Work As Meaning:

A Passion For Science

Jane F. Koretz

My COLLEAGUE JOHN (another biophysicist) and I were sitting around at work chatting about this and that of a biophysical sort, when he suddenly asked me how old I was. I told him, and he remarked that I looked ten years younger (a gross exaggeration!).

"Thank you," I said with a laugh, "it's the research that keeps me young and flexible. At least my *mind* is still flexible; I wouldn't want to comment on my knees."

To my surprise, John did not join me in the joke. Instead he looked thoughtful for a moment, and then said, "You know, I think you're right."

I retreated slightly then. "Well the research keeps me young but the teaching and administration and all the other crap involved in being here at Rip-Off U [my father's epithet for Renesselaer Polytechnic Institute] is aging me rapidly."

He ignored my words, and continued with what he was saying. "If you're doing something that you love," he said slowly, "it's as if no time passes, but if you're doing something you hate, or even something that you don't feel strongly about, then time seems to stretch forever."

"Yes," I replied, starting to see where he was going, "that's so true! I love my work, and I guess I hate all the other stuff mainly because it takes me away from the lab. I can go for

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hours doing the same repetitive thing without getting tired if that's what is needed to get the results, because I'm doing it for a reason, and the excitement keeps me going. But the same repetitive work in another context, and I couldn't sustain it for nearly as long.

"There's something else here, John, and that's the high involved in getting results and seeing, maybe for the first time, what is really going on, and then thinking about how it fits in with other things that we know, and then seeing a new way of approaching the whole problem."

"Yeah, it's better than sex and lasts a hell of a lot longer."

I agreed with him, but I was getting more and more excited, so I continued on. "I tell you that sometimes when we get a new result, I really get off on just speculating about what it means. Some of my ideas are complete nonsense, but that spiral of thought and excitement can keep me going for months. And, you know, you can't stop thinking about it. I think about work in the shower, watching television, eating, even reading, and sometimes in the middle of doing something completely different, an idea pops into my head. That's how I thought of the micelle hypothesis [a currently controversial idea about the nature of the proteins in the lens of the eye]; it came to me in the shower!

John started laughing at me. What was this Frankenstein's monster he had unleashed, he must have been thinking, but in fact what I was saying to him, and the way I was saying it, was coming out in precisely the same way that John and I play with ideas all the time. "And another thing, John. I think that reading science fiction is important here too. SF takes ideas to their limits within a human context, and really, that is what we try to do too. It's not the *content* of the books, but the *approach* that's so key in keeping the mind open. Come on, stop laughing, you read SF too."

"But not nearly as much as you, Jane. I have better things to do with my free time."

"Yeah, you're addicted to your own brain endorphins. You wouldn't be limping around like that if you didn't do that stupid running thing every day."

"Hey, Koretz, if you want to talk about addictions, let's discuss your smoking."

The conversation, if indeed it could be called that, deteriorated into friendly insults and name-calling after that, but it set me to thinking.

Jane Koretz's Passion (with NO apologies to Elizabeth Taylor)

Scientists, in my experience, tend to be one of the least self-reflective groups of professionals and, at the same time, one of the most dedicated. This is due to a combination of factors: a native inclination toward science that develops during childhood and adolescence, a concentration on courses and outside activities related to this inclination, and, at the college undergraduate and graduate levels, a curriculum that, by personal choice and by institutional fiat, leaves little room for exposure to the humanities and social sciences. At Renesselaer, where the undergraduate degree is a Bachelor of Science, all students are required to take one humanities or social science course each semester, for a total of eight; strict distribution requirements have had to be developed to ensure that students are exposed to both types of disciplines, and that they sign up for courses past the introductory level. Many of my advisees resent this requirement, and have even had the gall to ask me which of the available courses is a "gut" (translation: easy A).

