Presentation Given at Middleton Awards in 1965*

…On this occasion, rather than review the contributions made by this year's recipients, it appeared to me that a more useful purpose would be served by analyzing the factors involved in granting an award such as this. At the risk of appearing presumptuous I will endeavor to find some answers to the often asked question: "How do you judge the worth of a research program?" While it is frequently said that there really is no answer to this question, perhaps as a beginning, we can define those criteria that determine which programs are outstanding. Obviously we all cannot be doing work that fits this category, but it does seem to me that we all have a responsibility to try to do so. Perhaps we have an additional responsibility to recognize the men and ideas having the potential of future value. In addition, it has become apparent in recent years that scientific research is consuming increasing amounts of public funds, and we are all aware, that the propriety of this expenditure is coming to be questioned with increasing frequency. We are therefore obligated to search for valid yardsticks which can be applied to make the necessary value judgments. The public and those entrusted with the expenditure of public funds must be acquainted with our efforts in this direction before they apply inappropriate yardsticks with disastrous results.

While I am obviously ill equipped to initiate a discussion of this sort, it is my opinion that such self analysis is essential for science today. Since this opinion is apparently shared by others with much better qualifications than my own I am encouraged to air both their views and mine.

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* Speech given by Dr. Becker during his introduction of the winners of the William S. Middleton Award, investigators Dr. Lucien B. Guze and Dr. George M. Kalmanson, both from the VA Center, Los Angeles. Published in Research & Education in Medicine Newsletter, Veterans Administration, March/April, 1966.

Communication is the lifeblood of science and the current journals represent the marketplace for scientific ideas and concepts. It is here obviously, that the initial value judgments of first the work and secondly the man are made. The eminent British scientist, Sir Peter Medawar, recently made some observations concerning this in a BBC talk entitled “Is the Scientific Paper a Fraud.” One of his major points was that the present form and structure of the scientific paper bear little resemblance to the actual sequence of events, intellectual and experimental, that went into the work reported. The strictures placed upon the scientist by this rigid format, required by most editors and referees, inhibit the presentation of the hypotheses and speculations that perforce were involved in the work.

The presentation of mere data gives us little insight into the all important thought processes of the investigator, and a sterile empty paper results. Since the published paper is of tremendous import to the investigator from a career point of view, it is tempting to pursue studies of a pedestrian nature which predictably will lead to publication. In addition it is obviously economical to plan a program of investigation to fit the pre-existing rigid format for reporting the results. Medawar goes on to state that in the “criteria used by scientists when judging their colleagues’ discoveries, foremost is their explanatory value, their generality and span of reference, and secondly is their clarifying power, the degree to which they resolve what has hitherto been perplexing.” In this regard there is a famous quotation from Max Planck—the creator of the quantum theory—that is most pertinent. Planck states in his autobiography “A new scientific truth does not triumph by convincing its opponents and making them see the light but rather because its opponents eventually die.” Now obviously new concepts call forth a reaction from the general body of science and in particular from those established scientists who are involved in the same area. Even though the highly structured scientific paper of the present day has little place for hypothesis and theories, it is exceedingly difficult to get papers published which even hint at “rocking the boat.” Anyone who does succeed in this endeavor earns our praises for having triumphed not only over nature but over the editorial review boards as well. Without Medawar specifying it as so, I believe his remarks can be taken as dealing with the broad problems of creativity in science—not only how we can identify and encourage those who have this facility but also what obstacles are placed in its path by organized science itself. Obviously he feels that the very instrument by which creative thinking is communicated—the scientific paper—constitutes one such impediment. Perhaps we should think not only of liberalizing the rigid format of the scientific paper but also liberalize our modes of thinking regarding new concepts and theories, not only those of others, but perhaps even our own as well.

Lord Brain, in discussing Medawar’s remarks has stated that medical research may be exceptional among the biological sciences both in the “hypothetico-deductive system utilized by the investigator and in his method of reporting his results.” He feels that in medical research the factor of serendipity is operative to a far greater extent than in any other field. However, I feel that his definition of serendipity carries with it a rather insulting connotation for the medical investigator. His definition is “the faculty of making happy and unexpected discoveries by accident.” Now I do not believe that all or even most of the significant work in medical research is merely the result of accident and I would propose to broaden the definition of serendipity to include the ability to recognize the significance of apparently unrelated work reported by others and to apply it in an appropriate fashion to his own problems. I am prepared to defend this as being more than an integrative process and something that cannot now (and probably never will) be accomplished by computers.

Since I have already taken liberties with these comments of my peers, I will recklessly continue and propose some additional criteria of my own. While no one can dispute the value of a paper that “explains what was previously unexplainable,” as per Sir Medawar, what about the reverse situation? I believe that a paper presenting data that renders unexplainable what was previously satisfactorily explained is equally valuable. Such destruction of cherished dogma formed the basis for modern science and we must always have a place for it—even today.

Of equal import to these characteristics of the work done are certain characteristics of the worker himself. Two of the most important of these are, in my opinion, courage and intellectual curiosity. A generous supply of the former is needed by any investigator who deliberately chooses to work in a field of research that has been little explored, where there have been no noble predecessors to set down guidelines and where one cannot predict with any degree of confidence that some publishable data will be obtained. Such individuals are usually motivated by the second factor, true intellectual curiosity—a desire to find the answers to problems, almost solely for the satisfaction of having engaged nature in combat and having won. I might venture the opinion that the quality we call creativity rather than being 1 percent inspiration and 99 percent hard work may be composed of equal parts of serendipity, courage and intellectual curiosity.

When one examines the bulk of published scientific papers of today and attempts to assay their significance and the qualities of the investigators represented, we find for the most part little resemblance to the criteria we have discussed here. Most papers resemble pedestrian strolls through already well cultivated areas by investigators playing the academic game of publish or perish….