The Outbreak

Farthest advance

It was early evening on the fourth of May, 1989. A solid row of scientists lined the rear wall of a large hearing room in the Cannon House Office Building. They were there to support David Baltimore as he testified before the Subcommittee on Oversight and Investigations of the House Committee on Energy and Commerce. On a raised dais opposite the scientists sat members of the Committee, at their center the chairman, Representative John D. Dingell of Michigan.

Baltimore and Dingell were powerful men. Their clash, pitting fact against myth, would determine whether ethics would impose limits on power in the conduct of biomedical research.

Dingell had questioned Baltimore in inquisitorial style. When the questioning was over Baltimore asked to make a statement. As when Napoleon entered Moscow, this moment marked the farthest advance of the outbreak.

Status quo ante

Cheating in science

Some cheat in science for money, some for fun, some for pride and others for who knows what interior compulsions or combinations of motives. The snake oil salesman cheats for money -- or does not cheat at all if he thinks his product works. Whoever buried a piece of bone shaped like a cricket bat at the Piltdown site where had just been found the "antiqued" skull and jaw of "The Oldest Englishman" clearly did it for fun.

Like Gregor Mendel (or maybe his assistant/s) many scientists have "improved" data, and, like Mendel, many of these have guessed right about the phenomena that their data were improved to illustrate better. Mendel was found out because 1) his experiments became famous, 2) the data were improved to the point of extreme statistical unlikelihood and 3) the improved data received the attention of a good statistician (R. A. Fisher). Had the experiment been less famous or the cheating craftier the monkey business would probably never have been found out.

Contrary to statements by many scientists, data-faking is not an auto-convicting crime. Even if a scientist invents data that illustrate a wrong theory the resulting paper will most likely merely be forgotten, an experiment that went wrong for some unknown reason.

As Shakespeare's Mark Anthony did not quite say, "The lucky faking that scientists do lives after them; the unlucky is not in the citation index." Even an egregious phony like the Piltdown man outlasted all his likely creators.

Fake research is nevertheless a chancy business, especially to build a career on, because each new act of fakery raises the cumulative probability of being caught. Safer and more reliable is fake authorship of real research. It is quite easy to arrange. In one form it is traditional in biomedicine; many supervisors appear as co-authors of supervisees' papers to which they contributed little or nothing. Indeed the supervisee may not be an author at all.

Supervisees put up with this because they depend on mentors to get research funds. Those who complain are feared by their own and other supervisors as trouble makers who might ignite a peasants' revolt. They are disliked by fellow supervisees envious of their courage, and by conformists and apple-polishers at all levels. Pack behavior adds an instinctive response. Complainers are "different," and treated accordingly.

Authorship is faked by journal referees and reviewers of grant applications when they steal essential ideas, hurry through the relevant experiments if necessary, and publish the results. To give themselves more time they can and do hold up or reject the papers or applications that they steal from.

A common trick is to redo and publish research that someone else has already published, ignoring, misrepresenting or denigrating the earlier work.

In a field with strong science politics fake authorship by a major figure can be safe even when blatant. Other scientists pretend not to notice if they are afraid to do otherwise.

Science politics

The chain of dependency does not end with supervisees' depending on supervisors for support money. Supervisors too are dependents.

Science has long lived in large measure upon patrons. Until World War Two the patrons were private and numerous. If Rockefeller would not support you Carnegie might. After World War Two research greatly expanded, with the increase paid for by the Government (i. e. the taxpayers). The typical scientist is now either supported by Government or not supported at all, and in most fields most of the support is funneled through a single agency. In biomedicine this is the National Institutes of Health (NIH).

But NIH is not the arbiter of biomedical scientists' fate. Like Pontius Pilate NIH evades responsibility by delegating the decision on one's future to one's competitors. Grant applications are rated by panels of scientists, the Initial Review Groups, familiarly Study Sections. These panels do not advise the decision makers; they make the decisions. Later levels of review seldom have much influence. (When they do it can make news.) Funding or starvation depends almost entirely on study sections' numerical rating of applications.

The system is like an unsupervised school yard, a no-umpire arena for bullies and gangs of bullies. Nothing stops panel members from punishing enemies and rewarding friends.

Nor does much prevent panel members from perpetuating control of a panel by people sympathetic to themselves. Members serve four-year terms (usually) and cannot succeed themselves, but their successors are nominally chosen by the executive secretaries of the Study Sections, low-level civil servants who lack the status to face down bullies who want to choose their successors. (We know that executive secretaries are complaisant from the absence of public fights over succession. Powerful scientists make a loud and public noise when seriously displeased. There is no noise. Evidently the big operators are satisfied.)

The system encourages the growth of politics, and as total politicization is approached in science facts cease to be a factor in deciding whose paper is published, who gets a grant, or who wins in a confrontation between a whistleblower and a cheat. Further, in politicized science poor scientists outvote the good ones, whom they vastly outnumber. The effect is disastrous.

Thus the little discipline of joint lubrication research has been controlled for more than twenty years by a cabal around Professor Van C. Mow, late of Rensselaer Polytechnic Institute, now at Columbia University, who rose to prominence for no scientific accomplishment. On balance, government money has probably retarded our learning about joint lubrication. Funding was denied to people working on the chemistry of joint lubrication. It went instead to the cabal for work on the physical mechanism that progressed from inventing a wrong theory to taking a correct theory from M. A. Biot and renaming it for themselves, thence through non-germane and/or trivial work, and finally to a so-far incomplete rediscovery (but complete renaming) of the joint's physical mechanism I explained 42 years ago.

Typical scientists have to put up this sort of thing if they want to eat. I do not, so about 20 years ago I asked both NSF and NIH why they supported Mow's incompetent work. NSF's George Koo Lee, the manager of the program that supported Mow, in effect said, "Let's you and him fight," and gave me a preprint of the keynote lecture that Mow was to give at a national meeting. I asked William van Buskirk, the man in charge of speakers, for ten minutes for rebuttal. He gave it to me, but within a week withdrew it under pressure without informing me.

Lee's counterpart at NIH, Stephen L. Gordon told me to write a critique of Mow's work and he would have it reviewed. No review emerged. Gordon explained that Mow had just received the Melville Prize, and that civil servants could not -- mumble mumble, etc. etc. I complained to J. E. Rall, NIH Deputy Director for Intramural Research, and a review by Lyle Mokros emerged, reported, I later found out, by telephone. Gordon wrote me that mistakes had appeared in the literature -- his telephone notes, given me by a sympathetic bureaucrat, said simply, "agree technical part" -- went on to make excuses for Mow and said nothing would happen except that the critique would be sent to Mow "for his guidance." (Later Alan Grodzinsky, once a collaborator of Mow told me that Mow had told him that Steve Gordon would give him anything he wanted.)

I continued to push, and in 1989, William Raub, Acting Director of NIH proposed a multi-stage complaint, rebuttal, rerebuttal etc. slugging match between myself and Professor Mow. Herbert Pardes, Mow's dean, wrote to NIH objecting to the procedure, and progress stopped. Betty Riley secretary to Craig Wallace, the executive secretary of the panel, when asked the reason for the delay said, "They're dancing around it. They're afraid of being sued." The format was changed to a written complaint by me, a written reply by Mow (in the end unresponsive), and oral replies by NIH functionaries before a three-man, external panel that met in secret.

Confirmation of Pardes's threat of suit came when I mentioned to Craig Wallace that I had telephoned Pardes to complain that when Mow sent to NIH a manuscript of mine that he, as referee, had rejected he left out the half of it that proved my point, and Pardes obliquely warned me that what I was doing might get me sued. "He did the same to us," Wallace said, and went on to bemoan the fact. To cheer him up I said, "If he sues we can turn him inside out. We can look in his bank account." "They won't find much in mine," Wallace replied. Next day he denied having mentioned the threat, and Betty Riley, who had been present throughout the whole exchange, denied that he had mentioned it. Wallace and I, both retired from NIH, recently had a cheery exchange about this event that never happened.

The panel, two to one, said that Mow had not borrowed his correct theory from M. A. Biot. Later I found out that Professor Wilson C. Hayes, who voted against me, had been coediting a book with Professor Mow at the time the panel met. Told of this, NIH, in the person of its then top lawyer Robert Lanman, who does not pretend to understand science, found no evidence that this connection had influenced Hayes's verdict.

In addition Professor Mow verbally browbeat Richard Chadwick, my supporter on the panel for half an hour in the presence of Chadwick's wife while the decision was being deliberated. Informed of this, NIH did nothing.

By this time continued complaint by me had got NIH to appoint a four-person internal panel to look into Mow's influence on funding. (I hoped to get one or two dissidents appointed to the Study Section.) Evidence gathered for this panel by NIH's Division of Research Grants showed that an unusually large fraction of Professor Mow's applications were approved, as were those of Hayes and Richard D. Brand, both journal editors. The panel left this evidence out of its report and said nothing untoward was going on. I learned of the missing evidence via a Freedom of Information Act request for something else. In a memo to its members the panel's executive secretary, Tommy L Broadwater, wrote, "The table will not be part of a final report because of the 'high potential' for misinterpretation based on the numbers reflected in each category."

Evidently this part of NIH, at least, has less regard for science than for appearance, for it used those panels for damage control, bogus probing, not to find and correct mistakes.

Bureaucracies reward and punish. A bit before the second panel investigation Laura Lerner, until then an ally of mine told me, out of the blue, that I was an anti-Semite and asked if I had noticed that bad things were happening to me. About that time she received an NIH grant. Also about then Richard Chadwick, from the first Mow panel, told me he had been warned that I was someone to stay away from.

