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Boll Weevil Eradication

John H. Perkins

Summary. Some representatives of the cotton industry and of the entomological profession advocate efforts to eradicate the boll weevil. This coalition originated in 1958 from a complex of socioeconomic changes in cotton production and scientific developments in entomology. The results of a pilot eradication experiment (1971 to 1973) were controversial, and the debate was inhibited by social pressures upon the entomological profession. Substantial conceptual difficulties also attend evaluations of eradication experiments. A new trial eradication program is under way. If its evaluation is not to be warped by problems similar to the earlier ones, both the social and scientific aspects of eradication must be recognized and steps must be taken to ensure a full and open debate.

The boll weevil (*Anthonomus grandis* Boheman) (Fig. 1) has been a serious pest in U.S. cotton production since 1892 (*1*). Today it is a key pest in more than half of the U.S. cotton acreage and causes an estimated 8 percent loss of yield (*2*). Cotton producers spend an additional $50 million per year for insecticides (*3*). Environmental contamination from such efforts is high as an estimated 30 percent of all insecticides used in American agriculture is directed toward the boll weevil (*4*, p. 5).

Cotton growers have relied heavily on synthetic, organic insecticides to controll boll weevils since the late 1940's. Toxaphene and methyl parathion have received particularly high use as did toxaphene plus DDT until DDT was banned in 1972 (*3*, p. 2; *5*, *6*). Boll weevils developed resistance to the chlorinated hydrocarbon insecticides, beginning in 1954 (*7*), and entomologists fear it may develop resistance to organophosphates such as methyl parathion. Insecticides directed against the boll weevil also induced outbreaks of secondary pests, notably the bollworm [*Heliothis zea* (Boddie)] and the tobacco budworm [*Heliothis virescens* (Fabricius)]. *Heliothis* spp. in turn have developed resistance against insecticides that renders them difficult or impossible to control with chemicals (*6*).

Resistance, outbreaks of secondary pests, and environmental hazards induced entomologists and cotton producers to seek new control strategies for the boll weevil. One, insect pest management, aims to keep boll weevils at or below the economic threshold (the population density above which the returns from increased yields exceed the costs to suppress) without inducing secondary pests (*6*). Such schemes are now in use in some areas, particularly the lower Rio Grande Valley of Texas (*8*). A second strategy is to eradicate the insect from the United States (*9*). The U.S. Department of Agriculture (USDA) in conjunction with the cotton industry and the states of Virginia, North Carolina, and South Carolina launched a 3-year trial boll weevil eradication program (TBWEP) in 1978 (Fig. 2). Its objective is to determine whether technology is currently available to eradicate the boll weevil from the United States. A judgment that eradication technology is available could lead to the launching of a multimillion-dollar national eradication program. The technology being tested in TBWEP will be compared with an insect pest management strategy deployed in the optimum insect pest management trial (OIPMT) running concurrently in Panola County, Mississippi (*10*).

Difficult policy questions are raised by the simultaneous existence of two alternative and mutually exclusive control strategies: (i) What is the effectiveness of each? (ii) Does either require or deserve additional research? (iii) Are research needs for the two interchangeable? (iv) How should priorities be set on additional research needs? (v) If both strategies are successful, which (if either) should be implemented and how? (vi) Successful new control strategies for boll weevil might alter regional patterns of cotton production (*11*). What steps would be needed to alleviate possible socioeconomic distress?

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These policy questions indicate that evaluations of TBWEP and OIPMT will involve difficult biological, social, economic, political, and environmental issues. Similar factors confounded the evaluations of the pilot boll weevil eradication experiment (PBWEE) in 1973 and raise the possibility that the outcomes of TBWEP and OIPMT might also be subject to multiple interpretations. In this article, I review the history of the events leading to the eradication trials with an emphasis on how scientific and social factors have been related. The argument presupposes that the coming evaluations of TBWEP and OIPMT will benefit from a more thorough knowledge of the past. Discussions of biological and environmental factors important to the problem have been presented (*4*, *12*).

