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Frontier Connections*

Contactos de Frontera

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ABSTRACT

The author imposes an arbitrary gradient upon scholarly (and particularly scientific) activity- from the curiosity-driven frontier or *exploratory* edge to the application-driven *exploitative* edge. As an exploratory biologist he uses autobiographical materials to illustrate the diversity and significance of personal interactions (*connections*) in providing continuity and coherence to what appear at times to be isolated, private, and perhaps irrelevant pursuits.

Key word: Conections, Scholarly activity, Frontier.

RESUMEN

El autor impone una pendiente arbitraria sobre la actividad escolar (particularmente científica)- desde la frontera o límite de la -pura curiosidad- *exploración* hasta la -aplicación- *explotación*. El como un biológo explorador usa material autobiografico para ilustrar la diversidad y significado de la interacción personal (*conexiones*) para proveer continuidad y coherencia a lo que en ocasiones puede ser aislado, particular, o quizás irrelevante.

Palabras clave: Actividad científica, Exploración, Explotación.

Personal Perspectives

Before starting on this trip I visited my doctor. I was concerned about whether the unusual circumstances might call for extra medication. I explained that I was to be awarded an honorary degree, and that I anticipated a certain amount of stress. My doctor's reaction had nothing to to do with my physical condition, but was a comment on the circumstances. "Wow! You must have some important connections there." I acknowledged that I indeed had important connections in Pisa, and that those connections were very much on my mind. In fact, I had decided to talk about those connections in my invited lecture.

Before starting on that risky exercise, I must express my delight for the privilege of joining you in the celebration of 650 years in the life of this distinguished institution. I also want to say that I am not here strictly as an individual, but rather as a node in a web of connected coworkers who look on from distant places and even from remote times. It is the linkage of that wider academic community to this local community and the events that occur here today that is the subject of my talk.

I chose this unexpected subject because this is a special occasion and because this audience represents all the liberal arts. When scientists speak to each other on normal occasions, we shift into one of our several dialects, which are in a sense international dialects, but which are also comprehensible only to initiates of the discipline. Partly because of the specialized vocabulary, the average scholarly work written in the scholarly dialect is actually read by fewer people than are listed as authors of the work. This limited exportability of specific scholarly activity is especially characteristic of "exploratory" studies - high risk explorations of the unknown, such as I must classify most of my own research efforts. For such reasons I decided that it just might be appropriate for once to dispense with the highly specialized discourse about some exotic program of studies.

My presentation takes the form of an "apology" for exploratory studies, an interpretation of the place of such studies within the scientific culture. Instead of displaying a specialized **product** of my scientific activity. I want to explore the **process** by which more general scientific advance occurs.

***An essay prepared for presentation at the University of Pisa, on October 5, 1994, as a contribution to the celebration of 650 years of distinguished and continuing scholarship.**
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The only way I know to address this subject is in a highly personal way. And this approach has its own problems. Particularly in the sciences, personal experiences are expected to be discounted and ignored. In an effort to promote objectivity, or at least the appearance of objectivity, scientific publications often prohibit personal pronouns and insist on the use of the passive voice. Experiments are expected to perform themselves and data to interpret themselves without human intervention. A scientist who speaks as a person violates conventional standards of behavior.

The discouragement of personal discourse is an attempt to control the all-too human penchant for self aggrandizement and the distortion that goes with it, but it makes dreadful reading and listening, particularly for an audience unfamiliar with the vocabulary, and with limited intrinsic interest in the subject matter. In any case the attempt to obscure the actor in the drama is of limited effectiveness. Human ingenuity overcomes the rhetorical constraints. Scientists often manage to become public figures, celebrities; they are interviewed in news reports and talk shows. They are pictured in newsmagazines.

The attempt of scientific disciplines to limit the effect of personality and to restrict the expression of personal opinions is evidence of an uneasiness about the relative roles of the individual scholar and of the larger community in the search for understanding. And that balance has to be considered when we talk about connections.

A traditional view of scientific advance is encapsulated in the so-called "Great Man" perspective. Scientific discoveries are viewed as unique personal triumphs of exceptional individuals in personal struggles with ignorance and superstition. Understanding is considered cumulative in that later "Giants" stand on the shoulders of Giants who have gone before (See Merton, 1965). In this view the "scientific community" has a negligible role in scientific advance; it provides for the expression of rare genius; it transmits and disseminates the insights of the highly gifted; and it bends the pure understanding of natural phenomena to utilitarian purposes.

This interpretation is now falling into disrepute. In recent years historians and sociologists of science (Barnes, 1985; Brannigan, 1981) have been active in documenting the influence of social and political factors in scientific discovery and invention. They show the considerable dependence of the individual scholar upon both predecessors and contemporaries, upon "major" and "minor" players of the scientific game. They discount the apparent gulf between genius and ordinary talent. Scientific ability is assumed to be variable but with a continuous distribution. The discontinuities in perceived achievements are believed to reflect discontinuities in opportunities. Some students of the scientific process argue that "Discoveries" in fact are social constructs, developed by a consensual community, and are only arbitrarily assigned to an individual. They question whether our understanding of the natural world today would have been very different if Gregor Mendel or Galileo had never lived.

The pendular swings in the emphasis on the roles of the individual and of the society will undoubtedly continue because they represent one of the persistent dialectics of the human condition - like mind and matter, yang and yin, gradualism and saltation, stochasticism and determinism. It seems appropriate, however, on an occasion such as this to celebrate the continuity of this community of scholars without undue emphasis on the Giants who have occasionally appeared here. Most of us are inevitably "small players" in the high dramas, but we are essential at least in maintaining the social fabric. Those high dramas of major discoveries - like Italian operas - do not present conditions under which we would like to live every day, even if they indeed ever existed outside the imagination.

Perhaps paradoxically, I am speaking with personal pronouns about highly personal events while arguing that the personality and individuality of the scientist may have been inappropriately emphasized in the past. I am encouraged to speak personally by the examples of several recent scholars who have rejected the circumlocution and distortion of indirect discourse. The philosopher Steven Toulmin, for example, speaks of metaphysics as autobiography and argues that the pursuit of understanding is inevitably a personal journey. When dealing with human values, the source of understanding should not be disguised behind a pretended objectivity.

