respond in a relatively calm manner. They are attracted over distances of up to 10 cm to the area where the display occurred and tend to settle there in loose clusters. The result is a change in the overall spatial pattern of *Oecophylla* workers in those portions of the territory through which the intruders move, from random or weakly clumped distributions to moderately or strongly clumped distributions (*5*). An example is given in Table 1. In laboratory trials clusters of workers were able to retain and subdue invaders in much shorter periods of time than were single defenders.

Because short-term recruitment involves touching the substratum with a portion of the abdomen anterior to the anus and acidopore, with no sign of an extrusion of the rectal gland, we searched for another pheromone source farther along the ventral abdominal surface. And indeed a promising new gland was discovered near the center of the last abdominal sternite. This structure, which we call the sternal gland, is illustrated in Fig. 2. When preparations of the gland (*4*) amounting to approximately one-gland equivalent each were placed in dummies of folded filter paper and presented to *Oecophylla* on their territories, the ants responded in a manner indistinguishable from that attending natural short-range recruitment. Similar preparations of poison glands and Dufour's gland induced aggressive attraction, which was not followed by clustering in the manner observed during short-range recruitment (*7*). When *Oecophylla* workers are picked off the territory and held lightly between the fingers or a pair of forceps, they rotate the terminal abdominal segment upward exposing the shining cuticular surface that covers the sternal gland. When released back onto the territory, they immediately perform the typical display of short-range recruitment. From these several lines of evidence we conclude that the sternal gland produces a pheromone that is released during the display and induces the workers to aggregate into small local clusters.

When foraging *Oecophylla* workers encounter intruders repeatedly at short intervals, some shift from short-range to long-range recruitment, laying rectal-gland odor trails back to the nest. The result is the gradual increase of worker density on the territory, also illustrated in the data of Table 1. This increase, enhanced by local clustering caused by continued short-range recruitment, results in a greatly increased rate of capture of the intruders.

The use of pheromones in the establishment and exploitation of colonial territories is only part of a remarkably complex system of recruitment behavior in *O. longinoda*. In fact, our current studies suggest that communication in this species may be the most elaborate yet to be recognized in ants as a whole (*8*).

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References and Notes

1. E. S. Brown, *Bull. Entomol. Res.* **50**, 97 (1959); D. Leston, *Annu. Rev. Entomol.* **15**, 273 (1970). *Oecophylla smaragdina* has been used in biological control of citrus pests since at least as far back as the 12th century; see, for example, the apparent account of this species by Chuang Ch'O, cited by Y. Shiba, *Commerce and Society in Sung China*, M. Elvin, transl. (Univ. of Michigan Press, Ann Arbor, 1970).
2. The *O. longinoda* colonies were collected from several localities in Kenya, mostly in the vicinity of Mombasa. Each colony was maintained on a tree surrounded by a moat of mineral oil and from time to time was given access by bridges to foraging areas 70 by 140 cm in extent.
3. The newly discovered rectal gland of *O. longinoda* is not to be confused with the rectal pads and rectal papillae that occur in ants and most other insects. These organs are entirely different in form and serve in the resorption of water.
4. Body parts from 10 to 30 major workers were placed in 0.5 ml of ether and chilled for an hour or longer; the number of parts was kept constant for a given replication. Artificial trails were drawn with hardwood applicator sticks for 40 cm over a paper surface, and the number of workers following for half this distance was recorded during four or more successive 5-minute intervals.
5. The spatial arrangement of *Oecophylla* workers was defined as either random or clumped by estimating, through the chi-square test taken to the 95 percent confidence level, the fit of their distribution to a Poisson distribution. In a series of more than ten such tests, with one or the other of four species of insects as invaders, the arrangement invariably shifted from random or weakly clumped to a more strongly clumped distribution (Table 1).
6. So far as we know, no homologs of the rectal gland and the sternal gland have yet been reported in other kinds of ants. S. M. Hammad [*Bull. Soc. Entomol. Egypte* **49**, 133 (1965)] reported a "scent gland" on the terminal sternite of the formicine *Cataglyphis bicolor*, but this structure consists of paired lateral clusters of cells evidently distinct from the *Oecophylla* gland. The sternal gland of *Oecophylla* is also different in location, structure, and function from the sternal gland of termites.
7. The mediation of aggressive alarm in *O. longinoda* by pheromones in the poison and Dufour's glands was previously reported by J. W. S. Bradshaw, R. Baker, and P. E. Howse [*Nature (London)* **258**, 230 (1975) and *Symposium of the International Union for the Study of Social Insects, Dijon* (1975), p. 61]. The same authors further discovered that a mixture of mandibular gland pheromones (hexenal, 1-hexanol, 2-butyl-2-octenal) induces aggressive alarm.
8. More detailed accounts of territorial and other forms of behavior involving recruitment in *O. longinoda* are being prepared for publication.
We thank H. Engel for making the histological preparations and other technical aid, T. Hölldobler for Fig. 2, and K. Horton and R. Sekulic for collecting the live colonies of *Oecophylla*. The research has been supported by NSF grants BMS 75-06447 and BNS 73-00889. This article is dedicated to the memory of Alfred E. Emerson, eminent student of the social insects, who died 3 October 1976, in his 80th year.

