THE FIRST 30 YEARS OF

THE BUREAU OF ANIMAL POPULATION

by

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[The B.A.P. was founded in 1932. Below is the substance of a talk on its history, given by C.S. Elton at a meeting on 14 February 1962, in the Library of the Bureau at the Botanic Gardens. The text has had to be slightly modified to allow for the fact that a number of lantern slides were shown in the actual talk, and one or two small items have been added. It is to be understood that no attempt was made to describe fully the post-war work of the Bureau.]

In 1920 I was strongly influenced by Victor Shelford's book on *Animal Communities in Temperate America*, which contained an ecological survey of the neighbourhood of Chicago. In 1921 I had the luck to be asked to join the Oxford University Expedition to Spitsbergen, as assistant to Julian Huxley. I was then an undergraduate. He let me do what I liked, and this was an ecological survey of animal life. Spitsbergen lies not very far from the North Pole. Old Warden Spooner once remarked to Huxley in his quaint way: "I understand that Spitsbergen is no further from the North Pole than Land's End is from John of Gaunt"! It is heavily glaciated, ice-bound for most of the year and some parts even in summer, and because of its high latitude and geographical isolation from Eurasia has a very limited fauna. When I had finished reading a paper upon the very sparse beetle fauna of the archipelago, at the Entomological Society of London, I heard Professor Bateson, the great Cambridge geneticist, remark in a stage whisper: "I do not think these Oxford men know <u>how</u> to collect beetles"! In fact only six species were known.

This was the first of three expeditions to Spitsbergen organised by George Binney, and my contact with him was important for our future work. In 1923 I went again, with the Merton College Arctic Expedition. This time we tried to circumnavigate North-East Land, and were led to some very barren spots indeed. Indeed, on the whole of this island, about the size of Wales, I only found 9 species of dry land invertebrates – out of the 60 or so known from the whole archipelago. On the way home I happened to spend one of my last three pounds in a shop in Tromsø on Robert Collett's book about Norwegian mammals – *Norges Pattedyr*, and in it I discovered all about lemming migrations, the records of which I translated and mapped. It was then that I found that the migrations had occurred every three to four years somewhere in Norway. During the Oxford University Arctic Expedition in 1924 I was in charge of the scientific work, and spent the summer at the base camp on Reindeer Peninsula, a broad low-lying peninsula covered with Arctic fjaeldmark and other vegetation. Here I completed as far as I could the general ecological survey. At this camp I spent two weeks alone with the medical officer of the expedition, Dr. H.W. Florey, and this led to a friendship that also had very important later results for the work of the Bureau.

Two other influences on my ideas about this time, while I was an undergraduate at Oxford, must be mentioned. One day my tutor, who was Julian Huxley, gave me a book to read by Gordon Hewitt, on *The Conservation of the Wild Life of Canada*, which contained graphs of the fur returns of the Hudson's Bay Company, two of the most striking being those for snowshoe rabbit (or snowshoe hare) and Canada lynx. These could be seen to fluctuate in a remarkably regular manner and to have periodicity about ten years. Thus the two great Northern cyclical species, lemming and snowshoe hare, each with their special period of fluctuation, posed major population problems that we have even now not properly solved, although they have occupied the Bureau of Animal Population's interest for a long time. The other influences that focused my mind on population questions was a book by A.M. Carr-Saunders published in 1922: *The Population Problem*. This I devoured in the two days before my final Schools in Zoology, and although it was about human populations it was full of exciting ideas of a general nature.

I now come to the period 1925-30. There were two ways of doing research in population cycles. The first was mapping and following their trends on a wide geographical scale. In 1925, through my earlier contacts with George Binney who was now on its staff, the Hudson's Bay Company asked me to become its Biological Consultant, mainly to advise the Governor on this matter of cyclical fur crops. This enabled me to undertake two pieces of research. One was sending out and analysing the results of an annual questionnaire about the changes in numbers of various species in northern Canada. These reports from the fur posts gave a running record from year to year. The other line was a study of the past history of the cycles shown in fur returns and other records from Canada. Thus the Arctic fox is the staple fur crop of the Eskimos and large numbers are caught in good winters. But this species shows the same short three to four year cycle as that of the lemmings in Norway, and for the same reason lemmings in Canada also cycle and are the usual food of the white fox. This fluctuation is clearly seen in the nineteenth and twentieth century fur returns, and the annual reports for recent times. Similarly, it was possible eventually to trace the ten-year cycle of the lynx as far back as 1736 – surely the longest known fluctuation of a wild population and one that covers the whole of northern Canadian territories.

