ZOOLOGY: SHOULD IT EXIST ?

by

D. W. EWER
Professor of Zoology in the University of Ghana, Legon.

An Inaugural Lecture delivered on 6th November 1969 at the University of Ghana, Legon.

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THE funny stories told by English schoolboys fall into a very limited number of patterns. One of these emphasizes the alleged differences between the characteristics of various European nations. Such as tale is told of an Englishman, a Frenchman, a German and a Russian who were drinking together amicably on the afternoon of the 16th June 1904 in a café in Geneva. For reasons now long forgotten, the conversation turned to elephants. The four men found to their dismay that they knew remarkably little about these beasts and agreed to meet again a year later, each bringing the manuscript of a work upon the elephant. A year later to the day, in the same café they met again and unpacked their manuscripts for inspection.

The Englishman has spent the greater part of the year tramping through Africa and had written a short, practical guide called 'The Elephant and how to shoot it'. The Frenchman had spent his time contemplating the elephants in the Zoo at Vincennes and had written an elegant thesis, now much sought after by students of animal behaviour, entitled 'Les amours des Eléphants'. The German had remained ensconced in a library in Heidelberg and had completed the Historische Einleitung to a work, projected to take 13 volumes, the well-known "Beitrage zur Wissensch-aftlichenaturgeschichte der Elephant". The Russian had spent his time sitting, in dialectical contemplation, on top of the stove and had written a brief but highly polemical pamphlet "The Elephant: does it exist?"

Like the Russian, I have spent some six years in Ghana sitting, albeit not on top of a stove, watching the dialectical development

of the biological sciences, wondering about zoological research, asking myself what is the nature of this science I am called upon to teach, what is its relevance to Ghana, what is its future in Africa, should it indeed exist?

I have long accepted the dictum that Zoology is about animals, all about all animals; but this is rapidly ceasing to be true. The science appears to be in a state of disintegration. It is a disintegration which follows the law that the younger a zoological idea, the newer a field of zoological research, the more rapidly it recedes from its parent science, the sooner it takes unto itself an independent existence. The result is that what remains to the zoologist belongs to the Nineteenth rather than the Twentieth Century.

This disintegrative process stems largely from the development of two types of technique. The one allows both the identification and the analysis of minute quantities of chemicals, so that the dynamic events in living cells may be followed with relative ease; the other makes it possible to determine the full three dimensional structure of these vast molecules which are the characteristic working parts of all living organisms. It is these techniques which have led to the development of an outlook towards biological problems crystalised in the expression 'Molecular Biology'; and it is this development which has largely contributed to what I speak of as the disintegration of Zoology. Let me attempt to illustrate this by reference to various aspects of zoological research, and firstly to genetics.

Some twenty years ago genetics was, for the zoologist, a clear, comprehensible discipline, occupying a central position in his understanding of the mechanism of evolution. Research, particularly upon the tiny fly *Drosophila,* had produced decisive evidence that heredity was dependent upon units, the genes. The genes are the conservative elements of heredity that ensure that snails give rise to snails, worms to worms and cows to cows and bulls. The

detailed rules which determine the transmission of these genes from generation to generation were well understood. Further it had been shown that very occasionally a gene could undergo some type of change. This change provided the basis of the continuing appearance of variation required by Darwin's theory of Natural Selection.

But there was one question which remained. How did these genes bring about their effects? What was the difference between a gene which caused a fly to have bright red eyes and another which caused it to have pale yellow eyes? How did these differences produce their end results? The answer to such a question is clearly going to be in chemical rather than in zoological terms. It is a question which has been answered by the researches of the last two decades, but this answer has been won, not primarily by zoologists, but chiefly by the efforts of physicists and chemists. The active study of genetics has moved rapidly out of the ken, indeed out of the comprehension, of the orthodox zoologist; it has acquired a vast mementum of its own, so that the zoologist is no longer even equipped to bring critical judgement to bear upon matters currently in dispute. He may admire the work; he may find vast intellectual pleasure in the demonstration of the purely materialistic nature of the processes of heredity, but beyond this he cannot go.