Probably the net effect of my efforts has been to solidify NIH's support for Professor Mow.

This is the stew of intrigue that Government support has made of bioscience. Supervisees depend on mentors for money and mentors depend on politicking to get the money. Permeating it all is fear -- of not getting published, of not getting a grant, of not getting tenure, of offending powerful scientists by not agreeing with them. Into this jungle of brass-knuckled, fearful people drop accusations of fabrication, falsification, plagiarism and other forms of intellectual theft. Most sink without a trace.

Cover-ups

In politicized science, as in politicized anything, appearance comes first. Someone blows a whistle on cheating. How much better it would be for the cheater's laboratory, the university, the sponsoring agency, agency's parent department, science itself -- and, sotto voce, the cheater -- if no whistle had been blown.

In theory the bureaucracy is against fake science because it wastes the taxpayers' money. But the fact of fakery proves that the bureaucracy's management of science has been less than perfect, and the bureaucracy would rather conceal fakery -- even let it continue -- than prosecute fakers and thus admit that it sometimes blunders.

Everyone but the whistleblower benefits from cover-up. So in one way or another it is arranged that the whistle might as well not have been blown. Usually cover-up is done by local whitewash. The university (or other research institution's) authorities find that nothing wrong was done, therefore nothing need be reported.

Usually also the whistleblower is punished as a warning to others and for simple vengeance. Rationalization is easy. "Damage to the reputation of a great university and the good name of science consequent on revealing that fakery has occurred would injure mankind far more than the fakery itself. Silencing or discrediting the whistleblower is the only ethical course."

Once a whistleblower has been blackwashed the blackwashing cannot stop, for that would reveal that the original blackwashing was wrong, that he or she was not the bad person portrayed.

For example; when Robert Sprague realized that his research partner Stephen Breuning was inventing data and so informed the National Institute of Mental Health (NIMH) which was sponsoring the research, NIMH first investigated Sprague, then sank into inaction. Only when journalist Dan Greenberg threatened to expose this inaction did NIMH move. It found that Breuning had made up data, and blamed Sprague for not realizing this earlier.

The affair put Breuning out of research. Indeed he was indicted and, post plea bargain, sentenced to 5 years probation and 250 hours of community service. He was required to pay $11,352 back to the Government, while the University of Pittsburgh had to return $163,000.

But the bureaucracy had the last laugh. It failed to renew Sprague's grant. This was done at a stage after the Study Section had given a positive recommendation, and it made news. Potential whistleblowers took note.

Whatever the authorities thought they were doing, whatever the excuses they made to themselves for what they did, the de facto policy was and still is to silence and punish whistleblowers. The temptation to do so operates at all levels, from the cheat through the cheat's institution and the sponsoring agency to the Government department to the administration that is in power. All want to preserve the appearance of propriety, and all, with depressing regularity, have co-operated in cover-ups and the punishing of whistleblowers.

When fraud happens (almost?) all administrations are corruptible

Breuning's sin showed that there was at least one crooked scientist. This is to be expected. Inevitably there are bad apples among scientists. The attempted cover-up and the successful retaliation against Sprague was much more serious because it showed that the system was corrupt. It could not be the result of one or two bad apples in administration. Too many people cooperated in what happened.

And it was not only to Sprague that it happened. The experience of whistleblower after whistleblower, including me, has shown that cover-up is almost certain if the cheat is important. When important scientists do wrong "respectable" people in "respectable" administrations do wrong to conceal the sin. It has become a function of the system to protect corruption in high places and punish those who try to expose it. The outbreak was a reaction to this system-wide corruption.

What was new about that? People always complain about corruption -- in science as elsewhere -- and the system shrugs off the complaints and punishes the complainers. What marked the outbreak was the fact that, for the first time, powerful third parties took a hand and probed the system by trying to get justice for the complainers.

Cheating, for example the Piltdown man, evokes a "gee-whiz" reaction. Cover-up makes one think "oh oh," and fear for the well-being of the community. This "oh oh" reaction got the Congress interested. NIH administrators testified before Congressional committees, and promised to do something about both fraud and cover-up.

NIH formally recognized misconduct when it named Mary L. Miers to the newly-created post of Institutional Liaison Officer in 1983. Miers had been dealing with misconduct since 1980 as assistant to William Raub, NIH Deputy Director for Extramural Research. She was replaced by M. Janet Newburgh in July of 1987, and on March 16, 1989 the misconduct job was moved to the newly-formed Office of Scientific Integrity (OSI) under acting head Brian Kimes, though there followed some months of overlap in which both Newburgh and OSI handled cases.

Now that scientific misconduct was officially a problem it needed an official definition. Fabricating (making up) and falsifying (altering) data were clearly wrong, as was plagiarism, but scientists do many other dirty tricks, typically deceptions of one sort or another. So deception was included in the definition -- and soon objected to because many experiments in psychology involve deceiving experimental subjects.

Rather than narrow the application of deception to (say) “deception in reporting the content or the authorship of research” NIH replaced it with “. . . other practices that seriously deviate from those that are commonly accepted within the scientific community for proposing, conducting, or reporting research.” Establishment figures have said repeatedly that this definition would rule out innovative research, but I do not recall their giving any examples. In fact OSI and its successor the Office of Research Integrity (ORI) have never (or maybe just once) prosecuted anyone under this clause.

The term “prosecuted” raises another point. OSI was NIH’s science-police force. Police forces pursue criminals. Therefore cheating scientists are criminals, and deserve all the Anglo-Saxon rights of the accused that make the whole procedure slow and expensive.

Crime or civil wrong?

Cheating in science is not violent like mugging, nor does it involve theft of something tangible (money) like white-collar crime. Its distinguishing ingredient is deception, and deception about something one has been paid to do is a civil, not a criminal, wrong. Among those the deception injures is always the sponsor, -- in American bioscience almost always the taxpayer -- who does not receive original research truthfully reported, or honest advice from peer reviewers or study section members.

The research may be bogus, or unoriginal, or not done by the payee. Personal enmity or indebtedness may masquerade as technical advice in refereeing and in study sections. With the exception of concealed, formal financial conflict of interest in advisory panel members like Wilson Hayes, which is a crime under 18 United States Code 208, these are civil matters with civil remedies. The sponsor can stop paying and can sue to get back money already paid. If the payee thinks this unfair, he or she can sue in turn (for which the Government should not refuse permission). All of these actions would be in the public record.

This is the only course that private sponsors have available, and it seems to work, because one hears little or nothing about fraud in the research that they support. This cannot be because none occurs. They cannot be that lucky. For example I arranged that money be donated to Stanford University for research on joint lubrication, only to see it spent on something different. I complained, and Stanford returned the money, with interest.

Nor can it be because fraud is covered up. Whitewash makes whistleblowers shout, so apparently fraud is dealt with to the satisfaction of complainers.

And it seems that no one sues.

For NIH to use this procedure some NIH official or officials, as agents of the taxpayer, would have to make decisions that would embarrass NIH by showing that its grant system is less than perfect, and also embarrass erring scientists. The latter frightens NIH officials if the scientists are important, because NIH bosses are then unlikely to back them up.

Willingness to respond to influence rather than principle is a requirement for high public office, and the higher the office the more absolute this requirement is. (Anyone designing a government bureaucracy should keep this in mind.) Lower levels in a government bureaucracy can have a quiet life only if they do not offend people who have power that can be brought to bear on their bosses. (A business bureaucracy cannot let improper influence misdirect its actions too badly or the business will lose money, and it, and the bureaucrats' jobs, will eventually vanish. Government bureaucracies do not have this financial lash to keep them honest.)

OSI (later ORI -- the switch in names is confusing; please bear with me) wanted the quiet life, and prosecuted only low-level data-fakers unless its hand was forced in some way. (The forcing of its hand was a major event in the outbreak.)

OS/RI did not announce its self-imposed limits. It accepted complaints of theft of research and theft of credit and went through the motions of taking them seriously until, inevitably, it decided against the whistleblowers. One such was Jane Rosen. After much back and forth, ORI told her that to prove that her Ph. D. supervisor Peter Coleman had stolen her research she would have to show that he had contributed nothing at all to the work that he published without her as co-author and only the barest acknowledgement that she had worked on the problem.

All decisions against complainants, and that is most of the decisions, were kept secret. There was no way potential whistleblowers could learn what ORI's working definition of scientific misconduct was (if indeed it followed any definition consistently). It was weird. ORI would say a particular piece of slimy behavior was not scientific misconduct under the Public Health Service (PHS) definition (when it obviously was, under the "other practices" clause), yet refuse to publish the definition it was actually following.

In common law an unstated definition reveals itself in decisions on cases. By concealing its decisions ORI concealed its definition. Only after the Rosen case was aired in published letters to the editor did ORI declare that "authorship disputes" were not within the PHS definition of scientific misconduct.

The term "authorship dispute" is itself deceptive in that it suggests symmetry between the disputants. But what happened, or did not happen, was theft of credit, which is not symmetric at all. Someone steals and someone is stolen from. (By ORI thinking auto theft is an "ownership dispute.")

Other facets of ORI's working definition remain hidden.

In every way it could OS/RI operated behind a veil of secrecy, which it claimed was in the interest of all parties. Not only were most of its decisions secret, it routinely bound, or tried to bind, complainants to secrecy, when in fact their best hope of derailing whitewash and cover-up was publicity.