In any case, we "small players" are persons too, and the personal experiences of small players suggest how the fabric is woven. I realize that my personal experiences are uniquely mine in detail, but I am persuaded that many of these experiences are generically similar to those of most of you. Individuality is preserved in the special circumstances, but the general characteristics of our intellectual and spiritual journeys are shared.

I am also encouraged to take a personal approach by the examples set for me by three teachers in graduate college, who were willing to share their personal experiences, their values and their hopes with their students. What they revealed through the sharing of their teachings strongly influenced the course of my career. Those teachings were in many ways responsible for my being here today.

Before I can explain what those teachers gave me, I have to explain something about the vessel into which they poured their distillations. At least that is the rationale I offer for going into some autobiographical details.

A Prepared (?) Pupil

When I entered graduate college in 1946 I was a profoundly naive subject, particularly with respect to the ways of science. I was not even very interested in biology, having been in effect drafted into the study of zoology. To explain how I was drafted I have to make an even earlier diversion.

Rude Beginnings:

I had been taken as an infant to live in the then new state of Oklahoma. This area in the middle of the North American continent was flat and sparsely inhabited. Oklahoma Territory was of little interest to the "Americans", i.e., the descendants of the European invaders of America. It was set aside as a territorial prison for native Americans who had been removed from their lands in the east. The discovery of large oil deposits early in this century changed all that. The area was quickly opened to settlement by white Americans, the native Americans were directly or indirectly disposessed of their lands again, and the state of Oklahoma was organized in 1908.

The city in Oklahoma to which I was taken in 1926 was called Wewoka, and the home to which I was taken was on Mekusuky Street. Wewoka - Barking Waters in the Seminole language - had been the capital of the "Seminole Nation". Those Seminoles who had not escaped into the Florida Everglades when the tribe was forcibly removed from their lands in the east had come to rest in what was subsequently Seminole County. The Indians were no longer much in evidence, though their drums could sometimes be heard in the summer evenings.

The landscape of this region had been transformed in the 1920s from prairie grasslands and wooded hills to a forest of miniature Eiffel Towers from which wells were being drilled into the earth. The night sky was illuminated by torches that burned the excess natural gas from the punctured underground reservoirs. Other signs of civilization were minimal. The nearly new wooden houses called forth no memory of times past or ancient civilizations. Oklahoma was a frontier land looking to the west and to the future, not eastward nor to the past.

I evoke the geographical frontier of my childhood as a kind of metaphor of my professional career. Living on a frontier influences one's perspectives in both obvious and in more subtle ways. The social historian Frederick Jackson Turner (1920) attributes much of the "American character" to the experiences of the European colonists moving across the continent. That thesis has come to be challenged, but it has considerable appeal. The frontier experience is transient and not something that can be continued indefinitely, however, in the life of an individual, of a nation, or of humanity. At least the geographical frontier - though it has existed and has shaped values throughout human history - is coming to an end. The scientific statesman, Vannevar Bush, saw in the frontier an experience that needed to be continued for its effect on human values, and he spoke of Science as the Endless Frontier. Geographic frontiers may come to an end, but human ignorance is boundless.

The journey from Wewoka, Oklahoma in 1926 to Pisa in 1994 took place in several stages. Though Wewoka was a frontier city, it was not without some cultural pretentions. Most of the influences to which I was exposed, however, were in the humanities. My father was a protestant minister and had attended college for a couple of years, though he lacked the discipline to complete a degree. He was considered something of an eclectic scholar, and actually wrote some books. He was fascinated by history, and was also something of a sucker for the book salesmen who wandered the plains during the "dust bowl" days of the early '30's selling sets of histories, encyclopedias, and collected works of major authors. I read widely from his personal library as well as in the newly developed public library. History, however, was for me an alternate and indeed a lesser fiction. The names and places and events had no way of becoming real within my frame of experience. It is no wonder that I went into a profound culture shock when I first set foot in Europe - in 1957, and was taken immediately to Chartres Cathedral. I think I would have perished had I gone directly to Rome or Athens.

Toulmin and Goodfield (1965) characterize the cultural transition associated with Charles Darwin as *The Discovery of Time*, the general recognition of the indefinite extension of time, both forward and backward. That realization has had profound consequences for our understanding of our place in the universe and for the formulation of our value systems. As with many fundamental insights into the human condition, we must each make our way through the understanding personally, in a recapitulation of our cultural history. Although I believe that each of us has made that transition, I suspect that I have had more developmental difficulty than those of you who have grown up in sight of the Tuscan hills, in the shadow of ancient heroes.

My first intellectual challenge was an introduction to Latin, in the ninth grade. I was sufficiently intrigued to work hard and was chosen to represent my school in state scholastic contests for two years. Foreign languages were like history to me, however, meaningless exercises of no practical utility. I was unable to sustain interest for very long, probably because I had no early examples such as one provided for me by an Italian - at a genetics conference on Lake Como in the early '60s. Professor Barigozzi welcomed the guests as they arrived, in Italian and French, in German and English, yes and in **American**. In any case, despite substantial effort, I only achieved third place in the contests, and abandoned languages as a lost cause. My introduction to classical civilization stopped before it was well started.

For my undergraduate education I went to a near-by church school in Shawnee, Oklahoma, where the Potawatamie Indians had been assigned lands. I took courses in any subject that looked interesting. I changed majors nearly every semester in a search for a subject to my liking. The only firm decision I was able to make as I approached graduation was that I would like the easy life of a college professor. I decided to major in literature, because I liked to read and because the most interesting teachers at Oklahoma Baptist University were the teachers of English. OBU seemed less a launching pad for an academic career than an escape hatch from the southwest.

Higher Education

The next stage of the journey was to Indiana University in Bloomington, Indiana, in 1946. I noted earlier that it was never my intention to study biology; I was in a sense drafted into the field. The decision to go to Bloomington, particularly to study zoology in Bloomington, like most of the important decisions in my life, was controlled by external circumstances. I was attracted to teaching, and liked the idea of research, but I had no research experience and had demonstrated no special aptitude. Moreover, I had no financial resources with which to purchase an education in a discipline of my choice. I had to earn an education by selling my services as a teaching assistant. And I could find no respectable university willing to offer me a teaching assistantship in English or Philosophy in 1946.

