With regard to "objectivity" I have repeatedly emphasized the point that all scientific undertaking depends on inborn foreknowledge as well as on our inherited central propensity structure (see Lecture on Epistemology and Evolution). All science is selective in the sense that it weighs the facts of experience and that it chooses certain hypotheses, thus bypassing the multitude of possible hypotheses. Thematic presuppositions have always been part of scientific discovery (28). This kind of statement does not imply permission for uncontrolled phantasy or deliberate violation of the consensus principle. The statement means, however, that even objectivity in the strict sense of the consensus principle does not guarantee that we are unbiased or neutral against the real world "out there."

In evolutionary taxonomy (at least with regard to higher taxa) the consensus principle is strictly obeyed with respect to the rules that ought to be applied. The individual taxonomist's degree of freedom is strictly limited in this regard. And that is probably all that can be demanded.

Science is a social activity. The scientific community, past and present, represents the social dimension (1) of the scientific enterprise. The scientific community is commonly regarded as the internationale of fellow professionals who scrutinize each other's work, correct it, and carry it on. This latter point implies "tradition," in practical terms, the instrumental, intellectual, and moral training of the student as part of his preparation for future membership in the scientific community. In the first lecture (Prologue) I discussed the reasons why the scientific internationale is a purely professional one and carries no political weight.

There are two types of scientific communities depending on the forces that keep the communities together. The global community of all natural scientists is operationally defined by the allegiance of its members toward the normative code of science, the intrinsic values of science. Since this point is essential for the function of the scientific enterprise, the ethics of science deserve a thorough consideration in a separate lecture (see next chapter).

A great many scientific communities of limited scope and membership consist of the practitioners of a particular scientific discipline. We all know the main scientific professional groups such as physicists, chemists, botanists, astronomers, and the more concrete groupings, professional societies. However, communities in this sense exist at numerous levels, including the official academies and the unofficial "invisible colleges" around some outstanding person with a high degree of scientific leadership and charisma. These scientific communities play a role (sometimes disagreeable) as restrictive guilds or select fraternities since membership depends on election or some other kind of adoption. More recently, scientific communities have become established that are defined by instrumentation (Electron Microscopical Society), by particular techniques (Spectroscopical Society), by environmental factors (Photobiological Soci-
The acquisition of paradigms by a person is an act of mental relief. It frees the mind for high level research of the kind which Kuhn (2) calls puzzle-solving. Kuhn compares normal science, proceeding under the guidance of a paradigm or a set of paradigms, with puzzle-solving: it requires high skill; solving a puzzle does not necessarily have much value for anybody else; the existence of a solution is assured in advance and the kind of solution that is wanted is known. If I give a sample of crystalline anthocyanin to an experienced organic chemist and ask him to determine the structure, I know the kind of solution of the problem and I further know that a solution exists.
The notion of puzzle-solving is not to be taken as a *negative* metaphor. Rather, in a mature science a high value is accorded to puzzle-solving abilities.

“Normal science” and “puzzle-solving” do not imply the notion of a straightforward process, governed only by evidence and logic and virtually free from intellectual fights and irrational elements. All novel ideas and data, in “normal science” as well as in “revolutionary science”, will meet great resistance that can only be overcome gradually. Even a minor step can encounter strong and embittered reluctance if it contradicts an alternative concept that somebody else has staked his reputation on. Progress in “normal science” as well as in “revolutionary science” is made by man and thus involves all features of the nature of man, whether we like them all or not.

Another point in “normal” science, which is seriously underestimated by Kuhn, concerns the willingness and ability of the scientist to consider the possibility that paradigms must be revised or exchanged. Scientists in mature fields are certainly conservative. Their reluctance to accept novelties—on the level of constructs and theories—is legitimate. It would be very unwise to treat every set of discordant data as a falsifying argument against a theory. However, if the existence of data that are not compatible with existing theory has become an inescapable fact, no experienced scientist would hesitate to question existing theories. The decisive element is facts even though these facts have been obtained within the intellectual and methodological framework of existing paradigms. In science, objective data are stronger than paradigms. However, the objectivity of the data must be proved with the highest degree of confidence.

