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CONCLUDING REMARKS: THE RESEARCH PERSPECTIVE

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Attending this symposium was something of a pilgrimage to Mecca for me. I grew up hunting quail and rabbits along railroad tracks and osage orange hedges in central Illinois, but every *Field and Stream* story I ever read about quail hunting showed bird dogs on point in a piney woods. Later, when I got to graduate school and told my major professor (the late W.D. Klimstra) that I wanted to work on quail for my research project, he just handed me a copy of Herbert Stoddard's book (Stoddard 1931) and said, "Come back after you have read this and we can talk."

When I asked Lenny Brennan what he wanted me to talk about tonight, he said I should first describe past research-management interactions, then I should assess the current state-of-the-art in quail research, and finally I should discuss how researchers and managers can cooperate to ensure the bobwhite's future—all in 15 minutes. So, I guess I had better get started.

As to how research and management interact, well, I know how they are supposed to interact. Research is supposed to accumulate and synthesize knowledge about a particular subject, and management is supposed to apply this knowledge to achieve certain goals (Bailey 1982). Sounds simple enough, but we all know it is not.

First of all, when the knowledge we seek involves natural systems, the process can be very slow and difficult. One reason is the extreme complexity of these systems. Someone once said that nature is more complex than we think. In fact, it's more complex than we can think. Another problem is lack of direct access to the critters we are studying. We can not confine them to cages and observe them like laboratory rats. In addition, we have no control over the vast array of biotic and abiotic factors that affect these free ranging populations. Consequently, habitat studies are routinely confounded by changes in weather, and vice versa.

Finally, we have to remember that animals live the way they do because natural selection has been molding them into their environment for literally millions of years. Even the most rudimentary understanding of how this "evolutionary wisdom" works is extremely difficult because the time scales involved are almost incomprehensible to us.

Another problem is that all knowledge produced by research is not necessarily reliable. Unreliable knowledge can come about in several ways: one is faulty research in which the method of data collection and/or analysis is somehow flawed. Peer review at the proposal or publication stage is supposed to guard

against this, but it does not always do a perfect job. A second type of unreliable knowledge was described by H.C. Romesburg in his much-cited 1981 paper (Romesburg 1981). Ideally, research is supposed to follow the scientific method which involves 3 steps: (1) the collection of a set of facts; (2) the development of a hypothesis to explain these facts; and (3) the testing of that hypothesis with another, independent set of facts. Romesburg contended that wildlife research generally stopped after the first 2, and seldom proceeded to the 3rd step. Even worse, he noted that over time, some of the untested hypotheses acquired the status of principles or laws. In other words, they became dogma simply by being repeated often enough. Romesburg was not a quail biologist, but it's interesting that the example he used was Errington's threshold of security concept (Errington 1945) which for years formed the basis of our annual surplus theory of harvesting quail and other upland game.

There is still another type of unreliable knowledge. That is when knowledge obtained under 1 set of circumstances is mistakenly assumed to hold for all circumstances. Back in 1982 at the 2nd Quail Symposium, Klimstra (1982) pointed out that much of what we know, or think we know about quail was derived mainly from thriving, healthy populations occupying large tracts of optimum habitat. He suggested that it might be wise to reexamine some of these so-called truths in light of the fact that many quail populations are now persisting at much lower densities in habitats fragmented by bulldozers and contaminated by chemicals.

This brings me then to the current state-of-the-art in quail research. I think bobwhite research can roughly be divided into 4 periods: The 1st period was the 1920's, 1930's, and 1940's and could rightly be called the Stoddard-Errington-Leopold era. Many of the fundamental principles of quail management derived from their work and writings. The 2nd period spanned the 1950's, 1960's, and 1970's when people like Jack Stanford, W.D. Klimstra, Val Lehmann, Walter Rosene, Bob Robel, Ralph Dimmick and others expanded our knowledge of bobwhite ecology and management. The 3rd period roughly corresponded to the 1980's. As Brennan pointed out at the Quail III Symposium (Brennan 1993), this period represented something of a lull in quail research with a noticeable decline in numbers of papers published, percent of the total literature devoted to quail, and amount of funding for quail projects. Since that time, I think we have entered the 4th era, which is characterized by renewed interest

in quail research and management. I am encouraged by the quantity, and especially the quality of bobwhite research being conducted by people like Wes Burger in Mississippi, Fred Guthery and his students at Texas A&M, Tom Dailey and his colleagues in Missouri, and of course here at Tall Timbers and other researchers whom we have heard from over the last couple of days.