The genes are the conservative elements ensuring that worms give rise to worms. But worms, snails, cows and bulls start their lives as eggs with relatively little obvious structure and few obvious differences between them. Yet the end-results are vastly different. The study of this phenomenon lies in the field of embryology. Once again some twenty years ago, it was possible to look upon this subject as an integral part of zoological research in which particular emphasis was laid upon the mechanisms involved in the development of form, of morphogenesis.

But with our new understanding of gene action, it is necessary to reformulate questions about morphogenesis in a more fundamental way. During development, which may last from a few days to several months, different genes become active at different times. There is a definite sequence of events, a programme whose timing is of critical importance, for, should this sequence be disrupted, the resulting individual is abnormal.

So the basic questions in embryology have become "what is the mechanisms by which a gene is switched in and out of action?" and "what is the nature of the programme which controls the sequential action of the genes?" We do not yet know the answers to these questions. But it seems likely that they may come from studies upon very simple organisms; from research into such problems as the ways in which virus particles are assembled within cells. Certainly it seems unlikely that the classical type of embryological experiment upon higher animals, upon newts and salamanders and frogs, will contribute greatly to the solution of these problems, for the answers will be in chemical and not in zoological terms. Morphogenesis then, like genetics, is receding from Zoology to form part of the edifice of Molecular Biology.

One cannot, in considering the processes of living, neglect those of dying. What are the laws of death? How definite is the life span of an animal whose death is not attributable to accident or disease? Is the duration of life genetically determined? What other factors influence it? These are most excellent questions in Experimental Zoology and they are now being studied seriously for the first time. But it takes little imagination to see once again that the fundamental answers are likely to be in terms of chemistry; that once the basic facts at the organismal level are understood, the study of death will acquire its own momentum and leave its zoological ancestry behind.

The solution to the problem of the nature of dying offers a prospect with little appeal. Understanding leads to control. So we may well be faced, in the future, with the threat of a new pill which

ensures that everyone enjoys a continuous, sunny middle-age; that we will have to adjust to a society in which there is indeed no room at the top, for both politicians and professors will live for ever. Or maybe worse, like Swift's miserable Strudelbrugs, the process of aging may not cease. One will live on and on indefinitely, unable to do any thing except to be "opinionative, peevish, covetous, morose, vain, talkative."

One field of study which would seem to be the undisputed territory of the zoologist is that of the behaviour of animals. Here too the influence of chemical thinking is becoming increasingly felt. It has long been known that certain types of events within the nervous system have a chemical basis. But in recent years there has developed an exciting debate as to whether or not the processes of learning depend upon the synthesis of specific chemical compounds. Some people claim that if rats are trained in a particular skill and extracts of their brains are injected into untrained rats, these latter will learn this specific task faster than controls which have received no such injections.

Here too is excellent raw material for the writer of science fiction. If these findings should prove to be correct, and they are very far from being generally accepted, then in the future we may quite literally inject knowledge into our students rather than try to impart it by word of mouth. Even the most sophisticated teaching machines will become obsolescent and the cram shop will be replaced by the chemical factory. It may be possible, in a manner alarmingly reminiscent of Brave New World, to determine, by suitable adjustment of dosage, the intellectual ability of the chemically educated.

But the success of such a technique might also vastly aid the development of inter-disciplinary studies. Many of you will recall how Pepys, after witnessing an early and unsuccessful attempt at blood transfusion, speculated upon the consequences of injecting

the blood of a Quaker into an Archbishop. So, too, by suitable admixtures, one might produce individuals creatively receptive of the facts and ideas of different academic disciplines; of both political science and astronomy; of food science and meteorology; of geology and hagiology.

It thus seems clear that the future development of research upon these various central problems of zoology — of genetics, of development, of behaviour and of death — will all lie at the level of analysis of Molecular Biology. Indeed it has, from time to time, been seriously urged by the more fanatical prophets of the new enlightenment that no more money should be wasted upon the old, traditional fields of zoological research. Their day is gone. The future lies exclusively with Molecular Biology and Biophysics.

