

On the Domestication of Science

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** This use of the expression, the domestication of science was inspired by Fr. Donald J. Keefe's expression on "the domestication of worship." It is interesting to speculate how this may be an expression more than that, a concept with broad social and cultural implications.*



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The expectations of society: health, wealth and success

One of the ambitions of those who preserve sperm was to have on hand the material to recreate a genius. I suspect that that vainglorious notion has not entirely disappeared; but it is based on a poor understanding of history, not to mention biology and the social sciences. These people had not heard of, or did not believe in, the dictum that no one steps into the same river twice. But beyond that, as Socrates had his hemlock, Einstein today would probably experience the same ambivalence of a society pleased and yet frustrated by modern technology. The domestication of science, which began most noticeably after World War II and now is nearly complete, brought science under the influence of a society that never understood it, tolerated it briefly, and now has sufficient control over it to define it. The result has been a new science called research, or research and development. Its banner is significance and its solidarity is based on mission.

Those who saw a threat to science in the beginnings of extensive national subsidy after World War II had little to say publicly. There eventually were articulate spokesmen in defense of science when that need became unavoidably obvious; but it is significant to note that, although these people successfully interrupted the political effort to create a National Cancer Agency, for example, they did not stop the domestication process. Cancer research today is neither following a judicious plan such as might have been developed for a National Cancer Agency nor being conducted in a manner conducive to scientific discovery. Spiegelman could reasonably point out to the Nixon administration that trying to cure cancer with a National Cancer Agency would be like trying to go to the moon without knowing Newton's laws of gravity; but neither Spiegelman nor anyone else today can easily find the conditions necessary to do basic research. Philip Handler, president of the National Academy of Sciences, called the present state of affairs half-way technology. To understand what he meant, one has to see modern science policy as something analogous to what one might visualize if the attack on poliomyelitis had amounted to emphasis on developing a better, more effective, mass-produced and cheaper iron lung. Dialysis machines, organ, transplants, engineered cells and chemotherapy are seen by scientists as interim technology.

How did the expectations of society toward science develop and how did they lead to the domestication of science? There is evidence, that the process was straightforward enough. Before World War II scientists operated with more or less organization and more or less financial support; but regardless of the degree of organization or support, the emphasis was more on discovery than on significance, more on understanding than on product. In Europe, where science was largely subsidized by industry, the desire to capitalize on scientific discovery still did not contravene science. There were, however, two notable events in this country which may be cited as examples of the transition period which led to the domestication of science. The Manhattan Project was undertaken during World War II because it was thought that enough basic information was in hand to produce an atomic bomb. In another area of national interest, the March of Dimes sponsored a concerted attack on poliomyelitis. The success of both of these missions is history; and so is the post-war response of our government; the creation of an extensive national science subsidy.

At this point the relationship between science and society still seemed orderly enough, except perhaps to astute observers; and it continued so for over a decade. Transcripts of senate hearings published about 1960 finally began to show the unrest in clear and unmistakable form. "What," a distinguished congressman would ask of a distinguished science administrator (in session and on the record), "do you think about mustard plasters? My mother used them on us whenever we had a cold." Such entries were abstracted for the amusement of readers of *Science* in the early 1960's. By this time the cost of paying for American science was growing at a rapid pace. Budgets were often gratuitously funded at more than the requested amount. Research grants were easy to obtain, leniently administered and supplements were as near as the telephone. The dyad, science and society, was damaged and grown men were resorting to primal primary group instincts. Few scientists took the situation seriously and it was the Johnson administration that finally took the predictable steps of cutting science budgets and boldly manipulating the expenditure of those science dollars that were offered. Individuals in society were still frightened of heart attacks and cancer and pleased that their children didn't get as many of the previously crippling diseases of childhood. But that wasn't enough. A cultural lag had occurred in both science and society;

and it was society that reacted first. Simply (and incompletely) put, the affluence which society experienced in technological benefits interrupted the idealistic cultural theme which had brought them about.

There have been many changes since the mid-1960's and noble efforts have been made to communicate and to understand; but the bottom line remains the same: the domestication of science. The stimulus of sputnik and the success of the space program added energy to an already spinning slingshot of technological achievement.

The expectations of science: support, support, and more support

The notion of the dyad, science and society, implies a radical difference between the scientific community and the society of which it is a part. Under idealistic cultural conditions the difference may be implicitly accepted and not generally examined. But such is not the case during a period of conflict. What then are the characteristics of science, the community of science doers? How did their expectations of society develop and how did these expectations lead to the domestication of science?

Perhaps the most noticeable characteristic of the scientific community early on was its sense of lineage. Since science, as we have known it, is a relatively new thing, it was not uncommon for individuals to talk of identifying their place, or that of their mentors, in a line of accomplished scientists dating back virtually to the beginning of their discipline. The hierarchy of science was heavily determined by its sense of lineage.

