A Case History in Biological Research

In recent years I have been interested in some of the most recent developments in the field of biological research, particularly in the area of gene expression and its regulation. I have been particularly interested in the role of various transcription factors in the regulation of gene expression.

I have been fortunate to have worked with some of the leading experts in this field, and I have been able to contribute to the ongoing research in this area. My goal is to continue to contribute to our understanding of the complex processes involved in gene expression and to develop new tools and techniques to further our knowledge.

I have also been involved in teaching the principles of gene expression to both undergraduate and graduate students. I have found that teaching is a rewarding experience, and I believe that it is important to share our knowledge with the next generation of scientists.

In conclusion, I am excited about the future of research in gene expression, and I believe that there are many exciting opportunities for new discoveries in this field. I look forward to contributing to this exciting field of study and to continue to learn and grow as a scientist.
world, on the exchange of ideas, between disciplines. This interaction includes the vigorous exchange from theoretical science to empirical observations.

Scientific communities, in the cross-fertilization between disciplines, and last but not least, in chance, geographical proximity, and opportunity. It would, like finally to complete this case history with a description of the present state of the patient, and a prognosis of his future development.
Under the circumstances, I hope I will be forgiven if this presentation is given from a
personal viewpoint. After graduating from the
University of Wisconsin, I took my chemistry, a year's
work in biochemistry and microbiology.

The opportunity of taking graduate work at this university
under the direction and leadership of Dr. E.B. F.

Prof. W.H. Peterson and Prof. E.B. Fred. During this
period, I attended the course in the early 30's, the exciting area
of development was concerned with the study
of the so-called "growth-factors" for microorganisms,
for the most part, mycelia and unidentified.

At this time, I became deeply involved in this area, and
was fortunate to have been able, with Prof. H.G. Elwood, then
residing at Wisconsin, to identify one of the required
growth-factors for this fungus based bacterium, as
the recently synthesized vitamin B, or thiamine.
This was in the time before the universality of
original germ theory, and the bases of evolution had
been clearly defined. Although the vision
of P. A. Khorov and P. B. V. Kuchkov had
indicated the necessity of microorganisms
for "growth factors" beyond simple growth of syntheses, and
concluded their finding with evolution, particularly
in relation to the complex environment of
"pathogenic" pathogenic microorganisms, the tendency
at that time was to consider "growth factors" as
highly individual differences, peculiar to particular
strain or species of microorganisms, and

to consider their variation in these respects,
consistent with mutation in higher organisms. Actually, my synthesis
and

analysis of genetics was probably typical of
that of most biologists and microbiologists
of the time, with
primarily

genetic concepts being a course on mutation.
After completing graduate work at Wisconsin I was fortunate to be able to spend a year at the University of Leiden under Prof. F. Kuyt, the discoverer of vitamin, and to work in the same laboratory with Prof. Nils Fries, who already had contributed significantly in the field of nutrition. At this time, Prof. Bendel was just moving on a scholarship of the Stanford University, and invited me to join him in the study of the eye-color hormone of Drosope. While he and Prof. B. Ephrussi were in their work at Caltech and in Paris, Ephrussi had so brilliantly established as products of gene-controlled reactions that my first contact with modern genetic concepts, as a consequence of the behavior of factors—the observation of Mendelian inheritance of Ephrussi in Paris that the phenotype was concerned with the eye-color hormone production; these studies on the mutation of Drosope in sterile culture; and the chance contamination of the cultures of Drosope, were a particular challenge.
able to isolate the first hormone in bacterial crystalline state from a culture medium under Dr. H. J. Mayer as supplied with typhoid vaccine and to identify it as typhogenin. It is isolated by Nastuk and later structurally identified by Budensett. It might be possible that less typhogenin, has some been recognized to play a central role in many organisms, many mammals and humans.

As P. B. Beadle has pointed out, at about this time, out of many discussions and considerations of the general biological applicability of chemical genetic concepts, stimulated by the wealth of possibilities among the microorganisms and their variation in nature in respect to their nutritional requirements, we began our work with the mold Neurospora.
I shall not enumerate the factors involved in the selection of the experimental product, nor the details of the experimental procedure, but must take this opportunity of paying my unceasing gratitude to the finding of a number of talented investigators, notably Prof. B. O. Dodge, for his establishment of this ascorbic acid as a most suitable organism for genetic studies. It was through him that the chemical activity of ascobic acid was observed by Prof. T. H. Morgan, who became of great interest to Prof. B. O. Dodge, and to Prof. C. C. Lindberg, who became interested in the work of Prof. Dodge through a close friend of Prof. Dodge, Prof. T. H. Morgan's contact with Prof. Dodge.

Our use of Neurospora for chemical genetic studies would also have been impossible, if not much more difficult, without the availability of synthetic ascorbic acid as the result of the work of Kuhl and of the Vigneaud. In addition, the discovery of the mutation of ascormycetes...
The most helpful, as shown by the

findings, is that the synthesis medium

used for Neurospora was that described by

in 1935.

This, supplemented, with hydro, and referred

in the Neurospora formula

as "Fris medium". It should also be

pointed out that it should also be

pointed out that the feasibility of producing

the second, mutational deficient mutant strains

could not depend on the early work of

Rudiger, and in that of Prof. H. J. Muller,

and ultraviolet light

on the mutagenic activity of X-rays or Rontgen

light that was needed was to put these

facts and findings together, to produce a

with mutagenic, mutatorically deficient

(hermaphrodite) mutant strains of Neurospora.

Each single

cert and to show that these deficient genes

were associated with mutation of single

gene.