which students can interact, along with a set of commentaries by various ethics teachers from engineering and philosophy. He is currently developing materials for students that emphasize exemplary engineering practice, in contrast to merely avoiding wrongdoing. He is also exploring, with chemist Ted Goldfarb of the State University of New York at Stoney Brook, ways in which science education in grades K-12 might include ethics. This involves summer workshops for high school science teachers who want to include ethics in their science teaching.

Lawrence J. Prochaska

Lawrence Prochaska is professor of biochemistry and molecular biology at Wright State University. He received his Ph.D. at Ohio State University, his B.S. at Illinois State University and began his career at Wright State in 1980. His research focuses on the biochemistry and molecular biology of membrane-bound enzymes that are crucial in heart and bacterial energy conservation reactions. The recipient of Wright State's Excellence in Medical Education Leadership Award and the Distinguished Service Award from the American Heart Association's Ohio Valley Affiliate, Prochaska has also received the Dayton Academy of Medicine Outstanding Senior Faculty Award for Excellence in Research and Teaching. A member of Sigma Xi, he is president-elect of the Ohio-West Virginia affiliate of the American Heart Association and has been active in the Biophysical Society, among other organizations.

Arthur H. Rubenstein
Gustave L. Levy Distinguished Professor Arthur H. Rubenstein is dean and CEO of Mount Sinai School of Medicine. He also serves as executive vice president of Mount Sinai/NYU Health. He is a former chair of the department of medicine at the University of Chicago Pritzker School of Medicine. An authority on diabetes, Rubenstein is a widely sought counselor to academic health centers and a frequent panelist at the annual meetings of the senior research societies in internal medicine. He collaborated with Donald Steiner who discovered proinsulin. The widely used assay for the C-peptide of insulin, developed in his laboratory, has provided a means of studying insulin metabolism in diabetic patients receiving exogenous insulin. For his research, Rubenstein has received numerous awards and named lectureships. He has authored more than 350 papers and has served on the editorial boards of*Annals of Internal Medicine, Journal of Diabetes and its Complications, and Medicine*.

Cliff Stoll
While an astrophysicist at Lawrence Berkeley Laboratory in 1988, Stoll noticed a 75-cent accounting error in his computer. After a year of sleuthing, he tracked down a ring of computer hackers, who systematically broke into military and industrial computers, searching out defense secrets and retailing these to the Soviet KGB. His dogged investigation ultimately led to the arrest of the spy in Germany. Stoll has given talks on computer security to the FBI, CIA and NSA. More recently, he has become quite skeptical of the wide promotion of computers in the classroom: do they actually help to educate, or are they the filmstrips of the new millenium? Cliff has written three best selling books, including *The Cuckoo's Egg and Silicon Snake Oil*. On the side, he makes glass Klein Bottles.

Judith P. Swazey
Judith P. Swazey is founder and president of The Acadia Institute in Bar Harbor, Maine, an independent, nonprofit center for the study of issues concerning medicine, science and society. She also is an adjunct professor of social and behavioral sciences at the Boston University Schools of Medicine and Public Health. Her research, writing and teaching have focused on social, ethical and graduate and professional education. A Fellow of the American Association of the Advancement of Science and a member of the Institute of Medicine, Swazey has served on Sigma Xi’s Board of Directors and chaired its Science and Society Committee, and she has served on the boards, committees and councils of numerous other professional organizations. She was a member of the Department of Health and Human Services’ 1994-95 Commission on Research Integrity. Among other activities, she is principal investigator of The Acadia Institute Project on Bioethics in American Society.

Bill Valdez
Bill Valdez is director of planning and analysis in the U.S. Department of Energy’s Office of Science. His responsibilities include corporate strategic planning, budget planning, R&D

evaluation and corporate communications. He has held various positions at the Department of Energy since 1994; most recently, as senior advisor to the director, Office of Science. He also worked at the White House Office of Science and Technology Policy from 1998-99, where his responsibilities included developing technology initiatives, preparing multi-agency reports on scientific workforce and international energy initiatives, and monitoring agency energy sector activities. Prior to working at DOE, Valdez worked as a senior project manager in private industry where he provided strategic planning services to Asian and European multinational corporations.

P. Aarne Vesilind
P. Aarne Vesilind is R. L. Rooke Professor of Engineering at Bucknell University, a position he accepted earlier this year following retirement after 30 years on the faculty at Duke University. While at Duke, Vesilind served as the chair of the department of civil and environmental engineering for seven years and was twice elected to chair the Engineering Faculty Council. A member of Sigma Xi, he is a former trustee of the American Academy of Environmental Engineers and a past-president of the Association of Environmental Engineering Professors. He serves on many technical and professional editorial boards and has written nine books on environmental engineering, solid waste management, education and environmental ethics. His book *Introduction to Environmental Engineering* (1998) incorporates ethics into an undergraduate environmental engineering course.