The two cycles, the short and the long, and the food-chains with which they are associated, were summarised in an illustrated chart that I helped to prepare for the Leipzig Fur Fair about 1928. On it can be seen the whole sequence from lemming to fox to Eskimo, and from snowshoe hare to lynx to Indian, and thence to the sale room of the Company in London.

These enquiries were to be continued in various forms for 25 years, first by myself, then by the Chittys.

In the second major aspect of this research was that, since none of the species chiefly concerned now occurs in Britain, it was desirable, indeed absolutely necessary if scientific analysis was to go further, for a study to be made of some local rodent population subject to cyclical fluctuations. This need led to the Bagley Wood investigation from 1925-28, a piece of team work by members of the Department of Zoology and Comparative Anatomy, done in the time of the late Professor Goodrich. John R. Baker and I joined up together, he to do breeding biology and I to do parasites. E.B. Ford came in, as a protozoologist. Dr. A.B. Gardner studied the bacteriology and spirochetes. The first three all shared in the arduous fieldwork of nearly 600 trap-nights, from which over 2000 mouse and vole bodies were collected and analysed. At first we had Denys Kempson as an assistant, but later A.D. Middleton took his place. The populations of wood-mice and bank-voles built up to a peak of abundance and suffered a crash, though we did not manage to find the cause. This was the first systematic investigation of its kind anywhere in the world.

The team amicably dissolved and I began to take great interest in epidemiology. It was clear that research of this kind required some kind of continuity – you may have to wait at least three years between each manifestation of the crash in numbers. I tried to generate an artificial epidemic among field voles in special pens in a field near Botley. The small wooden hut from which this work was done later on served for Baker and Ranson's investigation of vole breeding, at the Department of Zoology; and at various times housed members of the Bureau and its vole stocks and

its primitive pre-war workshop. It was not until 1947 that we were to obtain any really adequate working quarters.

I must mention now a private circumstance that greatly influenced my actions. My elder brother, Geoffrey York Elton, died suddenly in 1927 at the age of 33. I owe to him most of my early training as a naturalist since 1909; and also many contributions to my general outlook on ecology that grew up in the early twenties. He was a very brilliant naturalist, and though not a scientist, was at one time engaged in fish farming, but his main work was as a school-master. When he died, I had an urge to create some kind of permanent memorial in the form of a research institute – from which it would be seen that right from the start I had intended the B.A.P. to be a permanent research unit not simply a means of expanding my personal work.

I first put up a scheme to the Medical Research Council (who had supported our Bagley Wood work in a small way), for the study of wild animal diseases. This failed; but fortunately the Assistant Secretary of the M.R.C., Dr. Landsborough Thomson, offered to help me to redraft the application. He admonished me for having mentioned Climate. “This worried them” he said. He then went through the application and altered the word “Disease” to the word “Health” throughout, and switched a few paragraphs round, and I sent it in to the Empire Marketing Board, who gave me a grant for three years. This was my first lesson in applying for grants. So Middleton and I settle down to work on what we called “The Oxford Rodent Investigation”. From now on we concentrated on the field vole, *Microtus agrestis*, at the same time starting a search of the world literature on these topics. I had the ambition to collect into one room all the publications concerned with lemmings – and here they are now in this Library. Middleton had the bright idea of using the new plantations made by the Forestry Commission on the Scottish and Welsh hills, for here were the conditions of the grass cover that encouraged full development of a cycle in numbers which we were pleased to find was not dissimilar in length to that of the lemming. On the long rides through the young trees we laid out trace sample points and did various other things on numbers.

Meanwhile John Baker and R.M. Ranson had embarked on the first proper laboratory study of the causes of breeding seasons in voles. Richard Ranson, like Middleton, had been a laboratory assistant at Stowe School. He had a special talent for breaking-in wild mammals to form laboratory stock, and was successful with the field vole, the Orkney vole, the Continental vole (*Microtus arvalis*), the wood-mouse, and the hedgehog. He died of over-work near the end of the War in 1944.

Here I digress a bit. When I read what the Vice-Chancellor said in his Oration last year, about the danger of the administrative machinery in Oxford University grinding to a halt, I like to recollect that I put in an application to the Empire Marketing Board for Baker’s work on a Friday, and received the grant, a quite substantial sum, on the following Tuesday. I do not think that administrators nowadays fully realise the tremendous strain and stress that is laid on research workers by having to wait such long periods of uncertainty for decisions, especially about grants. Nowadays it may take anything up to a year.