I want to discuss now some crucial problems concerning “normal” science proceeding under the guidance of a set of paradigms.

1. According to Kuhn, authorship of books ceases to be a principal sign of professional achievement in fields governed by paradigms. “The scientist who writes (a book) is more likely to find his professional reputation impaired than enhanced” (2). In a mature field ruled by accepted paradigms the highly specialized and technical research communication becomes the vehicle to spread information through the community of fellow experts. This has usually the consequence that subcommunities originate (e.g., the Drosophila group or the phytochrome group in developmental genetics) with relatively few members and with a strong tendency for intellectual inbreeding and self-flattery. As a rule, superior books or integrating monographs are indispensable for reuniting from time to time the diverging subcommunities. This kind of book is usually written by a strong personality.

2. Research in a mature field with accepted paradigms shows a strong tendency to proceed according to the motto: science for science sake. As already pointed out, Kuhn compares this kind of research activity with puzzle-solving, and Jevons (4) makes the criticism that “while a paradigm or set of paradigms may be very successful in guiding puzzle-solving, it may also orient scientists away from socially important problems that are not currently reducible to puzzle form. Scientists tend to tackle problems they can solve rather than problems that “need” solving from society’s point of view: to investigate fundamental particles or build up complex heterocyclic molecules rather than find a cure for cancer.” In my opinion this kind of critique—although quite common and to be taken seriously—misses the point completely. Certainly, the academic freedom of science has led to much “high-sounding humbug” (to use Jevon’s (4) expression); however, the fact that a closed discipline of the natural sciences is based on truly reliable paradigms and has a wealth of equally reliable laws and facts available by far compensates for some too esoteric adventures. It is the set of established and approved paradigms together with the reliable empirical laws and facts of a mature scientific discipline that can serve any time as a trustworthy basis for rapid technological innovations. None of us knows which kind of sophisticated technology we will need tomorrow simply to survive. However, if we have at our disposal the scientific information of a whole discipline, intellectually organized in paradigms, laws, and facts, the technological innovator can hardly fail in solving any problem that can be solved by technological means. From the point of view of human survival we must champion pure, basic research for the reason that it is the only means of providing for a flexible technology. The network of scientific information must be at hand when the need arises. Mission-oriented research must fail if the soil has not been prepared by curiosity-oriented research (see Fig. 22).

3. Editors of leading scientific journals and the reviewers they engage to examine the submitted manuscripts are in a particularly difficult position. They must demonstrate some allegiance toward the established paradigms and thus impede unjustified attempts to violate or to overthrow the paradigms (in other words, they must counteract any premature “scientific revolution”); on the other hand, however, they must remain agile and vivid enough not to decline those papers that possibly lead a field to new frontiers.

There are many reports (tales and fairy tales) about editors who failed to maintain the right balance between being conservative (favouring “normal” science) and being progressive (permitting a bit of “scientific revolution” once in a while). Since most of these criticisms stem from retrospect, they are rarely fair. Failure in this matter is not always an indicator for mental rigidity. While it is indeed hard to find and to keep on the small path between justified reluctance against surprising novelties and narrow conservatism, a rather clear distinction can be made between being cautious and being biased. The decisive point in the evaluation of a paper is the validity of the empirical facts and the internal consistency of the argument. Provided the facts are clear-cut beyond reasonable doubt and the internal
in the young scientist a critical attitude, a readiness for creative thinking and even for scientific revolutions. The motto of education in science could be: be committed to existing paradigms, but at the same time be committed to discovering new patterns of thought.

Education in science, i.e., teaching and learning of behavioral patterns and paradigms, is similar to teaching and learning in other fields of human life. As LEOPOLD noticed, “the budding scientist may make a strong set of behavioral fixes based on his earliest science experiences in graduate training . . . the same early fixation of behavior could be expected for any profession or craft which involved an extensive apprenticeship” (5). The imprinting influence of a major professor on the budding scientist is easy to discover, from speech mannerism and gestures to the loyal defence of the major professor's scientific hypotheses (5).