The stereotype of a scientist is the cold and disinterested seeker after truth (whatever *that* is!), wearing a lab coat and heavy glasses and tending to be personally untidy and absent-minded, thanks in large part to photos of Albert Einstein. There is, unfortunately, more than a grain of truth to this, as is so often the case with stereotypes. Scientists must at least give the *appearance* of objectivity in their work, and this is expressed as a detachment and coolness that the conversation above actually belies. They often wear lab coats to protect their clothing from laboratory hazards; a lab coat is also a convenient way of carrying around pencils and pens, keys, notepad, calculator, and other miscellany. Finally the dedicated, often obsessive and always intense, preoccupation with their work leads to a lack of consciousness of appearance and, often, absent minded responses to stimuli not directly connected to the work at hand. When I am working in the lab, I dress for comfort and ease of movement; a typical outfit (male or female) might be jeans and a sweatshirt or pullover accompanied by the most comfortable pair of shoes (usually sneakers or running shoes) in the closet. Hosiery tends to get snagged by mysteriously appearing corners and rough spots: suits tend to be ruined by acids or other chemicals sponta-

neously jumping out of bottles; hair tends to get disordered as one runs one's hand through it; smudges of this and that tend to appear on one's face and clothing; all in accordance with Murphy's Law (if something CAN go wrong, it WILL) and its various corollaries. The strangest phenomenon of all, however, is that one's clothes remain pristine and undamaged if one does not care what damage they sustain.

What are the special qualities of science, and doing science, that lead to this single-mindedness, this careless disregard for personal appearance and appropriate behavior, this openness to new ideas and concepts at the most inappropriate times? Why do people passionately devote their lives to the study of questions that they' know cannot be answered, and resent the time taken away from this pursuit for other purposes (e.g., eating, sleeping)? It is certainly not that they are searching for some truth of Truth (see Koretz, *Beyond Reason and Faith);* that is an interpretation given to science by non-scientists who equate science with certainty. As I or any other scientist will tell you, there is no way to prove that something is true. That is why currently accepted frameworks of ideas about the universe and living organisms are referred to as "theories" rather than as truth; we can prove theories false by a clever experiment or accumulate results consistent with the framework that gives it increased validity as a mechanism for understanding a fundamental process, but we can never unequivocally Prove a Truth.

Equally untrue is the assertion that scientists are people who like to solve puzzles (although many, including myself, do). This trivializes the motivations and dedication of scientists and is, in fact, an unrealistic appraisal of scientists' work. A puzzle is a limited problem that has a solution, usually unique, and that can be accomplished within a certain period of time. It is a game that is played for recreation. A scientific problem never has a solution is these terms, certainly never has an end, and is definitely not a recreational pursuit in the sense of relaxing the individuals involved. There is, however, a related term that is completely appropriate in a description of what scientists do, and that is "play." We play with ideas, with concepts, and with hypotheses; we juggle relationships and processes and mechanisms; we perform experiments in our heads to investigate the implications of a given idea and develop actual experiments to test the nature of

its place in our theories. We "play" in the sense of "toy with" and also in the sense of the antonym to "work."

There is a heady, intoxicating, addictive excitement in discovering, unraveling, and elucidating the fundamental relationships and processes of the universe, or at least that part of the universe to which we have access. And the roots of this are there for the mathematicians, the physicists, the chemists, the biologists... There is an implicit sense of direct connection with the underlying fabric of reality, a special understanding that feels almost like a privileged relationship with the cosmos, and a universality in discovery that transcends the specific. I have heard it said by friends that scientific inquiry - understanding the basis of a particular mechanism or adaptation or process - destroys the mystery and beauty of the world around us. For me and others like me, the opposite is true. Understanding enhances our appreciation of the magnificence of the world, adding extra dimensions to the way in which reality is experienced. The closest analogy I can formulate is, appropriately enough, an artistic one. A photograph of a sculpture, for example, gives the observer a sense of the three-dimensional shape and (perhaps) size and tenure of the work, especially if more than one view is provided. But to truly and fully experience it, one must also be able to view it directly - to walk around it and capture its spatial essence, to see and feel its surface, to experience through all the senses (including kinesthetic) its reality in space and time.

The universe is not only stranger than we conceive, it is stranger than we *can* conceive.