14 October 1976

The Biome Programs: Evaluating an Experiment

The article by Mitchell *et al.* (*1*) reporting the results of the Battelle, Columbus Laboratories, analysis of three of the five biome studies in the U.S. contribution to the International Biological Program (IBP) represents the first widely available evaluation of these integrated research programs. However, the article contains some ambiguities, limitations, and possible misconceptions which must be clarified in the light of its potential significance. First, the timing was inappropriate. Although the IBP officially terminated in June 1974 as an internationally coordinated scientific program, the National Science Foundation (NSF) has continued to fund certain biome programs for an additional 3-year synthesis period. Therefore, the (potentially) most important results were not available at the time of the study.

Second, the evaluation is based on incomplete data. The analysis does not consider almost 500 internal reports, takes no account of the interbiome and international aspects of the program, and attempts to evaluate management effectiveness based on limited access to information on the decision-making process.

The third limitation is that, for purposes of the Battelle study, a simplified view of the programs was adopted. This can be paraphrased as follows: the biome program planners intended to produce an ecosystem model; projects were chosen to provide data to the model; data were to be submitted in uniform format to a computerized data bank which would produce model parameters; the parameterized model would then be of sufficient accuracy to be immediately useful for resource management. The authors conclude that the Eastern Deciduous Forest Biome program (EDFB) failed to meet these objectives. This is not surprising, since EDFB never adopted such simplistic goals. Nowhere in the IBP biome planning statements or project plans does the explicit goal of the biome programs, *to analyze ecosystems*, appear.

Further, we cannot agree that technical problems of mathematics or computer programming were limiting factors. Modelers now have the skills to translate biological understanding into the shorthand of mathematics, but the science of ecology simply does not possess a sufficient understanding of processes at the ecosystem level. The real heart of the matter lies in our understanding of the

interactions between functional units in the system, while the challenge lies in elucidating relationships which are seldom studied by individual disciplinarians. The problem is a scientific one, not a technical one.

Mitchell *et al.* state that automated data banks were intended to be a primary integration tool. This simply was not true in the EDFB. Since we did not have a single ecosystem model for the program, there was no clear way to define how hundreds of data sets could be stored in any optimal format for retrieval and analysis. Information specialists from a number of major centers (including Harvard University and Oak Ridge National Laboratory) indicated to us that this type of problem was without precedent and that we would be exploring new ground, rather than performing a routine service. As a result, we designed our data bank for two purposes: first, to serve as an archive for data so that the investment in research would not be totally lost when an investigator left the program; and second, to form an internal experimental unit, attempting to gain experience in the types of data analysis that would be requested so that through time a numerical data base could be designed on sound principles. The data banks never reached the level of success we had hoped for, but we never misconstrued them as a major integration tool. They were a research effort.