By 1931 the Great Depression had happened and about that time all our grants were cut off – Empire Marketing Board and Hudson’s Bay Company. But in 1930 a rich

American gentleman from Washington, D.C., Mr. Copely Amory, had approached the Hudson's Bay Company and then myself, about organising a scientific conference to discuss wild life cycles. This was called the Matamek Conference on Biological Cycles, and took place in a French Canadian fishing village on the north shore of the Gulf of St. Lawrence that was Mr. Amory's property. I acted as its scientific secretary. It is the only conference I have ever known at which the Chairman walked in one morning and threw down two large river salmon that he had just caught, and at which not only professors and various biologists (including Aldo Leopold, and R.G. Green and William Rowan) were present, but also red Indians, game and fishery wardens, poachers and the captain of the Québec ice-breaker. (The last-named would be a wonderful help in Oxford). As a result of this meeting, the New York Zoological Society took up an active interest in my work, through their President Mr. Madison Grant and the Director of their Zoo, Dr. Reid Blair, the latter of whom had been at Matamek and subsequently visited Oxford. They made me a grant on which I was able to live for two years and without which the Bureau could not have started then. We also received a large grant from the Royal Society for the further study of vole cycles.

I sometimes claim that if I had learnt to write clear English, it is from learning Greek at school and from writing applications for grants. Anyway, the Bureau was permitted to be set up in 1932, on a trial run – which eventually lasted 13 years before permanent absorption into the University. It started in the Department of Zoology and Comparative Anatomy, with three of us – Middleton, D.H.S. Davis and myself, working in two rooms. Soon afterwards Ranson joined us. We had few resources, no workshop, no library except my own, but a great deal of energy and optimism. Problems which now look difficult or even insoluble, looked easy. We were the Prospectors, who preceded the Developers and the Sceptics. Tom Warwick also came for two years to study the muskrat invasion in Shropshire and Scotland, but was stationed at a distance from Oxford. In 1935 Miss Marie Gibbs came as our Secretary; she is still with us – the longest surviving member of the Bureau except myself.

From 1932 to 1939 our main preoccupation was with vole populations, although during the ruins of the Slump Middleton had with his characteristic Northamptonshire toughness started a national enquiry into the spread and distribution of the invading American grey squirrel which was later repeated by various people and is now one of the continuing projects of the M.A.F.F. Later he also developed an important bit of work on common partridge populations, and not long before the War left us to become director of the game research station of Imperial Chemical Industries.

We chose nine stations for measuring vole cycles, spread from Oxford up to North Wales and the Scottish Border, and further to the mountains of West Scotland and Aberdeenshire. (Here a lantern slide was shown of a massive mountain in Argyll, the top in mist and the sides sloping down for a thousand feet to Loch Eck). Studying an animal population in the field is less comfortable and circumscribed than working on animals in the laboratory, though both are very difficult. Our pioneer attempts to measure population changes involved very hard physical work and long journeys by car. In the winter to summer of 1933-34 we drove to the Scottish border once a month, 320 miles one day, did eight hours trace "census" the second day, and got

back home the third day. Starting from knowing practically nothing, we had by 1939 got quite a clear picture of the nature of vole cycles, which can be very violent and fairly regular.

In 1935 David Davis left us to undertake a study of rat populations in Sierra Leone, and thereafter became Ecologist and Chief Rodent Officer in the Union of South Africa's Department of Health in Johannesburg. Dennis Chitty, (an Englishman, son of a Bristol surgeon, who took his biological training in Toronto) came in his place, and did not leave us until 1961, when he took a professorship in the University of British Columbia. His Canadian wife, Helen, worked with him throughout much of this time, and since the war especially they made great progress in handling both the North American wild life enquiries and the vole population studies in Britain. Chitty was the first to apply ringing methods to wild mice and properly to elucidate their population structure.

Both before, during and after the War the Bureau owed a debt to the former University Registrar Mr. (now Sir) Douglas Veale, that is immeasurable. Without his pathfinding through the insane jungles of University and Government administration the Bureau could not have either started or survived. Among the very numerous recurrent crises affecting the Bureau's survival was the question of my own job. During these years I lived from hand to mouth: the New York Zoological Society, a Leverhulme Research Fellowship, a Christopher Welch grant. But in 1936 my personal position was assured by getting a Readership in Animal Ecology and a Senior Research Fellowship at Corpus Christi College.

