INSTITUTE OF CURRENT WORLD AFFAIRS

DRP-10

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Cheating in Biology

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Dear Peter,

October and November have been two of the best months yet for the development of the Automated Web for Canopy Exploration (AWCE). I traveled to Los Angeles in late September to encourage John Williams, the engineer of AWCE, to move faster on the design. The reasons for the delay in the construction of AWCE were a subject of DRP-9. It appears that the equipment may be ready by early January, at which time I plan on returning to Los Angeles to conduct field tests in the Santa Monica mountains. The system can then be shipped to Costa Rica in February.

While in Los Angeles, I was pleased to see my photographic adventure book, LIFE ABOVE THE JUNGLE FLOOR, reviewed by the New York Times Sunday Book Review on October fourth (see attached copy). The result of this review was that my literary agent, Luis Sanjurjo of International Creative Management (ICM), decided to approach editors at several publishing houses with my next book proposal, PRIMATE TEMPLE. This book will be a popularly written challenge to existing theories of human evolution.

Another facet of AWCE is the production of a documentary film about its construction and use. I have been looking for a producer for quite some time and since none have come forward, it was beginning to look like I would have to produce the film on my own.

It is quite remarkable how new business opportunities sometimes materialize. One Sunday morning I was sipping coffee and talking with total strangers at a favorite outdoor restaurant in Santa Monica. Susan Olson, an artist who makes paper from natural fibers such as banana skins and willow bark, joined the group and became interested in my stories about jungle treetops. She introduced me to Charles Pavlich, a documentary producer, who, after hearing my plans, became interested in filming the

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Donald Perry is an Institute Fellow who is developing a new system of access for conducting research in the tops of jungle trees.

building of AWCE. I am now putting the finishing touches on a proposal for a series about adventure and science in the canopy.

I met a number of individuals in Los Angeles who were very interested in AWCE. These people encouraged me to actively work on developing a foundation that would support continuing canopy research. As a result I am now designing a brochure and prospectus for the foundation. I am also looking for officers.

Upon returning to Branchport on October 18th, I was feeling much better about the prospect of constructing AWCE in January and February. Nevertheless, I was still plagued by the thought of beginning the project on a marginal budget with the possibility of funds becoming depleted mid-way through construction.

Between October 20 and 23 I made appointments with editors at four publishers, including Bantam Books and Dutton, to discuss my plans for PRIMATE TEMPLE. To save time I made all appointments for friday, October 24th. This was a hectic day. I rose at 4:30 a.m., drove to the airport in Rochester, and flew to New York. The questioning sessions at the four publishers were much like going through four sets of orals. I returned to Branchport that same evening.

On October 31, Roberta Halsey (my fiancee), her mother, and I went to Nashville, where I delivered a lecture on explorating a giant hollow tree to the American Society of Consulting Arborists. An urgent message was waiting for me at the Opryland Hotel. My agent, Luis, told me several publishers were vying for the rights to the human evolution book. A week of intense negotiation followed, and just when a deal with Bantam seemed imminent, Little Brown of Boston joined the bidding. Little Brown won. As a result, support for AWCE's installation and operation has been assured for a number of months, and I am confident that AWCE will soon thereafter become self-sufficient.

I will now return to some subjects that were discussed in DRP-5, a report entitle "Seeking a Professional Profession." I had said that universities see grant proposals as a means of paying for operations, and they can take up to 50% of a grant awarded to researchers. This is a low figure according to an article written by Daniel Koshland for SCIENCE (234:525).

"...No one really believes that an institution with a 30 percent overhead is very efficient and one with a 100 percent overhead is a bunch of fumblers. Rather, they regard the high rate as a clever way to enrich that institution with federal money. University administrators, spurred by either envy or altruistic passion, then try to get "their fair share..."

Koshland suggests that overhead be limited to 3.5 percent. The chance of this happening seems about as likely as balancing the U.S. budget. Koshland is justifiably worried that "the new tax bill may be particularly hard on private universities" and that they and public universities might see continued raiding of grants as a means of survival.

The grant-proposal-writing game is the flip side of the overhead game, which brings us once again to the "publish or perish" syndrome. I cannot overestimate the importance of publishing to those locked in the academic/science routine.

Publishing, and all it connotes -- a good position at a university, obtaining grants, and ultimately tenure -- is probably the greatest motivating force in the psyche of today's scientists. Thus, researchers have actually become workers for maintaining a given university's operations. The time is rapidly approaching when a wedge will have to be driven between university administrations and the grants of research scientists. Both research freedom and moral values have been eroded by the existing relationship.