But there are limitations to Molecular Biology. All organisms are the product of an historical process; so that when one moves from the general to the particular, one can make no more than very broad predictions. One cannot predict the works of Shelly from the Oxford English Dictionary nor those of Hesiod from Liddle and Scott. So too one cannot predict a pig, a porcupine or a pangolin from a knowledge of the biochemical events within a cell. The organisms themselves are the valid domain of study of the zoologist. They represent a level of complexity beyond the scope of Molecular Biology. In so far as we need to know about animals, or find pleasure from knowing about animals, so far we have need for Zoology. The classical core of Zoology certainly remains as something which constitutes, within a University, a legitimate field of study. But this is not the answer to my question which concerns itself, not with a University in a vacuum, but with a University in Africa. What is the future of research within the classical core of Zoology in a developing country in Africa today?

The description of the diversity of animal forms, the cataloguing of all the myriad types of animal that exist or have existed, has been and still is an undertaking of the utmost importance. Without it one can achieve nothing. It is estimated that about a million different sorts of living animal are known. But this is certainly nowhere near the end of the story. Any entomologist who has worked in Tropical Africa will tell how careful systematic collecting brings to light more and more new species; and this is not limited to insects. Dr. Edmunds of my Department has recognised 36 completely new species of sea-slug from collections made off the coasts of Ghana during the last few years. This nearly doubles the total number of known species of sea-slug in a relatively limited area. Similar work which Dr. Edmunds undertook during a brief stay in Dar-es-Saalam, gave a very similar picture.

Here then surely is an aspect of zoological research which should develop rapidly in Africa. Unfortunately it is not so simple. During the long years of colonial rule, the study of African animals, both living and fossil, was only rarely undertaken in Africa itself. Expeditions came, collected and departed, taking their spoils back to their various national Museums: to London, to Paris, to Brussels, to Berlin, to Lisbon. In later years this activity has been greatly augmented by collecting expeditions from many Museums and Universities in the United States, as well as from other European countries which formerly had no direct links with Africa.

It would be foolish not to recognise that in the prevailing circumstances such expeditions were, and still largely are, the only way in which the fauna of Africa can become fully known. In Tropical Africa between the Sahara and the Zambesi, there are few Museums or Universities which have the facilities to store collections of animals in conditions which will ensure their permanent preservation, under conditions which will make certain that neither mites nor mould will get in. Nor have there been many people living in Tropical Africa who have been able to dedicate their working lives to the study of those minutiae of structure which are the daily bread of systematic zoologists.

It is this situation which presents great difficulties and gives rise to the seemingly wanton behaviour of some of my colleagues. Thus Mr. Hughes has, over the last several years, been making an intensive study of the snakes of Ghana. In the pursuit of this research he finds it necessary to visit many European Museums. This was recently a matter of outrage in some grant-awarding committee where one of the members, a theoretical physicist if my memory is correct, could not comprehend why the study of snakes of Ghana had to be pursued in Bonn or Basel. True, there are written descriptions. But these are often inadequate. The final appeal must be to the original specimens, so that one can see precisely what was the animal that Pel took at Elmina in 1843 or Missionary Riis collected at Akropong in 1845.

Thus it must be accepted that, at the present time, the scientific study of collections of animals from Tropical Africa is more easily undertaken in Europe or America than in Tropical Africa itself. Urgent as it is, it is likely to be many years before the countries of developing Africa can provide all the necessary facilities to ensure that scientifically valuable material is adequately curated; before they can accept the financial burden of providing both an increasing number of trained taxonomists and an ever increasing demand for storage facilities. It is likely to be even longer before a solution can be found to the problems posed by the fact that key specimens of the African fauna are scattered, in a somewhat random fashion, over the Museums of the Northern Hemisphere. Once Africa can successfully curate this material, there will be no justification for its retention overseas, though there will unquestionably be many arguments found to justify the preservation of the status quo, whose roots lie in history not in scientific common sense.