Peculiar demographic circumstances, and a change of field eventually put me in graduate school. I had been spared military service because of my uncertain health, and I was one of the very few students who were ready for graduate school when World War II came to an end. American soldiers were ready to return to college as undergraduates and were financed by the GI Bill of Rights. A remarkably large fraction of these students wanted to become rich medical doctors, but colleges and universities were ill-equipped to accept the flood of premedical applicants. Faculties had declined during the war and few graduate students were in training and available to assist in classes. Anyone with minimal qualifications who could assist in teaching premedical courses was welcome.

When I learned of the shortage of graduate assistants in premedical courses, I applied for graduate study in Zoology. I had a mediocre academic record from a barely accredited school, and I had little training in science. But Indiana University immediately offered me a teaching assistantship. When asked to define the specific field I wanted to study, I wrote "the physiological basis for abnormal human behavior". I think the phrase must have come from a psychology course. My scientific education at this point consisted of one course in chemistry, one course in physics, a course in zoology and a course in the spring flowers of Oklahoma. The decisive factor was that I had had a course in the Comparative Anatomy of Vertebrates, the biology course required of all pre-meds. Never before nor since would a student with my formal qualifications be admitted to graduate study in science at a major university in the United States.

This is a long way around to the subject of the influence of teachers on an unformed student, but teaching must be evaluated on the basis of the pupils being exposed. In universities today we often complain about how ill-prepared our students are. I can't imagine a student much less well-prepared than I was in the usual ways of evaluating preparation. Perhaps my very ignorance was a strength. We are well aware that many of the most significant contributions of scientists are in their early years, before they have exhausted their reservoirs of ignorance. At least some aspects of scientific activity are best conducted by a mind not too well informed about how things ought to be. And some minds can maintain their naivity, their frontier spirit, indefinitely.

Prejudiced (?) Professors

Teacher I: Salvador E. Luria

The first teacher on my list, and the first Italian I ever met, was Salvador E. Luria, (1912-1991) then well on his way to the Nobel Prize that would climax his academic career (Luria, 1984). I took Luria's course in Viruses- along with his student Jim Watson, who would get the Prize even before his mentor. Perhaps the thing I remember most clearly about that class was his response to a student who complained about what he considered excessive class time spent in argumentative discussions. "What is life?" "What is a virus?" "What is proof?". . The student insisted that all he wanted from a teacher was "the facts, just the facts, please". Delicate considerations prevent me from identifying the student who complained.

Luria was gentler than the occasion demanded, indeed far more gentle than was characteristic for him. He explained why he conducted class the way he did: "The primary role of a professor is not to transmit information, but to propagate prejudice. And time spent sharpening words is never time wasted."

I don't know what Italian word Luria was translating into English as "prejudice". I knew that the prejudice Luria was talking about was different from the one that sounded the same in my vocabulary. I knew that Science should be a disinterested search for Truth, and that biases, personal feelings, unprovable assumptions had no place in the Academy. Of course, as a Jewish refugee from a fascist Italy, Luria knew things I didn't know. And he did not use the word to denote a narrow mindless bias. He used " prejudice" to refer to the total armament of the informed and disciplined mind, including values and judgements that cannot be readily articulated. To believe that understanding can ever be totally detached and impersonal is to deny the motive force of the effort, and to distort and misread the process. Luria's "prejudice" was a value system necessary to keep facts in harmony with Truth.

The way in which Luria influenced what I am saying today should be apparent. I am not here primarily to discuss a set of facts, but am reaching for a more elusive but more important goal, trying to understand how we explore the unknown, how we have learned and transmitted learning for 650 years, and indeed throughout human history. We do not follow some secret method or recite some magic formula. We have to use all our resources of intellect and spirit, and we have to be reminded how much the unknown overshadows what we know.

Teacher II:Herman Joseph Muller

The second professor at Indiana who had an important influence on my subsequent career was Hermann Joseph Muller (1890-1967). My first exposure to Muller was at the first social event organized by the Zoology Department after I arrived in Bloomington in the fall of 1946. Muller had just returned from Stockholm where he had been given the Nobel Prize for his discovery of the induction of mutations by ionizing radiation (Carlson, 1981).

Muller was the first Nobel laureate I had seen in the flesh and I found him fascinating. I was reaching another junction in my educational life. I deeply disliked the job whereby I paid my tuition - cutting up phenol-soaked sharks and pointing out rapidly decaying landmarks inside moldy rats. The remedial undergraduate courses I was compelled to take - organic chemistry, calculus, plant physiology, were not subjects I could bring myself to study, much less build a career upon. My grades had fallen below C-level. I was moving steadily toward an untimely and undignified exit from graduate school.

I listened intently to what Muller had to say. And what he said, he said with conviction and passion. I had been exposed to rhetorical passion before in the evangelical church of my parents, but I had never seen it burn so brightly outside a pulpit. Muller's topic was appropriate for the occasion:"The Place of Prizes in Science". Muller declared that one does not pursue knowledge with the objective of winning fame, or fortune, or prizes. The achievement of Understanding is its own reward, a sufficient return for the effort . "If prizes come, " he said, "they are gratefully received, but they are a byproduct of the life of science, not the end product".

I discovered later that the passionate ideals to which Muller bore testimony were sometimes beyond his own implementation. Of the scientists I have known, in fact, he was perhaps the one most preoccupied with priority, with prestige and prizes. Though I soon perceived the messenger as flawed, the validity of the message seemed to me to remain intact. I have never since doubted that the pursuit of understanding is more than an occupation, and that it can be a high calling. That perspective espoused by Muller engaged my idealism, and convinced me that science is not primarily the mindless accumulation of facts that I had previously perceived, but a way of life capable of engaging all one's energies and skills.

This idealization of the scholarly life is perhaps hopelessly outmoded. Certainly the public image of scientists is often carefully contrived to reject such sentimentalities. Jim Watson, my fellow student at Indiana in the 1940s, encapsulated the conventional image of the self-promoting scientist in his book The *Double Helix.* (1968). He develops the persona of his protagonist -"Honest Jim", the "young man on the make", the dirty trickster intent on winning the Prize regardless of the consequences. Such personifications have appeared again and again in fictional depictions of the life of Science, and most of us have observed them in the flesh.