J.B.S. Haldane

"Doing" science is more than playing with ideas, however, no matter how exciting, perceptive, or incisive they may be. There is a necessary precognition to this playing with ideas, and that is thinking of the ideas in the first place. It is in this respect that the scientist most nearly shares the experience of the artist, the composer, the poet, the sculptor - individuals whose life's work is described as creative. The insight into a system or a process leading to a new idea is rarely, if ever, deductive or even inductive; it is rather an intuitive leap, a new perception or shift in perspective that can be explored in the ways described above, that seems to come out of the blue. It is sometimes possible, once this leap has taken place, to

trace back the relationship of the idea to the conditions leading to its development, but more usually the idea is far beyond what might be considered its logical basis. It is at this point that the "playing" - both mental and experimental - begins.

An example out of my own experience might be useful here - the micelle hypothesis mentioned earlier. My research is concerned with determining the mechanism of how the human eye focuses, and why this ability is gradually lost as we age. In the process of studying this mechanism, I became interested in the proteins of the crytalline lens of the eye (that part of the eye which is responsible for focusing on things closer than about 20 feet away), since the focusing power of the crystalline lens will depend on the concentration and particular properties of its constituents. While quite a great deal is known about two of the major proteins *(B* and V-crystallin), the protein present in the highest concentration - a-crystallin -- remains relatively poorly characterized. At a meeting on the lens a few years ago, when I was seeking to learn more about a-crystallin, I was horrified to see prominent scientists in this field screaming at each other in a disagreement over the molecular weight of the aggregate; this made no sense to me, because that measurement should have been one of the easiest and most straightforward to make. I was further horrified with the models of the structure being presented, since they were not supported by any evidence being presented at that very conference.

I started reading the literature in this area and discussing it with a colleague (call him Bob) who works on a-crystallin, and was appalled by the experimental difficulties presented by this protein, and by the lack of progress in the years since it had first been isolated. My previous experience with aggregating proteins dealt primarily with actin and myosin, the two major proteins for motility (at the cellular level as well as in muscles and heart); each had unique properties, but their aggregation behavior could be described in a general fashion that made them similar to all other known aggregating systems (e.g., virus coat proteins or the constituents of cilia and flagellae). a-crystallin seemed to be different, or at least the conditions for its proper study had not yet been discovered. I even looked at the aggregates in the electron microscope to see if there was something I could discern that others had not seen; this is not a conceit, since I happen to be an excellent and experienced electron microscopist. What I did *not* see was a

uniform population of particles; instead, they varied in size and shape, and each particle seemed to be unique in ultrastructure. From their appearance, the only conclusion I could reach was that the current model was definitely wrong.

It was somewhere at this point that I stopped really thinking about the problem, at least consciously. It seemed to be an intractable one, and I was not enough of an experimental biochemist to deal with it; in addition, my research priorities were with the question of the human focusing mechanism, not characterization of protein aggregation mechanisms. So I cannot explain why or how, while taking a shower one evening, I suddenly KNEW that a-crystallin aggregates were micellar, [Le., the subunits have one end soluble in water and one end not soluble in water; in water or an aqueous solution, the subunits' non-soluble ends aggregate together away from the water non-specifically, leaving the soluble bits sticking out and interacting with the water. The size and shape of the resultant aggregates are concentration-dependent, but not unique for a given concentration.] Perhaps the soap provided the final stimulus to my subconscious, since it and other detergents are molecules with ends of different polarities; however, I have had a number of good ideas in the shower and bath separate from this one, so I do not think so.

I was very excited, and also very *certain.* The idea made sense of all the different observations - often mutually contradictory - in the scientific literature; it was beautiful; it could be tested. I tried it out on my a-crystallin colleague Bob, and he replied that he had thought of it, too, but that proteins do not behave in this fashion. I challenged him, since this was his area of specialization, to describe one and only one result inconsistent with this micelle idea, and he could not. [To me there was nothing strange about a protein aggregating in a micellar fashion; proteins, in my experience, do what they have to do.] At any rate, while remaining skeptical, Bob became less hostile to the idea. I wanted to write it up immediately and send it to be published and evaluated by the larger scientific community, but Bob was much more cautious, since he thought we would be ridiculed. I prevailed in the end, however, and, with the cooperation of an open-minded set of reviewers, it appeared in the literature, clearly labeled as a hypothesis. In the paper, we showed how the hypothesis was consistent with a variety of different experimental results, how it made sense of the fact that the lens is transparent despite a

very high protein concentration, and how the other models fail using the same criteria. The initial response was underwhelming.