So I am brought back to my central problem. If, in Tropical Africa at the present time, taxonomic research cannot be carried on effectively as a subject in its own right, that is as distinct from

its service value to agriculture or medicine, does Zoology have a legitimate place in an African University?

Inseparably linked with taxonomy goes the study of anatomy, and especially what is called comparative anatomy in which anatomical facts are interpreted in evolutionary terms. The great days of the study of the comparative anatomy of living animals are largely passed. The broad facts of the anatomy of the animals around us are known.

Occasionally there are excitements. The discovery of a living coelacanth off the coasts of Africa in 1939 was one such case, as it had been believed that this type of fish had been extinct for 75 million years. Significant new discoveries are still being made, but they are rare events. There have been five or six such in the last thirty years and they have all been of animals living in the seas. These have been found, not as a result of deliberate search, but as the spin-off of other work. These few discoveries certainly do not mean that comparative anatomy is urgently calling for many new recruits. It is not an area of research which alone could justify the existence of Zoology as a distinct discipline in the Universities of Tropical Africa.

This is not to say that anatomy is 'finish'. But it has become essentially something secondary to other types of research. Information is often required at a level of detail which never greatly troubled the classical comparative anatomists. Thus, for example, Dr. Kumar of my Department, has worked for many years upon the intimate details of the internal anatomy of bugs. This is not, however, an end in itself; rather it is part of an investigation of the classification of these insects at a higher level than that of providing a simple catalogue of the fauna. The material for such a study has therefore come from all parts of the world and not just from Africa.

Similarly, some years ago I found that to solve a physiological problem I was studying, it was necessary to have a far more detailed description of the anatomy of the nervous system of grasshoppers than we then possessed. The earlier anatomists had indeed described the nerves, but had omitted what would seem to be important details — exactly where they went and where they ended, whether in a muscle or in a sense organ. As often happens, such studies gather their own momentum; I soon learnt that this type of anatomical investigation is of value in a different way, as it reveals unnoticed structural arrangements which pose new problems to the insect physiologist.

One could easily multiply such examples from the anatomical research work which has been undertaken here at Legon. The point which I would stress is that these studies are essentially adjuncts to other types of research; they do not in themselves justify the preservation of Zoology as an intellectual discipline.

It is possible to study the anatomy of the lower animals with little regard to function, and indeed in the past functional interpretation has far too often been based upon speculative extrapolation from structure rather than upon experiment. The study of the physiology of the lower animals that branch of zoology which is dubbed comparative physiology, has grown slowly.

It is a subject which deals with what I find to be intriguing questions: how does a locust fly; how does an electric fish generate the radar - like pulses it emits; how do the toads which sing to us at night know when they should start to breed; how is the larva of a tse-tse fly nourished as it develops within its parents's body; of what value are its lungs to a lung fish, or their curious respiratory structures to a mud fish?

There is a vast number of problems such as these. But Comparative Physiology constitutes, on analysis, a curious hotch-potch

of empirical observations and, despite many attempts, no satisfactory intellectual scaffolding has been found which will turn a mass of intriguing facts into a self-contained, intellectual discipline. Despite this limitation the problems remain, at least to me, of the greatest fascination. There is perhaps as much of the aesthetic as of the scientific in this feeling.

Now all the examples I have quoted relate to African animals and, when I first came to Africa nearly 25 years ago, I believed that a major task of a zoologist in Africa was to study the physiology of the African fauna. It was that pathway which I attempted to follow. Today the situation is very different. This is for two reasons. Firstly, methods of investigation have become increasingly sophisticated, with the inevitable need for far more money, far more effective technical and maintenance staff. Certainly there are still questions which can be answered, at least in a preliminary way, with pink string and sealing wax. But far more critical results can be obtained, often with much less effort, when it is possible to use sophisticated technical methods.