In 1936 Professor (now Sir Howard) Florey did us a very good turn, by putting me in touch with Leslie. P.H. (better known as "George") Leslie was a trained pathologist who had been studying the whooping cough bacillus in the Sir William Dunn School of Pathology. Florey suggested that he might be interested in the epidemiological aspects of our work. At first Leslie helped us by looking into the question of mouse typhoid bacteria, but soon he began to get absorbed in the biomathematical properties of populations, and this was the start of the brilliant series of mathematical "models" which have done as much as anything else to enhance the intellectual standing of the B.A.P. Through the years he has also acted as a statistical adviser on innumerable topics. He is still with us. His first triumph was to work out, in collaboration with our animal-breeder, Ranson, the intrinsic rate of natural increase for the vole – the first time this had been done for any wild animal except man. Later on he did the same for Orkney voles and brown rats. This function, known as "little *r*" is one of the most important in population ecology.

Florey also gave us magnificent help in arranging that Dr. A.Q. Wells could come to this department, where we had built a small isolation laboratory for the purpose, and act as pathologist for our vole work. The Medical Research Council made his grant, which they continued until his death (which occurred in 1956, after over-exertion during deer-stalking in Scotland). After many blank months during which he examined voles sent in alive from all over Britain, through an organisation created by the Chittys, in 1938 he suddenly discovered vole tuberculosis, which turned out to be a widespread organism in wild small rodents. Until that time the tubercle bacillus (*Mycobacterium*) was implicitly believed to be attached only to man and domesticated species. This was a new, fourth, kind of tubercle bacillus, and a

genuine wild one. It proved to be a mild strain, with immunising properties not dissimilar to the well-known weak human strain "B.C.G." and both of these have been tested and used on a very large scale for human beings. But at the same time Chitty proved conclusively that the disease was unrelated to the cycle of vole populations. From about this time our interest in the parasitic diseases of wild animals diminished very much, as it became clear that they were not usually concerned with the regulation of numbers.

We undertook another cooperative research concerned with the effects of voles on vegetation. Here I want to digress again, to remark that cooperation with other departments and institutes does not depend upon living together in the same building like guillemots on a huge cliff. (Here a slide was shown, of an eleven hundred foot high guillemot breeding cliff in Hinlopen Strait, Spitsbergen). It may be just as difficult to fly from one ledge to another, as to visit another cliff! Anyhow, we got help from a botanist at Kew Herbarium, V.S. Summerhayes, my partner in the 1921 Spitsbergen survey. For five years he measured the vole pressure on vegetation, by means of exclusion cages and controls set up in forest rides on the hills of Wales and the Scottish Border. He proved that it is voles that, by concentrating on dominant grasses for food, keep down the grass canopy sufficiently to enable certain flowering plants and mosses to survive there at all. An example of such a species is the beautiful bog asphodel. Thus the vole, besides having a population problem, is the dominant influence in a whole community.

Just before the War we were joined by H.N. (better known as "Mick") Southern, now my right hand man. He decided to study the ecology of a rabbit warren. Here I want to tell a story about grants. I approached five agricultural bodies (private and national) and three forestry bodies (private and national) for grants. All refused, saying that the ecology of the rabbit was only of zoological interest. After the War, Harry Thompson, one of our war-time staff who later became director of an M.A.F.F. research laboratory, consulted me about research on rabbits. I suggested that the most useful practical thing would be to get unequivocal measures of rabbit damage to crops. This was eventually done and came to figures of about 3%. When the British rabbits died of myxomatosis, a figure of £15,000 was worked out for the total saving caused by cessation of rabbit damage. It is a curious coincidence that a sum of this order was at the same time knocked off the farm subsidy by the Minister of Agriculture. I think this is quite an interesting bit of social history.

With the help of other people at the B.A.P. (and a slide showing Francis Evans, our first graduate student, who rejoins us for a year as a University of Michigan professor this summer, and Dennis Chitty, putting fence stakes into the ground), the warren was surrounded with wire in which were controlled entrances. Rabbits had huge numbered ear-tags by which they could be individually identified by Southern sitting up a tree with a telescope. This was perhaps the first visual marking of a wild mammal population, and certainly the first serious population analysis of a rabbit society.

