mate of the total number of sighted birds with larger than average testes in all conditions, and so forth. In the 12 photoperiod treatments in which normal birds had heavier testes than blinded birds, there were 114 normal birds and 97 blinded birds. In the six conditions in which the blinded birds had larger testes, there were 48 blinded and 52 normal birds. This categorization generates Table 1.

When the population is dichotomized this way and the reliability of these differences in the numbers of blinded and sighted individuals in groups with larger or smaller average testes is evaluated by χ^2, then $\chi^2 = 37.826$; $P < .001$. As I noted above, a foolproof statistical evaluation would have to be based on Underwood and Menaker's raw data, but the present analysis strongly supports the conclusion that the retina is involved in the photoperiod response of house sparrows.

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Lott states that failure to reject the null hypothesis in our experiments does not mean that there is no difference between blind and normal birds but only that there is some *chance* that there is no difference. Clearly, failure to reject the null hypothesis does not prove that there is no difference but simply that no difference could be shown within the resolution of the experiment [see any of several discussions of power of tests of significance, for example, in (*1*) or (*2*)]. This is precisely why we concluded, "Our data offer no support for the hypothesis that the retina is involved in this response." The reader should be convinced by this argument only to the degree that he feels our experimental design and statistical analysis maximizes the chance of observing retinal involvement should it exist.

Lott claims that an alternative conclusion can be drawn from our data by use of a χ^2 test (Lott, table 1). However, in the construction of this table he ignores the fact that some birds which he places in the "blind-smaller" category had testes that were as large or larger than the testes of the normal birds in those samples. Lott places all blind birds of a particular sample in this category simply on the basis that their average testis weight is less. The same criticism applies to the way in which he forms the remaining three categories. A specific example will suffice to show that Lott's reanalysis of our data is inappropriate. The individual testis weights in one of the 12 samples in which the normal birds had the larger average testis weight are shown in Table 1. Lott (see his table 1) assigned the seven normal birds in this sample to the "normal-larger" category and the seven blind birds to the "blind-smaller" category simply because the average testis weight of the blind birds (185 mg) is less than the average testis weight of the normal birds (197 mg). It is quite clear, however, that many of the blind birds in this sample had testes as large or larger than the testes of the normal birds. A valid χ^2 could be performed on our data, for example, by using the median testis weight of all 311 birds in the 18 samples as the dividing line between "larger" and "smaller." The resulting χ^2 is not significant; $\chi^2 = 2.68$, $.10 < P < .25$ (Table 2).

It is true that there are other statistics one could apply, such as combined probabilities from tests of significance or signed rank tests of the differences between the means, in an attempt to get an overall view of the significance of the data. Neither of these tests show significant differences between the blind and normal birds at the 5 percent level, but neither these nor any other statistics with which we are familiar are completely adequate to test the overall significance of data drawn from a population that is changing with time. Accordingly, we employed a straightforward statistic, Student's *t*, to test for differences between blind and normal birds in each sample, and published the data in extenso. We see no reason to alter our conclusion that an extraretinal photoreceptor exists in the sparrow which is fully capable of mediating the gonadal response to photoperiodic stimuli.

Table 1. Individual testis weights from the birds in experiment C, day 26 (*3*).

Blind birds (mg)	Normal birds (mg)
5	12
24	102
35	142
226	197
308	212
308	276
391	436
Average 185	Average 197

Table 2. Number of blinded and sighted birds placed in larger or smaller categories according to whether or not their testis weights were greater or smaller than the median testis weight of all birds.

Category	Birds	
	Blind	Normal
Larger	65	91
Smaller	80	75

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1 June 1970

Controlled Fusion: Plasma Confinement with Lasers

Holcomb (*1*) indicates that the effort to produce controlled fusion is being aided by developments in laser technology. However, there is one laser technique that has apparently been overlooked by him and other workers in the field of nuclear fusion. Theory indicates that a circularly polarized laser beam in a plasma can create a strong magnetic field along the light path. Sufficiently strong fields would be extremely valuable in extending inertial confinement times, or in supplementing external magnetic fields used for long-term confinement.

The size of the magnetic field can be estimated by using the classical linear equations (*2*)

$$B = (e/2mc^2)(\omega_p^2/\omega^3)E_0^2 \quad (1)$$

$$\omega_p^2 = e^2 n/\epsilon_0 m \quad (2)$$

where m and e are the mass and charge of the electron; ω_p and ω, the plasma and laser frequencies in radians per second; E_0, the electric field in the light beam; and n, the free electron density. A field of over a megagauss results from a 2000-joule, 100-psec pulse of 1 μ wavelength light focused on a 50 μ^2 area if there are 10^{21} electron/

cm³. The high electron density was chosen to represent the expanding target just as it begins to transmit rather than to reflect the 1 μ radiation. It should be noted that the reflected light will not detract from the magnetic field created by the primary beam. In fact, the field is doubled when the beam is reflected back on itself and circular polarization is maintained.