The pressure to publish and obtain grants to support universities is so great that researchers are being forced to please administrations, funding agencies, peers, journal editors, reviewers, and more. What is a "high-powered" researcher to do if their laboratory or field work fails to show significant results? The researcher would be a disappointment not only to himself, but also to a slew of others who depend on that work. It is not surprising then that many researchers may doctor data, or resort to unscrupulous means to enhance the "significance" of papers.

Before this is shrugged off as a rare and isolated event, consider a case just reported in SCIENCE (234:534) by Eliot Marshall, where a junior scientist, Dr. Robert Slutsky of the University of California at San Diego, was found to have "fabricated data in three papers, listed coauthors without their permission, and falsified some of his qualifications on a curriculum vitae [C.V.]."

The fallout from Slutsky's alleged cheating is that 68 medical papers could be invalid, and the specter of cheating has spread to include a large number of researchers at UCSD. An interesting point in the article was that for a time, Slutsky was publishing papers at the unbelievable rate of about one every ten days.

"Peters [an investigator of the case] said it is understandable that someone seeking tenure might churn out a lot of papers. 'It's this damned business of counting numbers of papers for promotion, rather than quality.' But for the person on the top, Peters said, 'It really is ego: I have 150 papers in my bibliography, somebody else has so many.' It all comes down to a 'false sense of values,' and 'I don't think any of us are completely innocent of it'." (op. cit.)

Publishing is not a free-spirited and creative endeavor of the academic scientist. Publishing has become a treadmill created by university administrations, and by researchers adept at squeezing the most verbiage out of a data set. "Publish or perish" is the dominant value of academic science; it is the modus operandi of academia.

Dr. Slutsky is just one of a large number of scientists who hold publishing as the highest value. This value, under the constant pressure of the scientific social structure, produces researchers with an insatiable greed for "C.V. expansion" (the addition of papers to one's resume). In a shrewd political move, Dr. Slutsky capitalized on this corrupting influence by crediting a host of fellows and associates with authorship of questionable work without consulting them. Those who accepted these offerings are no better than Dr. Slutsky; by not revealing the farce, they

have been caught cheating on their own C.V.'s.

According to the article, the university has said that the whole affair could have come to a halt early on if some of the fellows (researchers working under Slutsky) had come forward and told what they knew. Whether or not the university's claim is true, it totally ignores the social pressures that operate to silence subordinate biologists. Dr. Slutsky's fellows were possibly worried about getting on the wrong side of senior biologists who had joined in the cheating. If a senior scientist is displeased with a subordinate, it can mean rough sailing for the career of the subordinate. Junior scientists cannot be expected to police senior scientists. Senior scientists that wrongfully accepted authorships must be held to account. As it stands, 38 subordinate biologists and seven senior scientists were stung by Slutsky.

My own experience is that senior biologists come down hard on subordinate biologists who speak their own mind about unflattering subjects of biology. Consider my report DRP-5, entitled "Seeking a Professional Profession." The comments I made in that article were not designed to compliment the scientific social structure at the National Science Foundation (NSF) and elsewhere, although there are many complimentary things to be said about these institutions. More good is always done when problems in these systems are brought into the open along with suggestions for their correction. Therefore, it was not a surprise when, by way of Peter Martin, I received a letter critical of DRP-5 that was written by Dr. Golly, a Division Director of NSF Environmental Biology.

Dr. Golly holds a post coveted by academic biologists. In this position, a senior biologist can have too much influence over who will receive grants for research. I quote from his letter:

"This young man [I'm 39 and not feeling too young] clearly does not understand science and the sociology of science. His ignorance and prejudices are so profound that it is quite clear why he is unsuccessful in obtaining grants and in publishing scientific manuscripts... Unfortunately he expresses a mythology that is repeated over and over among graduate students and younger faculty who really don't want to expend effort to understand the culture they live in and succeed. By repetition these stories become true and are very difficult to counter...I discussed these myths with many colleagues -- only some of whom believed me. It is sad that your Institute unwittingly contributes to the propagation of nonsense."

In my defense I could say that Dr. Golly's ignorance of publications in tropical biology and the needs of tropical biology is profound, or he would have seen a number of my articles (the cover of SCIENTIFIC AMERICAN for example), and he would have heard that I have received grants, and he would see that AWCE could change the way tropical research is conducted. But I believe Dr. Golly knows the importance of my work; he is just unused to a "youngish" biologist conspicuously plucking feathers from NSF's neck.

This brings us back to Dr. Slutsky's subordinates. They were in absolutely no position of power, and they would not have

gained anything by stepping forward to squeal. It is my view, and that of some of my colleagues, that cheating is a commonplace event. Dr. Slutsky was only doing what other researchers have done, his problem was that he was too prolific. He published and perished.

