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The IBRO History of Neuroscience Committee devised the following questions for Professor Mountcastle:

1. Where were you born and raised?

I was born in Shelbyville, Kentucky on July 15, 1918, but from an early age I lived in and grew up in Roanoke, Virginia. My family on both sides has a long history of residence in Virginia, and I have always considered myself a Virginian.

2. Where were you educated and which of your teachers had the greatest influence on you?

My mother had been a professional teacher before marriage, and she taught her five children with great skill and persistence. She taught me to read and write by the time I was 4 years old. Thus when I entered the public school system in Roanoke, Virginia, I rapidly skipped two grades. I graduated from high school in 1935, and then attended Roanoke College, in Salem, Virginia - a nearby town. I went through the standard college course in three years, and earned a B.S. in Chemistry in 1938, graduating from college at the age of 19. I then entered the Johns Hopkins University School of Medicine, graduating in 1942. During my medical school years I had every intention to follow a career in Neurosurgery, with no thought of a career in research. I interned in surgery in the Johns Hopkins Hospital under Professor Alfred Blalock. I then served three years in the United States Naval Amphibious forces, and participated in the invasions at Anzio, Italy and Normandy, France during the Second World War. After my naval service I returned to Hopkins for a fellowship in Neurophysiology, with the intention of going forward in a career in Neurosurgery. However, I became so intrigued by research into the physiology of the brain that I never left laboratory research. I never did an experiment until I was 28 years old, and I suppose I would be regarded as a latecomer to research.

I have spent my entire professional life at Johns Hopkins, becoming successively Professor of Physiology, then Director of the Department of Physiology, and lastly University Professor of Neuroscience. The University generously allowed me to continue research and scholarly work after the usual retirement age. During my post-laboratory years I completed and published two monographs and four extended reviews in Neuroscience. I finally retired completely in November 2005 at the age of 87.

The person with the greatest influence on me was Professor Philip Bard, who was Director of the
Department of Physiology during my time from 1946 to 1964, and whom I succeeded in that position. It was he who first showed me the pleasure, indeed the intense joy, that follows any discovery - however small - and particularly those of the way the brain controls behavior.

3. **When did you start your career in scientific research and in what area?**

I began my research with Bard in 1946, using the method of cerebral ablation in an attempt to dissect the various components of the Kluver-Bucy syndrome, in macaque monkeys; this was not successful, but revealed behavioral abnormalities that follow removal of the amygdaloid complex. After that, I was allowed to go on my own into electrophysiological studies of thalamus and cortex, which I continued throughout my research life.

4. **What do you consider to be your most important studies and contribution to biology or medicine?**

Some observers comment that the discovery of the columnar organization of the cortex is my most important contribution. However, I believe that there are others of equal if not greater importance, particularly because they address the questions of the dynamic activity of the brain, while columnar organization is a static property.

The first is the concept of the cerebral cortex as a distributed system, that signals are embedded in the population activity, and the development of methods for direct study of this activity in waking monkeys, following on the contributions of Jasper and Evarts. The second is the study of the dynamic activity of the cerebral cortex in waking monkeys as they executed sensory tasks, at first in the somatic sensory cortex. Later I translated this method and concept to study of the homotypical cortex, described in my paper on the parietal cortex in 1975.

5. **Have you invented any new techniques?**

I adapted the method of single neuron analysis to the study of sensory and other behavior in waking monkeys, working in learned tasks. But this method developed quickly in several laboratories, and is not in the least wholly my own.

6. **What do you think about awards and for you, which have been the most truly emotive?**

Some scientists believe that the prospect of awards is a strong motivating force for investigators. However, I believe that there is a powerful negative effect: they produce in the minds of many, particularly in the media, the idea that all non-award winners are second-class citizens. We all know that this is not true. Beyond that, the prospect of some awards produces an intense degree of unsavory activity on the parts of some who regard themselves as candidates.

The most 'truly emotive' award for me was the award of the National Medal of Science, conferred by President Reagan in 1986. The setting in the East Room of the White House was impressive enough, but beyond that they had invited my (then) five grandchildren, ages 5-12 years to come along. For an hour these children had free run of the public floor of the White House. They will never forget it, and neither will I.

Emotive is a hard word to define. The award of the Helmholtz medal in 1982 was intensely emotive for me because my long-time and very dear friend Donald Mackay gave the citation. Similarly, the Lasker award had strong emotional meaning for me because I shared it with a lifetime friend, Eric Kandel. The Fyssen award in 1983 was similarly emotive because the citation was given by my long-time friend, Yves Laporte.

In summary, I would say that the emotional appeal of an award has to do more with the individuals associated with it than with the award itself.

7. **Can you sum up the attributes that you possess which have made you a successful scientist?**

I give these answers in no particular order of importance.
i. The first is an enduring curiosity about the nature of things, and particularly about the function of the brain.

ii. The capacity and the endurance to work long hours without fatigue. This is especially true for the waking monkey experiment.

iii. To derive satisfaction from the company of younger colleagues in research, and the willingness to share credit with them, and to take the blame for whatever debit arise. For example: of my two papers of 1957, the second on columnar organization is authored by me alone. This is because my two co-authors of the first paper were so afraid of making a major error in presenting what some regarded as a radical hypothesis, they requested me not to include their names on the second paper. In a sense they were right, for I endured nearly a decade of calumny and contempt, before the fact of columnar organization was confirmed by others, in other areas of the cortex.

iv. The capacity to face the facts of experimental results, even though they may go against one’s most cherished hypotheses, and to challenge even the most well-established concepts in the field.

v. For a departmental director, the capacity to change rapidly to and from those (minor) problems that continuously arise, and to solve them quickly, without diversion from the ongoing experiment.