The result is that cases where the complainant loses are known only to the complainants and their friends. OS/RI buried whistleblowers in separate, unmarked graves. Because this gives the establishment a tremendous advantage over whistleblowers I tried to open the secrecy curtain.

Although the Freedom of Information Act says that reports of investigations (without qualification) are to be accessible, OS/RI would and still will reveal only reports of cases where the complainant won, and these reports became less and less complete over the years. I challenged in court ORI's withholding of information where the complainant loses, the vast majority of cases, and all those where cover-up has been successful. I sued for a list of all cases that OSI had decided, including the names of the parties. (When I originally asked for the list I had no idea that I would be refused, and I needed the list to see which reports I wanted to request. They were then the size of modest telephone books,) I won a partial victory in the court of first jurisdiction but lost everything on appeal. (The Government uses taxpayers' money to fight taxpayers through appeal after appeal.)

This should be familiar. A law is made against some sort of sin committed by powerful people. Decision by decision, administrators and judges then destroy the law.

Do not let my experience deter you from using the Freedom of Information Act. It does not work as the Congress intended. If what you request is embarrassing to the authorities you will probably not get it. But some closet believer in honest Government may send you something revealing that you did not ask for.

During the appeal the Federation of American Societies of Experimental Biology (FASEB) and other associations of scientists and universities entered the case as Friends of the Court on the Government side. This was part of a consistent pattern. In confrontations on the issue of openness vs. secrecy these organizations are regularly on the side of secrecy.

I never heard that any of them polled their members to see what they favored. The people at the top make the decisions and tell the rank and file what to think. As in bad labor unions, the leaders claim to be defending ordinary members while in fact looking out for themselves and their powerful friends.

History teaches that only public scrutiny can keep justice honest. Those who run these organizations fear it.

On one occasion the FASEB announced that five scientists had been victims of false accusations. (Waving the bloody shirt is a standard establishment tactic.) I asked Michael Jackson, FASEB executive secretary, who these scientists were so I could look into what had happened to them. He would not tell me. If dark bulbs were available -- turn one on and everything goes black -- the establishment would buy them.

Whether or not it was the original intent, until recently OS/RI served as a lightening rod to attract complaints that might otherwise be taken somewhere else with more effect. Both Carolyn Phinney and Pamela Berge took their cases of intellectual theft to court and won. Berge lost on appeal, but in Phinney's case defendants Prof. Marion Perlmutter and the University of Michigan elected not to appeal.

Both cases spanned the outbreak in time, and Berge's loss on appeal may have resulted from the anti-whistleblower atmosphere that took over after it ended.

But I get ahead of my story. Ground was prepared for the outbreak by reports of sin and cover-up in science that attracted the attention of Congress. And that attention frightened bioscience into appearing to act. Thus as well being in theory a policeman (though in practice a lightening rod), OS/RI was supposed to require and approve ethics courses in grantee organizations. This was a perfect bureaucratic response. It provided jobs for the boys and girls, gave the appearance of action, and did not in the least inconvenience practicing cheaters or their protectors.

Scientific ethics can be taught with a single sentence. "Do not deceive your audience, (or your experimental subjects unless the latter give you permission to do so)." (The "unless" part lets psychologists do their little tricks.) The problem is not that scientists do not understand this commandment. It is that in present-day science it is often a good career move to break it.

Ethics courses are the bureaucratic equivalent of the psychologist's displacement activity; something one does while putting off doing what one ought.

Another displacement activity was and still is conferences on misconduct. They are misconduct theater, with all the reality of the mice discussing how to hang the bell round the cat's neck. The same actors troupe round the country taking each other seriously, or pretending to, all with the blessing of an establishment that knows they are harmless.

Ethics journals are another form of pretended action. They discuss ethics in the abstract, not real cases where people get hurt.

One recent traveling show, post outbreak, was the Commission on Research Integrity, set up under a provision of the NIH Revitalization Act of 1993. It is usually called the Ryan Commission after its chairman, Kenneth J. Ryan of Harvard Medical School. It contained no whistleblowers. Witnesses were told not to mention real cases, but did anyway. I expected nothing to come of it but attended sessions in the Washington DC area out of curiosity and a sense of exhausting all legal remedies.

In the end the Commission proposed some wise changes in procedure but undid them all by demanding that whistleblowers respect confidentiality. As long as secrecy is guaranteed, procedures will be followed only if convenient to those in power.

The Commission's recommendations even so displeased the establishment and have been ignored. So has been later work by at least one Government committee charged with forming a Government-wide definition of scientific misconduct. More recently a Government group, meeting in secret has proposed a narrow definition of scientific misconduct that leaves out all but one of the sins of the mighty, stealing from grant applications, which is seldom easy to prove.

By the time of the Ryan Commission the Outbreak was effectively dead. A sign of this is the lack of whistleblowers among the Commission members. Earlier study groups typically had a whistleblower member. A whistleblower who had been worked over by university and Government administrators would likely have balked at requiring whistleblowers to respect confidentiality.

Vigilantes

Much more effective were Congressional hearings chaired by Reps. Ted Weiss and John Dingell. Dingell's hearings in particular were important because they derailed, for a while, the cover-up of cases involving powerful scientists. That they had this effect was largely the result of spadework by Ned Feder and Walter Stewart of NIH on the "Baltimore affair."

Feder and Stewart were not average scientists. They were self-confident to and sometimes beyond the threshold of arrogance. Ned Feder was the boss, Chief of the Laboratory of Biophysical Histology in NIH's Arthritis Institute. The section shrank through personal incompatibilities, and then through starvation by management until it comprised just Feder and Stewart.

They published very little, yet each occupied far more space in *The Science Citation Index* than the average scientist. For a while it seemed that every other cover of a biological journal was a photomicrograph of nerve connections made with Stewart's Lucifer Yellow stain .

Earlier, Stewart had served as a retained referee for *Nature.* In this capacity he reviewed a manuscript by Georges Ungar on "Scotophobin," a compound that Ungar said was found in the brains of rats that had been trained to fear the dark, and that, injected into the brains of mice, made them fear the dark also.

The manuscript was short on detail, and Ungar resisted requests for more. Eventually Stewart concluded that both scotophobin and its effect did not exist. Yet if they did exist they were important, so *Nature* published the paper along with Stewart's criticism of it and Ungar's rebuttal to the criticism, a solution in the much-praised but seldom-followed tradition of open debate in science.

Then there was the case of Harvard's John Darsee and his many fabrications in cardiology. Initial investigations by both Harvard and Emory University, where he had previously been, were ineffective -- perhaps willfully so. As alumni Feder and Stewart pushed Harvard to be more serious, and eventually real investigations showed that Darsee had published made-up results in many papers that he had co-authored.

This led Feder and Stewart to ask what, if anything, Darsee's co-authors had had to do with the papers, pointing out that some of the data were so strange that their falsity should have been obvious.

They described their analysis of coauthor behavior in a manuscript they circulated for comment. It attracted threats of suit from Eugene Braunwald, Darsee's supervisor and frequent co-author, and others. Braunwald applied pressure to NIH upper management to shut them up.

This was only one of many applications of pressure on NIH from the bioscience establishment to "rein them in," to forbid them from working on science fraud. Their institute was willing to do so, but action was blocked by NIH central management, specifically by William F. Raub and Joseph E. Rall, Deputy Directors for extramural and intramural research respectively. Rall wished that they would do something else (he said so often enough) but he genuinely believed in academic freedom, including the right of scientists to do what they thought ought to be done.

Eventually Feder and Stewart's analysis was published in *Nature*. It established the term "honorary authorship" for authors who had had nothing to do with an article they supposedly co-wrote. And it led to time-wasting attempts to codify the duty of co-authors, often with unreasonable demands on how much each should understand of a co-authored article. (A major reason for co-authorship is division of expertise.)

This article advertised Feder and Stewart's interest in bogus science. Whistleblowers started telephoning and writing to them, and they spent less and less time on their research, a long-running snail-genetics project. (This project started as an exercise in using Stewart's Lucifer Yellow dye to map connections between nerve cells. The nervous system of the snails had been expected to reproduce perfectly. Cell for cell in the nervous system, all the snails were expected to have the same layout, a happy supposition that turned out to be true in the round worm Elegans, where it is now the basis of a whole research industry. It was not true in the snails, but Feder and Stewart hoped that sufficient inbreeding would make it so.)

They responded, giving whistleblowers advice and comfort, and sometimes doing detective work. The bioscience research establishment wanted them prohibited from doing this. Such work should be left to OSI, they said. But OSI was a whitewash shop for well-placed accused scientists, and anyhow, the integrity of science is every scientist's business. If some put more time on fraud-chasing than others, if some did it better than others, that was merely division of labor. And as administrator L. Earl Laurence said to Deputy Director J. E. Rall, if science fraud and the covering of it up became publicly embarrassing it would be good if NIH had not silenced Feder and Stewart.

So they were permitted to work on fraud, at first a for set fraction of their time, which they regularly exceeded, and later all their time. For a while the snails were managed in their spare time, but eventually the experiment was dropped and the snails killed.

The Baltimore Affair

An early case was headline material. Charles Maplethorpe, a recent graduate student at MIT told Feder and Stewart that a paper by Weaver et al. in the journal *Cell* was not a truthful account of the experiments it reported. A co-author of the paper was David Baltimore, who had shared a Nobel prize with Howard Temin for the discovery of the enzyme reverse transcriptase, whose existence disproved the assumption that

RNA was copied from DNA and never vice versa. (Temin had been saying this for years, but the particular pair of similar experiments by Temin and Baltimore seem to have convinced people.)