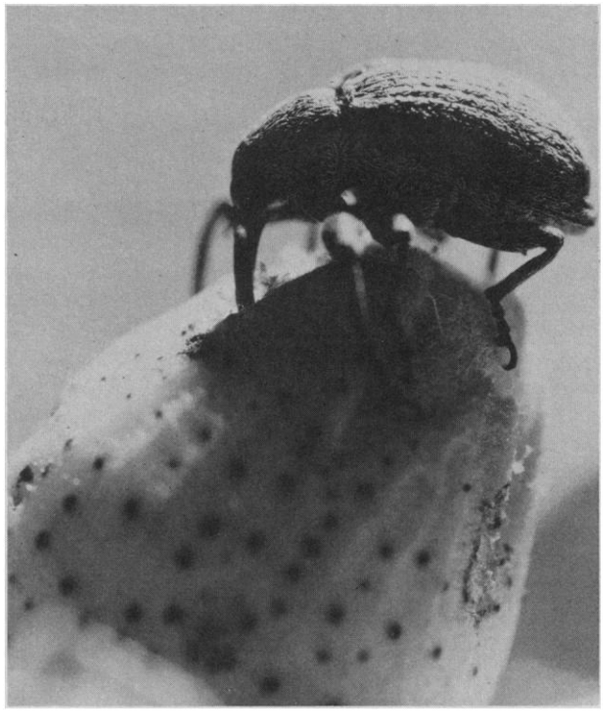

Fig. 1. Boll weevil (*Anthonomus grandis* Boheman) feeding on cotton. [Courtesy of U.S. Department of Agriculture, Animal and Plant Health Inspection Service]

Background of the Eradication Idea

Cotton producers faced a changing environment in the decade after World War II. First, the chlorinated hydrocarbon insecticides allowed the use of long season varieties with heavier fertilization and irrigation to produce more cotton; boll weevil reproduction was also favored, but the insects were controlled by the new insecticides (*13*, pp. 58–59; *14*). Resistance of the boll weevil to the chlorinated hydrocarbon insecticides was "solved" by the use of organophosphates, but growers and entomologists feared that resistance to organophosphates would render useless the new, profitable, production practices (*14*). Second, cotton was more widely grown as a cash crop in other parts of the world. Increased cotton supplies on the world market put severe pressure on cotton prices and profits of U.S. growers. Third, synthetic fibers were developed and widely used and thus put further competitive pressures on cotton profits (*15*).

A scientific and political movement to eradicate the boll weevil evolved from the 1958 annual convention of the National Cotton Council (NCC), the organization representing growers plus all other segments of the cotton industry. Robert R. Coker and J. F. McLaurin, both cotton producers from South Carolina, introduced a resolution declaring the boll weevil to be the number one enemy of cotton production. As J. Ritchie Smith, director of Technical Research Service at NCC later recalled, "It was [in 1958] that the National Cotton Council literally declared war on the boll weevil" (*16*). The NCC resolution led to an appropriation by the U.S. Congress for an inventory of boll weevil research efforts then under way (*17*).

Edward F. Knipling, then director of the Entomology Research Division, USDA, became the chairman of a study committee, The Working Group on Boll Weevil Research Programs. The group concluded in their report of 30 December 1958 that the future of conventional chemical control for boll weevils was seriously threatened, which posed the threat of disaster to the cotton industry. They recommended the establishment of a new interdisciplinary laboratory near Mississippi State University (*18*). Congress appropriated $1.1 million for its construction, and the Boll Weevil Research Laboratory (BWRL) was dedicated in 1962 (*19*, p. 95).