The psychological component of our educational system is far from perfect. I agree with LEOPOLD's analysis (5): “Out of the generous number of people trained to the Ph.D. in science, relatively few remain active in research, making for a low ratio of people trained to those active as professional researchers. I feel that this inefficiency is due in large part to the failure of the training system to give young students an enthusiasm for entering the competition and being willing to take the buffeting that is a natural part of the aggressive and competitive interactions with other scientists.” We must teach students as early as possible and with much more persuasiveness than usual that competitiveness is a major positive factor in bringing about good science and that an enjoyment of participating in competitive interactions with professional colleagues is an indispensable part of the scientist's life.

5. The scientific communities cannot afford a serious break in tradition. Even in periods of rapid changes in constructs, styles of thinking, and experimental techniques, a solid core of paradigms, normative behavioral rules, and proved institutional structures must be maintained. The scientific community is very fragile and vulnerable. It constantly requires stabilizing elements. Its existence is not compatible with anarchy or chaos. While it is no bad thing to question and reexamine fundamental beliefs and assumptions, we must do it in accordance with the scientific code of conduct, the ethics of science.

6. According to KUHN (2) a paradigm shift constitutes a “scientific revolution.” Indeed, when a paradigm or a set of paradigms is renounced, the world looks different. However, as KUHN (2) notices, “The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgement leading to that decision involves the comparison of both paradigms with nature and with each other.” Apparently it is an innate drive in human nature to replace obsolete paradigms by new ones. A horror vacui with regard to paradigms is indeed a characteristic of human nature. This is obvious in all human life: it has been amazing to watch our rebellious youth in Europe immediately replacing declined
cultural paradigms with new ones, even though the new ones look immature and miserable: le roi est mort, vive le roi! The crucial question of whether or not the new paradigms are really superior to the old ones, can only be answered in retrospect.

The Copernican revolution is a favored example for a dramatic change of paradigms: Ptolemy placed the earth at the center of the universe, Copernicus, the sun. Both looked at virtually the same data. Each system is perfectly logical and self-contained. Copernicus never ventured to give preference to his own system. In retrospect we notice that the Copernican revolution really occurred. In Kuhn's words: "The very ease and rapidity with which astronomers saw new things when looking at old objects with old instruments may make us wish to say that, after Copernicus, astronomers lived in a different world. In any case, their research responded as though that were the case" (2).

The transition from classical to molecular genetics, which most biologists might consider to constitute a paradigm shift, is another interesting case history insofar as the central paradigm in the new set was soon called a "dogma" (6). This "central dogma" of molecular biology includes the double helix model of DNA, the "autocatalytic" function of DNA in serving as a template for the synthesis of replica DNA chains, and the concept that genetic information, piled up in the nucleotide sequence of DNA, will be transcribed into a nucleotide sequence of RNA and translated into amino acid sequences of protein whereby the flux of information occurs only in the nucleic acid → protein direction but never from protein to nucleic acid. The central dogma is, of course, not a dogma in any ideological sense. It has not been intended—as CHARGAFF (7) feared—as a starting point for a new kind of normative biology that commanded nature to behave in accordance with the models. "Central dogma" is just a fancy (but psychologically interesting) name for a biological principle that is open to refutation any time. Actually, at least the mechanism of DNA replication in vivo is still open for debate and largely obscure.

Some sincere critics, in particular CHARGAFF, have blamed the avant garde of molecular biology for not being in line with the proved tradition of scientific inquiry. "When molecular biology appeared on the scene, ..., the publicity machines were all in position, and it was time for the saturnalia to begin in full force" (7). I leave it open whether or not this bitter argument is really justified. It could be that the creation of some new set of paradigms at the present state of science and society must necessarily be correlated with terminological fancifulness, unusual snobism, and even with the neglect of established rules of behavior. However, CHARGAFF insists that "many of the great constructions of our time—existentialism, structuralism, transformational grammar, the central dogma and some other sloganized tenets of molecular biology—have all looked, from their very beginning, somehow shoddy and overblown" (7).

