is no reason to reject the view that, in general, selection is directly responsible for the maintenance of these polymorphisms. What is needed, of course, is not so much in vitro studies of enzyme activities as experimental evidence of selective differences between different genotypes.

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Dermo-optical Perception

In "Dermo-optical perception: a peek down the nose" (*1*) Gardner takes exception to my research on tactual color discrimination, on the grounds that the various subjects (particularly Mrs. Stanley) were able to see the stimuli through "nose-peeks" and were not making the judgments from sensations in the fingers and hands. Gardner's comments are made on an a priori basis, since he has never seen my apparatus or witnessed my procedure, although his article conveys the impression that he has. His article combines details from my mimeographed reports with assumptions for which there appears to be no basis. Mrs. Stanley is not a magician. She is a housewife who, by chance, was found to have some tactual discriminating ability when she was in high school in 1939, ignored it for 24 years, and consented to some experiments in 1963. During the experiments, Mrs. Stanley was carefully observed. She was required to put her arms into the box containing the stimuli through thick black sleeves fastened around holes in the box and tight around her wrists, and she wore a sleep mask. She could not, as Gardner suggested, have poked the stimuli up a sleeve and used a "nose-peek," nor could she have observed the test material as it was being placed in the experimental box. Nor did she keep up "a steady flow of conversation with the observers, asking for hints on how she is doing." Nor did careful and continuous observation "seem unnecessary." Also, her ability was observed and confirmed by Donald DeGraaf, chairman of the physics department of Flint College of the University of Michigan.

My hypothesis of "wavelength temperature" discrimination seems more tenable. That a wide range of electromagnetic wavelengths, including the visible and infrared, does penetrate mammalian skin to a significant depth is shown by various investigations (*2*). Oppel and Hardy (*3*) showed that human skin has different absolute thresholds for different ranges of electromagnetic wavelengths. The sensitivity threshold, apparently in terms of subjective "temperature," is lower for wavelengths longer than 3 microns, as measured in gram-calories per square centimeter per second. For wavelengths of 0.8 to 3 microns the threshold in the same terms is 50 percent higher. And for wavelengths of 0.4 to 0.7 micron, the visible wavelengths, the threshold is still higher, being 2.2 times the threshold value for 3 microns or greater.

In each of my reports (*4, 5*) I have stated as my hypothesis that the tactual discrimination ability evidenced by the subjects was a product or variation of the cutaneous temperature sense. This has now been confirmed by further experiments of mine (*6*) and independently by W. L. Makous (*7*). When color discriminations are made with the hands and stimuli in a light-tight experimental box, the differences between the stimulus objects are related to the differential absorption, reflection, and emission of infrared wavelengths. The energy comes from heat emission by the hands in the range of 4 to 14 microns (*3, 8*).

In the 1963 investigations Mrs. Stanley was successful in her tactual discrimination judgments (85 to 95 percent, $P < .001$) when the colored materials were covered with Wratten neutral density filters down to about 13-percent transmittance; also when colored materials were covered with 0.003-inch cover glass or with clear plastic about 0.010 inch thick. She was not successful (her judgments were at chance level) when the stimuli were covered with 1/16-inch picture glass; or when her finger temperature was below 24°C; or when plastic stimuli and her hands were under water at 32°C. Her judgments were also at chance level with bits of colored wood or pieces of colored sponge rubber. These results were obtained during 55 to 60 hours of testing in the summer of 1963. The subject was less successful, although her score was still above chance, when tested in January 1964, and was not successful on 20 April 1964 or during 3 days of testing in June 1964.

From tests of 133 women college students, done with bib-screen plus blindfold, I estimate that 10 percent of the female college population have the ability to make statistically reliable discriminations of colored stimulus materials when the stimulus materials are illuminated (*5*). The hypothesis is again temperature discrimination. On the grounds of "parsimony," such explanations as "ESP" have been rejected. "Telepathy" has been excluded by double-blind experiments.

In view of the information now available, it is difficult to see how Gardner's comments on my investigations have any basis in fact.

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14 March 1966

I found Gardner's critique of DOP refreshing, but I feel compelled to come to the defense of Richard Youtz and his experimental subject. Having been invited by Youtz last August to test Mrs. Stanley's powers, I had the opportunity to observe her and her performance.

Indirect evidence leads me to the conclusion that Mrs. Stanley is not trying to cheat. When discovered by Youtz, she had not been employing her presumed powers for profit, and she agreed to ignore any attempts at commercial exploitation (she has been approached by television people). She does indeed talk while trying to discriminate the colors with her fingers, asking how she is doing, talking also about day-to-day topics. This, however, appears to be conversation to lessen the tedium and discomfort of the sessions rather than persiflage to misdirect the experimenter. Observing her, one gets the impression that she is a personable but not at all extraordinary housewife.