This is not, however, the most important change which has occured. The study of the physiology of the animals of Africa has, curiously, been revolutionised by the development of rapid air transport. African animals are no longer restricted to Africa. If you wish to study the physiology of locusts, you do not come to Africa, you go to London. There you will find an abundance of locusts of all sorts and also all the technical facilities you may require. If you want to study tse-tse fly, you go to Bath; if it is bilharzia snails, then you select Brighton. The physiologist no longer comes to Africa to study lung fish, electric fish, or mud fish, to study monkeys or apes, Goliath beetles or Giant snails. He arranges for the animals to be sent to him. As well as a brain drain to the laboratories of the Northern Hemisphere, there is also an animal drain and, as a result, physiological investigations which once required the scientist to live and work in Africa are now more easily carried out in better equipped and better serviced laboratories elsewhere.

I said that many years ago I had seen animal physiology as a particularly exciting field of zoological research which could be developed in Africa, but I have been overtaken by the scientific jet-set. So I am brought back once more to my central question. Is it possible to justify the existence of a Zoology Department in a University in Tropical Africa, if the outcome is to be that the young comparative physiologists will simply follow the animals to the laboratories of the Northern Hemisphere?

I have painted a picture of the forces making for the disintegration of zoological research in Tropical Africa. To a certain extent, it is a caricature. But all caricatures have some basis in reality. The present one serves to emphasize certain problems. It is, I consider, true to say that the development of classical Zoology in Tropical Africa is at the present time a dubious investment, since, sheltering behind the comfortable slogan of the "International Nature of Scientific Research," there is a growing scientific exploitation of Africa by the laboratories and research institutes of the Northern Hemisphere. Were this purely limited to assistance in the solution of practical problems, it might be welcome. In fact it goes further than this. At the same time, the newer fields of investigation, which are tied to no particular geographical locality in their choice of materials, depend upon levels of technical sophistication with which Tropical Africa cannot compete until its limited financial resources have met the far more urgent needs of everyday life and living.

Is there then nothing which we can usefully do? Do we possess nothing which remains unique and which cannot be transported by air to the laboratories of the developed countries? The answer is that we still have Africa, and this at least cannot be taken away.

Let me explain what I mean more precisely. One can certainly conduct interesting and significant experiments on locusts in Lon-

don. But one cannot fully understand locusts by looking at them as they live in cages. This can only be done if one studies them where they live. Similarly much may be learnt of the ways of the snails which spread bilharzia by studying them in aquaria, but many crucial questions can only be recognised and answered when the animals are studied in their natural habitats. One cannot ultimately divorce the physiology of an animal from its way of life in the place in which it normally lives.

There is then scope for the comparative physiologist in Africa if he accepts that his job is to relate the physiology of the animal very immediately to its environment.

I spoke earlier about the brain and its workings and more specifically about evidence suggesting a chemical basis for memory. But not all animal behaviour is learnt behaviour. If an animal had to learn everything by experience, it would be unlikely to survive for long. There must, for survival, be a considerable fraction of the total behaviour of any animal, including man, which is built-in, which is part of its hereditary equipment. This is what is spoken of as innate behaviour. It forms the necessary substratum of activity which may be subsequently modified or extended by experience, by the process of learning.

Just as the physiology of animals cannot be understood except in relation to their natural habitat, so too innate behaviour cannot be comprehended except in relation to the realities of the total environment in which the animals live. Caged animals, animals maintained over long generations in the laboratory and gradually selected to enhance their suitability for living and breeding in such artificial conditions, may be convenient, but from the viewpoint of behaviour, they may well be so abnormal that conclusions drawn from their study may be seriously misleading.

Here then also the zoologist working in Africa has much to contribute, by the detailed study of the behaviour of animals in their natural setting, by working out the natural laws of behaviour

at the organismal level, laws which must follow as consequences from the structure of any model of the underlying neural mechanisms of behaviour.

So we seem to be left, as the major areas of research for the zoologist working in Tropical Africa, with these two — environmental physiology and animal behaviour. While these may offer a vast variety of problems, they must be recognised as a very cramped stage upon which to develop a full academic discipline, more especially as much of their subject matter consists of isolated observations with no obvious pervading patterns.