7. A paradigm shift, and even a minor change in the set of established paradigms, is often followed by the super dominance of a particular approach. This may cause very serious and largely irreparable damage to some scientific disciplines. As a sad example, the rise of molecular biology has led to the virtual extinction of whole fields of traditional biology. The "noble study of botany" (to quote CHARGAFF (7)) has been all but banished from many universities. The diversity of methodological and intellectual approaches in experimental biology has been narrowed down to the use of high-speed centrifuges and scintillation counters. A still graver risk for the future of biology as a diversified discipline is the very "modern" anthropocentric attitude. This is a basic paradigm shift in biological sciences leading to adverse consequences on all levels of the scientific enterprise. It is a serious signal for the end of the free inquiry and for a severe constriction of the human mind if research in plant development must be justified toward the granting agency as a contribution to cancer research in man. While it is indeed probable that basic research with plants will contribute to the final understanding of the phenomena of cancer, it is not only a loss of diversity, but also a loss of dignity, if we can no longer afford to tell frankly that we are interested in studying plants for the sake of genuine knowledge about plants. The enforced anthropocentrism in present-day research and teaching is the greatest menace for the future of biology as an autonomous science.

Anthropocentrism in science is by no means to save our endangered world. It is true, of course, that we must understand man in terms of solid science. However, we also must understand and appreciate the rest of the universe in order to make our knowledge about man really fruitful. As I pointed out in the introductory lecture, it has been the autistic anthropocentric attitude that has driven man to the verge of extinction.

8. What does progress mean in science? The notion of progress always implies improvement, higher quality. A purely quantitative accumulation and compilation of largely unrelated data should not be considered as "progress." Rather, "a record of steady but pedestrian work over a period can finally convince observers that a field is irremediably boring" (8). Moreover, we all know that there is a category of "pointless publications" or what RAVETZ has called "shoddy science," i.e., immature, ineffective, or rotten fields of inquiry. As RAVETZ puts it: "I have explained how under certain circumstances it is possible for published reports of research to contain no substance, but to consist of unsound data, interpreted by an incoherent argument, which leads to a vacuous conclusion. This can happen in spite of scientists' good intentions, in an immature field; and it is brought about by a corruption of standards in the case of shoddy science in an established field. Whatever the cause, we are thereby reminded that publication does not equal progress" (8). However, progress in science is a real thing at least in many fields. There are useful internal criteria for progress in most scientific disciplines.

Progress in science means that the theories and laws become "better."
This implies higher accuracy of prediction and explanation, wider scope, increase of simplicity, and esthetic appeal. "Progress" further means that the trustworthiness of the leading system of paradigms increases. Indeed, scientific progress is not so much related to "scientific revolutions" but to the increasing consolidation of a given set of paradigms.

As a first approximation scientific progress may also be measured by the augmentation of the stock of objective data ("facts"). This implies, however, not only that the number of absolutely trustworthy singular propositions ("facts") increases, but also that the "facts" become organized and available for any actual need, be it theoretical or practical.

An estimate of the future progress ("potential," "fruitfulness") of a scientific discipline is difficult to achieve. Personal judgements of leading scientists and administrators of the granting agencies play a major part even if the reviewing process is performed by panels. As a rule, there is no straightforward choice of a research program in the competition with others that could be called "fully rational." It is in part at least a matter of science politics, rather than a scientific matter, and science politics are only slightly more rational than general politics. This is by no means a statement of principal criticism. I cannot think of any better way to administer science than it is actually being done by the leading agencies. However, as scientists we must be sober enough to realize that the demand for rationality generally accepted within the sciences cannot be met in science politics and science planning. Moreover, the public and the legislators will inevitably refer to "usefulness" when the further support of a scientific field is being considered.