One of the important misconceptions presented by Mitchell *et al.* is the evaluation of cost efficiency and size of the program. Comparing the biomes with the Hubbard Brook study, the authors conclude that the achievement of program objectives was a function of the size of the program and that an optimal size is approximately $0.5 million annually (that is, the size of the Hubbard Brook study and roughly equivalent to the individual research sites in the EDFB). We cannot agree. The size of a program is determined by the scope of the objectives. This can be illustrated by an analysis of the open literature from the EDFB, the Hubbard Brook study, and volume 56 of *Ecology*.

The majority of *Ecology* articles deal with a single variable and are outputs from single-investigator projects. The Hubbard Brook program emphasized abiotic-producer interactions in mineral cycling, and hence the majority of their papers fall into the two-variable category. The EDFB also placed considerable emphasis on interactions between two variables (process studies). These studies were centered at our site programs and Mitchell *et al.* accurately conclude that this level of research can be acomplished by programs of this size. However 25 percent of our papers (57 papers of 226 which contained discussion of state variables) deal with three- or four-state variables and the interactions between them. And it is this level of resolution which was unique to the objectives of the biome programs. To achieve this larger objective, larger teams of researchers are needed. We conclude that the size of the program is a function of the ecological objectives being addressed and the scale of resolution at which the ecosystem is viewed.

It also seems necessary to make some comments about the "bête noir" of the article, biome management. Originally, Battelle intended a suite of analyses based on interviews, revisits, objective trees, and the input of management analysts. These procedures were to lead to in-depth evaluation of the methodologies utilized and suggestions about which mechanisms should be encouraged or discouraged in future studies. However, Battelle was not able to perform these analyses. As a result, the authors' conclusions are based on loose impressions formed from conversations and interviews. These have led to some misconceptions which require clarification.

Mitchell *et al.* are wrong in assuming that management was not responsible for the scope of studies, emphasis on transfer studies, temporal trends toward greater synthesis, direction of modeling toward more profitable scientific goals, or feelings of optimism by our participants, and then concluding, "There is no evidence that management did much more toward integration than select a set of projects that seemed appropriate to the general problems described in the proposals." These judgements reflect a general naïveté toward the complexity of management in a multimillion-dollar research program.

The biomes involved the creation and operation of large-scale integrated research teams in ecology, almost de novo and without previous experience. Within this environment, the programs experimented with innovative approaches such as interdepartmental and interdisciplinary teams, new management strategies, synthesis, models, data banks, internal reports, newsletters, and information meetings. And each of the biome programs was innovative in different areas. Of course, some mechanisms worked better than others. Some fell flat. This is exactly what was anticipated, and the internal evolution of the programs reflects the experimental nature of the management.

The authors state that it is difficult to tell what happened between the time of the proposal and the actual funding of individual subprojects. As evident from the scope and coverage of the subprojects that were actually funded in each case, this was a period of intense management and leadership activity, involving review by and negotiation with NSF as well as extensive internal judgment and review. The process seemed to appear autocratic to investigators who did not receive funding and democratic to those that were approved. But the review and selection process was a critical management input to the programs which merits further examination, evaluation, and recommendation.

The costs of management and coordination were larger than would be experienced in individual proposals, but the magnitudes are smaller than those deduced by the authors from their examination of only summary budgets in the proposals. EDFB management consisted of a biome director (at no cost to NSF), a deputy biome director, a modeling coordinator (later scientific director), and an executive committee composed of these three plus five site coordinators. Only the deputy biome director spent more than 50 percent of his time on management and administration. The others spent the majority of their time as researchers. This is clearly indicated by the fact that 43 percent of our open literature publications are authored or coauthored by this group of individuals. Indeed, "management" (as perceived by Battelle) constituted our most productive group of "researchers." To split the biome program budgets into 60 percent for field studies and 40 percent for modeling, synthesis, and management is like analyzing an individual university grant with a professor and graduate student and relegating the professor's salary and all expenses to management and only graduate student expense to research, since only the latter will be gathering the actual field data.