A fraudulent paper co-authored by a Nobel prizewinner was headline material if indeed it was fraudulent. The bioscience establishment had so far blocked any real attempt to clean up bioscience. Perhaps, baited by Nobel-sized publicity, Congress might weigh in on the side of reform.

Was the paper indeed bogus? Feder and Stewart looked into it as well as they could with the limited evidence available. (Stewart is the chief detective of the pair, Feder the diplomat.) The original whistleblower was not Maplethorpe but Margot O'Toole, who had worked with Thereza Imanishi-Kari, a co-author of the Weaver et al. paper. O'Toole had been trying to perform an experiment that followed up on the Weaver et al. paper. The experiment refused to work. A falling out ensued, and Imanishi-Kari exiled O'Toole to mouse breeding. Among the mouse-breeding records were 17 pages of data, including some on a reagent called BET 1. In Weaver et al. BET 1 distinguished clearly between two particular sorts of antibodies. In the 17 pages it did not.

If BET 1 behaved as the 17 pages showed, O'Toole realized, the failure of her own experiments was explained -- and the Weaver et al. paper was certainly wrong and possibly a fraud. She took her questions to Henry Wortis, who had supervised her Ph. D. work at Tufts University, where Imanishi-Kari was slated to go, and to the authorities at MIT. At MIT ombudsperson Mary Rowe told O'Toole that if she wrote up her complaints she would receive a written response to them. But then David Baltimore entered the fight in support of Imanishi-Kari and this assurance evaporated.

At both MIT and Tufts investigations found things wrong with the paper but said that no correction need be published.

And there O'Toole, recognizing the power arrayed against her, let the matter rest. When her fellowship at MIT expired she went to work answering the telephone for her brother's moving company.

Then Walter Stewart started telephoning her. At first she wanted to stay silent, but eventually Stewart convinced her that she had a duty to science to tell what she knew, and she agreed to cooperate. Stewart began studying the case and the immunology on which it was based. In the latter he had the independent advice of an NIH scientist, whom, in the light of what happens to whistleblowers, I do not name. [He is Edward E. Max. (March 5, 2017)]

Imanishi-Kari was later to claim that the BET 1 sample whose data appeared in the 17 pages was a bad batch, an explanation that had long before occurred to Feder and Stewart, but when they asked to see the data on which the figure in the paper was based Baltimore, who was captaining the defense, refused all cooperation.

Baltimore's view of Imanishi-Kari throughout the affair followed a sinuous course. One assumes that as co-author he originally believed in the veracity of Imanishi-Kari's data. Soon after the trouble started, in a letter to Herman Eisen, chairman of the MIT investigating panel, he disowned Imanishi-Kari but said that any warnings about the paper would be delivered privately. Then, in testimony before John Dingell's committee he disowned his letter to Eisen, and reaffirmed his faith in Imanishi-Kari. When ORI said that she had forged data Baltimore said he had placed too much trust in her, and when the United States Attorney in Baltimore MD failed to indict her Baltimore's public faith in Imanishi-Kari began to recover. He changed sides four times in all.

Feder and Stewart wrote up their conclusions for publication. NIH would at first not permit them to submit their manuscript, but with the help of the American Civil Liberties Union they got the prohibition lifted. NIH sent the manuscript to several scientists for review. Most said the manuscript raised serious questions about the veracity of the data in the Weaver et al. article, but that it should not be published

Nevertheless NIH gave final permission for publication. None of the journals approached would do so. The nearest miss was with *Nature,* which kept the revision process going for a year, asking for lawyer-induced changes. Finally, after editor Maddox said again and again that the proofs would be sent next day and failed to send them, they withdrew the manuscript.

Years later, after ORI said that major parts of the data had been forged, *Nature* then published Feder and Stewart's paper.

Dissidents find an ally

With the Baltimore affair Feder and Stewart were able to attract the attention of Rep. Ted Weiss and Rep. John Dingell, the latter being preferred, being, as Stewart put it, a "400 lb. gorilla." Both held hearings.

Their dealings with Weiss had an odd consequence. In a meeting between scientists, not including Baltimore, and Congressional staffers Weiss aide Diane Zuckermann used German citizens' response the Holocaust as an example of educated people's letting bad things happen. (I think she had gotten this from Stewart.) Stewart repeated this analogy at the meeting. Scientists present, already disliking Stewart, and many Jewish, objected furiously. It was monstrous, said Baltimore later, to suggest that cheating in science was morally equivalent to the Holocaust. But this suggestion was Baltimore's invention, embroidered onto a historical observation by a Jewish member of a Jewish politician's staff about willful failure to see evil. Life has its ironies.

So far as I know Baltimore did not claim to be a victim of anti-Semitism. There were Jews and Gentiles on both the teams that eventually formed, Jewish Feder and Gentile Stewart on one side and a Jewish Nobel prizewinner on each.

Baltimore correctly said he was targeted by Harvard men. Many of his critics were connected with Harvard, as his defenders were with MIT. When the case broke he was being considered for president of MIT, and his involvement was no doubt one of the reasons he was not chosen.

Later he became president of Rockefeller University in New York City, but the case led to his resigning under pressure.

In the first Dingell Hearing various whistleblowers told their stories. Margot O'Toole was last. Baltimore was not called to testify. In effect he was hanged in effigy. This is better than being hanged in person, and gave him a great propaganda advantage.

It was the single low blow that Dingell made, and done to deny Baltimore a propaganda platform heavily attended by reporters.

Dingell staffers were accused, with justification, of leaking damning information, but, right or wrong, leaking is part of the investigation and hearing process. Leaked information baits whistleblowers into coming forward. When they hear information that came from whistleblowers they realize that their stories might be listened to as well.

A second reason to leak is to attract reporters to the eventual hearing; a third is to preview things that will emerge with agonizing slowness at the hearing where the question-answer format takes ages to make a simple point.

Information leaked by an investigating committee must be true. Dingell's power would have evaporated had he been unable to back up leaked information.

Congressional interest in the Baltimore affair impelled NIH to do something, so, its standard response, it appointed a committee. (A business executive would most likely have appointed one man to look into the matter and then grilled him about what he found out.) Two of the committee members were or had been associates of Baltimore. Dingell objected and the two were replaced. It was later (1991) discovered that one of the replacements, Ursula Storb, had in 1986 written a letter of recommendation to Tufts University for Imanishi-Kari. Storb stayed.

Robert Gallo -- virtual reality

In the meantime Robert Gallo had attracted media and Dingell interest. Gallo was the American co-discoverer, it was said, of the AIDS virus. But he discovered it a year after French scientists had discovered it, and many months after the French had sent him samples of their virus, two separate strains, it was eventually found out. Had Gallo discovered his virus, got it accidentally by contamination from the French samples, or had he stolen it?

Gallo's virus was said to have been grown in and isolated from a "fruit-salad" culture prepared by co-worker Mikulas Popovic by adding together samples from several AIDS victims. It could be grown in continuous culture in particular human cells. However Gallo's laboratory got its AIDS virus, be it by theft or otherwise, finding this strain of cells was an important discovery that eventually speeded AIDS research. The French still had to keep adding lymphocytes to their cultures because the AIDS virus killed them off faster than they reproduced.

Gallo at first claimed that he could not identify the culture cells, and did not freely share them with other scientists, thus retarding AIDS research. They turned out to be a known strain from the American Type Culture Collection.

Gallo's AIDS virus was eventually found to be the same strain as the second of the two samples that the French had sent him. (At the time they were dispatched the French had not realized the two samples were different strains.) The statistical chance that two independently-gathered samples would be of the same strain were minute. Gallo's virus was the French virus, appropriated by accident or theft.

This information emerged gradually, during which time Gallo's accounts of what had happened mutated. Toward the end, when reminded that he had once said he had not grown the French virus, he said, ". . . there has been confusion in the response of what we did to LAV. In my response during the passionate period . . . 'oh we never grew LAV' and of course we did grow LAV" (5/16/90, OSI interview; transcript p. 87). Had this been said much earlier it might have occasioned surprise, even shock, but by this time people had been desensitized. Gallo had contradicted himself so many times that observers ceased to expect his explanations not to change. From there it was a short jump to ceasing to think that they ought not to change.

Gallo had become a target of Dingell's curiosity, and thus interesting to NIH, so much so that Acting Director William Raub told me that he told Suzanne Hadley, acting head of OSI, to look into the matter and follow it wherever it led. (Why did this instruction need its second half? Because previously, when she looked at evil by well-connected scientists she could not see it. That was OSI's default setting.)

Gallo was an intramural scientist, and OSI was part of NIH, so for science-political protection Raub got the National Academy of Sciences to appoint a committee of outsiders to look into the Gallo case. Then, having wished the committee into existence, he refused it cooperation in gathering information. The committee ended its deliberations in disarray. It found no misconduct but said that Gallo's behavior set a very bad example for other scientists. What else could it say, considering that Gallo's many contradictory statements and apparently sparse record keeping, coupled with the Committee's lack of fact-finding power, made it hard or impossible to find out what had happened in his laboratory?

Years later, after more facts had emerged, Fred Richards, chairman of the committee said, "They stole the virus."

Abbs the penman

Scientist Stephen Barlow noticed that a paper by Abbs, Hartman and Vishwanat on Parkinsonism contained a triple, one-above-the-other "triptych" figure that looked as if its data curves were smoothed tracings of curves in a figure in an earlier paper by himself and Abbs. Barlow complained about this and other features of the paper to the University of Wisconsin, Abbs's employer, and to others. James H. Abbs was a major grant-getter for the university. The university appointed a panel of four faculty members and one outsider. The panel looked at Abbs's confident rebuttal and declared him innocent. His department chairman, Henry Schutta, sent Barlow a letter demanding that he apologize to Abbs and sent copies to 17 people including Barlow's employer, the Boys' Town Institute. Barlow did not apologize. Boys' Town did not fire him.