I am not at all comfortable with that image, however. Indeed, I don't even believe that the "Honest Jim" of the *Double Helix* is an accurate depiction of the James Watson who discovered the structure of DNA. Watson takes great pride in reporting "the truth and nothing but the truth"; errors of fact in his account are certainly rare, and inadvertent. But Watson is a master editor, quick to insert the confirming detail, cunning in pruning his images to eliminate ambiguities, converting in effect three dimensions into two. Honest Jim is a compelling caricature of a cynical scientist in part because he was created as a counter-image of the sympathetic and humane person who was the usual expression of that personality.

The main reason for rejecting such characterizations is that they are not consistent with the experiences we usually encounter with our colleagues. The scientific community could hardly continue to function if it were based solely on the cynical self-interest of most of the participants. Certainly no institution of learning could have persisted for 650 years without a central core of scholars committed to shared goals and a sense of community.

Teacher III: Tracy Morton Sonneborn

The third and most important influence on my development as a scientist was Tracy Sonneborn. (1905-1981). He is the one who opened the community to me and demonstrated the practices that hold it together. He is incidentally the one who most directly connects me to the University of Pisa. As with most of my happy encounters, I came to Sonneborn not by choice, but by default.

Shortly after the beginning of my second year in graduate school, I was informed by the graduate dean that I must select a research sponsor and begin independent studies before the year was over, or else terminate my work at Indiana. By then I was convinced that the life of science was, at least in principle, a high calling that could engage my best efforts. But I had no idea about how to go about being a scientist. I still had no specific interests in natural phenomena, and had few skills to apply to their understanding.

Searching for a sponsor, I began to examine the biology faculty at Indiana, and systematically rejected them one by one, sometimes because I found their subject matter hopelessly dreary, sometimes because I sensed a personal incompatibility. The time came when only one name was left on the list, and I dared not ask too many questions. I walked into Sonneborn's office, introduced myself, and asked if he would accept me as a student. I knew that Sonneborn worked on ciliated protozoa, and that he was widely recognized for his studies in cytoplasmic inheritance. I had never met him, and had no idea about what I was getting into. I did not choose to work with Sonneborn because I liked him, or because I admired what he did. I chose him because he was the only choice I had left.

Sonneborn immediately took me into his laboratory, assigned me a desk and a microscope, handed me two test tubes containing paramecia with different mating types, and a laboratory manual on their management. Then he walked out of the room.

Sonneborn accepted me as a member of a community dedicated to the understanding of life as manifested in the ciliated protozoa. The initiation ceremony was an invitation to look in the microscope to see what he had seen, to ask the organism any questions I thought they could answer. He did not burden me with what he had already done. He did not explain what he thought might be important. Within two weeks I was converted into a life-long student of the ciliates, and have never again questioned my calling. And throughout my teaching career I have taken it to be my primary task to put students in direct contact with living things.

A year later - when I submitted my first research report -Sonneborn gave me a reprint of a paper that he had published before I joined the lab, observing wryly that sometimes one can save time by reading the literature. This lesson I had to learn more than once, as we shall see. But frontier explorers are obsessed with the myth of our uniqueness, and we have to be reminded from time to time of the footprints in the sand.

Sonneborn's pedagogical method was characteristically to engage the student directly with the phenomena to be observed, and to deal with interpretations and literature citations later. The surprise at learning that someone has been there before does not seriously detract from the sense of observing something new.

Sonneborn personified the "explorer" mode of science, as opposed to its polar extreme - the exploiter (Nanney, 1981). The explorer above all else wants to "go where no man (or woman) has gone before," recognizing that much of what is seen is likely to be familiar and unprofitable, but hoping endlessly to find something novel and valuable. The "exploiter" comes in sharply focussed, with the questions written out in advance. One exploiter of my acquaintance claimed he would never do an experiment that was incapable of giving an unequivocal yes or no answer.

The scientific drama provides a multiplicity of roles, and requires the utilization of diverse talents. The prospector rejoices in the freedom of a lonely walk in the wilderness. The exploiter is excited by the shouts of the crowd urging on the front runner in the last lap of the race. Different talents are required for different roles, and different rewards are provided for different scientific functions. Players of different roles, however, are not always sympathetic with those drawn to alternative projects, even when we recognize our mutual dependency.

So long as the scale of science was small, and the pace was slow, the exploratory and exploitative components of the scientific enterprise seemed to remain in balance. Casual exploration provided sufficient observations as a substrate for decisive experimental analysis. Today, however, the scientific explorer is becoming something of an endangered species. Although exploration is commonly less expensive than exploitation, it is also less likely to produce commercially useful applications in the short run. Funding agencies are largely concerned with the economic consequences of their investments and have not developed satisfactory strategies for funding exploratory studies. Consequently most resources are given to "consensus science", to well designed projects using familiar technology to answer important questions understood to be near solution. Exploratory research is sometimes characterized as "private" research, essentially an asocial activity done to satisfy the curiosity of an individual, and not meriting public support.

Much of the stress encountered in the practice of science today is a consequence of the demographic damper on the exponential growth of the scientific culture (de Solla Price, 1986; Nanney, 1988). After 300 years of short doubling times (15 years), science has had to adjust to limited resources and a doubling time adjusted to the doubling of the human population (50 years). The demographic inflection shifts the ecostrategies of science from r-selection to K-selection, and makes "dirty tricks," and the Malthusian menaces (war, plague and famine) much more commonplace. That, however, is not the issue I address here.

Balance must be maintained within the scientific community regardless of the socio-economic circumstances. I will not try to deal with that subject either, however. Rather, I am celebrating the exploratory phase of science, that mode of science that is motivated primarily by curiosity about natural phenomena, and that is the real leading edge of scientific advance. Contrary to common opinion, despite the remarkable discoveries of recent years, the natural world is still essentially boundless. Its exploration will continue even if driven solely by curiosity.

I hope that my experiences at the exploratory front will provide some insight into the activity. The least expected feature of that front is the way it is connected. I was excited by the confrontation of living organisms, and with the challenge of asking them questions in languages they could answer. But I was also excited by the connections with other workers who shared common interests. I needed to share my observations and found ample opportunities to do so within the Sonneborn laboratory.

When I left that working group, however, I felt for the first time the loneliness of the frontier. No one at the University of Michigan, when I went there as an assistant professor, was interested in a day-by-day account of my observations. Sometimes I felt that the only person who ever read my papers carefully was Tracy Sonneborn. And when I wrote papers for publication, I soon realized that I did not need to order hundreds of reprints for scholars interested in my studies. Citation ratings may be appropriate for comparing exploitative investigators, but they have little meaning for the frontiers. Exploitative publications may have citation half-lives of only a few months, while half-lives of exploratory studies are measured in years or decades. I gradually began to perceive the different social dynamics at the exploratory and exploitative edges of science.