The foregoing calculation is admittedly crude, in that nonlinear effects or the field effects on electron orbits have not been taken into account. A self-consistent field calculation would be required for the latter. In any case, Eqs. 1 and 2 indicated that laser-produced magnetic fields are favored by high electron densities, and by long wavelength light as well as high light intensity. Resonance effects could enhance the field, but no laser now in use will resonate with hydrogen isotope fusion plasmas.

Polarized light is an extremely versatile source of shaped magnetic fields. If the circular polarity varies in handedness from one quadrant to the next in the cross section of a laser beam, a cusped field will be produced. If the beam diverges, the field will decrease along the beam and form a magnetic mirror. Thus, by splitting a beam and causing the two halves to meet each other on the same axis after divergence, a biconical cusped field can be produced. Since the polarity of the light can be rapidly changed (for example, megahertz modulation by electrooptic light modulators), the magnetic field can also be modulated.

It should be mentioned that ordinary, circularly polarized light will produce a magnetic field of the same strength as laser light of equal intensity. But no ordinary light sources available today are able to concentrate the light in time and space to the extent that lasers do.

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Acanthaster: A Rarity in the Past?

In "Acanthaster: A Disaster?" (*1*, *2*), two points of view were expressed. One questioned whether this starfish had been a great rarity before the observed outbreaks in 1962 and 1968 and suggested that outbreaks may have been occurring sporadically in times past on different islands without attracting attention (*1*). The other stated categorically that there was no evidence for earlier periods of abundance and that the reference cited in support of the first point of view was irrelevant (*2*).

The principal evidence offered for high population densities in the past was a comment made by the noted naturalist C. H. Edmondson (*3*). In the introduction to his book on the reef and shore fauna of Hawaii, Edmondson states: "That serious-minded investigators might know something of . . . the scarcity or abundance, and the relative accessibility . . . of marine animals available for purposes of research about the shores of Hawaii, has also been an important consideration." And then for *Acanthaster planci* he said, while not "common" in Hawaiian waters, it was "very common" (1933 edition) and "abundant" (1946 edition) about Christmas Island (Pacific Ocean) in 2 or 3 fathoms of water. Even without his introductory remarks, it seems unreasonable to infer that Edmondson could have meant that he searched the reef at Christmas Island and found only isolated clumps of a few individuals, much less just four or five specimens in one spot. To suggest that an experienced naturalist would consider an organism abundant on such a basis is incredible.

I have come upon several other remarks made by investigators decades ago that also indicate *Acanthaster* had been abundant locally. Thus it seems that the historical rarity of the starfish has been greatly overstated, and the possibility of populations having occurred sporadically but naturally in epidemic proportions on widely scattered reefs has been too summarily dismissed.

In the Philippines, Domantay and Roxas (*4*) studied the sea stars of Port Galera Bay and Sabang Cove every summer between 1924 and 1938; they observed that *Acanthaster* was "common among the corals and rocks." It has been argued (*5*) that if *Acanthaster* has been always going through cyclic or sporadic fluctuations in abundance, surely the Japanese would have noticed it during their relatively brief but intensive shallow-water studies in the Palaus before World War II. The fact is they did. Hayashi (*6*) reported the species as "very common" on rocky and sandy substrata in the Arakabesan, but rare in the Arappu region, where he collected many examples in 1934. And further, the noted Danish echinoderm specialist and field biologist, Th. Mortensen, in his report on the development and larval forms of echinoderms (*7*), stated that *A. planci* "was found rather commonly on the coral reef at the little island Haarlem off Batavia, near Onrust, crawling over the top of the madreporarian corals on which it feeds, sucking off all the soft substance, leaving the white skeleton of the corals to show where it has been at work."

Clearly then, population densities of *Acanthaster* varied widely in the past, without undue importance being attached to periods of abundance. The question now is whether the situation is any different. If not, and reefs are as adapted to such catastrophic events as are certain terrestrial communities to fire (*8*), more harm than good could result from indiscriminate use of control measures. If the situation is significantly different, and the activities of man are actually perturbing the environment in certain reef situations so as to precipitate the apparent epidemics, we should find out what the factors are so that they can be intelligently regulated. Even if *Acanthaster* epidemics are not an entirely new phenomenon, the possibility exists that human disturbances are increasing their frequency by generating epidemics in areas where they might not have occurred naturally in the foreseeable future. To resolve the problem will require intensive field and laboratory research.

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