The story then reached Feder and Stewart, and they got it mentioned in a Dingell hearing.

Also they showed me their file on the case. I had seen NIH hide behind ignorance of physics and mathematics to avoid taking a stand about Professor Mow. What would it do with evidence that was obvious to anyone?

First, could the apparent obviousness be false? I worked out non-subjective methods for smoothing the short wavelength wiggles of the earlier figure and did some simple statistical mathematics. The probability that the similarity between the two sets of curves arose by chance was minute. I sent the results to Katherine Bick, Deputy Director of NIH for Extramural Research.

Then I found that by adjusting the relative size of the two figures all the curves and most of the picture-frame lines of the triptych fell upon each other. The second figure had been traced as a unit from the first. This I also sent to Dr. Bick, who replied that it would be necessary to reopen the case.

The case, partly successful in the end, was to take nine years.

The Outbreak peaks

Justice fared less well in the Baltimore affair and Gallo case. Secret Service experts presented evidence that important entries in Imanishi-Kari's notebooks had not been written when she said they had, but rather added after the paper was challenged. It was later discovered that the notebooks themselves had been assembled, with the connivance of Baltimore and his lawyer, from loose sheets of data shortly before the hearing.

"Secret Service" sounds like "Secret Police," with cloak and dagger, but the Service's presence resulted from a quirk of our Government's organization. The Secret Service is the agency that investigates counterfeiting and forged documents for the Government.

Most of the prosecution case was originated by Walter Stewart, but Secret Service techniques of document analysis uncovered additional evidence. In particular the Service had a very good method for revealing indentations in paper beneath the sheet that was written on to show what had been written.

Also presented at the hearing was evidence that a dark band that tended to contradict the paper's conclusion had been suppressed in publishing a chromatogram.

Baltimore was questioned, among other things, about altered dates in Imanishi-Kari's "notebooks." When asked if he altered dates, he said he did if they were wrong.

At the end of his testimony he asked to make a statement, received permission and attacked Dingell's committee for interfering in matters it did not understand. Such temerity was rare, perhaps previously unknown in Dingell hearings. The subcommittee had given Baltimore the platform it feared. Dingell tongue-lashed him in return, and closed the hearing. The tongue-lashing was pro-forma -- Dingell's heart was not in it.

The effect of this exchange was not immediately apparent.

Impelled by Dingell's interest NIH enlarged its Imanishi-Kari panel, which quite soon found against Imanishi-Kari though two of the five members wrote a minority report expressing doubt about the use of statistical methods to demonstrate that data had been invented. (People who make up numbers have an unconscious preference for certain digits, and the resulting non-randomness is easy to detect.)

NIH handed the investigation to OSI's successor organization the Office of Research Integrity (ORI), which soon issued a draft report for comment by accused and accuser declaring that Imanishi-Kari was guilty, a report soon leaked to the press by Dingell's committee. Peter Stockton, a Dingell staffer at the time, says it was leaked because they feared that the report might be distorted by NIH upper management between draft and final form. Management soon showed its willingness to do so.

The report had been prepared by Suzanne Hadley, who finished reports on Robert Gallo and his subordinate Mikulas Popovic at about the same time. NIH Director Bernadine Healy said the Imanishi-Kari and Gallo reports were incompetent and ordered Hadley to rewrite them. Hadley refused to do so and was taken off the job.

Healy had her own reasons for being angry with Hadley. While Healy was under consideration for the job of Director of NIH Hadley had looked into her handling of a fraud case when she was director of the Cleveland Clinic and found it wanting. (Why Hadley investigated someone who was likely to become her boss is a mystery to me. Hadley had previously shown no inclination to swim against the political stream.) The matter came up in a later Dingell hearing, and Healy defended herself by attacking. To Dingell's standard gambit of asking for clarity because he was a simple Polish lawyer she replied that she was a simple Irish girl.

When OSI became ORI it was moved from NIH to NIH's parent agency, the Department of Health and Human Services (DHHS) in response to Dingell's well-founded suspicion that NIH had improperly influenced OSI.

There is a funny story about this. In the chain of command above OSI had been the Office of Scientific Integrity Review. Its head, Lyle Bivens, once told me of Jules

Hallum and Suzanne Hadley, numbers one and two at OSI "They have to compromise," and that it would be a pity if they were forced out because their successors would not be as good. As head of the new ORI Bivens was their successor, another of life's little ironies.

ORI proceeded with the Imanishi-Kari, Gallo, Popovic and Abbs cases, as well as many that were less known. Healy attacked ORI in public statements, calling them "Keystone Kops," and a further new element had entered the picture. The legislation that changed OSI to ORI made the latter's decisions subject to review by DHHS's Departmental Appeals board (DAB), which was to figure more and more in everyone's calculations as time passed.

Facts vs. propaganda and censorship

In the eyes of scientists Baltimore won his confrontation with Dingell. The stage had been set by his supporters in the bioscience establishment and a cooperative science press. The former portrayed Dingell as a modern Senator McCarthy and the latter broadcast this opinion. *Science* and to a somewhat smaller degree *Nature* had been consistently anti-whistleblower. Daniel Koshland, editor of *Science* said that 99.9999% of scientific papers were free of fraud. An item in *Science's* "Random Samples" section made fun of Robert Sprague. These journals were open to pro-Baltimore letters to the editor and nearly closed to the opposition.

Almost the sole exception to this bias was *Science* reporter David P. Hamilton. As a reporter for the student newspaper at MIT at the start of the Baltimore affair he had uncritically accepted the version of events presented by authority. It seems the experience taught him skepticism. Not long after the Dingell hearings he was hired away from *Science* by *The Wall Street Journal,* the paper most stridently in Baltimore's corner, and sent to Japan. He was not replaced by anyone of similar independence, though Ellis Rubinstein, publisher of *Science* invoked his name to me as evidence of the magazine's independence, yet another of life's ironies.

At first *Science* minimized the Baltimore affair. The problems with the paper were errors, nothing more. As the evidence mounted that there was indeed more *Science* became schizophrenic, sometimes reporting fairly straight what people had said, sometimes treating the affair as a joke. But never did it report results of its own thinking. Thus news editor Barbara Culliton told me that she believed that Imanishi-Kari had forged data, and that she would report thus if a Government body came to the same conclusion. (As editor of *Nature Medicine* she recently wrote that the Baltimore affair was neo-McCarthyism. The vicar of Bray would understand.)

One of Baltimore's arguments was that the paper must be honest because its findings had been confirmed (itself a disputed point). It is a standard data forger's defense, and is like a counterfeiter's saying that his home-made twenties are good because they look like those issued by the Treasury. *Science* and *Nature* let Baltimore's logic pass without comment.

Science and the AAAS

Science is published by the American Association for the Advancement of Science (AAAS), some of whose members are more equal than others. David Baltimore was on the editorial board. (His wife was recently on the Board of Directors of the AAAS.) So was E. Margaret Burbidge, with whom I once had a nodding acquaintance, and who, as an astronomer, should be clear of the politics of bioscience. When I wrote to her c/o the magazine to complain about a piece of biased journalism, *Science* intercepted my letter. It did not reach Dr. Burbidge. The Letters Editor told me that *Science* changed this policy shortly afterward.

Newspapers

At *The New York Times* ex-*Science* reporter Philip Boffey wrote a short article that explained the case as well as could be done in a few words, and Philip Hilts, who looked into the affair and wrote "The Science Mob," strongly anti-Baltimore, for *The New Republic* (May 18, 1992).

Newspapers without science reporters were easy prey for propagandists, and Baltimore had the most propagandists. *Washington Post* reporter Malcolm Gladwell wrote what the establishment told him. The rival *Washington Times* ran the expected anti-Dingell editorial, but then something odd happened. It sent reporter Diana West to the Dingell committee to measure its fangs. By this time Dingell had borrowed Feder and Stewart from NIH, and Stewart was in the subcommittee office. Stewart explained; West listened. She interviewed other people, me for one, and as many as would talk to her at MIT. Baltimore would not talk. Baltimore supporter Bernard Davis told her he had made up his mind without learning about the case. With a Nobel prizewinner on one side and a postdoc on the other he said he needed no further information.

Mark Ptashne supports O'Toole (and what happened to him)

She interviewed Mark Ptashne, a one-time Baltimore defender who had changed his mind after seeing the evidence. Ptashne supported O'Toole but was unwilling for his name to be used. Later he got O'Toole a job in a biotechnology company he had helped to found. Also, he became somewhat more public in supporting O'Toole, but then NIH told him that his 5-year grant was being terminated after 2 years. He got back some of the grant, but for a long time said nothing in public about the Baltimore affair. Still more recently he got his voice back. Having had trouble with Government support he had moved from Harvard to Memorial Sloane Kettering Institute, which has its own money.

The result of West's investigation was a two-part article favorable to O'Toole that contradicted the previous editorial. (*The Washington Times,* bankrolled by the Reverend Moon's Unification Church, neither needs nor hopes for establishment approval.)

West did not fare as well with an article about Margot O'Toole for the *Reader's Digest*. All seemed well; the article had, as expected, gone through several stages of revision. Then the magazine canceled her contract.