Exploratory science is certainly connected, and those connections are very strong and are essential to keep it alive. The private satisfaction of one's curiosity is not ordinarily adequate as a sustaining motivation for a human being As a graduate student I teased Sonneborn with the Doomed Island Scenario. If he were placed alone on an island designated for destruction on the day that he would eventually die, and if he were given whatever supplies and equipment he might wish for as long as he lived, but would never be allowed to communicate anything he discovered, even after his death, would he continue to do research on ciliated protozoa? His immediate unequivocal answer was, "Of Course".

I believe Sonneborn was mistaken on this point. But his activity during the postdoctoral decade when he was trapped in Jennings' laboratory at Johns Hopkins, supports his declaration. He continued working on ciliates day after day without news of general interest to report to the scientific community. The favorable attention he received eventually, when he discovered mating types (1937) and was finally able to do genetic analysis on paramecia, was gratefully accepted of course. But I am skeptical that he could have sustained the work much longer without at least the hope of eventual positive social feedback. Certainly he valued possible connections to future generations. In a letter to Geoffrey Beale (1982), he wrote, "It is a great satisfaction to know that now or centuries from now, anyone who is willing to give the time and effort needed for the job would be able to identify strains in conformity with our descriptions."

We hope, of course, for more immediate feedback. That social feedback for explorers comes about primarily through the connections among thinly scattered workers on a common frontier. The connections are quite different from those connecting exploiters. Relationships among frontier workers tend to be cooperative and supportive, not adversarial and competitive. The workers are spread so thinly that duplication of effort seems foolish. Sometimes the workers are so scattered that they have to be connected in time as well as in space. I will return to this phenomenon when I consider the role of Emile Maupas.

The Bloomington Ciliate Laboratory became the center of an international network of research scientists, most of whom were connected to the center by an initiation such as I have described for myself - an introduction to organisms and phenomena of persuasive fascination, and a continuing relationship of accepting but critical collaboration.

The phenomena that first opened up ciliate genetics were not exciting simply because of their uniqueness, though they do have their special flavor. I do not want to argue the validation of Sonneborn's initiatives, but I should indicate the general rationale employed. The principle of "least causes", related to Occam's Razor, holds that mechanisms operating in one context are most likely to function in others also. Different organismic contexts seldom reveal new mechanisms, but rather mechanisms less accessible to analysis in familiar circumstances. This comparative approach is the foundation of exploratory biology, originally implicit in the doctrine of common descent, and abundantly justified in the universality of the genetic code.

The Sonneborn interactive web literally reached around the world, to dozens of laboratories established by his graduate students, his postdoctoral fellows, and their students after them. The reason that I am here today is that Renzo Nobili went to Bloomington, Indiana and became inoculated with ciliates and infected with enthusiasm. He returned home and nucleated a new center for ciliate studies at the University of Pisa, a center that duplicates many of the features of the Bloomington Center while developing its own special projects in the inimitable Italian style. The Pisa Center is now one of the most comprehensive international centers for exploratory work in ciliate biology.

Supplementing the Prospector's Pack

The community of scholars is connected in a variety of ways. The connections we have been discussing are fabricated from shared interests in phenomena in the natural world, and common attitudes toward those phenomena. Other connections of very different kinds may also be important, as I hope to illustrate now.

An explorer on the frontier carries certain essential equipment that defines the kind of understanding being sought. The prospector - the geological explorer - in former times was equipped with a pick, a shovel and a tin pan to search for gold. In modern times much more sophisticated equipment must be carried along for the analysis of chemical compounds and for probing deeper structures than can be explored with a pick and a shovel.

The tools necessary to conduct decisive studies vary according to the discipline and the historical era. Late 19th century biology was dominated by the technology of the compound microscope because it opened to analysis a previously invisible scale of organization. From the middle of the 20th century onward the dominant technology has been molecular biology. Computer technology has steadily emerged later in this century as another major instrument for understanding biological phenomena. The biological explorer who hopes to discover new understanding in these days must be equipped with one, or preferably both, of these powerful tools.

The frontier worker must either become proficient in the new technologies, or must be able to engage the interests of those who are knowledgeable. Few of us, unfortunately, are by training or aptitude able to master all the multiple technologies that are often required. Though we enjoy the quiet life at the exploratory edge, we are constrained to connect with scholars who have the analytical tools we need.

Putting together multidisciplinary teams at the exploitative edge of science poses few problems because cooperation is rewarded by capitalistic advantages; disciplinary arrogance is suppressed. Multidisciplinary teams at the exploratory edge must be constructed with more cunning, or by using adventitious opportunities. If one cannot find gold, one must be content with diamonds or rubies, or petroleum.

I want to describe at this point the circumstances that made possible the exploration of certain evolutionary problems in ciliates using the technology of computer science. As I have recounted earlier, I am ill-prepared for any kind of modern technology. Though I entered biology at mid-century, in terms of technological sophistication, I was half a hundred years behind the times, and prohibited by my ineptitude in chemistry and mathematics from participating directly in the great technological advances. My only hope of focusing their power on my disciplinary concerns was by luck or by clever application of Macchiavellian principles. Lacking cleverness, I had to depend on good fortune.

Good fortune arrived one morning in the mid-1960s, when a knock came on my office door. Following the knock came an agitated young professor, asking my assistance in preserving his marriage. Franco Preparata had come to Urbana, Illinois, as a Professor of Computer Science. He had come from the University of Pisa. Unfortunately, he had failed to clear this move appropriately with all the authorities - and more specifically with his wife - Rosa Maria.

Rosa Maria (actually Rosa Marina initially, because she was born on the high seas, aboard ship in the Suez Canal) is a citizen of the world, but she is an Italian woman and she loved Pisa. She had a satisfactory position teaching biology here; their children had friends and were comfortably settled. Rosa Maria was putting down roots, and Franco had pulled his up. He was beside himself, and willing to go to great lengths to reconstitute his family.