Now I would like to challenge a simplistic value -- that it is wrong to use unscrupulous means to advance one's career in biology. Before you steadfastly answer yes, look closely at some role models of the profession. The father of modern genetics -- Gregor Mendel -- was a cheat. He has been posthumously accused of juggling his data to make the results look better. Charles Darwin, who is often credited with originating the theory of evolution, apparently took the basic principles of evolution from one of his friends, Edward Blyth, without giving acknowledgment. James Watson and Francis Crick used Rosalind Franklin's research without permission to devise the double-helical structure of DNA. They then won a Noble Prize for that work.

Why should less-distinguished scientists be honest when unscrupulous behavior is rewarded with celebrated scientific recognition? Science advances by finding answers to questions about nature. Whether or not a scientist is scrupulous has no bearing at all on scientific discoveries.

Cheating is so respected it has been institutionalized. Senior researchers often expect and demand that their name be placed on papers produced by their graduate students and subordinates. This most prevalent form of cheating is called, of all things, "honorific authorship".

One can run into problems when one uses moral codes to judge Dr. Slutsky's work. Mendel would have been criticized had he been caught fudging his data. Yet the important issue was that Mendel was essentially correct in his conclusions. Perhaps Dr. Slutsky is essentially correct in his conclusions also.

The most pressing problem of the sciences is reducing the temptation to cheat. The investigating committee of the Slutsky case had a few suggestions. "(i) peer review should focus on the quality, not quantity of a researcher's work, (ii) that each department should develop a means to identify 'the type and degree of participation of every faculty author in each published work,' (iii) that coauthorship should 'reflect scientific involvement and imply responsibility for the work reported,' including a responsibility to defend coauthored papers if called upon, and (iv) that the medical school should develop clearer guidelines for supervising trainees and 'realistic' standards of productivity."

The above guidelines are simply restatements of ethics that have always been known. Only one change, however, will reduce this serious problem in our country's institutions of higher education: UNIVERSITIES MUST BE SEVERED FROM THEIR DEPENDENCE ON GRANTS TO RESEARCHERS FOR SUPPORT AND SURVIVAL.

Since universities would not benefit directly from the prolific publications of a gifted writer, the treadmill would slow to a relaxing pace. Quality would then gain in importance.

Even if Dr. Slutsky does not ultimately prove to be an

excellent scientist, he has made a more important statement than most of the papers that are currently being written. Dr. Slutsky's talented effort revealed the depth of the malignancy in academic professions.

Is this malignancy limited only to biology? Other academic professions, probably all of them, suffer the same disease. An article by Colin Norman, in the September 26 issue of SCIENCE (233:1380), embodies several repugnant facets of the academic community. "David F. Noble, a historian who has gained widespread attention for two books analyzing the process of technical change, has sued the Massachusetts Institute of Technology [MIT] for denying him tenure." In one of these books, Forces of Production, Noble "challenged the role of MIT in the development of numerically controlled machine tool technology, crediting an individual entrepreneur rather than the MIT faculty members with key discoveries."

It is true that challenges such as the above do not make friends, but Noble claims that this aspect of his work falls outside of tenure decisions, in the domain of freedom of speech. Tenure decisions must go through peer review. When Noble's application came before the first group of four peers in an open session, he was unanimously recommended for tenure. His application, however, was then sent to another group of peers for a secret vote. Noble was fired.

Peer review, in secret, is probably the single, most-abused ritual in academia. This is where the ugly biases of peers and seniors become law. This despicable mummery should have long ago been abolished.

Noble has charged MIT with infringement of his freedom of speech, and is "asking the court to order MIT to appoint him to a tenured position or, alternatively, to reconsider his candidacy according to proper academic criteria. He is also seeking $1.5 million dollars in damages." (op. cit.) If MIT wishes to rid the faculty of people who exercise their right to speak openly and freely, then the school should have to pay. As more suits are brought against "secret dealings", various universities will have to pay heavily for institutionalizing personnel prejudices.

It is my view that academic social structure is a holdover from feudal times. There is no respect for freedom of speech, and speaking publicly and honestly about controversial subjects is implicitly forbidden, unless the speech supports the "party line." For some reason, my scientific community does not understand the bill of rights.

The power behind the academic profession's curtailment of the freedom of speech is a rigidly structured, social organization. The community has well-defined, hierarchical roles that are reflected in frequently used dominance titles such as senior, junior, fellow, post-doc, and young. Does using these terms help us evaluate research quality? There is one good place for these titles -- resumes.

To conclude, in DRP-5 and this report I have called for an end to several practices in academia:

1) secret peer reviews
2) writing grant proposals for no pay
3) overhead charges by universities
4) blocking the freedom of speech
5) the use of hierarchical terms

The social fabric of academic professions needs a total cleaning, although to be honest I doubt any progress can be made. The "alpha" males/females have a strangle hold on the careers of their subordinates.

