

(4). During the REM period, distinctive spike-like electrical discharges occur in the nucleus reticularis pontis caudalis and appear to spread to various parts of the visual system (5, 6). The close temporal association between periods of masseter contractions and REM periods could be explained on the basis of the anatomical proximity (within the brain stem) of the motor nucleus of the trigeminal nerve, which is directly responsible for the contractions of the masticatory muscles, and the nucleus reticularis pontis caudalis. Our preliminary findings thus hold promise for the hypothesis, suggested by Michel *et al.* (6), that the spread of activity from the pontine reticular formation may not be confined to the visual system.

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"Male-Producing Factor" in the Wasp *Dahlbominus*

The discovery of an "inherited male-producing factor" in a hymenopterous insect that produces its males from unfertilized eggs was recently reported by A. Wilkes (1). This factor was not identified but was said to be of genetic origin and sex-limited. The genetic situation was anomalous, the influence of the inherited male-producing factor being superimposed on the inherited derivation of all normal males from haploid eggs.

As Wilkes observed, the effect of this factor on the sex ratios of the species studied, *Dahlbominus fuliginosus* (Nees)—an ectoparasite of co-

cooned larvae of *Diprion* spp.—was "constant and not influenced by the female parent, the host, or the environment." He demonstrated that the effect can be increased and presumably decreased by selection and is independent of the agents previously considered to be responsible for altering the sex ratio of arrhenotokous animals. He concluded that the inherited male-producing factor might account for the remarkable variability of the sex ratio that characterizes many species of Hymenoptera, both in the laboratory and in the field.

Wilkes made the point that the inherited male-producing factor and its effect on sex ratio were contrary to Flanders's understanding of the subject. Such is not the case. Wilkes's presentation of the situation seems based on a misinterpretation of my beliefs concerning sex control in the Hymenoptera (2). Twenty-five years ago, after studying in detail the reproduction of various types of Hymenoptera (3), I concluded that in the species that reproduce biparentally the sex of the individual is a function of the environment—that the environment operates through the female's spermatheca (a mechanism specialized for bringing the sperm and the egg into contact) and that in the Hymenoptera the spermatheca functions as a sex-changing mechanism when stored with sperm and environmentally stimulated.

The essential features in the process of changing the sex of the egg, the derivation of normal males being from haploid eggs and of normal females from diploid eggs, are (i) the quiescence of the sperm stored in the spermatheca (except when activated by secretions from the spermathecal gland), and (ii) the secretion by the gland of sperm-activating fluids only in response to stimuli associated with the act of oviposition (4). The facultative nature of this second feature and the variability in the responsiveness of the spermathecal gland exhibited by individual females appear to be the basis for the sex-ratio variability in the Hymenoptera. Mated females with spermatheca filled with viable sperm may never fertilize their eggs. I found this to be the case with several females of *Macrocentrus* that had deposited their full complement of eggs (5). Oviposition was stimulated but not secretion by the spermathecal gland, the essential factor in egg fertilization.

The males from which the unused

sperm were derived would be classified as "sterile" according to Wilkes's use of the term. However, it is significant that all of the *Dahlbominus* males, sterile or not, examined by Wilkes were alike in the number and motility of their sperm.

Wilkes in his selections of low-female (SR) lines simply isolated the females characterized by a low glandular response to environmental stimuli, a response that precisely fits the effects characterizing Wilkes's male-producing factor. The "action" of this factor, being negative (a lack of spermathecal stimulation), is of course clearly independent of the agents considered to be responsible for altering the sex ratio of arrhenotokous animals, that is, for changing the sex of the egg from male to female.

Wilkes's failure to fully identify his male-producing factor may stem from a misunderstanding of the phenomena involved in the reproduction of the Hymenoptera. This is indicated by a misinterpretation of certain concepts derived from my 1946 paper (2). Thus he says, "Unlike what Flanders believes to be the situation in arrhenotokous species, females of *D. fuliginosus* cannot control fertilization of their eggs. . . ." My belief, based on quite adequate data, was and is that the environment acting through the female's spermatheca controls the fertilization of the egg. Again he says, "From the records of adult emergence, the sperm taking part in fertilization appears to have been random, and not selective as has been suggested for this as well as other arrhenotokous species by [Flanders]." To my knowledge, no one has suggested that in the Hymenoptera the sperms taking part in fertilization were selected on other than a quantitative basis. The structure of the chalcidoid spermatheca permits numerical selection, such as one sperm at a time, from the number present therein (3). It is noteworthy in this connection that the sperm nucleus as well as the egg nucleus when developing separately are male producing (6).