Knipling made clear at the laboratory's dedication that eradication, if feasible, was the goal: "Congress expects more than minor improvements. . . . Therefore, the objective of the research should be to find ways of reducing losses to a minimum or to eliminate the problem entirely. For my part, I feel that we should gear our thinking and direct our

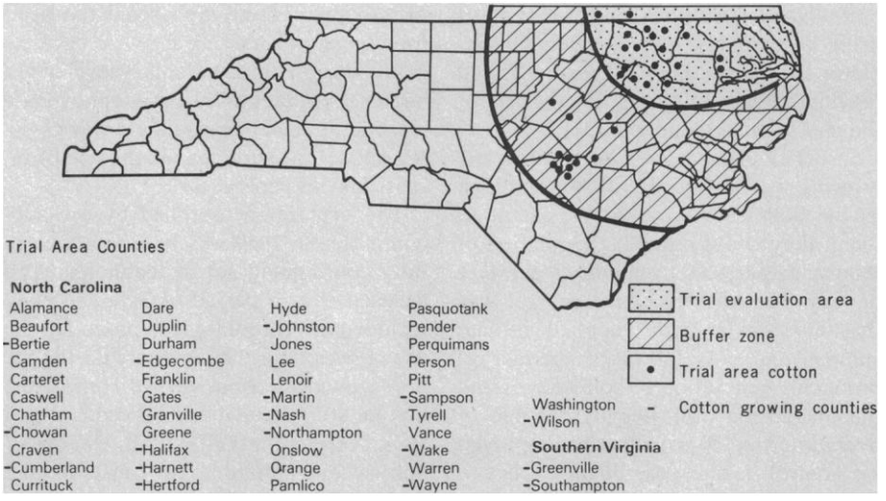

Fig. 2. Location of trial boll weevil eradication program, 1978 to 1980, in North Carolina and Virginia. "Trial evaluation area" is the zone in which experimenters hope to achieve eradication. "Buffer zone" is the area in which boll weevils will be carefully controlled so that migration of the insect into the eradication area will not confuse evaluation of the eradication technology. [Courtesy of U.S. Department of Agriculture, Animal and Plant Health Inspection Service]

research efforts to the development of practical ways of eradicating the insect'' (*19*, p. 2).

Upon Knipling's recommendation, Theodore B. Davich was appointed director of the BWRL, and they, together with the staff designed a number of research lines: basic ecology of the insect, pheromone attractants, host-plant resistance, improved methods of chemical control, basic physiology of the cotton plant, feeding stimulants and inhibitors for the boll weevil, methods of mass rearing and sterilization, and others (*20*). The projects on mass rearing and sterilization were of high importance to development of the eradication strategy because they were crucial to adoption of the sterile-male technique for boll weevils. Sterile males appeared to be highly useful for eradication efforts on the basis of their successes in eradicating screwworm flies from Curaçao and Florida and suppressing them in Texas (*21*). Knipling noted in 1963 that USDA financed research on sterile males by diverting funds from other avenues (*22*), and in 1972 such research constituted 25 percent of the public funds spent on boll weevil research (*4*, p. 23). Both Knipling and Davich felt that the diversion was justified on the basis of the potential for eradication or population management offered by the method.

The Pilot Eradication Experiment

Results of research at the BWRL and elsewhere led Knipling to conclude in early 1968 that technology had advanced sufficiently to justify a large-scale eradication experiment. The two most important advances were (i) a new method of using insecticides, the reproduction-diapause control method (r-d method), and (ii) improved possibilities for the use of the sterile-male technique (*23*).

Insecticides have been used during the growing season since the 1920's. As long as the boll weevil was not resistant to them, they could reduce a population of thousands per acre to hundreds per acre or less. The novelty of r-d control was that insecticides were applied in late summer and early fall to kill the last reproducing generation of boll weevils and the diapausing boll weevils capable of overwintering. A grower who employed r-d control in one year might expect to find weevil populations in the following season at a level of tens per acre or less. He might not even have to treat for weevils until late in the following season. Entomologists most involved with the research leading to the r-d method were James R. Brazzel, L. Dale Newsom, Davich, Knipling, Perry L. Adkisson, E. P. Lloyd, D. R. Rummel, and others (*24*). Knipling believed in 1968 that use of the r-d method might make it feasible to eradicate boll weevils with insecticides alone, but his recommendations for an experiment included additional suppressive technologies (*23*).

