

2000

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Recommended Citation

Potts, G. R. (2000) "Using the Scientific Method to Improve Game Bird Management and Research: Time," *National Quail Symposium Proceedings*: Vol. 4 , Article 2.

<https://doi.org/10.7290/nqsp04kr4n>

Available at: <https://trace.tennessee.edu/nqsp/vol4/iss1/2>

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FOURTH STODDARD MEMORIAL GAME BIRD LECTURE

USING THE SCIENTIFIC METHOD TO IMPROVE GAME BIRD MANAGEMENT AND RESEARCH: TIME

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ABSTRACT

Aware of the time lag that frequently exists between declines in biodiversity and effective conservation to correct and reverse the declines, I examine some reasons behind this problem. Experience with species as diverse as the shag (*Phalacrocorax aristotelis*) and grey partridge (*Perdix perdix*) shows the main problem to be the long period of time needed to detect problems, to define causation, to install effective changes in policy and, finally, to bring about restoration. The time needed to conduct research and implement policy to solve such problems often exceeds the time span of a career in ecology. Speedier results are therefore essential, but they will depend in part on removing the barriers between practitioners and theorists on the one hand and between practical applied ecologists and bureaucratic policy makers on the other.

Citation: Potts, G.R. 2000. Using the scientific method to improve game bird management and research: time. Pages 2–6 in L.A. Brennan, W.E. Palmer, L.W. Burger, Jr., and T.L. Pruden (eds.). Quail IV: Proceedings of the Fourth National Quail Symposium. Tall Timbers Research Station, Tallahassee, FL.

Two events in the early 1930's, the publication of Stoddard's (1931) monograph on the northern bobwhite (*Colinus virginianus*) and the establishment of The Game Conservancy, were unrelated—but not for long. Stoddard visited some Game Conservancy study areas at the beginning of June 1935 and further inspired our organization with the idea of game as a by-product of farm crops. In his case, of course, the crop was timber that was managed by selective logging, “carried out with the welfare of game prominently in the picture.”

Stoddard regularly corresponded with my predecessors. In one letter dated November 1945 he wrote to A. D. Middleton, “I am afraid you will be greatly disappointed with the game research that you will find under way in the United States.” Nevertheless, he continued to encourage bobwhite quail managers using, in part, his knowledge of successes with the grey partridge (*Perdix perdix*) in the United Kingdom (UK). Similarly, Middleton visited Stoddard in 1947 and immediately began to encourage partridge managers using his knowledge of bobwhite quail management in the United States.

All this is a long time ago, but it does introduce the theme of my talk, which is time. How much time do we need to carry out the research necessary to solve a problem? There is also the related question—do we need long-term monitoring? After all, say the critics, monitoring does not advance science in the way that experiments can. There is no virtue in long-term data gathering for its own sake. I suppose monitoring could become a completely mindless exercise, though it will not be mindless if the objectives are clear.

For as long as I can remember, there have been vacuous arguments about the value of long-term studies. Long-term monitoring projects have suffered, particularly where government departments have been involved (e.g., in the UK the Rothamsted annual aphid surveys financed by the Ministry of Agriculture, Fisheries & Food (MAFF) and the continuous plankton survey, once funded by MAFF, now financed with private funds by The Sir Alister Hardy Foundation for Ocean Science). My proposition here is that long-term work is not a virtue, it is a necessity that stems from the long-term basis on which nature itself operates.

I will show that the length of time necessary to diagnose a problem, get action on that problem, and monitor the remedy, often must be measured in decades. I will argue that things must be speeded up, and that it would help if scientists were to become more involved in policy issues, and less detached from practical considerations. By the same token, policy makers need to have more practical and scientific experience.

METHODS AND RESULTS

Below I draw on some of my own experiences to illustrate the amount of time it takes for research to provide solutions to problems.

