low-level creative artistic productivity, in her cultural studies in the South Pacific. Historical studies of some highly creative artists also reveal a high incidence of neurotic or psychotic-like behavior among these persons.

The Maslow Security-Insecurity Inventory was included in this study in order to investigate this problem, and all groups were classified in the "average" range on Maslow's norms. Regarding his test, Maslow stated that "the purpose of the Security-Insecurity Inventory is to detect and measure the feeling of security (which as defined here is one of the most important determinants of mental health, almost to the point of being synonymous with it) . . ." (*7*, pp. 2–3), and again ". . . security as defined here is almost synonymous with mental health . . ." (*7*, p. 7). If Maslow's definition is accepted, the present results certainly offer no support for Roe's hypothesis or for the implications of Mead's studies in relation to creativity in science. These findings give little support to the hypothesis that creativity is associated with the highest level of mental health (degree of personal adjustment), since the average scores for the groups were not in the "very secure" range, but rather were only "average."

The religious factor, also, is of interest here. It has usually been found that creative scientists show a preference for the Protestant religion and that very few eminent scientists prefer Catholicism. This was noted by Roe (*1*), Knapp and Goodrich (*12*)—in relation to the production of scientists by Catholic institutions of higher learning—and by others. This report therefore supports Knapp and Goodrich's findings in that only 1½ percent of all scientists included in the study attended undergraduate schools with Catholic affiliation. Further, only 6 percent of all subjects came from homes in which the Catholic faith was preferred, while 77 percent came from Protestant homes. However, no relation was found between the achievement of creative status and religious preference. It appears, then, that religious preference is much more strongly associated with the choice of science as a career than it is with achievement of highly creative productivity within a scientific discipline.

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Human Cones

I was greatly pleased with the magnificent experimental results on visual pigments recently published (*1*). Although the maximum density values for cones are similar to those obtained by fundus reflectometry in the gray squirrel (*2*)—the first living cone retina to yield a result comparable with recent measurements—they seem somewhat low when compared with data obtained by this method on living, intact foveal human cones. Double passage through the retina gives in this case $\Delta D_{max} \doteq 0.2$ (*3*). Brown and Wald's values (*1*) are, as the authors point out, much too low when compared with Rushton's recent measurement (*4*). However, two points have to be made in this connection. First, the authors examined what appears to be a normal retina, whereas Rushton dealt with a protanope. Second, Rushton's value of 0.35 is wholly inconsistent with other data he published in the same series (*5*). He claims to obtain good agreement between his difference spectrum and Pitt's spectral-sensitivity (S) curve (*6*), which he did not correct for preretinal absorption losses. But if the density in protanopic cones is as high as he estimates it to be (on the basis of a theory that is unclear to at least one reader), then the S-curve ought to be much broader, as shown in Fig. 1 (*7*)—preretinal losses being ignored also in this curve for the sake of consistency. It can be shown that much of Rushton's third paper (*8*) in this series—notably his conclusions regarding photosensitivity and also the relation between pigment regeneration and dark adaptation—are invalidated (*7*) by the inconsistency between the second and first papers.

The reason is clear. The computed value of σ, believed by Rushton (*5*, p. 371) to represent the fraction of stray light, is numerically linked to the high density value of 0.35. He writes: "Though 0.35 might seem rather a high value for cone density, it would have come out a great deal higher if the stray light factor σ were not as small as 2.0." Thus if Rushton's theory is valid and his measurement correct, σ must be smaller and his claim to have made it low by attention to instrumental design a modest understatement. Now Rushton's two comparisons—(i) between predicted and computed values of the intensity variation of the fraction of pigment bleached, and (ii) between the fraction of pigment regenerated as a function of time and the course of dark adaptation—show fairly good agreement (but see *7*). However, the computed values that these coincident pairs of data are based on are derived by means of a nomogram pinpointing the above value of σ. It might be deduced that the value of σ is not of great importance, but it

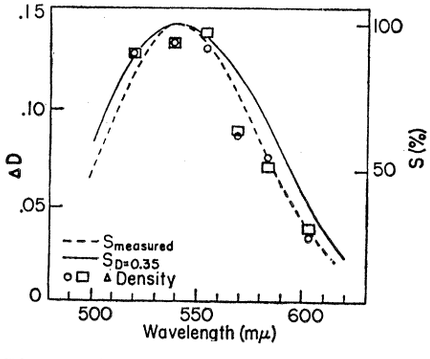

Fig. 1. The solid line indicates the shape of the protanopic V_λ curve if Rushton's density value of 0.35 is correct. The broken line is Pitt's curve.

can be shown that the agreement depends to a considerable extent on the value of σ chosen by Rushton. If it is reduced (for example, by substitution of the measured rather than the computed values), the agreement loses some of its splendor. There is no doubt that a correction for wasted light has to be applied (*7, 9*), and it is almost certain that it is substantially less than Rushton's value. The concomitant reduction in the *in situ* density of the green-sensitive value follows then even on Rushton's theory, but the agreement, particularly between pigment regeneration and dark adaptation, disappears. Possible reasons for this have been studied experimentally (*10*).