Davich and his colleagues tried five small-scale eradication trials utilizing sterile males alone or with insecticides in 1962, 1964, and 1967. They judged that only two of them demonstrated the suppressive powers of sterile males, and an introduced population of gravid females was eradicated in one area. Raising and sterilizing the weevils plus the migration of ''wild'' insects into the test areas frequently presented problems in the experiments (*25*). Despite the difficulties, Knipling, Davich, and others were encouraged, and other successes, such as the development of pheromones (*23*), reinforced the dedication of Knipling and others to conducting an eradication experiment.

The Technical Committee of the NCC prepared a resolution to establish a special study committee on boll weevil eradication that was approved in 1969 (*26*). Robert Coker was appointed to chair the committee consisting of growers plus entomologists Knipling, Brazzel, Davich, Adkisson, and David Young. In May 1969, Coker appointed Knipling as chairman of a subcommittee to select a site for the proposed eradication trial (*27*). Knipling led his group of six through a whirlwind of visits across the South in June. Their report, finished by August, recommended a location centered in Jefferson Davis and Covington counties, Mississippi. They emphasized the need for urgency in moving forward because of the danger that the boll weevil might become resistant to the organophosphate insecticides and because there was growing public pressure against the use of insecticides in general (*27*).

The experiment outlined by the subcommittee in 1969 was based on a carefully coordinated set of technologies to be used over a period of 3 years. They included in chronological order (i) the use of insecticides by growers during the first growing season, (ii) r-d control the first fall, (iii) defoliation, desiccation, and stalk destruction the first fall, (iv) use of boll weevil pheromone traps in the spring of the second year, (v) early season insecticide treatment the second year before boll weevil populations were actually damaging, (vi) release of sterile males the second year, and (vii) use where necessary of insecticides to control any detected outbreaks of boll weevils. Steps (ii) to (vii) would be repeated during the second and third years (*27*, *28*). The planned 3-year sequence ran for only 2 years because of a shortage of funds, a factor that contributed to the difficulty of evaluating the results.

At the initiation of the PBWEE in 1971, Coker simultaneously asked the special study committee to begin thinking about a systematic eradication effort across the cotton belt (*29*). Launching the experiment was thus combined with efforts to initiate a national eradication program.

Three expert committees evaluated the trial. First was the Technical Guidance Committee (TGC), a group cochaired by Knipling and Brazzel that oversaw the operations of the PBWEE. On 30 August 1973, they gathered in State College, Mississippi, for their last session. Knipling had prepared in advance of the meeting a draft statement declaring that the experiment demonstrated the technical feasibility of eradicating the boll weevil. The TGC discussed it at some length, and Perry L. Adkisson (Texas A & M University) emerged as the reluctant leader of those who hesitated to agree with Knipling's statement. Adkisson's position represented a change for him, because he had been optimistic about prospects for eradication until at least April (*30*, *31*).

The disagreement within the TGC must be understood on both biological and sociopolitical grounds. Two main biological problems faced the committee: (i) Were boll weevils detectable in the eradication zone? (ii) If yes, were they native or immigrants? If no boll weevils were detectable, then a prima facie case existed for eradication. If they were, then the argument became absolutely dependent on the demonstration either that they were immigrants or that they could not reproduce because of the sterile males.

Intense visual and machine surveys were the primary detection methods. A third method was the use of pheromone traps that underwent substantial development between 1971 and 1973 (*4*, pp. 34–40). During the last month of the experiment, visual and machine surveys detected no boll weevils in the southern two-thirds of the eradication zone, but the northern part was infested (*4*, pp. 75–81 and 108–112). Pheromone traps, however, provided detection of a few boll weevils in the southern part of the eradication zone during the last month (*4*, pp. 82–89; *30*). Furthermore, Adkisson received informal reports that the visual surveys might have missed some infesta-

tions. He considered the reports reliable, and they were a key factor influencing his conclusion that boll weevils had not been eradicated. The informal reports were not widely discussed at the time because of apprehension that careers could be damaged by pressing the issue (*30*).