Wilkes's observations do not preclude the possibility that his male-producing factor is merely an inherent low responsiveness of the spermathecal gland to environmental stimuli associated with oviposition. Wilkes could determine the validity of this conclusion by propagating his high-female lines on several host populations (*Diprion* spp.) that differ either in kind or in size.

The cocoons of *Diprion hercyniae* (Hartig), for example, exhibit a disparity of 1 to 2 mm in length (7). As with other parasitic Hymenoptera of similar habit, the female may generally deposit haploid eggs on small hosts and diploid eggs on large hosts (3).

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Flanders's major criticism of the report (1) appears to be that my analysis of the situation in *Dahlbominus fuliginosus* is based on a misinterpretation of his belief that *facultative* variability in responses of the spermathecal gland in freeing sperm from the sperm capsule is the basis for sex-ratio variability in Hymenoptera. The fact is that in the hymenopter *D. fuliginosus* the production of male- and female-producing eggs during oviposition is not influenced by the environment or by the host (2). It is clearly established that the initial sex ratio of *D. fuliginosus* is not affected by the size of the host. Large hosts differing in volume by 37 percent and in weight by 41 percent produce only 2.7 percent more females than smaller hosts, and the reduction in number of males was shown to be due to differential mortality. The stimuli that result in the release of sperm from the female sperm capsule of *D. fuliginosus* are no doubt elicited by passage of the egg along the oviduct, as in *Nasonia* (*Mormoniella*) *vitripennis* (3), and not through contact of the antennae, legs, or ovipositor of the parasite with its host. In any event, the hosts used in all tests in the study criticised by Flanders were

of the same species, and of the same size and age (1).

The suggestion that selection "simply isolated the females characterized by a low glandular response to environmental stimuli" is difficult to reconcile with the facts reported. If this were so, why do the selected, high-male-producing females when known to be inseminated by wild, low-male-producing males always produce low-male progeny? Flanders's interpretation would require the spermathecal gland to alter its response according to the type of sperm to be released when SR and wild sperm are both present in the one sperm capsule. As indicated in my report (1), the proportion of the two types of marked sperm taking part in fertilization under such circumstances was random and could not have been influenced by the host or the environment. That discontinuity in the release of sperm is the regulating mechanism for male production by inseminated *Dahlbominus fuliginosus* females, as suggested by Flanders, appears to be unlikely indeed. It is much more likely to be due to some form of genetically regulated dimorphism of the sperm (4).

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Radon-222 in Mine Atmospheres

We have measured in several mines what we believe to be barometrically induced changes in the flux into mines of radon-222 (Rn), a noble gas introduced into the interstices of rock surrounding the mine by the decay of radium-226 contained in the rock. A falling barometer is regularly associated with an increased flux of Rn into the mine; a rising barometer is associated

with a decrease in Rn flux. This is consistent with the report by Raymond F. Boyer—"Coal mine disasters: frequency by month," *Science* **144**, 1447 (1964)—citing the increased frequency of major mine disasters during periods of barometric minima.

The effect is perhaps a combination of convection and diffusion. A change in atmospheric pressure is transmitted more rapidly into the mine atmosphere than into the interstices of adjacent rock. If pressure has decreased, pressure in the mine will be less than pressure in the rock interstices, and a convective flow of interstitial gas into the mine will be induced. If pressure has increased, pressure in the mine will be initially greater than pressure in the adjacent rock interstices. A convective flow into the adjacent rock of mine air will occur and will dilute the interstitial gases in the surface rocks and hence reduce the Rn concentration gradient in the shallow rocks. Such a reduction will cause a decrease in the diffusion-driven flux of interstitial Rn into the mine.

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Telemetry of Dolphins

Mackay's excellent report [*Science* **144**, 864 (1964)] on radio telemetry of physiological variables of an untethered dolphin prompts me to call attention to an earlier physiological measurement made on a free-swimming porpoise by other means [C. E. Hendrix, *U.S. Naval Ordnance Test Station Tech. Note* 304-50 (1960)]. The purpose of this particular measurement was to determine skin temperature, but the principle appears applicable to other slowly changing variables for which adequate transducers exist.

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