INSTITUTE OF CURRENT WORLD AFFAIRS

DRP-5

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Seeking a Professional Profession

Peter Martin
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Dear Peter,

There is little to report on about the Automated Web for Canopy exploration (AWCE). Amos Bien, who owns Rara Avis, the property in Costa Rica where the station will be built, has a carpenter at the site. The carpenter will remodel a nearby farm-house then start building the Heinz ground station.

I am traveling to Costa Rica in mid-June of 1986 to find the exact site for the station. I plan to shoot a line across a chasm as a first step in constructing a footbridge that will link the station with a newly built road. Lines will also be placed between several trees. In the fall, these lines will be used to lift the steel support cables into place. By stringing these lines, I will know precisely how much cable will be needed for the AWCE system.

I have begun preparations for soliciting a donation of stainless steel cable from steel companies. I will also try to have gas-powered generators donated to the project.

I will be traveling to Los Angeles around mid-August to assist John Williams, the engineer, with the prefabrication of AWCE. A full-scale test of AWCE will be done in a canyon of the Santa Monica mountains to make certain the system functions properly.

John is now preparing a complete list of the materials that must be imported for the project. The list will be sent to the U.S. embassy to be cleared for Costa Rican customs. The materials will then be shipped by air freight, as that is the only way to be certain nothing disappears at the port of entry.

The rest of the report is about why I have turned to popular writing as an outlet for scientific hypotheses, ideas, and newly discovered aspects of natural history, rather than publishing this material in scientific journals. This will also be a

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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.

discussion about research grants and the socio-economic pressures of the scientific community to which I belong.

Perhaps the best way to begin is to relate certain interactions that have occurred between myself and my peers. Not long ago a colleague felt it necessary to tell me to "start doing more field research", as if my decision to concentrate on photo-journalism was ill-conceived. Another incident took place while visiting Cornell University in Ithaca, New York, when I bumped into one of my peers, whom I shall refer to as Jane. Jane works as a biologist at Finca La Selva, a research site in Costa Rica where I also work. She was appalled by one of my recent popular works that appeared in Science Digest, a magazine that likes to squeeze every bit of sensationalism out of a topic without becoming inaccurate. She was compelled to offer this ominous warning: "If you don't quit writing like this you will be very sorry." Long ago I grew used to this negative mentality, so my retort was surely a disappointment. "Wait until you see some of my future work," I replied. I am beginning a book about the evolution of human behavior, and suspect it will illicit some negative response.

I happen to be confident about my popular work, and no one has ever complained about the factual basis of the subjects I have discussed, so I began to wonder what prompted these statements? After considerable thought, I believe the source of frustration for many of my peers is the economic inadequacies of their profession. These inadequacies come into plain view when one examines the traditional process of scientific publication and grant-writing.

From a practical point of view, the only reason to publish in scientific journals is for prestige. Prestige is the reward commodity that organizes the social hierarchy of science. Numerous publications in excellent journals will help an aspiring professor hook a position at a university, while for professors, these articles are potential springboards to receiving large grants for research.

Publishing gives biologists the first competitive taste of professionalism. The only requirement for submitting articles to scientific journals is that the articles must be prepared in the typing format characteristic of that journal. Any and all research is accepted for consideration, but because so many articles are received by these journals, only a small fraction are published. This can take two years.

When the editor of a journal receives a manuscript, it is read to see that it exceeds certain minimum standards. The manuscript is then sent to several reviewers for their opinions. These reviewers usually belong to the author's circle of associates because they are the best judges of the findings that are reported in the manuscript. Reviewers can remain anonymous, because, according to one view, the editor is assured of receiving an honest appraisal of the manuscript. However, a much more likely reason for why reviewers prefer anonymity is that they are highly concerned about peer pressure and the effects of revenge on their chances of receiving a grant or promotion.