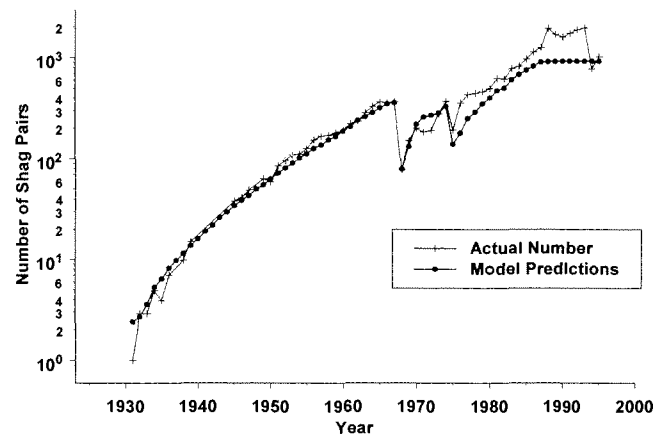


Fig. 1. Comparisons of actual and modeled population trend of the shag (*Phalacrocorax aristotelis*) on the Farne Islands, United Kingdom from 1930 to 1995.

Monitoring and Modeling

The first example I shall give is that of the shag (*Phalacrocorax aristotelis*) on the Farne Islands. The population model used to predict annual changes was a simple one written in 1978 but it took nearly 30 years of monitoring to accumulate enough data to develop it. It was only after 14 years of annual ringing, mostly by John Coulson of the University of Durham, that sufficient data had been collected to start the studies that we began in 1962. Seventeen years later we were able to produce a model of population growth. The redistribution of sites and breeding success following the red tide of 1968 was as predicted, and this gave us the confidence to publish (Potts et al. 1980). However, the question remained: How good was the model?

Our model certainly fitted the retrospective data well (Figure 1) but for the future it turned out to be too pessimistic, because we had not studied the shags for a sufficient amount of time. We had not foreseen that the birds would spread to many other islands in the Farne Islands group. The model basically represented what would happen on 4 of the 11 Farne Islands, and when inferences from the model are limited to these 4 sites it remains satisfactory. The birds, however, now breed on a further 4 other islands, all of which are less suitable than the original 4, but support significant numbers of birds, nonetheless. Neither did we consider that the strict control of disturbance by tourists would have such a benefit to the birds by reducing predation by herring gulls (*Larus argentatus*). Nor did we even consider that guillemots (*Uria aalge*), which return earlier to nest sites than shags, would increase and eventually take over many of the best nest sites.

Although we predicted a peak shag population of a little less than 1,000 pairs, the population now looks as though it might reach a ceiling at about 2,500 pairs (Figure 1). Before we leave the shag example it is instructive to suppose that we had checked the numbers only once, in 1995, 16 years after the model was

completed. In that year there were 1,016 pairs and we might congratulate ourselves since the number is very close to what was predicted. There were, however, already 1,948 pairs by 1993, over twice what we had expected. The comparatively low numbers in 1995 were due to the effects of very high mortality during 1994. Such results show the benefits of annual monitoring and a danger of using only limited spot checks as a substitute for monitoring.

My Sussex study on partridges had its origins in a monitoring project started by others in 1954 (a farmer, Christopher Hunt, and a gamekeeper, Fred Allen). Their ideas were based on the Damerham study which began in 1947 and on 7 years work by A.D. Middleton prior to World War II. Thus, my partridge model developed in 1977 was written after no less than 37 years work, mostly by others (Potts 1986). The model accurately predicted the changes in the Sussex study partridge population through to the present time. However, density-dependent nesting mortality was higher than expected, offset by lower density-dependent winter losses than expected (Potts and Aebischer 1994). Furthermore, various experiments justified the basic model parameters. The structure and role of nest predation was verified in the Salisbury Plain experiment (Tapper et al. 1996), and there was validation of this point from North Dakota (Carroll 1992) and Poland (Panek 1997). The supposed effects of pesticides on chick survival were verified in a number of experiments (Rands 1986, Sotherton 1991).

The Sussex partridge model, however, has not been able to predict the situation accurately on the 824-acre (333-hectares) farm managed by the Trust since 1992. In that year, there was one pair of partridges and by now we had predicted that we would have 12 pairs, whereas in fact we have only 4 or 5.

Although 57 years of study have been insufficient to develop a model that will produce really robust predictions, we do have a model which has proved very useful in partridge conservation. In particular, it drew attention to the intensity of density-dependent nest predation that was otherwise obscure, and implemented the management needed to overcome its effects.

One factor discounted in the partridge models was raptor predation. In retrospect, this seems to have been entirely justified and still is; however, the continuous increase in raptor numbers in the area (see Figure 2) draws attention to the need to embed conclusions in the time frame of the study involved. It could even be that at some future point raptors will have overwhelming importance, and could possibly prevent the recovery of partridge populations when all other factors have been controlled by good management. I emphasize we have no data to show this yet but raptor numbers are increasing in the Sussex study area and need to be monitored. During our annual surveys the ratio of partridges seen to raptors seen has changed from 1:150, to 1:15 over the 30-year Sussex study (Figure 2).