There are a number of obvious obstacles to progress in the sciences that have their root in human nature. Max Planck once said: "It is not that old theories are disproved: it is just that their supporters die out" (9). As every caricature, this statement contains the usual grain of truth. However, as I pointed out, some obedience or even allegiance to paradigms is essential, not only for the sake of continuity (tradition) in science, but also for the actual maintenance of stability in science as a largely intellectual enterprise. The paradigms determine the intellectual climate. This implies (at least for most of us) that they determine the manner in which we look at things in science and that they prevent us from seeing or believing things or features of things not in agreement with ruling paradigms. As Bondi puts it: "Sometimes attitudes that are possibly underlying particular points in theories and outlook are referred to among the cognoscenti as 'folklore'. There is a good deal of such folklore in physics, and in our interpretation and understanding of physics: a great deal of mythology even . . . ." (9).

It requires a very strong, independent mind to advance a new conception or theory and to face and stand the attacks that will inevitably follow. It is to a large part a question of temper and natural boldness (and not predominantly a matter of intellectual brilliance) that determines whether

a scientist prefers to work within the framework of established paradigms or dares to question and even to overthrow the venerable paradigms he grew up with.

The present state in elementary particle physics may serve as an example. As Heisenberg recently pointed out (10), "many experimental physicists nowadays look for "quark" particles, particles with a charge of one-third or two-thirds of the charge of the proton. I am convinced that the intense search for quarks is caused by the conscious or unconscious hope to find the really elementary particles, the ultimate units of matter. But even if quarks could be found, for all we know they could again be divided into two quarks and one anti-quark, etc., and thus they would not be more elementary than a proton. You see how extremely difficult it is to get away from an old tradition."

Those who act as leaders of a scientific community must encourage the unconventional approach to a problem and be willing, e.g., as editors, administrators, or advisors in granting agencies, to invest risk capital. In most instances the usual peer review process for grant applications will favor—mostly unconsciously—the conventional and average approach. Too often creative minds become inhibited in their thinking and approach to fundamental problems by the criticisms of their less creative peers. Enthusiasm and devotion will deteriorate. The problem for the leaders of a scientific community is to recognize and defend the potential and fruitfulness of a new approach much ahead of the applause of the majority. This not only requires a sharp, independent, and imaginative mind, but also personal courage—a rare combination. On the other hand, the danger in investing too much risk capital may not be overlooked. The major problem is to maintain a balance between the support of well-established, high-quality endeavors and the support of new untried investigators and institutions.

I do not know of a system that would be better in principle than the present peer review process, "an institutionalized method of evaluating the quality, potential value, and feasibility of research proposals submitted to a funding agency" (11). "The peer review system . . . has deep roots in the structures and procedures of the scientific and scholarly community" (11). However, some obvious shortcomings and signs of corruption (or lack of fairness) could be eliminated. The shaken confidence of the public and the politician in the peer review system must be restored. This is not only a matter of administrative reforms. The question is primarily to what extent fairness, responsibility, wisdom, and a broadly educated mind are permitted to play the major part in evaluating the proposals advanced by the individual scientist. But ultimately, getting the right projects is fundamentally a choice between projects on the basis of promised quality. Under no circumstances should it be a question of responsiveness to political issues or pressures.
In recent months (fall '75), the process of peer review of scientific projects has been the target of severe criticism in the USA. Some major questions—as summarized by MacLaine (12)—have come up: Is peer review fair? Does it provide for the support of the best science? Can it recognize potential breakthroughs? Are the reviewers chosen well? Do they respond objectively? As MacLaine points out, "such questions as these do not have any simple answer because they really refer not just to the immediate issue of how peer review works, but to the structure and nature of science in general and in particular. On closer examination, there are many different versions of peer review, each adjusted to apply to the science at issue. All of these versions have one purpose: to help decide how the limited funds available for the support of science can best be spent to advance both science itself and the national purposes to which it contributes. Science is a complex and multifaceted attack on the unknown. Guesses as to how it will work can most accurately be made by people who have themselves succeeded in such attacks on the unknown. These people are the peers." (12).