Other support for O'Toole came in Daniel Greenberg's *Science and Government Report* and in circulated letters by Serge Lang, a Yale mathematician who has long been worried about rising dishonesty in academia. Far fewer people saw these than read pro-Baltimore op. ed. pieces and letters to the editor. Maxine Singer made the publications of the National Academy of Sciences available to Baltimore while the Congress was lobbied by paid professionals on his behalf. This lobbying reached the Republican minority members of Dingell's committee. At one hearing Rep. Norman Lent of New York attacked O'Toole for being disloyal to her boss, Imanishi-Kari. He said that had someone someone who worked for him done the same thing the person "would be out of there in flash," and "wouldn't be rehired by anybody that I could call up," thus showing how to run a blacklist without creating a paper trail.

Baltimore's lawyers' bill goes to the taxpayer

Dingell later investigated misuse by universities of Government payments intended for research overhead. Substantial sums were returned, including $68,000 (I think the figure was) from Baltimore's Whitehead Institute at MIT, money paid a Washington law firm for the above lobbying of members of the Dingell Committee and billed to the Government. Baltimore said the billing had been an oversight.

Baltimore propaganda exploited pack loyalty, "us against them." Dingell was meddling in things he did not understand (which was true of Dingell, but not of Stewart). The specter it evoked was of Congress ignorantly attacking science. Scientists trusted *Science* and *Nature,* which fed them this false story -- to the extent that at a conference on orthopedics and biomechanics an old friend, out of the blue, told me what a terrible man Dingell was.

It was strange, an inversion of the Church vs. Galileo. Dingell, the politician, had facts on his side. Baltimore, the scientist, had the Dingell-equals-McCarthy myth, and the ability to censor his opposition in the places that counted most.

Dingell gives up

Eventually it became clear that Dingell had privately conceded defeat. Perhaps his advisors convinced him that the contest was unwinnable, or not worth winning. He had, after all, invested time and prestige in trying to make science better for rank and file scientists, and their thanks was to believe that he was the new Senator McCarthy. Further, the election of William Clinton as President made the Department of Health and Human Services part of a Democratic administration, and thus not to be attacked by a Democratic congressman.

A warning of the change was felt by Margot O'Toole in the impact of a file box thrown across a conference table by a Dingell staffer at a meeting. (He said it was an accident.)

Whistleblowers, in science or not, who testified before Dingell's committee were likely targets for retaliation. With this in mind Dingell made a practice of saying that the committee took a continuing interest in those who had testified before it. His loss of interest in scientific misconduct put whistleblowers like O'Toole into a free-fire zone.

When the Democrats lost control of the Congress in the 1994 elections Dingell lost control of his subcommittee, and his power vanished, but he had lost interest in cheating in science two years earlier.

Proof of his loss of interest had come in the Oates affair.

The Plagiarism Machine and historian Oates

Because authorities had again and again looked straight at fraud and seen nothing Feder and Stewart decided to concentrate on a form of fraud the authorities could not pretend not to see, plagiarism. Universities had for years expelled undergraduates for plagiarism. Surely they could not ignore it when done by professors.

So after a false start trying to use computer software designed to allow lawyers to compare different drafts of the same document (it is good for finding differences, not for finding similarities) Stewart, using principles developed for comparing strings of genes in DNA, wrote software to find identical strings of words in pairs of documents. Thus was born NIH's plagiarism machine, perhaps not the first plagiarism machine, but at the time the most famous.

The professoriat, or at least its self-appointed protectors, reacted like Count Dracula seeing the first light of dawn. Lawyer Robert Charrow told the professors what they wanted to hear in his column in the *Journal of NIH Research*. The machine, he wrote, was of no use even for scanning for plagiarism, a statement precisely false because that is exactly what it does. More irony:-- the machine provides the only certain way to prove someone innocent of word for word plagiarism. Only its untiring eye can guarantee that no sequence in a particular document is the same as a sequence in another particular document.

Not much turned up in early trawls through the scientific literature. Scientists are more likely to steal ideas than words, except when they lift entire background sections from other people's grant applications to use in their own applications on the assumption that no real harm is done, and anyhow, who will know? (The original author will, and plagiarizees have received plagiarizers' grant applications for review.)

Historians use words, thousands, tens of thousands, hundreds of thousands of them, that take much effort to assemble. Enter Steven W. Oates, historian. Oates is a repackager. Much of his output is paraphrased from other history books. Oates does not deny this. An article about him is illustrated with a photo of Oates with an open book and his word processor in front of him. He writes for non-experts, and his many books sell well. Evidently he fills a need.

But repackaging is frowned on in the profession. A historian is expected to work directly from original materials (documents, photographs, and now recordings). The

American Historical Association (AHA) includes paraphrasing in its definition of plagiarism.

Historian Michael Burlingame said that much of Oates's biography of Abraham Lincoln, "With Malice toward None" was paraphrased from Benjamin Thomas's "Abraham Lincoln". Oates threatened to sue Burlingame. Burlingame asked Feder and Stewart to check Oates's output for plagiarism. With the knowledge of their superiors, they did. (Perhaps the superiors thought it would distract them from making trouble in science.)

They found that short bits were identical in the Oates and Thomas texts, many more than occur in truly independent compositions. Paraphrasing is like trying to walk along a narrow valley without using the exact route of a previous traveler. In most places one can, but there are spots where only one route (i. e. phrase or word) is possible.

In one delightful spot Oates turned Thomas's, "Day after day the temperature rose no higher than twelve below zero. For nine weeks the snow lay deep" (Thomas page 21) into "For nine weeks the temperature held at about twelve degrees below zero" (Oates page 15). Oates's version is meteorological nonsense. Imagine a thermograph record that is a horizontal line at -12 degrees F for 63 days. The need to use different words to express the same facts led to altering a fact.

Oates fell for a dramatic image in a different book. He took the phrase "dark clouds of inferiority in their little mental skies" from Martin Luther King Jr.'s autobiography, changed "their" to "her," for King's daughter, and used it without attribution in his own biography of King.

Healy silences Feder and Stewart

Feder and Stewart wrote up their conclusions and sent them to the AHA. Oates's response was to write to Senator Paul Simon of Illinois, who had himself written on Lincoln, complaining about the use of health research facilities and money to harass a historian. Simon wrote to NIH Director Bernadine Healy relaying the complaint, and within days Feder and Stewart received a memo from L. Earl Laurence, top administrator of the National Institute of Diabetes, Digestive and Kidney Diseases, informing them that they were being separately transferred to other duties.

They fought back with a flurry of memos to officials within NIH and within DHHS, but to no avail. They no longer had protection near the top of NIH. Director Healy, in a classic purge sequence, had got Deputy Director Raub to fire Rall as Deputy Director, as she looked on in the same room, and a few weeks later she fired Raub. Nor did Feder and Stewart have Dingell's protection. Dingell sent Director Healy a pro-forma letter of objection, but from its wording experienced Dingell-watchers could tell that it was not a true Dingellgram. It was not intended to be acted upon.

Dissidents such as myself visited people, telephoned people and wrote letters. Senator Simon was sympathetic once his staffers were told that he had stumbled into a charged situation. He wrote to Director Healy that he had not wanted the Feder-Stewart anti-fraud operation shut down; he only wanted them to stop bothering historians. All efforts availed nothing. The bioscience establishment had what it wanted and would not give it up.

It was clear that pressure had been applied right at the top. NIH officials said that the matter was out of their hands. All decisions were being made "downtown." i. e. at DHHS headquarters.

Note that some officials were less supine before the bioscience establishment than they might have been. Feder and Stewart were not fired from Government service. Though outsiders wanted it, and insiders proposed it at times of exasperation, it did not happen. [I have since been told that Senator Grassley would not let NIH fire them. Aug. 15, 2016.]

Walter Stewart started a hunger strike that lasted for 33 days. Probably this was counterproductive. Hunger strikes are acceptable if done by politically-approved people far away in distance and time, Gandhi for example. Done right now by an

American a hunger strike suggests self-importance and presumption, and a disquieting whiff of strangeness.

I do not criticize. Stewart sensed, rightly, I think, that the act of silencing himself and Feder was a declaration of victory by the bioscience establishment, and that if it were not reversed reform was dead for the foreseeable future. If there was a time for desperate measures that was it.

The plagiarism machine has been, in the words of lawyer Charrow, "locked away," even though it was developed with public money and would be useful to ORI and others investigating charges of plagiarism, which remains one of the recognized forms of science fraud -- a situation that illustrates the priorities of the bioscience establishment and its friends in Government.

Whom to blame?

Feder and Stewart's bosses in the National Institute of Diabetes, Digestive and Kidney Diseases (NIDDK) knew that ORI was not providing justice, and would not do so unless something, perhaps public pressure, presented the bioscience establishment with a find-some-ethics-or-else choice. The Feder-Stewart operation had a chance of evoking such public pressure. NIDDK knew that by muzzling Fewer and Stewart it was derailing a force for ethical reform. Why did it do it?

Start with L. Earl Laurence, Executive Officer of NIDDK, who wrote the reassignment memos. Suppose he had refused to write them. His superior Philip Gorden would probably have written them, and Laurence would have been disciplined. Had Gorden refused, Gorden's superior would have probably written them and Laurence and Gorden would both have been disciplined. Appealing the disciplining by arguing that the order was wrong and could not properly be obeyed would have done no good. A bureaucracy that gives an order will not admit that it was wrong. (Can readers think of a single case where a soldier has successfully invoked the Nürnberg laws as a reason for disobeying an order?)