I think the people I just mentioned would be the first to tell you that their research has benefitted from the body of knowledge accumulated by workers that preceded them. That is how science is supposed to progress. In all honesty, however, much of the earlier research conducted by us old-timers tended to be mostly descriptive or correlative in nature, often lacked proper experimental controls, and used questionable statistics or none at all. I think that most quail researchers today recognize these problems and are attempting to address them.

As a researcher, I tend to judge the current state-of-the-art of quail research primarily on the basis of its quality and how it contributes to the overall body of scientific knowledge. Managers, understandably, are more concerned with its applicability to their specific goals or objectives. And this brings up the old question of practical versus basic research. There are probably managers here and elsewhere who would disagree, but I do not think this is really an issue with quail research—in my opinion, the vast majority of studies, past and present, have been practical in nature. In fact, I would say that perhaps we have tended to neglect basic research in favor of the practical. Only a very small fraction of the literally thousands of quail studies that have been conducted have focused on such fundamentals as population genetics, sociobiology, and behavioral ecology including optimal foraging strategies, spacing behavior, and the proximate and ultimate factors involved in habitat selection. I would argue that such basic information will ultimately be necessary if we are to ever fully understand what is happening to this bird we are all so concerned about.

Some have suggested that a good deal of the more practical, site-specific types of studies (e.g., optimal burning schedules, disking rotations, or even harvest strategies for that matter) could and should be done as part of management itself. They have even given this a fancy name: Adaptive Resource Management (Walters 1986). The rationale is that because we really do not learn very much from systems at equilibrium, and because management often involves some type of manipulation, we are missing opportunities to obtain new knowledge by not attempting to evaluate the effects of these manipulations in a scientific manner (Macnab 1983). To do this successfully, however, requires the imposition of certain conditions on management operations such as applying only 1 treatment at a time, randomly assigning different levels of this treatment, maintaining untreated or control areas, and collecting data in a statistically sound manner (Sinclair 1991). In the real world, many of these conditions and constraints have proven unacceptable to administrators, managers, and the user public (e.g., Gratson et al.

1993). Still, it is something that we should consider whenever possible.

In closing, I would just like to remind you that as necessary and vital as research is, it is not an absolute cure-all for the current problems faced by quail and other forms of wildlife. The widespread decline in bobwhite abundance over the past 3 or 4 decades did not result from lack of knowledge on the part of biologists and managers. It resulted from fundamental changes in land use and landscape composition and pattern. Given enough time, space, and opportunity, I think we have sufficient knowledge and skill to produce locally abundant quail populations. To be a viable game species, however, it is not sufficient for quail to be only locally abundant. They must be reasonably abundant over relatively large portions of the landscape. The problem, of course, is that quail biologists and managers do not control large portions of the landscape. As Brennan stated a few years ago: "Clearly, the fate of the northern bobwhite hangs in the balance of how we farm our land and manage our forests" (Brennan 1991:553). Finding ways to accommodate the needs of quail in emerging agricultural and forestry programs will be challenging, but absolutely essential. Workshops and discussions here and at the previous quail symposium clearly demonstrate that there is a general appreciation for, and commitment to, this approach.

I thought long and hard about ending my remarks right here—on a reasonably positive note. Instead, I am going to say something that I think most wildlife biologists already know, but for some reason seem reluctant to talk about. In my opinion, the problems we've discussed here tonight and throughout the symposium, important as they are, are still just proximate concerns. There is a more fundamental problem that confronts not only quail, but all other wildlife species as well. I am talking about the continued growth and expansion of the human population, coupled with a land use philosophy that ignores the future in favor of financial priorities and the sanctity of property rights.

I dislike ending on such a pessimistic note—but it is my opinion that in the face of an ever-expanding human presence on the landscape, only a relatively few wildlife species will ultimately thrive, and the bobwhite will probably not be one of them. Hopefully, the expertise and commitment evident at this symposium will be sufficient to prove me wrong.

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