For many, speaking "off the record" is almost a code of ethics. When it comes to controversial subjects, biologists

often avoid being candid, even when their opinions are non-committal. A good example of this appears in May 1986, SCIENCE DIGEST, page 32, in a sub-article entitled, "Are Dissenting Views Being Suppressed?". The article is about the possible suppression, by scientific journals, of articles opposing the hypothesis that dinosaurs became extinct after an asteroid collided with the earth.

> "...there is little objective documentation that critical papers have been rejected for improper reasons. Indeed, several scientists on both sides of the issue said they could not cite specific cases of bias or exclusion from journals or meetings, even though nearly every scientist interviewed on this question requested, and was granted, anonymity."

Since biologists can be very touchy about controversial subjects, it does not surprise me that no one gave SCIENCE DIGEST "objective documentation" of the alleged suppression of articles. First, in the vast majority of cases "objective documentation" of bias does not exist in the realm of scientific publication. Second what would a biologist hope to gain by claiming <u>their</u> article was being suppressed? It would be impossible to prove, and ultimately someone would have to step forward and take the heat. This holds absolutely no reward in the biological world, where the best strategy might be to propagate rumors.

This social structure, where biologists cannot safely step forward publicly to take positions on critical issues, is counter-productive to the pursuit of science. More often than not, those who review manuscripts for publication are very biased peers who are competing for the same grants and positions sought by the authors. The shadows of secrecy allow reviewers to be inaccurate and dishonest without repercussions. Further, it occasionally happens that reviewers will dash articles or proposals out of spite, or to actually steal the ideas for their own work. Clearly not all biologists could be operating in this manner or the system would collapse; nevertheless competitors and unfriendly peers should be the last people to see a person's work prior to publication.

Prestige within the group is the only reward for scientific publications; authors are not paid, which is the main reason few outsiders try to publish in these journals. This system, I believe, originated centuries ago when science was the pastime of monks and the well-to-do -- when the social elite dabbled in natural history. There is little chance journals will be able to shake this relic tradition because their readership is not large, and journal sales are for keeping the publication machinery rolling. In fact, the wheels to this machinery are so poorly greased that many journals ask authors to pay to be published. In the event that the author cannot pay, the cost is covered by general revenues.

This archaic, aristocrat-biologist tradition of writing-for-free causes today's professional biologists to give away a substantial amount of their labor.

Normally when field-biologists finish their doctoral

program, they immediately apply for teaching positions at universities, curatorial positions at museums, or postdoctoral fellowships and the like. But with the shrinking enrollments of the post-baby-boom generation, there are few openings for field-biologists with Ph.D.'s. Any job requiring a trained field-biologist attracts dozens or even hundreds of applicants. One is either extremely lucky or well-connected to get any of these positions. The thin icing on the cake is that these jobs may have a beginning salary of less than $20,000 per year. I personally know of several excellent biologists who have not found (and may never find) a teaching position. Many, like myself, are searching for a profession that can honestly be called professional.

What will happen to the glut of disillusioned field-biologists is anyone's guess. Some turn to Wall Street, law, computer sciences, and other well-paid professions, while others may become bartenders or cab drivers. Some remain in the field and eke out their livings by assisting other Ph.D.'s who have grants; the perpetual hope of these unfortunates is that that an academic position awaits them at the end of their ordeal. This will never come true for most.

Almost every college graduate has heard of "publish or perish", which means if beginning professors fail to publish in scientific journals, high quality journals at that, there is a chance their positions will become open to "more successful" biologists. Do not get the idea that "publish or perish" is a process where by the "intellectual cream rises to the top" making it easy for universities to keep their ranks filled with quintessential intellectuals. "Publish and perish" has a purely economic facet. For articles to be written, new research must be done, and for this to take place, grants must be written and accepted. Grants interest universities because they often can claim 50% of these monies under a concept called "overhead." This is one of the avenues through which universities are funded by granting agencies. Thus, not only are beginning professors obligated to teach several classes, they are also expected to help support the school by writing successful grants that result in scientific articles.

