Comments and Communications

Of Spots Before the Eyes

THE observation of so-called flying disks in so many areas and the relative consistency in the description of these objects have led the writer to wonder whether they may not represent some form of natural phenomenon. Should these ephemera exist in the macrocosm, it is likely, if they are indeed natural phenomena, that they would be known to astronomers, meteorologists, and other observers of the atmosphere. Since the scientists have given no explanation of the oft-reported disks, it is necessary to ponder the problem of their existence in man himself and in spheres other than the psyche. A ready and reasonable explanation may be found in a smaller orbit, the eye of man. *Muscae volitantes,* the flitting flies we have all seen, may well be the "saucers" we wonder about.

Muscae volitantes is the term employed for the appearance of spots (motes) before the eyes. . . . They are caused by the shadows cast upon the retina by the cells normally found in the vitreous and are present in all eyes under certain circumstances, such as exposure to a uniform bright surface, or when looking through a microscope. They are found more frequently in errors of refraction (especially myopia), and the symptom may be aggravated temporarily during digestive derangements. They are annoying and sometimes alarm the patient, but are of no importance and do not affect the acuteness of vision. The treatment consists in correcting errors of refraction, or in relieving the disturbance of digestion. They often persist until the patient ceases to look for them and thus forgets their existence (*1*).

Anyone who has observed this visual phenomenon will recall that the object seen is brilliant and that it moves erratically, its erratic motion being a compound effect related to the motion of the shadow on the retina and associated movements of the eyeball and head. These objects also agree with some "observations" made on flying disks in that it is impossible to judge their distance or speed.

Another visual phenomenon which may be observed in the dark, as well as in the daylight, is the scintillating scotoma. Scotomata may be of various colors but otherwise are of uniform appearance as judged by the descriptions given by many persons suffering from migraine. They are of fairly consistent duration, usually lasting about 20 min, with an initial period of increasing density, then of stable appearance until they fade away. They are thought to be of cerebral origin (*2*).

It is thus likely, in the opinion of the writer, that flying disks are motes in the eyes of a dyspeptic microcosm or perhaps some abnormal cortical discharges in the migrainous.

EDGAR F. MAUER, M.D.
1930 Wilshire Blvd., Los Angeles

References

1. MAY, C. H. *Manual of Diseases of the Eye,* 264 (1941).
2. ALVAREZ, W. A. *Oxford System of Medicine,* VI, 936 (9).

Editorial Prerogatives

EDITORIAL NOTE: *The Editorial Board has reluctantly but unanimously agreed to publish the following letter from K. H. L. Key, solely because it raises an issue on which the board members themselves are not in complete agreement, and which is bound to benefit from free and frank discussion. Dr. Key has informed the board of the name of the journal to which he refers—and the board has stipulated that its name shall be announced at the same time his communication is published. Although he has demurred, he has not withdrawn his request for publication, and the board persists in its stipulation for two reasons: It refuses to be party to the guessing game that would ensue if charges are made against an unnamed scientific periodical and it insists that fair judgment of any dispute depends upon the simultaneous presentation of both sides. For these reasons Dr. Key's charges are followed by a rejoinder from the editors of the* Quarterly Review of Biology.

FIVE years ago R. W. Gerard (SCIENCE, **106**, 289 [1947]), made some excellent observations on the editing of scientific papers and editor-author relationships. I had made use of some of his points in trying to influence editorial practices in Australia, but it was not until some time later, when I came to submit a paper to a highly reputable American biological journal, that I was able to appreciate fully the criticisms made by Gerard of the methods of some editors. My experiences with that journal show that the arguments of Gerard and others have had no effect at all on some editors. A simple recital of the events as they occurred will, I think, be as effective as any amount of pleading, for they carry their own emphatic condemnation. They may perhaps serve to bring once more to the attention of scientists the very unsatisfactory situation that still exists in editor-author relations.

I submitted my manuscript—a lengthy review article—in July 1949. In November 1949, I was informed by the editor that it would be accepted for publication, subject to certain alterations that were necessary in order that the paper should conform to the style of the journal (e.g., deletion of the summary, incorporation of footnotes into the text, and changes in the list of references). The request for such alterations was, of course, reasonable and normal, and I met it fully. In January 1950, the editor informed me that he was now turning the altered manuscript over to an associate editor for "editing for the printer." In my innocence I imagined that this would mean the insertion on the manuscript of instructions to the printer regarding type style, spacing, etc.