Gardner remarks that he was "unsuccessful in persuading" Youtz to put a box over Mrs. Stanley's head during the testing. It should be noted that Mrs. Stanley is the sole subject available for Youtz's experiments. She sometimes refuses to consent to experimental sessions because of chores at home. She likes to pause for a cigarette and coffee, or merely to rest, at random moments. Because of the nature of Youtz's hypothesis, some experimental sessions are run under conditions of high temperature and humidity. In the circumstances, considerable tact and flexibility are required of the experimenter in order to achieve the cooperation of his only subject. Youtz's present sleeve-and-bib apparatus seems more than adequate to prevent peeking. If Mrs. Stanley were required to put her head into a box, she would just plain refuse to serve. One hopes that her attention has not been called to Gardner's article. Youtz has already had his troubles persuading her to continue.

On the basis of the evidence thus far I am inclined to agree with Youtz that Mrs. Stanley is sensitive not to electromagnetic energy but to thermal energy. The effects are subtle; the sole subject is short of time. Teasing out the physical variables on which Mrs. Stanley's performance is undoubtedly based is a formidable long-term task.

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Gardner's article offers a reasonable explanation of certain reports that conflict with what is known about sensory processes, and calls attention to some of the precautions that are necessary (though not sufficient) in a serious investigation of such questionable phenomena as those reported. Gardner neglects to point out, however, that it is because these reports are incompatible with present knowledge that they are likely to be explained by flaws in the experiments, such as inadequate precautions against trickery. Both character recognition and trichromatic color matching through "dermo-optical" means are among such questionable phenomena; not in this category, however, is the detection of differences in radiant heat exchange between the skin and different objects that may appear to be identical with one another except for color (hue, saturation, or lightness).

I describe elsewhere (*1*) a theoretical and empirical analysis of cutaneous sensitivity to differences in radiant heat exchange with divers objects. Application of the Stefan-Boltzmann law shows that, under some conditions, radiant exchange between skin at body temperature and a good radiator at room temperature is approximately 9.3×10^{-3} watt/cm^2, which is 3 to 15 times as great as reported values of threshold irradiance (*2*). Among the variables considered in the theoretical analysis are skin temperature, temperature and spectral emissivities of the objects to be discriminated, conduction, convection, and factors influencing the cumulative effects of thermal exchange. The computed effects of changes in radiant exchange on skin temperature were compared with empirical measurements. In spite of the inherent errors in such measurements, comparison reveals skin temperature changes many times as great as reported thresholds (*2, 3*).

The ability of human subjects to discriminate between objects on the basis of differences in their emissivities was tested under the following conditions: in a "completely" dark room (illuminance $< 11 \times 10^{-7}$ lu/m^2) with electronic monitoring against physical contact between the subjects and the test objects; with skeptical subjects, with subjects having no previous interest in magic or in mentalism, and with a totally blind subject; with a plastic laboratory apron (optical density > 10) snugly tied around the subject's neck and bound around his head in a way that restricted vision as effectively as the box described by Gardner; and with a double-blind procedure to eliminate suggestion and to preclude even telepathy. Of the five subjects who were tested carefully, none failed to perform significantly above chance in the ten trials given. The three subjects further tested since the publication of Gardner's article have performed equally successfully while wearing a box of the kind he described.

Anyone can, in an hour or two, prove to himself his ability to discriminate via his cutaneous senses between radiant exchanges with objects of differing emissivities. After applying flat black paint to half of a square plate (about 15 cm on a side and 0.3 cm or more thick) of polished metal, he can discriminate the painted (highly emissive) side from the unpainted (poorly emissive) side merely by holding his hand half an inch from the surface and attending to thermal sensations. He can take any precautionary measures he deems necessary, but after two or three practice trials he will be able to perform the discrimination correctly on about 90 percent of the trials.

To avoid misunderstanding, I must add that the discrimination just described is not analogous to color vision; the multidimensional color space is compressed here into a single dimension, rate of heat exchange. Rate of heat exchange between observer and object, however, is correlated with the hue of the object as well as with its lightness. Thus, a general term that subsumes both properties, such as color sensitivity, serves to relate the sensory function to the visible differences between the objects discriminated. But, because the discrimination actually depends upon thermal exchanges that are only statistically correlated with visible properties, perhaps emissivity sensitivity (or ϵ-sensitivity) is a more accurately descriptive term.

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3 March 1966

I would like to add to Gardner's observations a note about some research he does not mention. I tested a group of 80 college students on a task which required them to detect a single odd color from among three colored papers covered by plastic (*1*). The observed mean percentage of correct identifications was 33.7, against a predicted chance level of 33.3, a statistically insignificant difference ($t = 0.007$); increasing the relative differences in hue and brightness failed to produce significant improvements. In a follow-up study of three subjects whose detection performances were about as good as those reported by Youtz (*2*), the subsequent daily scores varied from significantly above to significantly *below* chance.