From a business perspective, the perspective of a university's administration, scientists are little different than salesmen -- except in one major way. When a salesman lands a major contract he will receive substantial economic reward. In the biology profession, the reward for a grant winner is merely respect within the "profession", along with an expense paid "vacation" to do what is often grueling field research.

Obtaining grants is as competitive as securing teaching positions, and merit is not the only criteria for judging a project. Connections and politics often determine who does or doesn't get the coveted prize. This political side of obtaining grants was driven home when I visited Washington D.C. to interview with Warren Una, Tony Shub, and David Binder for this fellowship. In my spare time I made an appearance at the National Science Foundation to test the waters concerning funds for the Automated Web for Canopy Exploration (AWCE). I was told that unless I submitted a research proposal, there would be no

chance of getting funds for the web. However, I knew that NSF can give funds just for equipment: $100,000 alone was spent at Finca La Selva to build a footbridge over a river; this would have also built a complete canopy research facility.

I could have submitted a proposal to use the web for studies on pollination biology, my area of research interest. I knew however, that my type of research would not receive funds. Why waste several months writing a research proposal only to be rejected by NSF? After all, my main interest has been in designing and constructing a system that others could use for studying the canopy -- a major habitat that remains virtually uninvestigated.

Further revelations of NSF's political nature came through a discussion with one of its own administrators. We talked about an eminent tropical biologist. I think of him as the father of modern tropical biology, as his theories and research seem to have touched most ecological aspects of tropical forests. It is difficult to write an ecologically-oriented paper about the tropics without citing his work. In my opinion he was a shoe-in for receiving a grant, yet after talking with the administrator, I discovered that this biologist's proposal was not going to be funded. The administrator went on to say that his co-workers thought it was funny that the biologist should wear field cloths into the NSF building. "We are looking for new leaders of tropical biology," was his ending comment. Usually I don't mind playing the dress-code game, but at that point my tie seemed tight, and I pulled at the knot.

How, I wondered did this come about? It is difficult to know for sure, but from listening to the "grape vine", I gathered that this biologist's popularity (among some scientists) had declined.

For the majority of biologists, getting a grant is a raffle. After the favorites receive their grants, some are left for the general competition. This is a sordid affair that lures many into spending months writing and rewriting a proposal. They must be careful to cite the right people and not to discredit any work of the many who may be judging the proposal. And one never knows who all those people are. It is an odd requirement since science often progresses by discrediting prior work. Grant winners may not propose the best research, but they are best at walking this tightrope.

Months can be spent preparing the proposal. Following submission, the hopeful wait months for an answer, which is a gut wrenching no for many. It seems idiotic to gamble five to ten thousand dollars worth of professional work time just for a chance to get funding to continue to work. That would be like requiring ten construction companies to each build a foundation and framework for a house after which the purchaser can choose the one he likes best. Is this a system? Spending a few dollars on a state lottery would give a much higher return for the investment of time, that is if a professional biologist's time is worth money.

If I had any power at all to change this system it would be to demand that all granting agencies, public and private, treat biologists as professionals and not ask them to

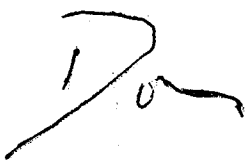
expend unnecessary effort chasing ephemeral grants. People who love their work should not be treated like dogs.

Everyone should be paid for their proposals. To avoid abuse by grant writers, all funding agencies could ask for a simple one page summary of the work that is being proposed. This could be accepted or rejected with no payment. But if the agency accepts the brief proposal, it should then be committed to paying a page rate to have the author elaborate on the project. The more the author is expected to write, the higher he should be paid per page. This would dissuade agencies from encouraging useless stuffing of proposals. Considering how difficult it is to write a science proposal, a starting rate of $100 per page would seem fair.

I would also remove secrecy from the peer review system. Reviewers that are afraid to stand behind their comments should claim a conflict of interest and exclude themselves totally from making judgments.

Will I ever again write for a scientific journal? Perhaps, but will my peers allow me to be published?

Sincerely,



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