Data indicating the presence of weevils thus forced the argument into one of migration and the capacity of boll weevils to reproduce in the eradication zone (*30*, *32*). Boll weevils can migrate at least 45 miles and possibly more (*33*). Because most cotton in the eradication zone was within 45 miles of infested cotton, it was not unreasonable to argue that boll weevils found in the area were immigrants. Migration data lent no support to the hypothesis that boll weevils present in the eradication zone during the last month were immigrants (*4*, pp. 103–107), but they could not refute the converse. Acknowledged imperfections in the sterile-male technique made it difficult, but not impossible, to argue that reproduction was fully blocked in the eradication zone (*4*, pp. 95–102 and 119–121; *30*, *34*).

Adkisson reasoned that (i) boll weevils of unknown origin were in the eradication zone and (ii) the sterile-male technique was not yet working for boll weevils, a prerequisite he considered essential for eradication. He therefore concluded that technology was not sufficient to eradicate the insect. Knipling, Brazzel, Davich, and others concluded that it was. Both judgments rested on the biological data, different sets of assumptions about migration and reproduction, and varying levels of commitment to eradication as a control strategy.

The sociopolitical factors thus became crucial to the matter. Key entomologists, including Adkisson, and the NCC had been preparing a plan for a national boll weevil eradication program for more than a year (*35*, *36*), and the NCC staff was predisposed to eradication at the time of the TGC meeting (*30*, *37*). Adkisson was unwilling to sign a statement declaring eradication feasible because he knew that the statement would be used to support launching an expensive program costing growers and government substantial sums. He believed that it would have been professionally unethical for him to tell cotton producers that technology was available to rid them of their boll weevil problems when he believed that it was not (*30*, *32*).

Adkisson's stance led him to suggest a compromise wording in which "eliminate as an economic pest" was substituted for "eradicate." He believed that his compromise language was good at the time because it allowed a committee that was clearly split to avoid a minority report. His sense that some other members of the TGC were in a difficult position politically led him to devise his compromise wording and thus avoid a traumatic fight over a split decision (*30*).

Ambiguity in the compromise language made subsequent policy-making contentious. Coker, J. Ritchie Smith, Knipling, and Brazzel simply went on behaving as though "eliminate as an economic pest" was entirely synonymous with "eradicate" as they launched a movement for a national "elimination" program (*38*, *39*). Adkisson used the language in an entirely different vein: He believed technology could eliminate economic damage by the insect, but any program based on it would be open-ended. It would have to be used year after year because it would not really reduce the boll weevil population to zero. He felt that as long as cotton growers understood the difference between open- and closed-ended programs, they were well served (*30*).

A second evaluation was performed by a committee of the Entomological Society of America (ESA), whose origins came from "certain [unspecified] members . . ." of the TGC (*40*). The ambiguity of the TGC report was only slightly alleviated by the ESA committee, which was sure that eradication meant reduction of a specified population in a particular area to zero and therefore "eradication" and "elimination as an economic pest" might be different. The committee could not, however, agree on whether there was any significant difference between "accomplishing eradication" and "demonstrating feasibility of eradication." Furthermore, the committee was divided over whether or not technical feasibility of eradication was demonstrated in the experiment. They expressed "reservations" about undertaking any massive program of eradication until further research had improved the techniques used in the PBWEE. Despite reservations, the committee explicitly made no judgment on whether a national eradication effort should be attempted. Rather, the committee said the matter was a sociopolitical decision that should be made "objectively" (*41*). Indecision was the hallmark of the ESA report and it suggests that something about eradication is conceptually troublesome to the entomological community.

The third evaluation of the PBWEE was performed at the National Academy of Sciences (NAS) by a committee chaired by Donald Kennedy (Stanford University) and for which I served as the principal staff officer. Kennedy's committee performed a general technology assessment of pest control methods, and eradication came into its purview as a concept of insect control. Evaluating boll weevil eradication fell primarily to a subgroup, the Cotton Study Team, chaired by Stanley D. Beck (University of Wisconsin); Adkisson was a member of both the parent body and the Cotton Study Team.