Refusing to do something that one lacks the power to stop is a kamikaze attack with a non-functioning bomb. One can applaud it, perhaps feeling that in the long run it will make the world a better place, but one cannot demand it.

Power to refuse to muzzle Feder and Stewart, or to unmuzzle them, lay in the hands of NIH Director Healy and DHHS Secretary Shalala. I and no doubt others wrote to both without success. Either could have discharged their duty to science and the taxpayer by keeping Feder and Stewart in business. Neither did. Instead they did as bid by powerful people who had things to hide. Further, when she was Chancellor of the University of Wisconsin Shalala herself had crossed swords with Stewart over a big-money case in which Professor De Luca of the University was accused of stealing an idea from a paper he was reviewing. The case was settled out of court for a secret sum.

Healy, Shalala and those who told them what to do are the people to be blamed, and behind them the system that puts such people in positions of power.

An ugly scene

In all my telephoning to whomever would listen in NIH and DHHS upper management the only useful response was from Philip Chen in NIH, who said a starvation expert happened to be visiting NIH and ought to look at hunger-striking Stewart. A visit would be arranged. Later, embarrassed, Chen said he had been ordered not to proceed. He did not say by whom.

The fall of the fall guys

Perhaps unaware that Representative Dingell was its umbrella, its shield from science-political storms because his interest forced the bioscience establishment to at least pretend to do something about crooked science, and also that he had lost interest, ORI continued with the Imanishi-Kari, Abbs and Gallo cases.

Margaret O'Toole told me that ORI's continued investigation of the Imanishi-Kari case was vigorous. "They were tigers," she said of their investigators.

ORI rewrote history

But there was an oddity. The investigators did not talk to Walter Stewart, who knew a great deal about the case, and in their final report his name did not appear, not even in the narrative description of the history of the dispute, an absence that rendered their account partly false. ORI was playing politics.

Baltimore claimed to be a victim of a politically-inspired witch hunt, and he pointed to Stewart as the prime hunter. Stewart was unpopular with the bioscience establishment for washing its dirty linen in public. So ORI left Stewart out. Politics were more important than truth

Where does such thinking stop? Where is the threshold where truth is grave enough to be more important than politics? This is a slippery slope that ORI should not have moved an inch down. (Did it occur to ORI people that in pretending that inconvenient events never happened they were behaving like the politicians in the USSR?)

Not talking to Stewart led to blunders in their presentation of the case.

ORI's final report declared Imanishi-Kari guilty. She had also been indicted by the United States Attorney in Baltimore, MD. David Baltimore said he had trusted her too much. Editorials in journals and newspapers spoke of Baltimore's tragic flaw of arrogance.

Imanishi-Kari appealed to the Departmental Appeals Board (DAB).

A warning and a reversal

In its final reports ORI found both Gallo and Popovic guilty of misconduct. Both appealed to the DAB. ORI pursued the Popovic case first. In the meantime, in response to a motion from Gallo to dismiss his own case, the DAB sent a letter to ORI asking that it prepare an "offer of proof." The letter described what ORI would have to do to prove Gallo guilty. I have read this document. Not explicitly stated, its message was nonetheless clear. Whatever evidence ORI presented, the DAB would exonerate Gallo.

In spite of this sledgehammer hint ORI provided the offer of proof. Then the DAB decided against ORI in the Popovic case. The language of the decision was insulting to ORI. Its reasoning was legalistic. Nothing that Popovic may have done wrong was "material." (Had I been the board I too might have exonerated Popovic, but on the ground that he was as honest as he could have been and still kept his job.)

ORI dropped the Gallo case. (Does an agency that drops a case because it knows the judge is fixed retain the moral standing to pursue any case?)

Next the United States Attorney in Baltimore dropped the case against Imanishi-Kari. Her lawyer in Baltimore was Benjamin Civiletti, an ex-United States Attorney General and a power in the Democratic Party. David Baltimore began to reassert his faith in Imanishi-Kari.

ORI had lost every case it had tried before the full Appeals Board. Yet it did not drop the Imanishi-Kari case. The Office of the General Counsel of DHHS told ORI it had a good case and should proceed. DHHS lawyers must understand the DAB as well as anyone does. Was this honest advice, or was it bait?

I did not attend the hearing, but I have read all of the 6500 page transcript. Right at the beginning the Board posed ORI a surprise problem. It refused to let ORI introduce its report as evidence -- saying that the case was to be tried de novo. Then it said it did not want to be "walked through" the case in detail. How, then, was it to present its case, said ORI, and was never clearly answered by the DAB. Arcane interpretation of the rules of evidence made it even harder for ORI to introduce matter.

Ned Feder, who was there some of the time, said that ORI's lawyers acted dispirited. Can one blame them? According to Peter Stockton the Justice Department had been asked to lend ORI a hot-shot litigator, but refused. Judged from the transcript ORI's lawyers Marcus Christ and Steven Godek were far from incompetent, but they must have felt that the hearing was a formality that had to be gone through before the predetermined verdict was announced.

The DAB exonerated Imanishi-Kari in a decision that was savage in what it said about ORI. Thus, "The panel found that much of what ORI presented was irrelevant, had limited probative value, was internally inconsistent, lacked reliability or foundation, was not credible or not corroborated, or was based on unwarranted assumptions." I was surprised at the verdict. People with better political antennae were not surprised. Earl Laurence had earlier told me that Imanishi-Kari would be found not guilty, and (a bonus) that O. J. would walk. Some insiders were told what the verdict would be months before it was announced.

Baltimore turns his coat the fourth time

Baltimore's faith in Imanishi-Kari recovered completely. Some editorials spoke of a triumph of justice. Others, more cautiously, said ORI was poor at its job.

Facts do not count with the Departmental Appeals Board

I was worried by the decision. Could I have been wrong? I got ORI's investigation report, the DAB decision, ORI's rebuttal to the decision, and later the entire transcript of the hearing. (ORI was so upset by the decision that it was willing to talk to me.) I had not been wrong. Imanishi-Kari's lawyers Joe Onek and Tom Watson were indeed witty and charming, ORI's Christ and Godek sometimes awkward and peevish, yet on the whole they presented their case well. But the Appeals Board was not listening. Imanishi-Kari would have been exonerated had she been prosecuted by Daniel Webster and defended by Harpo Marx.

Repeatedly the board accepted far-fetched explanations of things for which the obvious explanation was fabrication. One example: The DAB said it was impressed with Imanishi-Kari statistical expert Terence Speed's objections to ORI expert James Mosimann's mathematical analysis of Imanishi-Kari's uneven digit distribution but it never mentioned that Speed himself agreed with ORI that the unevenness resulted from human intervention. Imanishi-Kari had earlier admitted this, blaming it on "casual" rounding of the numbers. However ORI presented 66 cases of her rounding, and they were sloppy but not weird in a way that would have explained the digit distributions, a fact that DAB left out of its decision document.

Robert Lanman, NIH's top lawyer, told me that before the hearing ORI should have made clear to DHHS the consequences of a reversal of its decision. I agree with his implication that the hearing would be guided by politics, but I believe those consequences were what DHHS wanted.

Whistleblowers beware

With this decision it was obvious that the outbreak was over. The decision told the naïve that all was well in bioscience, and it told the rest that potential whistleblowers must be silent about cheating by or involving their betters or be crushed. If facts could not win her case for O'Toole they will not win a case for you or me.

The obvious dishonesty of the decision gives it the power to shape the future of ethics in bioscience. By brazenly accepting cock and bull explanations of incriminating facts the DAB gave warning that facts were not and would not be important. Power and influence would decide.

The DAB gave its decision on June 21, 1996. In July of 1996 Julius S. Youngner, the scientist member of the DAB panel -- the other two members were lawyers -- was appointed Chairman of the Committee on Ethics of the American Society for Microbiology.

[The Science Practice Group, Joe Onek, Tom Watson and Lisa Greenlees, of the law firm of Crowell and Moring soon disbanded. (Aug. 16, 2016)]

More censorship

If the public knew that raw power had arranged a miscarriage of justice, perhaps the miscarriage could be reversed. I have repeatedly tried to publish the above example, and another, of the DAB's goal-directed statistical stupidity. My modest notes have been rejected. The censors are at work. Even J. E. Rall, who had had the courage not to be intimidated by Eugene Braunwald, would not, at first, look at the evidence (or perhaps not admit to having looked at it). Some knowledge is too uncomfortable to possess. Remember the Holocaust analogy.

After the decision Horace Freeland Judson, author of best seller "The Eighth Day of Creation," about the discovery of the double-helical structure of DNA, wrote an article about Margot O'Toole. Arrangements for magazine publication collapsed.

In contrast, historian of science Daniel J. Kevles wrote "The Baltimore Case, A Trial of Politics, Science, and Character." Pro Imanishi-Kari and fulsomely praised by all but one reviewer that I know of, the book is not trustworthy. An extreme example: Kevles dealt with the incriminating 66 cases of Imanishi-Kari's rounding mentioned above by writing on page 347, "The O. R. I. had not obtained any such information." I wrote to him of his error. His reply proposed to replace the offending sentence with one that ignored the evidence rather than denying its existence -- this from a major historian of science. A paperback copy of the book bought early this year had the original wording.

Where are they now?

Professor Mow is now a member of the National Academy of Engineering, an honor society.

In 1996 Professor Hayes, the man who served on the panel to look into Professor Mow while co-editing a book with him, was on sabbatical leave directing the ARI research institute in Davos, Switzerland. There he distributed to four subordinates copies of three NIH grant applications that were in his possession as a member of the NIH Orthopedics and Musculoskeletal Diseases Study Section. (These applications are privileged documents. They describe ideas for future research.) One subordinate, thinking this unethical, refused to accept the applications and told a friend, Slobodan Tepic.