But in reality this is an artificial view, as we are treating the animals as isolates within their environment, much as they are treated as isolates within a laboratory. This is not to say that such an approach is barren: it has been and is, highly productive. But it is limited. Between animals inter-relations exist. Some of them are unpleasantly familiar, such as those two eternal traingles between man, the malaria parasite and the mosquito and between man, the bilharzia parasite and freshwater snails. To break these triangles requires an intimate knowledge of all three animals and most especially of the vectors of these diseases, the mosquitoes and the snails. There was a time when it appeared that hammer blows might solve these problems; that the ingenuity of the chemists in producing toxic materials would rid the world rapidly of these diseases. Unfortunately, we forgot, in our optimism, that the most fundamental of all biological laws is that organisms evolve. Evolution had been a vague, and to many people a distasteful, academic idea. But in man's attempt to control his environment, he has now been forced to realise that evolution can no more be disregarded than gravity. The mosquitoes evolved, so as no longer to be affected by the insecticides sprayed upon them, just as the capsids on some of the cocoa farms have also evolved so as to become resistant to gammelin.

This ability of animals to evolve may be expressed in a variety of different ways which must constantly be remembered, and are so easily forgotten in the enthusiasm of the moment. The blunderbuss techniques of the chemists have moreover brought with them serious secondary consequences and, as a result, the Governments of Denmark and Sweden as well as certain American State legislatures have made the use of DDT and certain other insecticides illegal. The developed countries are now coming to realise that the indiscriminate use of insecticides must be stopped and it is to be hoped that the developing countries will rapidly learn this lesson and not wait until they too have had direct experience of the consequences of heavy insecticide pollution.

If the blunderbuss is not as valuable a weapon as we once believed where should we turn? There is no single cure for many of our problems. It is, I believe, completely evident that we cannot control bilharzia on the Volta Lake by some one magic recipe; that it will be necessary to introduce a wide variety of control measures, some sanitary, some biological, some perhaps chemical, whose cumulative effect will produce the result we seek. Only by an integration of a variety of different methods will control be achieved.

This same idea is relevant to cocoa. The development of gammelin resistence in capsids, indeed the vast expense of chemical control itself, requires that we should see whether there may not be other weaknesses in the armour of the capsids which can be exploited, together with limited, rational use of insecticides, to produce an integrated and effective control. Whether, by a far more intimate understanding of the relations of the capsids with the animals around them, we may not be able to encourage and especially protect from insecticides those animals which destroy capsids. This is a problem being intensively studied by Mr. Leston in my Department: he is attempting to elucidate the relationship between the capsids and the surprising variety of ants which run upon the cocoa trees. Some prove to be potential friends of the farmer;

others his enemies. This, and this is the point I want to emphasize, is a type of zoological research which cannot be done elsewhere than in the cocoa forests of Ghana. That it has potential practical value is, in the immediate context, beside the point. It represents a type of zoological research which must be done in Africa, in this case must be done in Ghana and can be done nowhere else.

Cocoa trees suffer attack, not only capsids, but also by a virus disease, swollen shoot. Control of the disease is effected by removing the source of infection, by cutting out and destroying trees which are already infected. But control of mealy bugs, which spread the virus from tree to tree, is clearly also desirable. Here too ants enter in, as some species of ant encourage and protect mealy bugs. One thing which is essential is to find all possible places from which mealy bug infestation can be built up after insecticidal treatment. One such possible place has been discovered by Mr. Room in my Department who has been studying the fauna which occurs on mistletoes which grow on cocoa. The mistletoe is bored out by various insects and in the protection of the galleries thus formed both mealy bugs and their attendent ants occur: from such localities new infestations may possibly develop.

But here we have a more complex problem. This is not simply a matter of the inter-relations between animals, but also of the inter-relations between animals and plants. A similar situation arises in the control of bilharzia. You cannot consider the snails in isolation from the water weeds upon which they feed; any suggestions for control must take this aspect of the whole complex into account.