In late 1974, the draft of the Cotton Study Team's report expressed severe doubt about the technical feasibility of eradicating the boll weevil. The draft was subjected to the usual NAS peer review by a committee chaired by S. Hendricks (USDA); Knipling was a member. Knipling disagreed with the conclusions of the Cotton Study Team on eradication, but perhaps more importantly he was absolutely outraged by what he considered the heavily biased manner in which the conclusion was reached and presented. He considered Beck a longtime opponent of eradication efforts and that his appointment to chair an NAS committee was bound to lead to predetermined conclusions. Furthermore, he questioned whether Adkisson adhered to a consistent position on eradication (*36*).

Knipling believed that the NAS simply should not put out a statement that described only the problems of eradication, which he said he agreed with, without at the same time presenting the benefits to be gained should eradication succeed. A flurry of negotiations ensued among Knipling, Adkisson, Beck, Kennedy, and Philip Handler, president of the NAS, and resulted in a compromise by late 1975 (*30*, *36*, *42*). The revised Cotton Study Team report was released in early 1976. Its toned-down statement continued to express strong reservations about the feasibility of eradicating the weevil, but it went along with the eradication proponents to the extent that a trial program in North Carolina should be conducted before a final decision was made (*13*, pp. 4–5). The concept of continuing large-scale eradication trials was thus legitimized.

The Trial Eradication Program

The origin of a limited trial program on the eastern edge of the boll weevil belt came from extensive discussions held before and in the wake of the PBWEE. Coker appointed a committee to develop a plan for a national program in early 1972, more than a year before the end of the PBWEE (*35*). Knipling chaired the

group, which included Brazzel and Adkisson among its ten members. The committee recommended, on 4 December 1973, that a national "elimination" program be started in 1975 in western Texas and proceed eastward. All boll weevils were to be eliminated by the end of the eighth year (1982) (*39*, pp. 5, 6, and 57). Coker and others from the cotton industry met in Washington, D.C., on 12 December to discuss the subcommittee's report with Secretary of Agriculture Earl Butz, other USDA administrators, and a number of congressman (*43*).

Proponents of eradication, however, could not organize a sufficiently strong base of support for a national eradication program begun in Texas. The transition began during conferences on boll weevil research and elimination strategies held in February 1974. J. R. Phillips (University of Arkansas) presented a statement from himself and seven other southern entomologists who had severe doubts about the technology then available (*4*, p. 169). Charles Lincoln (University of Arkansas), Dale Newsom (Louisiana State University), and Dan Clower (also LSU) expressed similar opinions independently (*4*, pp. 149–153 and 172; *44*).

Fowden G. Maxwell, chairman of the entomology department at Mississippi State University and a member of the TGC, proposed a compromise in April 1974, for a limited trial eradication program that, if successful, could be expanded to a national program. He also recommended that a research team be established to evaluate the effort and recommend appropriate follow-up including options to abort, hold, or expand (*45*). Maxwell's proposal was quickly adopted by university and cotton industry people in Mississippi and by the directors of the southern agricultural experiment stations (*46*). Adkisson completed the transition by explaining to key Texas cotton growers why he felt that a national program was premature (*30*, *32*). His opposition was probably influential in the judgment made on 6 June 1974, by Assistant Secretary of Agriculture Robert W. Long, that a trial program should be held in North Carolina (*47*). In October, the limited program received sufficient endorsement to proceed from federal and state entomologists, regulatory personnel, and the cotton industry (*48*). NCC's original proposal for a national program had discussed and dismissed the proposition of beginning in Virginia and North Carolina (*39*, pp. 56–57). A complex of political and scientific considerations led to the adoption of a limited trial in the eastern United States.