Tepic complained to the institute's trustees, who forced Hayes to write a letter of confession to Daniel F. McDonald, executive secretary of the Study Section, copy to Harold Varmus, director of NIH. There was no response from or at NIH. Anthony Demsey, head of study sections, was told nothing. In the meantime Tepic resigned from his job under pressure. Only four months later, after Tepic wrote to Varmus, did NIH investigate, and, I heard informally, drop a plan to extend for a year Hayes's and others' service on the Study Section. Harvard too investigated. Reports of both investigations are secret, though Harvard, but not NIH, wrote an inquirer that it concluded that Hayes had done wrong.

Hayes is not debarred from receiving NIH grants, but I understand that the Arthritis Institute, the institute most likely to put him on a study section, has been told of his misbehavior. There has been no official announcement of his misconduct or any actions in response to it. More recently he was proposed for membership in the National Academy of Engineering. He was not elected, but starting July 1, 1998 he has been Vice-Provost for Research at Oregon State University in Corvallis Oregon.

Abbs, a champion grant getter with the full support of his university, sued NIH, claiming that OS/RI had violated his constitutional rights. He won, but lost on appeal.

Then for years nothing visible happened. I thought Abbs had protection, and wrote ORI a succession of needling poems in large print on postcards. To my initial surprise Abbs finally signed a voluntary exclusion agreement. Then I looked at the text of the agreement. Abbs promised not to serve on Government advisory panels for three years and accept monitoring of any research for that period. But he is not debarred from receiving NIH grants. Perhaps he did have protection.

Why, with a good case, did ORI settle with Abbs for wrist-slap penalties? Perhaps because it feared that he and his university, which had fought hard for him, would otherwise appeal to the DAB and receive the Imanishi-Kari treatment.

On the other hand, why did it not drop the case entirely? Perhaps because, in 1989, when OSI seemed to be dragging its feet and need prodding [I had been telephoned by Martin B. Blumsack, lent to OSI by NIH, who said that the case had been so mishandled that it might have to be dropped. (April 11, 2017)], I published the disputed figures and their superposition in *Neurology*. Abbs replied. A year later *Neurology* published an accusing letter by neurologist Gary Weismer with reply by Abbs. The evidence was thus well known in the discipline, and so obvious that it may have shamed ORI into sticking with its prosecution.

Few editors are as accommodating to dissent as *Neurology's* Robert Daroff. Writing to him later I asked him why, and said that my own actions may have resulted from reading too much Nevil Shute and Dashiell Hammettt. (The latter's writings especially concern the successful fighting of crooked city halls.). The mail soon brought me a little package from Daroff, a biography of Hammett.

Daroff said that when the first exchange was in the making he asked Abbs if he would sue. No, said Abbs, he would just write a rebuttal. I salute Abbs for his decision.

Henry Shutta, Abbs's old chairman, says that Abbs is innocent, a victim of trial by Dingell.

ORI again rewrote history

ORI's report on Abbs described impressive work to demonstrate that he could not have taken the data that it said were fabricated. In addition, however, it accused Abbs of something he had not done, and that ORI must have known he had not done. In his lawsuit Abbs had claimed that NIH had investigated, cleared him and closed its case, which was therefore *res judicata* (in English, "something decided") when it started probing him again. NIH claimed that the investigations were one; that the case had not been closed between them. But an NIH document prepared between NIH's two bursts of interest gave the status of the case as "closed." Abbs had a right to see this document and I had a copy of it. (I forgot that I also had a memo from Katherine Bick that referred to NIH's "previous conclusion.") I asked NIH to send him a copy. A Department of Justice lawyer threatened me with unspecified harm if I did not keep silent about it. I told NIH to send Abbs's lawyer a copy or I would do so. NIH finally did, but long after the deadline I had set, and after I had sent a copy.

ORI's report on Abbs is consistent with NIH's false position.

Though it gives a glimpse of a better world that might have been, the Abbs case is a sideshow. Center rings are the Gallo case and Baltimore affair, and the disposition of these showed that the outbreak had been crushed.

Gallo now runs a tax-supported institute of virology in Baltimore. David Baltimore is the new president of Caltech, the small but very good university in Pasadena, once home to Richard Feynman. Search Committee chairman Kip S. Thorne said the University wanted a someone with clout. It got one.

When Imanishi-Kari was exonerated Philip Hilts, who had written "The Science Mob," was called on the carpet at *The New York Times*. He kept his job, barely, but no longer writes about cheating in science.

The great silence

Indeed, few people write about cheating in science in the U. S. A. *Science* and *Nature* tell of cheating in England, on the continent of Europe, in India, Japan, Australia, every where, it seems, but seldom here. Has cheating stopped in the U. S. A.? Not likely. It is not news fit to print. Thus I have been told by *Science* that Wilson Hayes's misuse of others' grant applications, plus NIH's unwillingness to investigate the event and concealing that it happened, and the punishing of whistleblower Tepic, are "not a story." (And because it and similar events are not published, editors point to the lack of news as indicating that scientists are not interested in misconduct. Are you listening, Joseph Göbbels?)

Donald Kennedy, ex president of Stanford, who resigned following the revelation that Stanford had charged the Government for, among other things, all or part of a yacht, a shopping center, silk bed sheets and an antique fruit wood commode -- Stanford gave the money back -- is the new editor of *Science*. An evident believer that top people can do no wrong, he seems likely to tighten the censorship.

Behind this wall of censorship universities and other research institutions are free to deal with accused and accusers as they wish. Until recently the Office of Research Integrity could pursue little crooks while rubber stamping university exonerations of big ones. (Big fish were safe because ORI expected that, absent fuller public understanding of the evidence than is likely, the Departmental Appeals Board would exonerate anyone who could, or whose university would, pay lawyers to make the appeal.)

Now ORI only has the rubber stamp (though it seems to peek a bit at what it is stamping). Investigations are done by the universities or other research institutions where the alleged misconduct occurred, and any complaints made to ORI are forwarded to the institutions. If they wish the institutions can ask the Office of The Inspector General of the Department of Health and Human Services to do the investigations. Who else can summon the services of the Inspector General is not clear, except that I have been told that whistleblowers are not able to.

ORI is supposed to be informed of all cases of alleged misconduct, but this does not always happen.

ORI may even lose its role as czar of ethics courses. Establishment associations have complained to Congress that the courses are an "unfunded mandate," adopted via an illegal short-cutting of administrative procedure. Is this just the victorious establishment dishonoring an expensive, time-wasting commitment made as the price of receiving total control of fraud investigations?

Or do university lawyers see that these courses set out codes of practice that professors routinely break, and are thus weapons that exploited underlings can use to sue professors and universities for damages? Do not bet on the underlings' success. Power will have its way. Supervisors will take credit for supervisees' work, and those who object will be frozen out of science -- along with those who oppose the reigning cabals.

More money for science would not upset this applecart. The warlords would just award it to each other. The outbreak has been contained. The new dawn has been indefinitely postponed.

An analogy

My father used to say that no socialist experiment long outlived its founders. He was thinking of small-scale trials like the Oneida Community, but he turned out to be right about nations as well.

He reasoned that only the original zealots could keep their followers from succumbing to the temptations of selfishness. I think that equally or more important is that the new generation of followers grows up under the new system, and whereas the old generation saw communal property as a trust the new one sees it as something to get for oneself before someone else does.

State-supported bioscience lasted about as long as the socialist states of Eastern Europe before brazen exploitation of the system became obvious in both. The eastern European regimes collapsed because the ultimate repression apparatus, the Red Army, was controlled by Mikail Gorbachev, who would not use it to keep thugocracies in power.

No such reluctance existed in the U. S. A. when dissent arose in bioscience. As happened after the brief "Czechoslovak Spring" of 1968, the full power of DHHS and the establishment was eventually used to crush dissent.

The analogy is not perfect, and therein lies hope

American bioscience is now in a Brezhnev era, but it differs from the USSR in an important way. The USSR took care of all its citizens, not always well, but it recognized an obligation to do so. Science does not. Older scientists have long sponged on the labor and creativity of the young trainees in their laboratories, trainees who became older scientists and sponged in their turn. The expectation of becoming exploiters kept the exploited docile.

As each older scientist, on the average, trained several Ph. Ds., fulfilling this expectation required continuous expansion of university science departments to absorb them, an expansion much faster than that of the population as a whole. This expansion has now slowed drastically. Many scientists entering middle age now have a choice of joining Marx's "reserve army of the unemployed," or the migrant labor pool of permanent postdocs, or industry, or finding a different line of work. Industrial scientists are out of the war-lord world of Government-supported science (and, incidentally, into one where employers' response to being lied to by scientists is immediate and volcanic.) Further, more and more undergraduates are choosing not to enter the pipeline leading to exploitation and uncertainty, and those who do will have scarcity value and be harder to exploit.

The outbreak was a peasants' revolt that tried to force good ethics upon a warlord class that believed its personal superiority entitled it to the power it wielded, and justified whatever methods were needed to maintain that power. The warlords crushed the revolt, but the power of warlords depends on having peasants to exploit, and the peasant class is getting smaller. Market forces may accomplish what science's dissidents and their political allies could not. Years hence people may look back on the science-warlord era as an aberration consequent on ill-thought out central planning, a mistake that human nature eventually repaired.

Charles W. McCutchen

Feb. 20, 1998 (date of first draft) -- Sept. 14, 2001