He had finally worked out a "deal". Rosa Maria and their children would rejoin him in Urbana provided a) he would build her a house to her specifications, b) he would purchase her a Navajo squash-blossom necklace, and c) he would find a job for her. The last condition - a job- was turning out to be a considerable challenge. Rosa Maria lacked the formal credentials to teach in the public schools of Illinois, and her biological training was too general to qualify her for the specialized jobs being advertised. I was touched by his problem, and had been sensitised to the problems of women in the workplace by my own wife's experience. I agreed that Rosa Maria should join my staff.

As luck would have it, the relationship with Rosa Maria and Franco Preparata became one of the most agreeable relationships in my entire professional career. Rosa Maria is an extraordinarily skilful and imaginative laboratory worker, and remarkably capable in personal relationships. She saw that my laboratory needed to import molecular technology, so she cruised the halls for new techniques and parasitised the molecular laboratories; we were soon publishing sequences of the nucleic acids of ciliates, and could begin asking deep questions about the evolutionary connections between ciliated protozoa and the other forms of life. In trying to connect the protozoa we were also making intellectual connection back to a precursor in the previous century - Charles Darwin, who dominated that cultural landscape.

Sequencing nucleic acids brought a new development, in the direct engagement of Franco Preparata with the molecular evolutionary relationships of the ciliates. Rosa Maria and I prevailed upon Franco to confront the problem of extracting evolutionary connections out of sequence data. Franco is a computer theorist, not a computer programmer, and he was not very interested in this "applied" problem, but he considered that one good turn deserved another. With his usual facility, he surveyed the methods for building evolutionary trees, and decided that the only rigorous method, was one constructed by a fellow computer scientist - Sankoff -(Sankoff and Kruskal, 1983) and studiously ignored by all biologists in the business of building trees. Using this foundation he wrote PHYLOGEN - a computer program for constructing ancestral sequences and trees from the sequences of putative descendants (Nanney *et al.*, 1989).

The original Preparata program was limited in application to a small number of relatively short molecular sequences, and required much time for computations. The wider application of string programs awaited improved computer technology and this has been supplied by a talented young female computer scientist from Korea - Chaeryung Park. The program is still under testing and development (Nanney *et al.*, 1998). But it promises answers to questions about ancient junctions that are eluding biologists who use simpler but less rigorous analytical methods.

While reflecting on the contributions of Rosa Maria Preparata and Chaeryung Park I am led to a more general cognizance of the predominant role of women in what I am almost embarrassed to call **my** research career. Much of the research that over the years has been carried out in my laboratory and published under my name has been carried out by women - from Arax Tefankjian, Pat Caughey and Sally Allen in the early years, through Barbara Lindquist, Margaret Chow and Barbara Wozencraft in the middle years, to Ellen Simon and Barbara Meyer still around at the end. And this list of names includes only those who were around for substantial periods of time and were not working on degrees.

The contributions of these women can be dismissed as "merely technical", but I must deny that judgement categorically. These people were not my servants or extensions of my hands but full-fledged co-workers. Many of them were fully capable of directing research programs of their own, and they would have done so had the gender biases of our scientific culture been less constraining.

My experiences with gender differentials are certainly not unique, and may in fact be generalizable. We men have taken the credit, but our contributions rest on the shoulders - not of Giants but of women. For every Barbara McClintock and Rita Levi-Montalcini, hundreds of equally accomplished, or at least equally capable, women have their names buried in footnotes or as secondary authors of major papers.

The gender bias almost unconsciously implemented within our information industry gives an undue emphasis to the contributions of male scientists, and an inappropriate proportion of the rewards of scientific research go to them. The distribution of scientific credit is perhaps no more gender biased than is that for artistic credit, but that is faint justification.

I am sure that I am sensitized to inappropriate gender roles and gender rewards by the experiences of my co-workers, and especially by the experiences of my permanent co-worker. That experience, however, also reveals the reverse side of gender bias - the deprivation of males of some of the rewards of females whose efforts are invested more heavily in human relations. I can come to Pisa and be recognized (by a very few) as "Professor Nanney - distinguished protozoologist." But if I go the shopping malls, the public schools, the music halls of Champaign-Urbana, I am recognized - if at all- as "Mrs. Nanney's husband," and then ignored. Though my wife modified her professional career to make it compatible with mine - and though her accomplishments are less well recognized in Pisa than are mine, her personal rewards from a career in teaching and public service are far more frequent than mine and perhaps more heart-felt. I am not arguing that either of our rewards are inappropriate, only that the choices for careers (and rewards) should not be restricted because of gender.

I do not have time to describe in detail the Phylogen Program, or to give other examples of fortuitous associations that have multiplied my meagre efforts. Instead, I want to take a few moments to develop further the backward extension of the connected network of exploratory biologists. We do not need to leap directly back to Darwin, but can go back in stages. The search backward in time seems especially appropriate when we are celebrating centuries of connected humanistic and scientific explorations. Nevertheless I get a little nervous in historical explorations, particularly because I recognize my penchant for attributing qualities to both heroes and villains that I need for them to have.

Prominent Precursors

Forerunner I: Herbert Spencer Jennings

Tracy Sonneborn was an unusual occurrence in experimental ciliatology, but he did not spring from the head of Zeus. He was a graduate student in the laboratory of Herbert Spencer Jennings (1868-1947) at Johns Hopkins University in Baltimore, Maryland. I mentioned that he was trapped in Baltimore for a decade after finishing his doctoral thesis (Nanney, 1982). The Wall Street Crash and a world wide recession coincided with his completion of his doctorate, and few jobs were available. He had to stay at Hopkins and he had to work on *Paramecium*, because the Rockefeller Foundation had given funds to Jennings for *Paramecium* research. Sonneborn eventually made major contributions, but Jennings must receive significant credit for seeing the potentialities of such studies and for maintaining support through years of little return.

Herbert Spencer Jennings, it turns out, was another worker of the frontier, and in his own right an important student of ciliates (Sonneborn, 1948;

1975; Burham, 1973). Jennings, incidentally, also had a significant early connection with Italy. In 1897, having finished his doctoral work at Harvard, Jennings obtained a postdoctoral fellowship which he spent at Jena and at Naples. At Jena he was introduced to protozoa and made the observations on the behavior of *Paramecium* (1897) that are reported in today's elementary texts, under the term "avoiding reaction."

At Naples he became better acquainted with the invertebrates, and perhaps more important, he was exposed to "experimental biology" which had scarcely begun to be understood in America.