Since that time, a number of experiments to test the micelle hypothesis have been performed by myself, by Bob, and by others. In every case with which I am familiar, a-crystallin behaved as would be predicted by the hypothesis. Bob has gone from believing the hypothesis only on even-numbered days to really believing it. In addition, others refer to the hypothesis in their papers as a serious alternative to the old models. At a recent meeting, a biochemist essentially tried to rip us off; since he was unsuccessful, I consider this a compliment and an endorsement, even though back-handed. Finally, I am in the process of writing another paper describing three completely independent tests of the micellar nature of the protein, each of which conclusively gave results consistent with our idea. I am ready to promote the micelle hypothesis to the status of theoryl

This example illustrates, I hope, the creative process (whatever it may be) in the formulation of the new idea, followed by the scientific "playing" with it that Bob and I did, and the final stages of the process; experimental testing and verification, the foundations of which were also established through "playing." I still do not know where the initial idea came from (although I have learned over time that my subconscious is much, much more clever than I am!), but I could not have even begun to formulate it, let alone test it, without a broadly based scientific education, a through familiarity with relevant (and irrelevant) literature, and a willingness to think unconventionally. In general, scientists (like anyone else) do not like to be proven wrong, and in other circumstances, I would have waited until I have some supporting data before even broaching the idea in print. But I cannot even begin to describe the unassailable *certainty* with which the micelle hypothesis came to me, and I believe that even if I were proven wrong (which, at the time, seemed impossible!), it would nevertheless stimulate research on a-crystallin in directions that had not yet been explored; if I turned out to be wrong, I would be *magnificently* wrong.

In my euphoria, I knew I couldn't lose.

A scientist's professional stature depends in large part on the development and exploration of new ideas, and, while the showerlbathtub is a professional necessity for me (in more

than one sense!), an idea can come at any time or any place. I have had a number of very good ideas, and a number of very bad ones. If both types come to one in the kind of intuitive leap that I mentioned earlier, how does one tell the difference? My personal touchstone and that of some others with whom I have discussed this, is the aesthetics of the idea. Is it beautiful? Is it elegant? Is it simple? Does it create a new perspective? Does *itfeel* right? (Again, for lack of the right words and phrases, I have had to return to the artistic model of creativity in order to describe this evaluation process.) This is not as insane as it might sound at first: any scientist or mathematician has been immersed in the minutiae of their specialty for many years, and has attained an almost instinctive grasp of their system in the process. A new idea can have a beauty and rightness that is almost beyond description, because it induces a new perspective that illuminates the system in a more complete and satisfying fashion.

I described above the process by which I thought of and developed the micelle hypothesis. The certainty I had about it was, I think, based on its beauty - beauty in that it was consistent with and explained all previously reported observations, even the apparently contradictory ones, it provided an elegant explanation of maintenance of lens transparency despite the extremely high protein concentration in vivo, and it was easily testable in the laboratory. It was one of those ideas that led me to say, "Of course!" and others to say, "Of course! Why didn't I think of that?" There are other beautiful ideas - physicists are especially fond of them - that satisfy such criteria with one exception: they are not experimentally testable, at least at the present time. (One such is string theory.) They are beautiful because they are elegant and clever, but they are not ultimately satisfying because (in my opinion) they do not lead to the sense of illumination, the "ah-ha!" response, that a shift in intellectual perspective gives. For me, scientific beauty without experimental verifiability is a form of intellectual masturbation: it makes you feel good, but does not accomplish anything. It is much less aesthetically pleasing.