The peer review is largely designed to maintain quality of research. It is not the appropriate place for determining political priorities. Decisions concerning national priorities must be made by a political authority—on the basis of "if…then statements" supplied by the governance of the federal granting agencies or the leading academies and on the basis of national goals that transcend the realm of the scientific communities, e.g., geographical distribution of research support within the nation or actual needs on the level of "mission-oriented" research. However, once money is allocated to a particular field of research, the peer review system seems to be the most appropriate means to determine which projects within the field should be supported.

Progress is inevitably accompanied by some regress. The progress of science is seriously threatened by several regressive phenomena, at least two of which are obvious and interrelated: "big science" and "scientific mass society." I want to describe these phenomena in the words of two bitter critics who know intimately the merits and shortcomings of modern science.

Charaff on "scientific mass society" (7):
In those prehistoric times (BWC) [i.e., Before Watson–Crick, the creators of the central dogma; author's note]... science... was small; it was cheap; it was open. One could still do experiments in the old fashioned sense of the word. Now, everybody is working away at 'projects' the outcome of which must be known in advance, since otherwise the inordinate financial investment could not be justified... The small numbers of scientific workers engaged in research had other consequences. It was easy to open new fields and to go on cultivating them; there was no fear of immediate dispossession as is bound to happen now. There were relatively few symposia, and those that existed were not attended almost exclusively by hungry locusts yearning for fields to invade. Bibliographies were comparatively honest, whereas now entire packages of references are being lifted by a form of transubstantiation, as it were, from one paper to the next; so that if some work gets into the habit of not being quoted, it never will be so again. The break in the continuity of the tradition has, perhaps, been one of the most disastrous effects of the scientific mass society in which we are living now...

Another negative side effect of the scientific mass society will probably have immediate, serious consequences, since it is an important factor in widening the gap between the "Two Cultures" (13): overspecialization and concomitant underrating of the broad philosophical mind. This tendency in the modern scientific mass society is seen not only by concerned senior scientists such as Weisskopf (14); it is recognized in particular by those intellectuals who watch the scientific community from outside. Scholem, president of the Israel Academy of Sciences and Humanities, bluntly states in a discussion about the "population explosion" within the scientific communities: "I maintain that the number of scientists who are aware of the implications and fundamentals of what they are doing has not grown very much... I have often wondered at how little people who had studied science,—chemistry, physics or biology,—knew about fundamental questions which should have been foremost in their minds" (15). And Weisskopf admits: "... most of the so-called scientists are not really scientists in a true sense, but they do constitute the majority of the scientifically trained. And in the science establishment the philosophical idea, the mystery of things, has become lost" (14).

It could be that the present cutting craze in America and Western Europe will turn out to be useful. However, the pruning of the tree of science must be done with wisdom and courage. The most important point is to cut out the dead wood—mediocrity.

Hagstrom on "big science" (16):
Like other professionals... modern science is characterized by the splitting of the professional role into the roles of the administrator and the technician. Leaders necessarily become political, oriented to obtaining funds and access to facilities and coordinating the efforts of others. Technicians become means-oriented, interested in performing their specialized skills for extrinsic rewards and uninterested in the recognition given by the scientific community for the attainment of scientific goals.

If this occurs... control is exercised by hierarchical authority within research groups and by political power outside of them. Scientists become more interested in their particular organization and in the reactions of politically powerful leaders than in the opinions of the wider scientific community.... When recognition from the scientific community loses its
value, recognition will be sought from other users of research. As the norms of independence for professional scientists become abridged, scientists will come to feel less responsible for achieving scientific goals.

10. Progress in science has always been threatened by resignation. It happens from time to time that scientists who have achieved a strong position in their discipline are willing to say that the end of their discipline is in sight. Rutherford said at the turn of the century that atomic science is all over, and Max Planck proved that Rutherford was wrong. GUNTHERT STENT recently said [58. The Coming of the Golden Age: A View of the End of Progress (17)] that biology has gone from the period of charismatic innovation and is now in the dogmatic and academic period. At least those biologists who work in fascinating fields such as neurophysiology or developmental genetics usually do not share this opinion. However, many of them show signs of concern about the predicted gloom of their science. It is generally felt that a crisis of progress in science will inevitably lead to a crisis of self-confidence among scientists. Self-confidence includes audacity and rigor; it implies that we trust our capacity to conjecture and to improve theories about the world in which we live; theories that deserve to be called consistent, coherent, comprehensive, beautiful, and deeply satisfactory; theories that please the creative human mind.