Best wishes,

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with our sense of time, and they even wander as (seemingly) far afield as ethical relativism. Their orientation is primarily humanistic, and their book is in part a passionate plea against the use of formal mathematical reasoning as a method for solving mankind's problems.

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lem is that the answer depends on nonmathematical considerations. For example, since one person is ahead and the bet is all-or-nothing, it could be argued that he should receive all the money. Or, since the score is five to three, perhaps the pot should be split in that proportion. On the other hand, given the nature of the initial bet, wouldn't it be fairer to calculate the probability of one or the other winning six times had the tosses continued?

about man's ability to adapt to an increasingly computerized world, but unfortunately their discussion is often too superficial or fragmented to give adequate guidance. Nevertheless, their book does provide an antidote to the Cartesian view that mathematical and scientific knowledge will suffice to solve the central problems of human existence. □

Visiting the Great Potoo

LIFE ABOVE THE JUNGLE FLOOR
By Donald Perry.
Illustrated. 170 pp. New York: Simon & Schuster.
$16.95.

By Roger B. Swain

TREE CLIMBING is primarily a childhood activity, and with good reason. When we become adults, our bodies are heavier than our bones are strong, and we are more likely to be injured in a fall than we once were. Sooner or later, every human being who climbs trees does fall out of one, principally because the human body is no longer designed for an arboreal existence. Our hands are still designed for grasping but not our feet. Our big toes are a shadow of the thumbs they once were. Our soles have been thickened and flattened by the pressures of walking upright. In short, evolution has clipped our wings as certainly as it has the ostrich's.

Whatever the advantages of walking erect, it means that we no longer know what is going on up in the trees. Nowhere is this ignorance more acutely felt than among field biologists, those men and women whose business it is to study other species in the wild. Just as early marine biologists lamented not being able to freely explore the submarine world, so terrestrial field biologists have mourned our loss of tree-climbing ability. It is this loss Donald Perry set about restoring, as he tells us in "Life Above the Jungle Floor."

Consider what is to be found in the canopy of a lowland tropical forest. Preliminary sampling with the aid of an airborne insecticide has yielded so many new species of insects that our calculation of the number of species in the world has had to be drastically revised. Whereas 20 years ago the number was put at 3 million, it has now climbed to a total of 30 million.

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Before this arboreal fauna can be fully inventoried, let alone studied, people must devise a means of getting into and out of treetops safely. Tropical trees aren't like the familiar oaks and maples of temperate forests. First of all, many of them grow taller: 150-foot specimens are common. The first 60 to 80 feet of trunk may be branchless, but this does not mean that it is bare. Trunks are frequently sheathed in a thick mass of vegetation — climbing vines, descending roots, orchids, ferns and moss — not to mention wasp nests, ant nests, snakes, scorpions and other rude surprises. Trees can be climbed using spurs and a safety belt. But it is slow, dangerous, messy work that can be bad for the health of both the tree and the climber.

Mr. Perry, a biologist who has written up his discoveries in various popular magazines, borrowed his exploratory techniques from rock climbing. He strings ropes over lower branches by first shooting arrows into the tree canopy with a crossbow. Once the climbing line is in place, he uses ascenders. These are simple devices used in pairs, one locking onto the rope and holding the climber's foot while the other ascender is raised. Using progressively more elaborate networks of ropes, Mr. Perry has achieved a surprising amount of mobility.

"LIFE ABOVE THE JUNGLE FLOOR" chronicles his experiences climbing trees in the lowland forests of Costa Rica. He has captured both the wonder and the terror of the task. Up among the tree frogs, sloths, kinkajous, parrots and great potoos, the author sometimes forgets for a moment that he is venturing where no one has been before. But at other times, as when he elects to spend the night in the jungle canopy and a storm fells a nearby giant tree, his position is brought home to him. The only way for the reader to get a better seat would be for Mr. Perry to winch the armchair and its occupant into the canopy alongside him.

There may come a time when the human climber moving through the treetops will be no more unusual than a scuba diver swimming through a coral reef is today. Donald Perry, whose goal is to promote canopy exploration all around the world, may someday be remembered as the Jacques Cousteau of the tropical forest. If the present report lacks anything, it is a fuller explanation of what investigations can be conducted up in the trees. But future books will no doubt remedy this. In a first-time visitor to the canopy of a tropical forest, one should always excuse a bit of overenthusiasm about the scenery. Think of it as rapture of the heights. □

Roger B. Swain is the author of "Field Days: Journal of an Itinerant Biologist."