The advocates of DOP seem to alternate between two hypotheses. One hypothesis implies that DOP is a previously undiscovered sensory channel possessed, in varying degree, by all human beings. The group data from my 80 subjects failed to support this hypothesis, thereby raising the question of why this alleged new sensory channel should behave differently from all other sensory channels. The other hypothesis, which is the one toward which the convinced scientists have characteristically gravitated, is that only certain individuals are gifted with DOP. Such individuals are usually identified by their statistically significant performances. On the basis of the follow-up study of high-scoring subjects, I have pointed out (*1*) that, when Youtz (*2*) used the usual statistical test of significance on several hundred trials by a star performer, he reduced the standard error of the mean to the point where the increment of a few percentage points above chance appears to be significant (*3*). While this is technically legitimate, it is possible that during this period of time subjects may adapt to the situation, learn to detect stimulus differences on other dimensions, improve their ability to pattern their guessing behavior, and, as Gardner points out, perhaps learn how to nose peek, all of which might contribute to successively rising scores. Another possibility, evident from the data from my three subjects, is that the highly significant overall performance scores would mask the fact that the daily scores fluctuated widely from significantly above to significantly below chance. These possibilities make an overall test of significance very questionable indeed.

Since the "gifted person" hypothesis is so often used in the fringe areas of science, how are we to regard the many people whose performances on screening tests are significantly *below* chance? Are they to be included among the "ungifted"? It is certainly possible that continued testing with the ungifted might show patterns of above- and below-chance scores such as I found with initially high scorers.

Or is it possible that the convinced DOP researchers are focusing on the positive tail of a normal distribution?

The main problem with the gifted-person hypothesis is that it is so open-ended that it is not subject to refutation. It can always be said of critics of DOP that they have not been lucky enough to find a star subject. And being, unlike the DOP supporters, constrained by rules which require that hypotheses be expressed in such a way as to be both testable and refutable, the critics cannot assert that the null hypothesis is true, that is, that DOP does not exist in man. The final irony is that, despite the focus on the gifted-person hypothesis, in the discussion of the results the DOP supporters very often wander back to the unproven claim that DOP is a new sensory channel.

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References and Notes

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8 March 1966

Measurement of Anesthetic Potency

In "Temperature dependence of anesthesia in goldfish" (*1*), Cherkin and Catchpool introduce a technique for making quantitative measurements of the anesthetic potency of water-soluble agents. Goldfish are kept swimming by an applied stimulus (an electric shock); anesthetic potency is measured as AD_{50}, the dose at which 50 percent of the experimental animals fail to respond to the stimulus. The technique was developed in order to examine the relation of potency to temperature in the hope of distinguishing among current theories of anesthesia.

Cherkin and Catchpool found that, for each of a number of anesthetic agents, AD_{50} increased as temperature increased. From this they conclude that increasing temperature is antagonistic to the anesthetic process, a conclusion which supports the Pauling (*2*) and Miller (*3*) microhydrate hypothesis of anesthesia. The conclusion would be valid if it were demonstrated that the rise of AD_{50} with temperature is not the result of the effect of temperature on processes other than the unit anesthetic process itself. In other words, a suitable control of the effect of temperature without added anesthetic is required. The Cherkin and Catchpool experiment lacks such a control. At any temperature over a wide range, approximately 100 percent of the goldfish were successful in their response to the test; it was implicitly assumed, therefore, that at all temperatures in this range the goldfish were in a single baseline state, and that AD_{50} at different temperatures could be directly compared. The assumption that organisms are in equivalent states at different temperatures is at variance with a vast body of experimental knowledge. Most of the vital processes of poikilotherms, including those influencing motile responses, speed up as body temperature rises from 0°C to about 45°C. It is widely thought that this speeding up is due to the involvement of rate processes with activation energies. Therefore, it does not seem reasonable that the baseline performance of the goldfish should be independent of temperature. That it appears to be so is a fault in the experimental design, in which the measure used is success or failure in performing at an arbitrary level rather than quantification of a graded response. There is evidence in the report itself of a graded response as a function of temperature. At 1.6°C, even in the absence of anesthetic agent, 50 percent of the fish did not respond. (The authors' broadening of the definition of anesthesia to include this low-temperature effect is unwarranted, in view of the possible involvement of activated processes.)

The temperature coefficients of most of the numerous life processes which have been studied are so large (*4*) that, had they been taken into account, the conclusion drawn might have been qualified or reversed. For example, the temperature coefficient of the rate of opercular movement in goldfish is 16.5 kcal (*5*), while that for AD_{50} is 8.6 to 13.2 kcal. If the significant process in the Cherkin and Catchpool experiment has an intrinsic temperature coefficient similar to that of opercular movement, the fact that the coefficient for AD_{50} is smaller would indicate that anesthesia is more effective at higher temperature. Such a finding has been reported for the influence of anesthetics on the contraction of frog muscle (*6*).

The objection raised here may arise whenever an arbitrary criterion of performance is made the basis of a study. A recent modification of the Cherkin and Catchpool technique, using the brine shrimp *Artemia* (*7*), also employs such a criterion, and would be subject to the same objection if used to study the temperature dependence of anesthesia.

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