He moved beyond the observations to seek the mechanisms for the behavioral patterns he had documented. Jennings, with Jacques Loeb, was among the first to treat unicellular organisms as organisms in their own right and to apply mechanistic interpretations to simple behavior patterns. Jennings' *Behavior of Lower Organisms,* published in 1906 was an important precursor of subsequent behavioral studies in the mechanistic tradition, such as those of John B. Watson.

Jennings' field of interest broadened considerably when Gregor Mendel's interpretations of breeding studies were rediscovered in 1900. He was one of the first American biologists to welcome the new developments and to incorporate them into the new experimental biology. He also foresaw the coming together of evolutionary and genetic mechanisms, and made important early contributions on the role of breeding patterns in evolutionary change.

He contributed relatively little to the application of genetics to protozoa, though he was a major spokesman for the field (Jennings 1929). He was frustrated because controlled matings among the protozoa were not possible until Sonneborn's (1937) discovery of mating types finally made such studies possible. Despite the difficulties of studying genetic mechanisms without crosses, Jennings nevertheless made some ingenious and still troubling observations, notably concerning the inheritance of dentition in the sarcodinid protozoan, *Difflugia* (Jennings, 1937).

One area of Jennings' contributions of which I was strangely unaware until recently was in the application of genetics to human affairs (Kevles, 1985). Mendelian genetics was heartily welcomed to America at a time when the enthusiastic adoption of technology seemed to promise the solution of all major social problems. The Progressive political party incorporated the new genetics into their social planning and pushed an energetic program of eugenics, buying into the assumptions of the Galtonian program for improving human beings by selective breeding. This program was rejected in Britain where it originated, being considered premature and dangerous by leading geneticists. Haldane, Huxley and Hogben managed to divert the forces attempting controlled breeding of social misfits, sterilization of criminals, and genetic screening of immigrants.

This early political linkage between genetics and human affairs is a subject not well publicized in America, but it was responsible for much of the funding that pushed American studies into the forefront of international genetics. And most American geneticists protested little or not at all, as the social agenda drove applied human genetics to incredible levels. Only the adoption and extension of that social agenda by Nazi Germany in 1933 forced American reevaluation, the eventual rejection of eugenics, and the establishment of a new Genetics Society. With only a handful of other American scientists, Jennings had earlier pointed out the flawed foundation of the eugenics movement (Jennings 1930).

I know very little about the personal relationships between Sonneborn and Jennings. Sonneborn seldom spoke about Jennings, and his comments in his own informal autobiographical sketch provide little illumination. Sonneborn (1975) waited for 28 years after Jennings' death to publish his detailed memoir of Jennings life. And despite the meticulous documentation provided in that memoir, little is learned about their relationship. One can scarcely doubt that Jennings had a powerful impact on the young man unexpectedly caught within his support system for what must have seemed an endless time. How those relationships developed we will probably never know, because of Ruth Sonneborn's aversion to historians' perusal of family and personal documents.

Precursor II: Francois Emile Maupas

The last connection I want to probe briefly is a different kind of connection, a connection that is not mediated by personal association, or by institutional formalities, but which is nevertheless a significant force for coherence and continuity within the scientific community.

I became significantly aware of Emile Maupas (1842-1916) only in 1952. I only vaguely remember hearing his name while I was a graduate student. And, as I mentioned earlier, I was not at first even interested in reading Sonneborn's "obsolete" publications.

In 1953, ancient history suddenly became relevant to me. When I left Indiana University, I was resolved to also to leave *Paramecium*, and Tracy Sonneborn. Primarily I believe I needed to leave Sonneborn, not because I no longer respected him, but because I respected him too much. He had introduced me to the research community, and programmed me for the rest of my life. I felt that I needed to declare my independence, but to do so within the only research tradition I knew - that of the ciliated protozoa.

When I took my first independent position, at the University of Michigan, I discovered Alfred E. Elliott, and *Tetrahymena pyriformis*. Here was another person with experience with ciliates, and a ciliate whose nutrition was far better understood than that of *Paramecium*. The ability to grow this ciliate in simple axenic medium was considered important in the rapidly developing era of biochemical and molecular genetics. The only problem with using *Tetrahymena* as a genetic tool was that *Tetrahymena* was thought to be asexual. All the recent workers with this organism were convinced that it did not mate.

Elliott and I both thought that some attempt should be made to test the assumption of asexuality and should begin with newly isolated strains. David Gruchy, a graduate student of Elliott, was sent around the city collecting dirty water, and eventually isolated several new clones of *Tetrahymena*. Elliott washed the new tetrahymenids in antibiotics and placed them in axenic medium. He examined them periodically both in clonal cultures and in mixtures but never observed conjugants.

Finally Elliott brought the cultures to me, saying he was abandoning the effort. I promptly put the cultures into bacterized medium, and mixed them. Two days later, on Thanksgiving Day, 1952 (much like San Ranieri Day in Pisa) all the cultures were swarming with conjugating cells (Elliott and Nanney, 1952). I made microscopic slides, drew diagrams of meiosis and mitosis, and worked out the genetic consequences of mating. Elliott was out of town, so with great excitement I sent a report of the observations to Sonneborn.

Sonneborn fully shared my enthusiasm, but he suggested that I should check with Maupas before submitting anything for publication; i.e., Maupas, 1889. I was shocked to compare my drawings (Nanney, 1953) with those of Maupas, and to realize that Maupas had been there decades before, seeing exactly what I had seen, and interpreting the events precisely as I had. The only important difference was that he called the organism *Leucophrys*; the term *Tetrahymena* hadn't been invented yet (See Corliss, 1952). Maupas had even noticed (as I had) that in conjugation the exchanged pronuclei always passed to the right - in the continental rather than the English traffic pattern. I had been scooped. My first reaction was of disappointment, almost of disbelief.

Here was a connection of which I was totally unaware, but which reached to me across large spans of space and time and culture. I am pleased to hope that Maupas would have been as surprised and as pleased as I became after I had adjusted to the correction of history. My frontier attitude had at first robbed me of an understanding of the invisible and unconscious connections which tie us together even when we are unaware. Moreover, the accidental replication of observations is often the only independent verification of frontier research. That it works supports our confidence in the reality of the natural order.