We consider that approximately 10 to 15 percent of the budgets were actually expended on coordination and management. While this figure is higher than individual investigator grants from NSF, it must be remembered that the existence of the biome program management took considerable burden away from the NSF management of grants. A report of an NSF task group makes this judgment (*2*, p. 8): ". . . it is the opinion of this study group that no significant difference exists

in the 'cumulative administrative costs' between the multi-investigator grant and the 'principal investigator' grant when both the Foundation and the university are considered *together*."

The integrated research programs of the IBP were an experiment. It seems that little is to be gained at this point by debating whether they were a good experiment or a bad experiment. The complexity of the programs defies a simple yes/no answer, and the conclusions reached by individuals examining the data are likely to be correlated with personal experiences and individual preferences. The important observation is that attainment of certain scientific objectives dealing with understanding ecosystems do require large programs. None of the reviews and evaluations have suggested that programs larger than $0.5 million should never be undertaken. Some problems will necessitate programs of this size. Both the mistakes that were made and the mechanisms that tended to work well need to be identified, discussed, and further evaluated.

A detailed analysis of the management successes and failures of the biome programs is still needed. The management structures were a significant part of the IBP experiment, and the evidence may soon be lost as management staff and leaders are siphoned off to other assignments. We feel the scientific community should set about the important task of making concrete suggestions on the management mechanisms to be encouraged in future studies of a similar integrated nature. Certainly EDFB (and the other biome program planners) made mistakes, and some negative conclusions are quite correct. But the reasons for failure or success are *not* dealt with by Mitchell *et al.* and still critically need analysis.

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1. R. Mitchell, R. A. Mayer, J. Downhower, *Science* **192**, 859 (1976).
2. *Larger but Fewer Research Grants* (report of Task Group No. 13 to the Research Advisory Committee, National Science Foundation, Washington, D.C., 1975).

15 July 1976

A reply to the preceding comment is incorporated in the reply by Downhower and Mayer to letters from Gibson and Blair in this issue (Letters, page 822).

Is Delta-9-Tetrahydrocannabinol Estrogenic?

Several authors have reported that various cannabis preparations demasculinize male rodents (*1–4*), depress plasma testosterone levels in men (*5*), and may lead to gynecomastia (breast growth in males) (*6*). Solomon *et al.* (*7*) claim that delta-9-tetrahydrocannabinol (Δ^9-THC) has estrogenic properties; this would be an attractive and simple explanation for the mechanism by which cannabinoids demasculinize or feminize (or both). Unfortunately, the contention that Δ^9-THC has estrogen-like properties is not supported by the bulk of experimental evidence.

Prior to the submission of their report by Solomon *et al.*, our laboratory reported that cannabis (in high doses) demasculinizes male rats but is *not* estrogenic (*2*). In our experiments oral administration of cannabis resin (54 percent Δ^9-THC, 19 percent cannabidiol, 14 percent cannabinol) at doses between 0 and 250 parts per million in the diet caused linear dose-related *decreases* in uterine weight, both in ovariectomized adult female rats and in prepubertal female rats. Uterine weight was decreased even when corrected for the severe loss in body weight which occurred at higher doses. Decreased uterine weight in nonovariectomized adult female rats given chronic oral Δ^9-THC has been reported by Thompson *et al.* (*3*). In preliminary experiments we administered cannabis resin by intraperitoneal injection (as done by Solomon *et al.*); the results were erratic and unreliable, since intraperitoneal injection of large doses of Δ^9-THC leads to inflammation of abdominal organs. Intraperitoneal injection is not a suitable mode of administration for studying the possible uterotrophic effects of Δ^9-THC.