In 1939 University scientists were told that their services would be reserved for special work in the event of war with Germany. I did not quite believe that this work would ever be organised (as indeed it was not), so in April I submitted a plan to the Agriculture Research Council, for the staff of the B.A.P. to turn over to applied

research upon rodent control if war came about. This was approved in time (it took four months), and we actually started on the job a week after war broke out. (Here a slide was shown of a horde of rats attacking the farmer's stores in a barn, a picture by Gustave Doré done for La Fontaine's *Fables*).

I am not going to say much about this war work. It occupied about 15 people for up to eight years, more if we include the writing up and publishing of the results, which were eventually printed by the Clarendon Press, with the aid of a private donation of £2000. (This donor, on being told that some of the money had not been needed, and that we had also received some royalties, generously agreed to let the surplus be used for foreign travel by our staff. With it, useful visits have been made for research or conferences, to North America, Czechoslovakia, Poland, Sweden, Denmark and Uganda).

I shall only select a very few examples to show what this work involved. When we started 1939 the Government had only one man, in the M.A.F., engaged in organising rodent control, and he was on sick leave. At the end of the war there was a well-trained organisation covering both town and country, and a research unit at the M.A.F.F. (since split into two units), which took over our developed functions for permanent peace-time work. Parkinson's theorem had indeed operated among men, but among rats, mice and rabbits it acted in reverse. Much of our research was done in rather odd habitats, including rubbish dumps and even the Bodleian Library. Chitty's classical discoveries about the shyness of rats towards new objects, including bait, were made in a hen-house at Bristol and in an old tin trunk on the Port Meadow municipal dump. Some of the early observations on house-mice were made in the back stores of a grocer's shop in Market Street, and in food depots situated in all kinds of buildings. Police once nearly arrested Middleton for spooning cyanide powder into rabbit holes on a railway embankment, under the impression that he was trying to blow up a bridge.

Among these curious places that we used was a small, decrepit pig-farm near Port Meadow, called "Giles'", in which many important experiments were made on rat behaviour. This place has no architectural dream qualities. There was a mummified dead cat nailed over the door, and the ancient car of the owner, which we had thought to be a static part of the habitat, was one day driven out carrying with it bits of the experimental equipment. Among other things, rats were photographed by infra-red light by Southern and others.

Research was not confined to small sample populations or experiments on poisons: we took it forward to the operational research stage ourselves, organising rat control by the latest means on 11,000 acres of downland farms; in the whole of the Oxford market; in the whole of the Port of London Health Authority's territory; and along large stretches of Oxford's and London's sewers.

Corn-ricks were not only a practical problem as reservoirs of rats and mice, but enabled measured unit populations to be studied exactly. Outside this country, J.S. Perry and the late J.S. Watson made a special expedition to Palestine and the Sudan to adapt our methods to rat control in these conditions.

My remarks about our post-War work will necessarily be very brief, and are not intended to give any adequate description of the 35 or so separate research projects that have been done or are still in progress. The Exhibition is intended to supplement what I can say. In 1947, after another of our recurrent crises, the B.A.P. was separated from the Department of Zoology in a new Department with the title of Zoological Field Studies, which also included the Edward Grey Institute for Field Ornithology. We were given new temporary quarters in war-time hospital huts in the garden of St. Hugh's College in Banbury Road: it had no marble floors, no lifts (it was only one-storey), no rosewood desks, not even any gas, but it was spacious and very peaceful after the slum conditions in which most of us had been living. And there was room for graduates. Denys Kempson came, to begin producing his cornucopia of technical inventions.

We shall never cease to be grateful to our Professor, Sir Alister Hardy, for saving the situation then, and making conditions for peaceful and uninterrupted and fertile work during the following fifteen years. He has given us the maximum support with a minimum of interference. Later we moved into the present building, which is as ideal as anything we are likely to get. It is worth mentioning, in an age that thinks in terms of hundred thousand pound edifices, that the conversion of the old Botany Department for the use of our two institutes cost the University about £5,000 at the time, and not much additionally since then.

In drawing towards a conclusion I shall only pick out four topics for special mention, since any other details can be consulted in the Annual Reports of the Department. These are Foreign Contracts, Wytham Woods, the Tawny Owl work, and the Use of Extensive Surveys.

Already, through our North American wild life enquiries, we had for many years kept in contact with every significant piece of population work over there, besides having actual annual reports on some species over an area of two million square miles or more. In the early thirties, through the accident of a small text-book of mine being translated for use in Russian universities, we developed a close contact with Russian ecologists and obtained most of the literature we wanted, some 200 papers being fully rendered into English by our special translator, D. Jackson. This contact has been maintained to some extent since the war, and one man, Kalabukhov, continued to write during the war convoys period, so that we have exchanged ideas with him for about 30 years.