I still do not know much about Emile Maupas as a person (Sergent, 1955; Theodorides, 1974). His available photograph is decidedly intimidating but it was made late in his career and when he was ill; it should not be used in personality assessment. He had no scientific training in his formal education in the school at Chartres, and was introduced to natural history belatedly by a pharmacist friend. He was so captivated by microscopic creatures that he spent much of the rest of his life peering at them in the microscope and asking them questions.

He took a position as archivist in Algiers (where he presumed the warmer climate would provide a steadier supply of organisms). He performed his official duties scrupulously, but retired after work to his primitive laboratory in the bedroom of his three-room apartment. His observations began before the modern microscope was fully perfected, and his methodology remained primitive but powerful. He never married but kept up his lonely interrogations of nature for 47 years, until he died in 1916. His work was noted, however, and he made a marked impression on the scholars who occasionally encountered him. He received the Legion of Honor decoration in 1909. A bronze medal was struck in his honor in 1913, by subscription among his friends.

Maupas' isolated life, absent from institutional support, inevitably remind a protozoologist of another monkish investigator - Vance Tartar (See Frankel and Whitely, 1993) Maupas' studies, like Tartar's, however were published, and through publication were connected to the mainstream of biological thought. Maupas' ideas continue to have a significant impact on the course of biological investigation. The "Maupasian Life Cycle" was based on his observations of ciliates in culture (Maupas, 1888). Those observations still haunt us. Jennings and most of the protozoologists of his era, and Sonneborn and his students have all invested effort toward understanding clonal ageing.

Graham Bell (1988) has only recently written a book on the subject: *Sex and Death in Protozoa: The History of an Obsession* . In it Bell tries to show that Maupas, and students of clonal ageing after him, were mistaken in their interpretations. Bell does not decisively dispose of the phenomenon, however, and the life cycle of ciliates will probably continue to tantalize us, entangled in many of the still unresolved questions of sex and survival. Maupas was close enough to natural phenomena to merit continued attention from modern students.

I do not believe that Jennings ever met Maupas. I doubt that Maupas was ever at the Naples Biological Station. Jennings obviously knew of Maupas' work, but the quotations in his 1906 book were from an English translation of Maupas' observations, and he makes no reference to the original publications. Jennings learned much more about Maupas later, when he became interested in problems of senescence and natural death, but I suspect it was after the death of Maupas in 1916. Jennings may, however, have been involved in the subscription to award Maupas a medal in 1913. In any case, at some time Jennings came into possession of one of the medals. Sonneborn (1975) does not mention it in his thoroughly documented memoir of Jennings' life. But Jennings had a medal and gave it to Sonneborn. Sonneborn in turn gave the medal to me, so that it becomes a physical symbol of connectedness from the Algerian microscopist in the last century to workers of the same frontier today.

Sumation

That metallic connection brings me near the end of my story today. Before completing the story of the Maupas metal, however, I need to pull together the loose strings of this very personal story about the connections in the scientific community. Here near the end of a career the boy raised in the oil fields of Oklahoma finds himself still on a frontier, but on an intellectual frontier rather than a geographic frontier. A startling feature about this new frontier is how little the geography matters. Not only do I have close associates in Italy, but throughout Europe - in France and England, Germany, Denmark, and Poland. I have colleagues in Mexico and Canada, in Japan, Taiwan, Mainland China and Korea. These are not casual aquaintances but warm friends and valued companions. The community in this frontier is small but it is tightly knit by shared values and common goals.

The community is not only free of geographic barriers, but even the barriers of time. I am a part of community that includes H.S. Jennings and Emile Maupas, though I never saw them in the flesh. And I hope that I will still be included in that community when all that remains of me are words on a page. Human beings need to belong and we can be truly human only in appropriate social contexts. I have had the good fortune to be included in one scientific community, in a rather small frontier of knowledge, and it has provided for me a coherence and continuity that is deeply satisfying.

I find it difficult to sort out from this personal story the elements that are generalizable. Circumstances that seem purely fortuitous in an isolated story may acquire weight when found repeatedly in independent personal histories. I think of Anne Rowe's (1952) observations on a sample of distinguished American biologists - nearly all of whom suffered severe illness or were by some other means separated from their peers at a critical adolescent juncture. She suspected that the peer separation might have shaped personality characteristics related to subsequent achievement. Such correlational observations - like observations on twins - need to be evaluated carefully. I am a little uncomfortable with the observation that Maupas, Jennings, Sonneborn and I all began our education in literature and stumbled onto the protozoa as if by chance.

A continuing story

The Maupas medal acquired by Herbert Spencer Jennings, was subsequently given by Jennings to Tracy Sonneborn. And Tracy Sonneborn gave the Maupas medal to me. It is my pleasure today to pass the medal on once more, while we commemorate not only a century of experimental studies of ciliated protozoa, but 650 years of connected scholarly activity.

When I decided to pass along the Maupas medal to a colleague in Pisa, I had a delicate decision to make. Several Pisan candidates are qualified to carry that token another generation, and my personal connections with all of them are significant to me. By specifying one recipient I did not want to minimize the contributions of others. I finally decided that the medal should go to the one whose area of exploration most closely resembled that of Jennings at the time he acquired the medal. Once that decision was made, the nominee of choice was easy.

I have recently had the opportunity to review the research contributions of Nicola Ricci (1990), and I was truly astonished at the progress he has made in what must have been initially a lonely exploration of a seldom visited frontier. His characterization of behavior patterns of ciliated pro. oa begins where Jennings' studies started at Jena nearly 100 years ago, but he has added to that description a formal mode of analysis - the ethogram - which enables him to make rigorous quantitative comparisons of the behavior of different species.

The ethogramic comparisons open up an understanding of the biology of ciliates to fundamental dimensions concerning which we have been largely clueless up to this time. One concerns the diverse but remarkably conservative morphotypes of individual species that had been to this point without a functional explanation. Another provides a rationale for the way different ciliate species exploit a common environment.

Though this is clearly frontier stuff, one can scarcely doubt that the territory explored will soon become more densely populated as we begin to deal with the critical role played by mesoorganisms - those organisms intermediate in size between microbes and multicellular creatures - that occupy the critical ecological interfaces in our overburdened environment.

I am very happy to transfer the custody of this medal to Nicola Ricci, and I believe that Tracy Sonneborn would be happy too, and Herbert Spencer Jennings, and Emile Maupas, and who knows how many other explorers of the frontiers.

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