This indeed is a recurrent theme: the success, for example, of a game reserve depends upon a full comprehension of the relationships of the herbivores to the vegetation which forms their food. No rational scheme of reserve management can be developed

without this information; otherwise over-grazing may occur with resulting disaster. Any game reserve is an artificial unit and requires as much husbandry and more skilled husbandry than a cattle or a sheep ranch.

These various sorts of problems belong to that branch of Biology which we call Ecology. It represents, if you find profit on such formulations, the dialectical opposite of Molecular Biology, for in Ecology the whole organisms are the basic units within the complex of relations. Indeed the organisms are sometimes regarded so much just as units that their highly complex habits, their complex adaptations, their surprising inter-relations one with another are forgotten, or at least neglected, in the hopes of constructing some type of useful theoretical pattern against which to determine action.

With such a starting point, theory is essentially restricted to quantitative generalisations. Given certain initial conditions, this or that quantitative consequence in terms of numbers or at least mass of organisms will result. This is an approach of vast practical value; but a difficulty which faces the ecologist in that the diversity of units within a natural habitat is vastly greater than the number of particles upon which the physicist is called to exercise his ingenuity, vastly greater than the number of chemical elements upon which the chemist is required to build his fundamental theories of the structure and properties of molecules. So great is this diversity that every ecological problems seems to end by being something which is a special case, requiring detailed study on the ground.

Yet science depends upon the recognition of regularities. There seems in fact to be every reason to believe that such regularities do indeed exist and that these will allow us to unravel the factors which determine the biological structure of the few natural habitats which remain largely unaffected by human interference, unaffected by the march of technology and by the disastrous technological imperative that "what can be done, must be done."

Let me give you two example of such regularities. We have had the good fortune over the years to watch some of the changes which have occurred since the Volta River was dammed to form the Volta Lake. Almost immediately the dam closed, Dr. Biswas of the Volta Basin Research Project reported a massive growth of the tiny algae, *Anabaena*. More recently this same phenomenon has been observed when the dam across the Niger River at Kainji was closed, and a similar outburst of this alga appears to have taken place in a newly formed reservoir upon the Blue Nile in what are very different environmental conditions. Here then is a regularity. Since it is likely to occur repeatedly where dams are built in Tropical Africa, it becomes open to detailed causal analysis which would lead us to an understanding of one of the first changes to occur in the dynamic series of events which take place in man-made lakes.

The other example relates to a different type of regularity. Mr. Hughes and Mr. Leston have recently completed a survey of the snakes of the cocoa farms near Tafo. The number of different species found, nearly forty, may seem surprisingly high. What is of interest is not this simple numerical fact, but that two other surveys of the diversity of snakes in forest regions, one in the Congo and one in Panama, have shown that in both these areas as well some 40 different species of snake occur in the forest. Here then we have a regularity of a different order, suggesting that within the tropical forest, whatever may be the detailed differences, some type of unifying principle is at work which influences the structure, as opposed to the abundance, of the fauna.

I have stressed these cases because they bring out a point I wish to emphasize. While one can develop quantitative ideas about populations of plants and animals, the practical man is concerned as much with the particular organisms he encounters, as with their numbers alone. If indeed regularities in the structure or the development of ecological communities are to be understood, one can no longer treat the individual species as simple counters.

It becomes necessary to know their biology in detail; it becomes necessary to turn back again to their physiology in relation to their environment, to their behaviour and their behavioural inter-relations. The analysis requires a detailed biological study of each of the organisms concerned. These findings can then be used to build up a structured picture of the whole; a picture which will be far more analytical in biological terms than one built from the more general paremeters likely to affect population density.