In the United States my main contacts were with Aldo Leopold, the doyen of American wild life management and conservation; and R.G. Green, the only man ever to do a complete population study of a snowshoe hare cycle. They are both dead now. Since the war, Professor Tom Park, of the University of Chicago, has been the greatest friend and mentor of the Bureau. To him we owe the idea of expanding our graduate visitors, up to the present pattern of about six research staff and about six or seven visitors. He also prodded us into starting some vacation teaching courses at Wytham, which were very successful. From such foreign contacts there are always chain reactions, people sending over their students for training, and some of these students later coming back, perhaps as Professors (as with Peter Larkin now, from Vancouver). One of his colleagues was Dr. E.W. Fager, a brilliant biochemist who decided to change over to animal ecology. He came over to the Bureau for two years,

and is now an oceanography professor in the Scripps Institution in California. One of his graduates, Ray Ghelardi, is with us now. Another of Professor Park's graduate students, Monte Lloyd, has been here five years. These two are jointly studying the populations living in tree litter – an interesting variant from measuring marine kelp communities by under-water diving, and the cannibalism of flour beetles, respectively. The distribution map shown among the exhibits illustrates the present location of people who have worked or trained here, and it may be noticed that they are concentrated in Britain, West Europe, North America and Australia.

In Europe are most valuable relationship is with Dr. Christian Overgaard Nielsen, in Denmark. He came over for two years to do research at Oxford, and returned as Director of the Mols Laboratory in Jutland. This fascinating nature reserve offers a contrast in acid-soil conditions with those at Wytham, which are alkaline, and has been visited by several of us.

The Library in which we now are represents one of the chief consequences of our international contacts, and contains most of the publications we need for population research, other than those cognate things handled by the Edward Grey Institute and Hope Department of Entomology libraries. The three together provide a unique ecological assemblage of ecological publications.

Not much will be said about Wytham Woods, although it is the focus of our fieldwork. Members of the B.A.P. have visited and used it since 1943, when it was first acquired by the University. Perhaps the Singing Way, with its great beeches and its views across country best symbolises the mixture of aesthetic and scientific pleasure that the ecologist gets up there. But it is not only the inside of the Woods, with its rich mixture of habitats, including so many formed of or from dead wood and dead leaves, that concerns our fieldwork. There is now an extensive series of limestone meadows and marshes and streams also preserved for research. The Ecological Survey is concerned with the whole lot.

Nothing better illustrates how the whole of the Woods may be needed for an ecological study than Southern's long-continued measurements of the tawny owl populations and their preys (here was shown a photo of a tawny owl bringing a large earthworm to its young in one of the huge owl nest boxes up a tree). His maps of territories cover the whole hill.

One last example, to illustrate a principle of our work. The usefulness of wide mapping of distribution, such as was done for the grey squirrel has by no means been outmoded. Kitty Paviour-Smith has been studying a beetle, abundant in the dead stages of the birch-bracket fungus, which has spread from the London area, probably from a specimen of fungus in Kew Herbarium, and is now known to be an Australian invader (here a map of the zones of spread was shown).

So we can see that the investigation of single species populations, of predators and prey, the structure and interspersing of whole communities, and of foreign invaders into them, are all connected parts of the animal ecologist's field of study.

It will be observed that whereas before the War our research was deeply concerned with mammal populations, afterwards it has spread to a much broader field, making our work more that of a general ecological institute.

Professor Goodrich once remarked to me, at a rather bad time: “You know, there is nothing so permanent as a temporary arrangement.” I hope the reverse will never be the case. After another thirty years, I may not be taking much interest in things, as I should, if I lived, then be 92! But even then I would like to know that the B.A.P. were still going strong as an institute, and that its research had advanced so far that I could hardly understand anything that they were doing.

{Caption for images on the next page}

These buildings in the grounds of St. Hugh’s College were built for an occupied by the Head Injuries Department of the Radcliffe Infirmary during World War II. In 1947, The Bureau of Animal Population and the Edward Grey Institute under the title of Zoological Field Studies moved in and were based there until 1952. Then both units moved to the buildings facing the Botanic Gardens until the new Zoology Building was completed in 1970 when they moved there. The EGI still retains its name but the BAP had by then, with Charles Elton’s retirement and the appointment of John Phillipson, been renamed as the Animal Ecology Research Group.