How many years does it take to complete a controlled and replicated experiment to verify a model?

Consider the Salisbury Plain experiment (Tapper et al. 1996). This cross-over test was first designed in

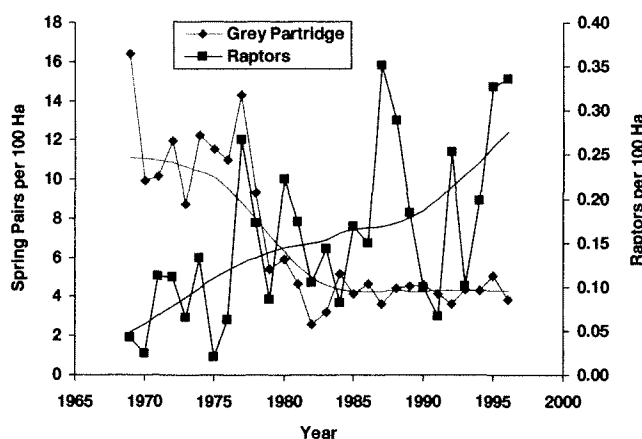


Fig. 2. Results of annual counts of partridge (*Perdix perdix*) and raptors in late August/early September 1969–1997; data from the Sussex Downs study area, United Kingdom.

1982 following the experience of Marcstrom and colleagues on islands in the Gulf of Bothnia (Marcstrom et al. 1988). Thus, 14 years elapsed from planning to publication. My estimate would be that it could not have been done more quickly than in 9 years. Game Conservancy Trust (GCT) experiments on conservation headlands took 8 years (Sotherton 1991). GCT work on insecticides and the recovery times of insects affected would also suggest that a minimum of 9 years field work will be necessary from planning to final publication, where a large scale cross-over experiment was involved (Aebischer 1990).

Most experiments with partridge populations appear to need up to 10 years from first planning to final publication in the refereed scientific press.

How many years does it take to (i) detect a problem, (ii) diagnose its causes, and (iii) start remedial action?

- (1) *The insecticide pp' DDE.*—In a sense, the monitoring that detected the thinning of raptor egg shells due to pp' DDE began in the 19th century with the collection of eggs for museums. Although, the effect of shell thinning started prior to 1950, it was retrospectively detected in 1966 and proven experimentally in 1969 with bans increasingly effective over the period 1969–1976 (Ratcliffe 1980).
- (2) *Seed dressings incorporating the insecticide diel-drin.*—The direct effects of these seed dressings began in 1956, and were detected during the first season of use (Anonymous 1957). The lethal dose (LD₅₀) was only established in the mid-1960's (Robinson et al. 1967) with effective bans over the period 1962–1966 (Ratcliffe 1980).
- (3) *The foliar insecticide dimethoate.*—The direct effects on beneficial insects were first quantified in 1975 (Vickerman and Sunderland 1977). Indirect effects on partridge chick survival were first reported in 1990 (Potts 1990). The first measures to restrict the use of this insecticide were to exclude it from the outer 6-meter wide margins of cereal

crops, part of the UK's Pesticides Safety Directorate Review in 1993.

- (4) *The demise of traditional undersowing in cereals.*—The indirect effects of this change in cereal growing began in the mid-1960's, with effects first suspected in 1969 (Potts 1970). Adverse effects on insects, particularly sawflies, were documented over the period 1971 (Potts and Vickerman 1974) to the present (Barker and Reynolds 1999). Worthwhile incentives for farmers to restore the practice have been introduced in only 1 ESA recently and in 2 pilot areas of the Arable Stewardship Scheme, which will begin to be effective in the years 1999–2004.
- (5) *The indirect effect of non-insecticidal broadleaved herbicides.*—These were first quantified in the mid-1960's (Southwood and Cross 1969), with further documentation accumulated to the present time. Grants for using conservation headlands to mitigate damage for some species were introduced gradually beginning in 1992 (Potts 1997).

How long after the remedial action would it take for the populations to recover?