One of the effects of the growth of science was to diminish the importance of this status system; but it was the effect of federal training grants, over and above that of the growth of science generally, that diminished the sense of personal lineage even more dramatically. Although the peer review system awarded training grants to prestigious mentors wherever it could, the sheer numbers of projected trainees demanded awards to individuals and institutions that otherwise would not have been qualified. As the training program declined, Darnell and others finally proposed that federal training dollars for medical science should be restricted to 50 select universities or departments. The quality of training in a department, university or other institution had always been part of the status system of science, but it is interesting to note that this proposal emphasized the institutional aspect to the exclusion of the prestige of the mentor. Training grants are now virtually gone but in their wake there has been considerable loss of the sense of scientific prestige based on either lineage, mentor or institution.

What was the importance of lineage as a status system? Was it to ensure proper training and appreciation of science? Was it a way for young scientists to use recognized scientists for their own interests? Was it a way for recognized scientists to feather their caps? As a matter of fact, it was probably all three. Discovery is a complex and subtle activity usually unformulated in the minds of those who practice it. Furthermore, discoveries are rare and genius is rarer still, so it is not surprising that while competence, curiosity and love of science may be important prerequisites, scientists need the esteem and reassurance of associates while they work. The thrill of discovery, which is widely acclaimed as one of the greatest pleasures of life, is eagerly and competitively sought. It is the chief reward of the scientist. But in the lonely search for discovery, the scientist needs reassurance that he or she is in fact capable. Nor does this need for reassurance disappear with discovery, because scientists, of all people who look at discovery, see the serendipity and the commonplace in their own work.

Another development which diminished the traditional status system of science occurred just after World War II. No one could predict that the discoveries in biochemical genetics during a relatively short period beginning about 1950 would coincide with the growth of national science funding. These discoveries were among the most socially influential discoveries in the history of science. We may not yet fully appreciate the way in which the elucidation of genetic mechanisms has led, partly by shock and partly by concluding from similarities to identities, to the covert assumption that the human is a definable animal. That may be an aside, but whatever the full significance of these discoveries, science at least was changed by them. The following quote from an issue

of *Time Magazine* in 1971 probably summarized the situation best: (referring to Watson and Crick's famous development of the structure of DNA) "Together, in less than two years of work at Cambridge, these two spirited young scientists showed how it is possible to win a Nobel Prize without really trying." That wasn't just a Time-ism. That was the reaction many scientists had to the incredible way in which the story of biochemical genetics, or more generally, molecular biology as it came to be called, unfolded during those years. There didn't seem to be any sense to the information, or to the way in which it was appearing. Belief in the "Central Dogma" actually became for some the mark of an insider. In some situations, talking about molecular biology could be supportive, so scientists assumed superior cliques by formally talking together about the subject. In other more knowledgeable situations where that was not possible, the pressure of discovery sometimes surpassed support systems; individual scientists absented themselves from seminars, from their peers generally, and even abandoned their work. Again, the pressure was not absent in the discoverers: In 1959, Crick wrote: "In the comparative isolation of Cambridge, I must confess that there are times when I have no stomach for decoding." Compare this with the way, in 1954, he and Watson "had sat down in the Eagle at Cambridge" and drew up the standard list of 20 amino acids which we recognize to this day. In time the discoveries of molecular biology became more familiar and we entered a new phase of biological science which was characterized by a curious disinterest in discovery. The new scientist seemed content to add detail to previously existing information, almost to the point of discovery. This is contrary to one of the axioms of science: breaking the paradigm is better than spectroscopy.

Little of the history of molecular biology accords with the traditions of scientific lineage. Furthermore, there have been no enduring new lines of descent from these remarkable discoveries. Those which have appeared have generally been chaotic, institutional and sterile. Although massive funding provided the opportunity for confirmatory and extending laboratory work, particularly in the United States, few landmark discoveries in molecular biology can be ascribed to American science dollars. Watson and Crick discovered the structure of DNA in England. Crick deduced the existence of transfer nucleic acid in England, Jacob and Monod discovered the concept of messenger nucleic acid in Paris. The first breakthrough in the genetic code by Nirenberg came in an American laboratory, but from relatively simple and inexpensive experiments. This is significant because funding has become an eagerly sought and generally accepted status symbol for scientists.

I have spoken of the domestication of science, of society expecting more and more from science and science depending more and more for support on sources outside of its own community. The examples came from my own observations as a biochemist, but I trust I speak accurately for science in a broader context. Others will have to judge that; I cannot. Perhaps the best summary of this paper would be a comment on a statement by Eric Hoffer: "Intellectuals? Give them everything they want, everything except power." That statement makes no sense. If one understands power as influence over others, then to give intellectuals or anyone else everything they want is to give them power. The only way to withhold power is not to give them everything they want or even the promise of it. Had that been the case, perhaps we would not have seen the domestication of science. It was the prospect of unlimited success, either way, that led to the present state of affairs. [Remember, this was written in 1978] It will be a recognition of limits that will turn the wheel of history around again from a pragmatic to a more idealistic cultural theme.