The scientific enterprise is fragile and vulnerable. With regard to what happened at the turn of our era in Alexandria, Toulmin writes: "Fragmented into a diversity of subdisciplines, confused in the public mind with technology and craft know-how, divorced from the broader questions of natural philosophy, which had been the source of its original impulse and interest to educated men at large, the clear spring of the scientific debate inaugurated in Ionia and Athens disappeared into the Egyptian sands." (18). Toulmin leaves no doubt that he describes a situation that was in some respect not unlike our own. He continues: "When I look at the fragmentation of the scientific enterprise today, at its divorce from any broader concern with natural philosophy, and at the wide-spread confusions of attitude that one finds both inside and outside the profession, as a result of which science has become so firmly linked in men's minds to technology—rather than to its traditional allies, such as cosmology and theology—I find the threat of Alexandrianism never far from my mind. While Copernicus was, undoubtedly, the man who initiated the rise of modern science, we may in our own day be in danger of witnessing the beginning of its fall" (18).

The feelings of insecurity and threat within the scientific community are being exploited ruthlessly by the press and other mass media for ideological or simply commercial purposes. I think Samuel is right when he says: "This feeling of impending doom has been taken up by the press and other media, presumably because doom sells better than destiny, and the gloomy view of life is more newsworthy than an optimistic approach" (19).

Appendix

A Case History: Plant Morphology as a Mature Discipline Ruled by Paradigms

Fields that have come to a close, i.e., have reached full maturity, include classical thermodynamics of reversible processes or optical atomic spectra in physics. In biology most fields are still much less consolidated; they are still wide open for progress in the sense of normal science but also open for revolutionary progress. Some biological fields, however, have indeed reached a high degree of maturity, which makes dramatic changes of paradigms improbable, among them plant morphology. The paradigmatic state of this discipline has been challenged recently by Sattler (20, 21).

In plant morphology the ruling function of paradigms, both in methods and techniques and in constructs and models, is particularly impressive, even for the dispassionate observer. The construct "leaf," e.g. (later on referred to as "phyllome"), has been of fundamental influence (in essence comparable to a Platonic idea) in the thinking of many generations of plant morphologists since Goethe. The construct "leaf" permits the interpretation of all lateral appendages of a plant such as seed leaves (including cotyledons and coleoptiles), foliage leaves, sepals, petals, and stamens as manifestations of the same organ. The construct "plant" is based on a particular idea of plant construction, namely that the sporophyte of every higher vascular plant consists of only three types of organs: root, cauline (stem), and phyllome (leaf). All observable organs are manifestations of one of the three "subconstructs" of the construct "plant." The fruitfulness of these constructs can hardly be overestimated. The construct "plant" became a paradigm that seems to be resistant against any scientific revolution. As an example, the construct has survived the rise of the theory of evolution.

Evolutionary morphologists who claim that the higher plants with roots, stems, and leaves evolved from plants that lack these organs do not deny the eminent usefulness of the constructs "plant" or "phyllome." However, they reject any ontological or metaphysical interpretations of these constructs (as an example they reject Troll's essentialism in plant morphology (22)). Homologization within a group of organisms that can be subject to the construct "plant" (in the foregoing meaning) does not imply the concept of evolution, but it has been used by evolutionary morphologists as a basis for inference on phylogeney. For fossil lower vascular plants, the construct "telome" (as originally invented by Zimmermann (23)) is now generally used by evolutionary morphologists. They assume, of course, that a continuous evolution has occurred from lower to higher vascular plants. However, they also realize that distinct constructs are indispensable.
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