I began this section with a quote from J .B.S. Haldane, the noted British scientist, that I first encountered many years ago when I was still studying to become a scientist. At the time it seemed like the most profound statement about the cosmos, and of our endless and futile striving to understand it, that I had ever seen. I have a much different attitude toward Hal-

dane's statement now. I do not believe that *anything* about our universe is inconceivable to our minds. What was inconceivable to him and others earlier in the century (perhaps quantum mechanics?) is assimilated by students at the undergraduate level today, and what is beyond our understanding today will be the stuff of scientific theories tomorrow. Imagine how inconceivable the concepts of today's science and the products of today's technology would be to someone transported from the eighteenth or nineteenth century, and then reconsider his statement. From my own experience, from the experiences of others, even from science fiction, it is clear that the creative capacity of the human mind is capable not only of conceiving of and encompassing our cosmos, but also conceiving of ideas that are inconceivable in *this* universe, but possible in others that we cannot physically explore. Elliot Gruenberg, a noted computer specialist, once called me a "techno-optimist;" if a consuming belief in the flexibility and imagination and creativity and curiosity of the human mind is being a "techno-optimist," then I certainly am.

Work: 1. Physical or mental effort exerted to do or make something; purposeful activity; labor; toil

In considering what it means to be a scientist, I have touched on the creative joy and satisfaction of initiating new ideas, and the intellectual pleasures of playing with them theoretically and experimentally. A third factor, which is a necessary pre-condition to the other two, is the recognition of where one's greatest abilities lie and the overwhelming compulsion to develop and use them to their fullest. As I discussed very briefly earlier, many - perhaps most - people who are scientists and mathematicians became interested at a comparatively early age, although it should also be noted that not all of those who were fascinated by science and mathematics ended up in these disciplines as adults. [In some cases, the talent is not sufficient; in others, there are more compelling priorities, such as making a lot of money; in still others, the saddest group, explicit or implicit discouragement of the individual's interest leads to alternative career choices. But that is another essay...]

There is an indescribable satisfaction in developing and using one's unique combination of skills and talents to their utmost. I keep wanting to use the words 'Joy," because indeed

there is that soaring and transcendent feeling, and "completion," because through the expression of your special talents, you come into direct knowledge of your own deepest self. Abraham Maslow uses the terms "peak experience" and "self-actualization" for the concepts I am trying to express, but they seem too cold and clinical. The word "passion" means an "extreme, compelling emotion; intense emotional drive or excitement," and this comes much closer to describing what one feels when one is doing what one does best, and why one does it. It does not seem the least bit incongruous to me to describe scientists and mathematicians as passionate about their work. The paradox only arises from preconceptions about and stereotypes of scientists promulgated in the media, especially third-rate horror films, and from the stilted language of scientific papers. Believe me, the way science is experienced in the lab and the way it is reported in the literature are light-years apart!

It is possible, even probable, that other people in other, unrelated, disciplines or professions obtain similar kinds of satisfaction through *their* work, and have recognized at least some of what I have been trying to describe from their own experience. While I obviously cannot speak with any authority on this point from my own background, I know others whose attitude toward their life's work is very similar to mine, people who are not nine-to-fivers and who are as passionate and obsessive about what they do as I am. It is easy enough to single out the artists and artisans; again, from *their* stereotype, we would expect eccentric dress and behavior, an inability to keep track of time, a focussed attention on their craft that makes them appear absent-minded... But I know others - lawyers, teachers, secretaries, social workers, gardeners, doctors, auto mechanics, even a janitor - in whom I see the same sort of joy and completion. These and other work choices are not necessarily "creative" in the accepted sense, but they are clearly and obviously providing the same sense of fulfillment to these others that I can get from my own work. The most important unifying point here is that, without the underlying passion - which itself arises from an individual's recognition and development of his!her unique arsenal of interests, skills, and abilities - the special satisfactions that a given type of work can offer is absent.

When I was in college, I spent two summers doing secretarial work. I was good at it, but I found it tedious and stul-

tifying, and desperately looked forward to the end of the day every day and the end of the week every week. If I had looked forward to the end of the summer as well, I would have driven myself crazy; I took things an hour, a day at a time. Yet I know secretaries who take a great deal of pride and satisfaction in their elegant formatting of letters and manuscripts, in innovative filing systems that make any documents almost immediately accessible, and in the professional way they answer the phone and handle inquiries.