It is, I believe, only in the relatively undeveloped areas of the world, as well as in the seas, that ecological analysis of this sort can profitably be undertaken. Where human interference becomes all pervasive, the resulting maintained pressures by insecticides, rodenticides, herbicides, fungicides and so forth produces so distorted a picture as to yield little of fundamental value. Within the tropics we still have places in which we can carry on research so as to understand these ecological inter-relations. Scientifically this is one of our major assets in Tropical Africa. But this understanding is also essential for development, even if this is not immediately obvious to those anxious for rapid and maximal exploitation of our resources. We must understand the total workings of the environments around us, for otherwise the future pathway of development will be marred by as great or greater biological follies and calamities than have occurred in the past. To look only to immediate gain, to think only of exploitation for today is to fail in the trust we have for posterity. We must act in the light of the full implications of Schweitzer's thesis that the ultimate morality is a reverence for life.

This all brings me back to the question which is my central concern. Does Ecology justify the continued existence of Zoology as a distinct discipline in an African University? Clearly, in so far as animals are an integral part of any ecological system, Zoology is formerly involved. But Ecology is not, despite tradition, divisible into Plant Ecology and Animal Ecology. Dr. John in the

Botany Department is, I understand, studying the growth of algae upon the rocks on the shores at Tema. This cannot be understood without at least some attention to the animals which feed upon the algae. The work becomes inter-disciplinary and Dr. John is working in collaboration with Mr. Pople of my own Department.

The division which exists today among the biological sciences has arisen from the original paramount importance of taxonomy and morphology. But, as I have tried to show, neither morphology nor taxonomy, essential though the latter is, are any longer the intellectual growing points of the biological sciences and to retain a division purely because it reflects what was a reality of the Nineteenth Century can become a hindrance to advance.

If, as I have argued, Ecology in all its ramifications must be the major area of biological research in Tropical Africa, then it appears to me that the existence of Zoology as a distinct discipline is not really justified. This should not be interpreted as meaning that this inaugural lecture is some quixotic valedictory address. I am not intending to resign tomorrow, for there is a final question which we must ask.

Does it really matter? Does the preservation of traditional divisions between different branches of science have any serious consequences? However we may fit ourselves into administrative cubby-holes, young research workers are all compelled, in the face of the very rapid advance of scientific knowledge, to become specialists in some one limited field or other. As a result one may well be able accurately to label many of them 'zoologist'. If young research workers must indeed become narrow specialists, some with a botanical, some with a zoological bias, then there would appear to be nothing to gain from any change. Furthermore, however we structure a Faculty, research work which transcends departmental boundaries will go ahead: there are no real barriers to inter-disciplinary research. So once again, it would

appear that nothing would be gained from any change. If this is true, then all this argument is indeed a great fuss about nothing.

However the duty of a University lies beyond simply the conduct of research. It is also charged with teaching. There seem to me to be two ways in which the formalistic divisions which we make between the sciences may have educational consequences, and since in a developing country the quality of the graduates is of paramount importance, this has to weigh.

Firstly and simply, the type of label you attach to a subject has very great influence upon the priorities of what is taught; to preserve the old labels, the old divisions, can too easily lead to a continuation of teaching along the old, familiar lines. Secondly, all teaching is a form of indoctrination. The pattern of thinking is engendered is inevitably influenced by outward administrative forms. If we wish to develop the type of integrated biological outlook towards the world about us which I believe to be essential, then the outward forms must reflect our purpose. And this is necessary, not only for those who are being trained to be future research workers, but for all who are being educated in biological ways; if we fail to teach our students to think in in all-inclusive ecological terms, to recognise the central importance in their lives of the ecology of man-made change, then indeed the pathway of development is going to be strewn with disappointments and false starts. For these reasons then, the old forms, based upon taxonomic divisions, should, perhaps, be discarded in favour of some more integrated pattern reflecting the unity of biology at both the molecular and ecological levels.

Mr. Vice-Chancellor, Dr. Johnson once said that contemplation of death by hanging concentrates a man's mind wonderfully. I have found that the contemplation of Ghana and her biological problems has broadened my thinking wonderfully. Having once been highly critical of Ecology as a scientific discipline, I find

I find myself fully converted to a different outlook. And it is a pleasure to acknowledge my debt to the University which has so broadened my intellectual horizons as to force me seriously to consider whether Zoology, at least as I understood when I first came to Ghana, is really necessary.

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