Problems of coordinating legal and financial arrangements between the federal and state governments and cotton growers proved to be time-consuming. Mississippi congressman J. Whitten, chairman of the Agricultural Appropriations Subcommittee, raised a series of objections to the program between 1974 and September 1977. His committee included concerns over (i) the regulatory powers essential to the program amounted to undesirable federal land-use control (*49*), (ii) insufficient grower and state cooperation with the program (*50*, *51*, p. 458; *52*, pp. 346–347; *53*), and (iii) lack of technical feasibility of the eradication technology (*50*, *51*, pp. 453–456; *52*, p. 385; *53*). The delay, until October 1974, of agreement on the size and location of the trial precluded USDA's request for funds for the trial for the fiscal year 1976 budget (*54*, pp. 92–94 and 130–132); the Senate provided an appropriation of $3.5 million anyway, but acceded to Whitten's position for delay in conference (*50*, *55*). Secretary Butz arranged a meeting of cotton industry people, the governors of North and South Carolina, and high USDA officials with President Gerald Ford on 9 January 1976, and Ford allowed USDA to request $1.7 million for an eradication trial for fiscal year 1977 (*56*). The Senate in 1976 urged immediate implementation of the trial, but Whitten again prevailed in conference and refused to allow the expenditure of the $1.7 million until all states had passed and implemented legislation authorizing their participation, and the director of the BWRL certified a technical breakthrough that justified trying eradication again. Whitten in 1976 specifically cited the doubts raised by the recently released NAS study as justification for his skepticism on the adequacy of the technology (*57*). The stalemate lasted until 1977 when Whitten agreed to USDA's argument that the required developments had been achieved with (i) improved mass rearing techniques, and (ii) improved sterilization techniques (*58*). The TBWEP began in 1978.

The Policy Lessons

Two items of utmost importance for policy considerations are suggested by the examination of the historical development of TBWEP.

1) It is not yet demonstrated that we have adequate institutions for evaluating eradication experiments. Evaluations of PBWEE do not inspire confidence: the TGC released an evaluation that was used exactly as each individual on that committee wanted. The ESA committee was a bit more clear in that they expressed reservations about continuing eradication efforts without further research. They failed, however, because they did not explain what the important issues were in evaluating the eradication experiment. The Cotton Study Team's report was influential in the Congress; yet its recommendations, too, were not free of the deep splits in the entomological community. The report merely said that eradication probably would not work but that final judgment should be held in abeyance pending the results of the trial in North Carolina (*59*).

The underlying problem was that representatives of the cotton industry, working in collaboration with key federal and state entomologists, dominated the politics of boll weevil eradication from 1958 to 1973. Had the coalition had its way in 1973–1974, the United States would now, in 1980, be in the midst of a national program to eradicate the boll weevil based on technology that in 1973 did not inspire the confidence of the entomological profession as a whole.

Some entomologists who judged eradication infeasible felt that they had to express their opinions guardedly, but others spoke out openly and forcefully. Unwillingness to risk careers and alienate the political support of such organizations as the NCC were the causes of reticence among part of the entomological profession. It is my judgment that the commitment to eradication in 1973 by the NCC and certain key entomologists stifled what should have been an open and wide-ranging debate about the feasibility and desirability of an eradication program compared to its alternatives (*60*). Each of the policy studies on PBWEE was warped because of the intensity of feelings on the eradication issue. It is likely that the evaluations of the TBWEP will occur under pressures similar to that of the PBWEE. Failure to recognize and correct this situation will in all likelihood create further muddled studies.

The USDA has established an elaborate process for evaluating the TBWEP on biological, economic, and environmental grounds. Eradication will be explicitly compared with the alternative strategies—(i) optimum insect pest management and (ii) current insect control practices with insecticides. The broader scope of the proposed evaluation compared to that done for the PBWEE is clearly an improvement for which USDA should be commended. Moreover, the

USDA has asked the NAS also to perform an independent assessment of the trial programs and of the USDA evaluations (*61*). Whether the evaluation apparatus will be able to sustain open debate remains to be seen.