The recovery times of the sparrowhawk (*Accipiter nisus*) (Newton 1986) and peregrine falcon (*Falco peregrinus*) (Crick and Ratcliffe 1995) were approximately 20–25 years. The shag (this paper) and some raptors (Newton 1994) are still recovering from past effects; their recovery time could be in excess of 75 years. The recovery of the stock dove (*Columba oenas*) could take as long as 20 years (O'Connor and Shrubb 1986). The calculation of recovery times from modelling gives 7 years for the grey partridge (Potts 1986) and 4 years for sawflies (Aebischer 1990).

To summarize, with consideration of length of time needed for monitoring, the time needed for modeling, experimentation, remedial action, and restoration is to be measured in decades. Allowing for overlaps (Potts and Robertson 1994), the total time needed to conduct research on factors limiting the abundance of wildlife populations is in excess of a full career in ecology.

DISCUSSION

There are, of course, several monitoring studies that are effectively permanent but these are rare and exceptional scientific initiatives. Among the best examples are those at Rothamsted, England, where the Broadbalk and Park-grass experiments have been carried out for 155 and 142 years, respectively. Even at Rothamsted, however, this long-term work contrasts with the relatively few studies there that have lasted more than 3 years (Woiwod 1991).

Some of our longest studies are not very long-term in biological time. One of the most well known is that of the fulmar (*Fulmarus glacialis*) on Eynhallow, Orkney Islands, which took place during the equivalent of the late George Dunnet's entire working life (Jenkins and Wynne-Edwards 1996), yet it encompassed little

more than 2 fulmar generations. The larch bud moth (*Zeiraphora griseana*) research in Switzerland, examined after 34 years, covered a period of only 4 cycles long (Clark et al. 1967).

In fact, it is astonishing that we can have a debate at all about the value of long-term work given the age of many organisms; up to 1,400 years for trees in the Amazon (Chambers et al. 1998), possibly longer for the yew (*Taxus baccata*) in the UK (Mabey 1996), 4,700 years for the bristlecone pine (*Pinus aristata*), and 10,500 years for the huon pine (*Lagarostrobos franklinii*) of Tasmania (Anonymous 1995).

One feature of any long-term research is that it would have to persist through the many changes in fashion that seem to dominate animal ecology. The temptation is often to divert wholly into theory problems such as density dependence versus density independence; diversity and stability theory; intrinsic versus extrinsic population regulation; ecosystem energy flow; ideal free distribution consequences; chaos; optimal foraging; acid rain; metapopulation theory; global warming; special effects observed through satellite imagery, and many others including diversity and stability, which are coming round for the second time in a generation (compare, for example, Way 1974 with Tilman et al. 1996). It could be that the attractions of theoretical ecology thwart long-term field work, but there are lots of other reasons ranging from fossilization of scientific careers through to the difficulties of securing long-term funding. What funding organization will today give open-ended career length commitments of the kind that were available to chemists and physicists in Germany in the 19th and early 20th centuries? Yet ideally, planning should be embedded in a time-frame which is greater than the length of individual scientific and administrative careers.

The long time-scales are, I believe, an actual cause of unnecessary reductions in biodiversity. We need a better system, we need to speed up the research and make it more effective and, frankly, more useful.

Given there is insufficient time and support for long-term studies, most policies are driven by incomplete research. It might be possible to make up some lost time or take short cuts by revisiting some of the classic study areas, e.g., those of Paul Errington in Iowa or of the Craigheads in Montana. Common sense could help make up the shortfall too, but it depends on long-term practical experience, something that has been seriously neglected by many ecologists. For example, recent results showing the benefits of plant biodiversity in grassland ecosystems in the USA (Tilman et al. 1996) have been the basis of traditional ley farming in the UK for 200 years.

In today's world, policies are often driven through the media by pressure groups, often on single issues. But who is to blame? How many of us at Quail IV have been regularly involved in trying to influence government policies? The very idea that science should be applied is anathema to some ecologists, but surely we can all agree that research is a better driver of policies than dogma. Please become involved, like Stoddard did all those years ago. It need not detract

from your science. At present, however, in both the UK and in the USA the main problem is the lack of effective communication between policy makers and field-based practitioners.

ACKNOWLEDGMENTS

I would like to thank my colleagues and Wendy Smith for helping prepare this paper.

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Note added in proof: Since this paper was delivered on 6 May 1997, research has started on the possible effects of raptors on partridges, and the diversity-stability debate has moved a long way, for example see McCann et al. (1998) Weak trophic interactions and the balance of nature. *Nature* 395:794–797.—GRP.