Janice Voltzow
Janice Voltzow is associate professor of biology at the University of Scranton. After earning her B.S. in biology at Yale University and a Ph.D. in zoology at Duke University, she was a postdoctoral fellow at the Friday Harbor Laboratories of the University of Washington. From 1986 to 1996 she held a faculty position at the University of Puerto Rico, where her research projects ranged from the rain forests to the coral reefs. Her current research focuses on the functional morphology and evolution of marine invertebrates. A member of Sigma Xi, she is president of the American Malacological Society. Voltzow has also served as chair of the public affairs committee of the American Society of Zoologists and as the representative of the Society of Integrative and Comparative Biology to the American Association for the Advancement of Science. She is a frequent reviewer of children's nature books for *Science Books and Films*.

Jeffrey Wadsworth
Jeffrey Wadsworth is the deputy director for science and technology at Lawrence Livermore National Laboratory, with responsibility for the Science and Technology Office, the University Relations Program Office, the DoD Programs Office, the Office of Planning, Policy and Special Studies, the Industrial Partnerships and Commercialization Office and the Joint Human Genome Institute. Prior to this appointment, Dr. Wadsworth was associate director for the

lab's Chemistry and Materials Science Directorate. He has also served as manager of the metallurgy department at Lockheed Missiles & Space Company, Inc. His many publications include the book *Superplasticity in Metals and Ceramics*. Dr. Wadsworth is a Fellow of both the American Society for Metals and the Minerals, Metals and Materials Society. He is the recipient of the Brunton Medal for Excellence in Research and the Metallurgica Aparecida Prize.

Vivian Weil
Vivian Weil is professor of ethics and director of the Center for the Study of Ethics in the Professions at the Illinois Institute of Technology. She has also served as director of the Ethics and Values Studies Program of the National Science Foundation. Weil specializes on issues of professional responsibility, primarily in engineering and science. She is editor of *Beyond Whistleblowing: Defining Engineers' Responsibilities; Biotechnology*, *Professional Issues*, and*Social Concerns*; and *Owning Scientific and Technical Information: Value and Ethical Issues* (with John Snapper). Among her publications are the monographs "Engineering Ethics in Engineering Education" and (with a co-author) "Ethics and Relationships in Laboratories and Research Communities." Other publications include "Comments on 'The Psychology of Whistleblowing' and 'The Voice of Experience,'" and commentaries in*Research Ethics: Fifteen Cases and* Commentaries (Volumes I-IV).

Caroline Whitbeck
A philosopher of science, technology and medicine, Caroline Whitbeck holds the Elmer G. Beamer - Hubert H. Schneider Chair in Ethics at Case Western Reserve University, with appointments in the departments of philosophy and mechanical and aerospace engineering. Her work focuses on the place of practice in the development of scientific, medical and engineering concepts. Her work in practical and professional ethics centers on the perspective of the agent, the person who must respond to the problem. Her emphasis on problem-solving has widely influenced pedagogy in science and engineering ethics education. The author of *Ethics in Engineering Practice and Research*, Whitbeck serves on the editorial board of *Science and Engineering Ethics* and is founder and director of the Online Center for Ethics in Engineering and Science, the foremost ethics site for science and engineering. She is a member of Sigma Xi.

William A. Wulf
William A. Wulf, president of the National Academy of Engineering, is on leave from the University of Virginia, where he is a University Professor and AT&T Professor of Engineering in the computer science department. Prior to joining the UVA faculty, Wulf was an assistant director of the National Science Foundation, responsible for computing research, the national supercomputer centers and the NSFnet (predecessor to the Internet as we now know it). He also founded and was CEO of Tartan Laboratories, a software company in

Pittsburgh based on research he did while on the faculty of Carnegie-Melon University. Wulf has conducted research in computer architecture, programming languages, optimizing compilers and computer security. A Sigma Xi member, he is a Fellow of the Institute of Electrical and Electronics Engineers, the American Association for the Advancement of Science and the American Academy of Arts and Sciences, among others.

Robert Zand

At the University of Michigan, Robert Zand is a professor of biochemistry, a professor of macromolecular science and engineering and is also a research scientist in the Biophysics Research Division. He earned a B.S. degree in chemistry and physics at the University of Missouri and an M.S. at Polytechnic University of New York. After service in the army, he returned to graduate school and received his Ph.D. in chemistry from Brandeis University and went on to conduct postdoctoral research at Harvard University. Zand's interests include using techniques of biophysical chemistry to problems of structure and function in biological macromolecules. He has been active in Sigma Xi at the local and societal levels, as a chapter president and on the Society's board of directors.