8. What do you think about the use of the impact index to evaluate scientific performance? What do you think is the best way to evaluate a scientist?

The impact index is useless. The best way is by (a) intensive study of his published work - which takes a lot of time; (b) by measuring the excellence of the work of the people he has trained; and (c) by spending a day in his laboratory, and if possible observing an ongoing experiment. I have found the latter the most important. The performance of an investigator as he executes an experiment is the most revealing, at least for central Neurophysiology.

9. What suggestions have you regarding the best ways to support scientific research and to safeguard its integrity?

I believe the current method of the USPHS is almost ideal, when executed properly. The secret is exposure of the critiques of an application, by two assigned reviewers, to the public criticism of the entire panel of 12-15 experienced scientists. In this public debate any personal prejudice on the part of the assigned reviewers is revealed, and thus occurs rarely. I think all observers with experience will agree that about 50% of the research funded by NIH is useless. The problem is, which 50%? So, one leans backward and tries to avoid error. The problem now is that competition is so great that applicants dare not propose very radical and untried experiments, though these may in the end be the most productive.

How to safeguard integrity? Firstly, select individuals of noble character. Secondly, never select men who enter science for other reasons - to make a career, to make money, etc. - let them go elsewhere.

10. What in your opinion makes a review of a submitted paper of most value to the author and the journal?

1. Evidence that the reviewer has worked at it, e.g., has read the important references.

2. Evidence that the reviewer knows the field, and has no personal bias.

3. Evidence that the reviewer has penetrated into the experiment methods themselves, as well as the results.

11. What are your views concerning the balance between applied and basic research that should be considered by funding agencies?
I suppose this question refers to what is termed (by Deans!) as ‘transitional research’. What an abomination! The basic scientist must be allowed to move wherever his curiosity and his results take him. The historical evidence is unequivocal: many basic discoveries which led to important advances in medical therapy were made in studies that had no clinical objective at all. On the other hand, there is a zone of research of great importance in the 'translation', but this is no substitute for the basic research that precedes it.

12. **What do you think are the neural substrates that make a human being human? In other words, what is special about the brains of humans and how does it differ from that of other species?**

What is special is, of course, the explosive expansion of the homotypical cortex, in all lobes of the brain. What is special in terms of brain function, is that humans observe themselves braining! That is, we are aware of ourselves perceiving, thinking, recalling from memory, generating action upon the environment, etc. Whether this extraordinary degree of internal awareness is present in other species is unknown, but given the facts of evolution, I believe it is present in the higher primates, to a gradually increasing degree, but with a step function to man. Whether this internal awareness is equivalent to consciousness is a question of intense discussion and debate. I believe the way to close in on this problem is to study the brain mechanisms in the simpler forms of awareness, and then gradually increase the complexity of the questions asked. However, I do not believe that methods which answer the question where, as in brain imaging experiments in humans, will answer this question. Where is not how.

13. **What are the three most important scientific questions you would like to answer?**

1. What is the local intrinsic operation in the cerebral cortex?

2. How are neural 'codes' embodied in the population activity in distributed cortical systems?

3. What are the neural mechanisms of the trans-cortical sensory motor linkages, e.g., between parietal and frontal lobes?

14: **Do you think there is life outside of our planet? If so, do you think that is possible that a sophisticated brain like that of human beings exists?**

Impossible to answer. But given the large number of stars and planets it seems likely that somewhere there exists a planet on which life evolved; that life, if it evolved, may quite different from our own. The distances are so great that we will probably never discover whether this is true or not.

15. **Do you think that computers will some day be capable of simulating the human brain.**

No, but one can never prove a negative!

16. **Do you think that the development of neuroscience research, and in particular the advances in our knowledge of the human brain, will change the course of the history of humanity? In other words, do you think that neuroscience will have a deep social, cultural and educational impact in the future?**

No. Not until we understand why we keep killing each other at such a rate.

17. **What factors in your general life influence you most? How have your family influenced your scientific work?**

i. The total and lifelong love and support of my beloved wife, Nancy Clayton Pierpont, and her tolerance of intensive work schedules.

ii. The ambience and total support of the Johns Hopkins University School of Medicine. When I returned to a fellowship after the Second World War, I soon discovered that the Dean and my
revered teachers thought that I was better than I thought I was! This pull from above has driven me all my life.

18. What are your hobbies?

I have been an outdoors sportsman all my life (up until about age 85). I played tennis from the age of 8, and after coming to Baltimore began sailing on the Chesapeake Bay. For many years we have lived in the country north of Baltimore, and enjoyed horsemanship (Arabians), and most recently, gardening in my old age. I am an avid reader, mostly of classic English literature, and of ancient history.

19. What you makes you most proud as a professor and scientist?

i. That some of my own research accomplishments have now become part of the general neuroscience zeitgeist.

ii. The phenomenal success of many of my postdoctoral students; n = 50.

20: What is your advice to young researchers?

i. Find that subject that intrigues you so that you must work on it, regardless of all else, e.g., career, money, etc.

ii. Get to work on it. If less than 60 hours a week, you are a part-time neuroscientist!