

it is—or was then in the process of—becoming. I would insist that the new paradigm I have tried to describe is taking hold, particularly among the younger members of the profession. On balance, however, there is little doubt that most of the work in the field is still descriptively, rather than causally, oriented.

In relatively few studies is the central problem of explaining change directly considered in terms of either the relevant data sought or the conceptual apparatus used. In spite of the widespread acceptance in theory of regional-ecological models that should depend on rigorous statistical sampling, the predominant focus of research for most investigators remains the arbitrarily chosen slice of a particular ancient site that is excavated. Specialists in the natural environment are still too often employed as technicians "expected to provide ready answers to poorly formulated questions" (9), rather than engaged in a genuinely collaborative study. Problems and objectives too often are formulated only as arm-

chair generalizations, rather than as sharply focused hypotheses to which definitive answers must be sought in regional or local sequences. Meanwhile, separated by a wide gulf from the former, the conduct of much research at the local level remains practically innocent of relevance to any theoretical problem whatever.

Holistic presuppositions about societies in general also have not been sufficiently clarified and tested. As a result, there may well be an excessive emphasis in archeological interpretation on stability rather than instability as the salient human condition. To phrase this differently, an oddly antiquated, almost Victorian emphasis continues on institutions and behavior that performed integrative functions—art styles, rituals, elites—at the expense of conflict, marginality and dissonance as sources of creativity and change. Possibly for the same reason, most reconstructions of archeological sequences still consist of a succession of qualitatively distinct, smoothly functioning phases or stages rather than of the con-

tinuous interplay of forces marked at intervals by new transformational forms. Finally, most of us remain excessively timid, reluctant to tackle the grand problems of comparison, generalization, and synthesis, even though the certainty of being found in frequent error if we did so ought to be heavily outweighed by the opportunity to deepen, sharpen, and ultimately justify our inquiries. But, before I allow these criticisms of the present state of archeology to seem overwhelming, perhaps I should ask whether at least some of them do not have analogs in the natural sciences as well.

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## The Development of Retinal Neurophysiology

Ragnar Granit

For some 20 years my main experimentation has not been within the field of retinal electrophysiology; my interests have shifted to problems of motor control—chiefly muscular sense organs and quantitative properties of firing motoneurons. For this reason my lecture will be as retrospective as the Award and deal with the rebirth of retinal studies that was inspired by the increasing accessibility of electronic equipment from the mid-1920's onward. Before this era, vision was chiefly studied by psychophysical methods; these had led to well-established correlations between perceptions of such things as color and luminosity and physically defined entities within the centimeter-

gram-second system of units. But in 1926 Adrian and Zotterman (1), using the afferent nerve of a stretch receptor in a frog muscle, had shown that it was possible by amplification to record the impulses in single nerve fibers, and in 1927 and 1928 Adrian and Rachel Matthews (2, 3) published their important experiments with the mass discharge in the long optic nerve of the *Conger* eel. Evidently the electrophysiological approach was the one now destined to pave the way for deeper understanding of how this, our noblest sense organ, had organized its interpretation of the world of light, form, and color.

Frithiof Holmgren (1831-97) (4) at

Uppsala, who in 1865 discovered the electrical response of the retina to light, the electroretinogram of today, had realized that many problems of vision could be analyzed by what he called his "method of objectivating the impressions on the retina," but amplification with the aid of the vacuum tube gave this general idea quite a different dimension; it was—Lord Adrian said—as if we had got a new, very powerful microscope to work with.

The basic idea of my own approach was formed during my study of Ramón y Cajal's (5) classical description of the retina as a "true nervous centre," as clarified by his silver-stain preparations and likewise suggested by its embryological development from the brain. It seemed to me likely that psychophysical data might, with some profit to the field, be translatable into neurological equiva-

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Professor Granit won the 1967 Nobel Prize for medicine or physiology, along with H. K. Hartline and George Wald. This is the text of his lecture before the Royal Caroline Institute on 12 December 1967. He is professor emeritus in the neurophysiological department of the Medical Nobel Institute, the Royal Caroline Institute, Stockholm 60, Sweden. The article is published here with the permission of the Nobel Foundation and will also be included in the complete volume of *Les Prix Nobel en 1967*, as well as in the series *Nobel Lectures* (in English) published by the Elsevier Publishing Company, Amsterdam and New York.