2) The eradication concept is conceptually troublesome to the entomological profession. I have argued (*62*) that two paradigms for guiding research in entomology developed after 1955. Both were intended to lead insect control practices away from heavy reliance upon chemicals. One paradigm is integrated pest management (IPM), which envisions the containment of insect populations below an economic threshold. IPM guided the design of the OIPMT. The second paradigm, which I have labeled total population management (TPM), envisions the suppression of total populations of insects over large areas with a variety of control techniques; for a few key species, one of which is the boll weevil, eradication is seen as the goal toward which entomologists ought to bend their research efforts. PBWEE and TBWEP are the most sophisticated experiments ever designed under the influence of the TPM paradigm.

Thomas Kuhn made the concept of paradigms highly influential in the study of how scientific change takes place. He asserts that scientists who adhere to different paradigms frequently have trouble communicating with each other because they are envisioning different problems, using language in different ways, and designing and interpreting experiments based on different a priori suppositions (*63*, pp. 320–339). This article is not the place for a thorough discussion on the use of Kuhn's ideas in the study of entomologists, but as a first approximation he has given us a conceptual framework for understanding the underlying dispute between proponents and opponents of boll weevil eradication. The opponents have been associated largely with the IPM paradigm, whereas the proponents have been associated with TPM. Because the two schools see the solution to insect problems in quite different ways (containment versus annihilation), they are inclined to judge scientific data by different criteria. Adherents of the TPM paradigm saw the dramatic decreases in boll weevil populations in the PBWEE experiment and concluded that the technology available, with refinement, could drive the population to zero. Adherents of the IPM school looked at the same data with a different set of suppositions and concluded that the boll weevil could not be eradicated.

Conclusion and Summary

The TBWEP grew from a close working relationship between particular entomologists and the cotton industry over a period of years. One previous experiment to eradicate the insect, PBWEE, engendered considerable controversy within the entomological community, and the evaluation of the new trial experiment may face similar difficulties. The USDA, the NAS, the scientific community, and the cotton industry must cooperate in a full and open debate during the evaluation period. It must also be recognized that difficulties of scientific communication will attend the comparison of an eradication strategy with a pest management strategy. The nation must avoid either launching or canceling a billion-dollar eradication program based on inadequate technical, environmental, and social evaluation. The story told here should provide some insights into what will be a difficult task.

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59. Although I was not deeply involved with the writing of the Cotton Study Team report, I accept, as a staff member for that effort, an equal share of the responsibility for any of the report's shortcomings. I believed at the time when the report was released that it was fully adequate as a policy study. I continue to believe that the biological reasoning underlying the report's skepticism on eradication is sound. The shortcomings became clear to me only after the passage of several years and lay in our inability at the time to comprehend the sociopolitical dimensions of the eradication movement. I continue to believe that the five-volume set from the Kennedy Committee is still one of the finest studies ever done on the problems associated with pest control.
60. This conclusion will undoubtedly be one of the most controversial of all judgments contained in this article. It is a judgment that is not subject to conclusive proof. During the course of my interviews and through correspondence, I have come upon at least six specific instances that might be interpreted as inhibitions of free expression or harassments of open debate. Some I received in confidence and cannot divulge names. In others, the evidence is tenuous, disputed, and probably unprovable. In no case has an "inhibited person" wanted to press the matter. Therefore I simply present my conclusion precisely for what it is: a considered opinion.
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64. This study would not have been possible without the generous cooperation of many individuals who consented to be interviewed, shared their unpublished papers with me, or commented on draft versions of this manuscript. I thank D. L. Dahlsten, C. B. Huffaker, P. Kenmore, W. H. Newell, B. B. Perkins, R. van den Bosch, and anonymous reviewers for criticisms of the manuscript. M. T. Sebrechts provided illustrations. Of those interviewed some may disagree with my account of boll weevil eradication, and I relieve them of any responsibility for my holding the views presented in this article. Some of the materials incorporated in this work were developed with the financial assistance of the National Science Foundation (SOC 76-11288) to Miami University. Some of the work was done through contract from Miami University to the University of California. The opinions, findings, conclusions, and recommendations expressed in this article are mine and do not necessarily reflect those of Miami University, the University of California, or the National Science Foundation. The hospitality of the Division of Biological Control of the University of California is acknowledged.