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30 April 1964

Atmospheric Data May Be Misleading

The studies of Rasool [*Science* **143**, 567 (1964)] and of Arking (*ibid.*, p. 569) are significant preliminary investigations of the heat balance of the earth and its atmosphere and of the very closely related distribution of cloud cover, as observed from meteorological satellites. We are pleased both that such studies are actively in progress and that *Science* is becoming increasingly a medium for publication of meteorological research findings.

At the same time, we are concerned that, because of the necessarily stringent limitations on the length of such papers, and because the majority of the readers of *Science* are not specialists in the atmospheric sciences, these preliminary results may, through the years, be accorded a more general validity than can be justified by the data sample on which they are based. In the past, similar initial results published in journals of wide distribution have all too often been applied as engineering data without either understanding or indication of their severe limitations.

Although Rasool discusses the known degradation of the channel 2 data in Tiros II and III, there appear to be other significant errors from still unknown effects. For example, degradation in the high equivalent temperature portion of the observations can be detected from the maximum channel-2 temperatures observed over the South Pacific subtropical high, where ocean temperatures are relatively constant. Studies by one of us (Wexler) indicate that observations in this region show a degradation of 8°C between orbits 1–5 and 42–47 of Tiros III, a further degradation of 5°C between orbits 72–77 and 98–105, and still another 5°C by orbits 212–218. These values of degradation are quite different from those supplied by NASA. The low equivalent temperatures associated with high clouds also appeared somewhat erratic during the later orbits.

Direct application of the visible spectrum channels of Tiros III (not used by Rasool) is limited by the fact that the observed albedos appear too low when compared with known albedos for clouds and various types of terrain. Nevertheless, the relative values are reasonable, and they can be corrected to give an estimate of the net incoming radiation as good as or better than that determined from climatological considerations. It seems unfortunate that Rasool did not use the Tiros data at least to verify Budyko's estimates, especially over oceanic areas where direct pyroheliometer data are nearly nonexistent.

But even if one disregards these uncertainties as to the accuracy of specific items of data, unfortunate misuses of these findings by those without thorough training in meteorology remain probable if Rasool's Figs. 1–6 and Arking's Fig. 4 are accepted as necessarily having long-term, quasi-climatological validity. These figures certainly include climatological effects and, in the case of Rasool's data, seasonal trends. But they doubtless also incorporate shorter-term atmospheric variations, whose influences cannot be judged from presently available data; and there are definite indications that the data are biased by diurnal factors introduced by the Tiros orbital characteristics.

For example, Rasool's Figs. 1–4 show relatively little change from month to month over and near North America and Australia, where data samples could be obtained twice a day, at approximately 12-hour intervals, with consequent reduction of diurnal effects. But over North Africa, orbit characteristics limit data to once a day and to varying times over the 9-week cycle Rasool discusses. Here the observed change from January to February could be largely due to the greater probability of cloud cover during the predominant afternoon and evening data-acquisition period in January as compared to the lesser probability of cloud cover during the morning February data acquisitions. Only significantly greater data samples will permit determining the relative importance of these diurnal effects as compared to seasonal changes.

In Arking's findings, effects resulting from the data necessarily being grouped near local noon are suggested, since the satellite-observed cloud cover exceeds the climatological values in the more continental northern hemisphere and the reverse is true in the oceanic southern hemisphere. There are also suggestions, in the satellite values near 30°N and 20°S latitudes, that the bright sands of the Australian, African, and North American desert areas are of greater significance than the author assumes; while these areas are only a small fraction of the earth as a whole, they do tend to concentrate along relatively narrow belts of latitude.

There are other matters, such as the effects of the diurnal variation of cloudiness on outgoing radiation, and the various complexities in the relationships between cloudiness and vertical air motions, that deserve more discussion than they were accorded or than space here allows. It is important, however, that these results be interpreted as only what they are—highly desirable but necessarily preliminary findings. Nonmeteorologists are advised to use them only with caution and preferably with the assistance of scientists fully aware of the inherent limitations to any attempts to assign them more general validity.

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