Although all three Δ^9-THC doses used by Solomon *et al.* "had [statistically] significant uterotrophic effects," the data lack one important characteristic necessary for a satisfactory estrogen bioassay; that is, uterine weight did not increase

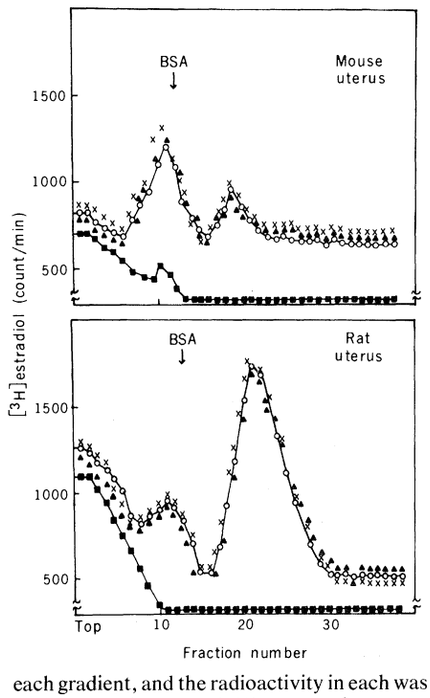

Fig. 1. Lack of effect of delta-9-tetrahydrocannabinol or cannabis resin on [^{3}H]estradiol-17β binding to uterine cytosol receptor. Uteri from sexually mature C3H mice (top panel) or 21-day-old Wistar-strain rats (bottom panel) were homogenized in three times their volume of TED buffer (0.01*M* tris-HCl, 0.015*M* ethylenediaminetetraacetic acid, 0.001*M* dithiothreitol; *p*H 7.4). All procedures were done at 0° at 4°C. The homogenates were centrifuged at 4000*g* for 15 minutes, then the supernatant was removed and centrifuged at 105,000*g* for 1.5 hours to obtain a supernatant (cytosol) fraction. Cytosol (about 3.5 mg of protein per milliliter) was incubated overnight with $2 \times 10^{-9}M$ [^{3}H]estradiol-17β in the presence or absence of potential competitors for estrogen receptor sites. After incubation the samples were mixed with an equal volume of dextran-coated charcoal (0.5 percent charcoal, 0.05 percent dextran, in TED buffer) to remove unbound steroid. Charcoal was sedimented by centrifugation at 4000*g* for 15 minutes; then samples (0.5 ml) of the supernatants were layered onto linear sucrose gradients (5 to 20 percent) prepared in TED buffer. Gradients were centrifuged at 40,000 rev/min for 22 hours in a Beckman SW.41 rotor at 4°C. Forty 300-μl fractions were collected from each gradient, and the radioactivity in each was measured by liquid scintillation counting. Bovine serum albumin (*BSA*; 4.6*S*) was used as a marker to determine approximate sedimentation coefficients of the radioactive peaks. Sedimentation peaks for the two samples illustrated correspond to previous reports (*11*) that uterine cytosol receptor sediments predominantly at the "8*S*" in immature rats (bottom panel) and nearer "4*S*" in sexually mature animals (top panel). Competitors were added in 10 μl of ethanol; an equal quantity of ethanol was added to the control tubes. (○) Binding in control (no competitor); (■) extinction of binding by incubation in the presence of $10^{-7}M$ diethylstilbestrol, a potent synthetic estrogen. Binding to either "4*S*" or "8*S*" receptor is unaltered by incubation in the presence of $10^{-5}M$ THC (x) (95.6 percent pure, NIMH lot No. SC 75518) or by the presence of cannabis resin (54 percent THC; 19 percent cannabidiol; 14 percent cannabinol) equivalent to $10^{-5}M$ Δ^9-THC (▲). Cytosol also was incubated with $10^{-3}M$ Δ^9-THC (data not shown); when that concentration was added to cytosol, a visible flocculence occurred which clarified during overnight shaking. Binding peaks in these samples were equivalent to the control.