Another summer, just before starting graduate school, I worked as an orderly on the women's psychiatric observation ward of a county hospital. Unlike my stints as a secretary/typist, this was work that I had directly sought and was interested in, largely because of courses I had taken in theories of personality and abnormal psychology, but also because of the possibility that there was indeed a link between mental disorders and physical disfunctions. Work, with the "mentally ill" was something I could visualize myself doing as a career, since it would directly benefit others as well as fulfilling my own need for doing science. But, as I painfully learned, interest was not enough; in dealing with the problems of others, one must be able to feel (and show) empathy while maintaining one's objectivity. I could certainly be empathetic, but I could not be objective, and that summer ended up being one of the most emotionally excruciating experiences of my life. Looking back many years later, I know that I learned some very important things about people and about psychology, but i also learned something very important about myself - that I could not directly help others without getting involved. In a sense, this trait has stood me in good stead as an academic advisor and teacher, but would have been disastrous in any of the helping professions.

I spent yet another summer (between my junior and senior years) as a research technician to a scientist; my responsibilities were to grow large enough amounts of slime mold for the scientist to do experiments, to make up solutions for the extraction and purification of selected proteins, and to do any other scutwork that needed to be done. In essence I was doing precisely the same kind of mindless repetitive work I had been doing as a secretary, but I loved every minute of it and hated to see the summer end. The difference, to me, lies only partially in the rational of the work, that I knew and appreciated why I was doing what I was doing, how it fit into the

scientist's overall research aims, and the importance of precision in the process. Primarily, this work fulfilled two important inner needs: it enabled me to perform tasks for which I was especially capable, and it allowed me to do work that I found interesting. Thus, boring as it may sound, that summer's experience allowed me to explore simultaneously my talents and my interests, and helped lead me to my life's work. The secretarial work was something for which I had some talent, but no interest, while the work on the mental ward was something in which I had great interest but no talent.

The key to meaningful work, then, appears to be a combination of aptitude and fascination that is unique to each of us. Whether our work, whatever it may be, is sheer toil or something that gives us a sense of personal accomplishment and satisfaction will depend on the relationship of the kind of work we do to the capacities, interests, and talents we have, and have developed. I suspect that the fulfillment obtained from different kinds of meaningful work may also be, at least in part, unique to that work. The personal and professional rewards of being a scientist, for example, are very different from those of a health care provider or a gardener or a librarian, as I have tried to show. However, for those of us lucky enough or perceptive enough about ourselves to have found a vocation that fulfils us in every way, we share a joy and completion in the process of doing that enriches and adds meaning to our lives.

The sense of vocation - of meaningful work and all that accompanies it - is a fundamental part of living a fully realized existence, independent of the nature of that vocation. It is fundamental to personal growth, because it is inherent to the process of self-discovery. It is fundamental to a life of meaning and value and richness, because it enables one to attain a personal perspective from which one can make sense of the world and one's relation to it. It is, or should be, fundamental to any humanist or Humanist philosophy, because work without vocation diminishes an individual's sense of worth, hinders self-development, and thwarts the realization of each person's unique potential. Work as vocation, brings joy and completion, opens an inward path to the ineffable transcendence of which we all are capable and should be the birthright of every human.

Acknowledgments

I am grateful to Elliot Gruenberg for the term "techno-optimist," which made me start thinking about work and meaning, and to my colleague John Salerno for many past conversations that illustrated play-as-work and work-as-play, when everyone else thought we were goofing off.

The joys - and travails - that accompany meaningful work have been described better than I ever could in a number of books, either explicitly or implicitly. *The Existential Joys of Engineering* by Sam Florman or any of the books of essays by Lewis Thomas are good sources in the science/technology area. Science-for-the-intelligent-nonscientist books, such as those by Stephen Jay Gould, are accessible primers that can communicate the sense of wonder and discovery the authors feel at the same time that they are communicating their descriptions and explanations.

The unity of the creative experience across disciplines is described with loving thoroughness and a number of diary extracts in *The Act of Creation* by Arthur Koestler. While the thesis of his book is not directly related to the subject of this essay, it is nevertheless a fascinating and cogent exploration of the potential of the human spirit.

Finally, I must acknowledge my continuing and deepening debt to the dictionary, which has provided me with definitions, synonyms, and, most important, insight.