It is unlikely that the previously reported effect of propranolol at low temperature[3–7] was due to inadequate diffusion of the drug. Benfey found that atropine produced the same block at low and high temperatures after 10-min incubation. Hypothyroidism also reduced the inhibitory effect of propranolol, although all tests were at the same temperature and after 30–40-min incubation[8–10].

In conclusion, Benfey's data seem to be inadequate to support his contention that the hypothesis of adrenoceptor transformation is an artefact attributable to nonspecific alkylation by POB.

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BENFEY REPLIES—The above comment[1] on my letter[2] misses the point. The real issue is not whether a change of temperature alters the rate of alkylation by phenoxybenzamine (although it obviously does), it is not how much phenoxybenzamine is required to block α-adrenoceptors (although Kunos *et al.*[3] loaded the tissue by four 10-min exposures to 2.5μg ml^{-1} of the highly lipid-soluble drug), and it has nothing to do with hypothyroid rats (although it may be noted that mammals have cardiac α-adrenoceptors in normal conditions at normal temperature[4]). The issue is whether lowering temperature transforms the adrenoceptor of the frog heart from its characteristic β-type into an α-receptor. If this were so, one might reasonably expect that phenylephrine would be more potent at a low than at a high temperature, that the reverse would be true for isoprenaline, and that the blocking ability of propranolol might be changed by lowering temperature. These predictions were not supported by my experiments, and this was the reason for not accepting the hypothesis that the nature of the adrenoceptor changes with temperature[2].

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Aedes aegypti complex in Africa

We wish to discuss aspects of the communication by Scott and McClelland[1]. It was pointed out long ago that sound ecological studies depend on the accurate determination of the evolutionary status of the organisms under study[2]. This is especially crucial in applied biology[3]. If two forms are common and sympatric there are three possible situations. (1) They are reproductively isolated and do not exchange genes under natural conditions; in other words they are distinct species. If they are not reproductively isolated (that is they mate with each other in nature), they may represent (2) two subspecies (races) whose ranges have recently intersected to form a transient area of sympatry where hybridisation occurs, or (3) morphs of a single polymorphic species[4]. The concepts behind these possibilities are well established and backed by sound bodies of theory, and many studied examples of each are documented[5].

Scott and McClelland dealt with the sympatric occurence of the dark and pale forms of the current taxon *Aedes aegypti* L., a mosquito of considerable medical importance. The pale form has a wide distribution within and beyond Africa, and is domestic in its habits. The dark form is confined to Africa south of the Sahara, and is an outdoor mosquito[6]. These facts and the data on the alkaline phosphatase and protein loci provided in Table 1, of ref. 1, are sufficient to eliminate the possibility that we are dealing with a single polymorphic species. In fact, the data for these two loci provide strong evidence for the existence of positive assortative mating in the field, thus supporting the view that they are distinct genetic species. It should be emphasised that the evidence for random mating in cages and for the interfertility of the two forms is of little relevance in assessing the status of the forms because it is well established that distinct genetic species are often interfertile in artificial crosses[7,8], and cases are known where distinct species mate at random in cages[7,9] but positively assortatively in nature[7,10,11].

We think that it is unfortunate that Scott and McClelland used the vague and imprecise terms "ecotype" and "incipient species". "Ecotype" is a term introduced for plants and is not appropriate to animals. The term "incipient species" is not appropriate here, as it is usually used in narration to imply some well marked subspecific stage. Neither term is backed by a satisfactory body of theory. The opening sentence of the report immediately leads to confusion by implying that "ecotypes" are not populations.

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SCOTT AND McCLELLAND REPLY—We agree that the status of the two entities is crucial to the paper. We believe that field hybridisation has been amply demonstrated (refs 4 and 9 of our report in *Nature*[1]). A computer simulation of this situation containing three genotypes, three habitats, two seasons, parameters for rate of movement, habitat selection, natural selection and assortative mating, is now under review. It demonstrates that polymorphism is highly probable, provided only that: (1) there is a dry season when breeding occurs only in the indoor habitat; (2) fitness of the indoor ecotype is greater in houses, and fitness of the outdoor ecotype is greater in the natural habitat; and (3) movement between indoor and outdoor habitats is not large (less than about 25% of the indoor inhabitants must be immigrants each generation). The computer model shows that, when mating is random, polymorphism is probable.

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1 Scott, J. A., and McClelland, G. A. H., *Nature*, **256**, 405–406 (1975).