To my complete surprise, in October 1950, I received a letter from this associate editor stating that the galley proof was on its way to me, and that it incorporated a number of changes that *he* had made in the manuscript. He outlined the principles he had followed in arriving at these changes, all of which related only to my use of English, and said he hoped

I would feel perfectly free to restore my wording in any instances where I thought the change had not been for the better. When the galley proof arrived on Nov. 3, it became apparent that several hundred changes had been made, few of which could be justified in my view, and many of which changed my meaning or were completely arbitrary. I was obliged to restore my original wording in a great many cases, although I did my best to avoid unnecessary restorations; I justified the restorations in a lengthy letter to the editor.

In a letter dated Dec. 5, the associate editor consented to only about one third of these restorations. He said he was forced to "cancel" the remainder, "unless you would like to pay for the alterations," which would cost some $71.00. Since the journal was due to be issued by the end of that very month, I cabled the editor that I had regretfully to insist that all my changes in the galley should be allowed to stand, and that I was trying to arrange for payment of the necessary $71.00. I confirmed this in an air mail letter, in which I explained that I had already accepted all the changes I could possibly agree to and that, if he could not accept my decision, I would have no alternative but to withdraw the paper. To the cable the editor replied, on Dec. 20, that he regretted that before my cable was received the galley was in the printing presses, and that "under no circumstances could I stop or interfere with the manufacturing schedule of the ——— Press." My air mail letter was never answered.

From this sorry little history the following facts stand out: (1) The paper that was published was not the same paper that I was told had been accepted for publication. (2) In spite of a period of nine months between the acceptance of the paper and my receipt of the galley, my first intimation of hundreds of important editorial changes was in the galley proof, less than two months before the due date for publication. (3) The editor's statement that I was free to restore my original wording was a gesture without any substance, because most of my restorations were "rejected." (4) The later implication that I could have all my restorations if I paid for them was quite improper. If the editor was in difficulties with his costs, that was due solely to the fact that he had allowed his own alterations to appear in the galley without taking the precaution to ascertain first whether they would be acceptable to the author. In any case, this supposed "offer" was as hypocritical as the assurance that I was free to restore my wording, for the galley had been returned to the printer before even a cabled reply could be received. (5) Statements were published under my name which I had expressly repudiated. Yet there was, of course, no editorial note to say that the author was not responsible for all the words attributed to him. (6) A paper was published which its author had explicitly withdrawn.

This whole correspondence, including the transfer of the proofs in both directions, was conducted by air mail (except where cable was used), so that the distance between Australia and the United States does not enter as a significant factor.

That the events recorded under (5) and (6) above could occur at all in scientific publications (except by some accident or misunderstanding) must come as a shock to many scientists. One would have supposed that no extenuating circumstances of expense, inconvenience, or delay could possibly be held to justify such action. The dangers, both to science and to the individual scientist, that such action implies are too obvious to need mentioning.

I have deliberately avoided any detailed consideration of the material points at issue between the editor of the journal and myself. My criticisms of the editor's procedure, and of the ultimate outcome, would, I consider, have full force even if it could be established that, on every question in dispute in regard to the text of my paper, strict interpretation of English usage showed the editor to be right and me wrong. If, however, the editor was in error in many instances, then the criticism is certainly strengthened. That this was in fact the case is shown, in part, by the editor's own admissions in correspondence and his acceptance of one third of my restorations. Further, the paper in its original form was approved for publication by the leading Australian scientific institution to which I am attached. The professor of English at the Canberra University College, who has studied both the original manuscript and a duplicate copy of the galley proof, has written to me as follows: ". . . I should say that in general the editorial changes made in the galley sheets do not appear to depend on any important question of style or grammar, and that, as far as English is concerned, I see no reason why the preferences of the author should not have been conceded."

All the documents in this case, including the duplicate galley proof with its restorations and the editor's "rejections," are on file.

K. H. L. KEY

Division of Entomology
Commonwealth Scientific and Industrial Research Organization
Canberra, Australia

THE editors of the *Quarterly Review of Biology* are sorry to have incurred the ire of their esteemed contributor K. H. L. Key, of Australia. At the same time, they would like it to be understood that the *Quarterly Review of Biology* has a literary tradition, as well as standards of scientific excellence, to maintain. All manuscripts are edited for clarity of expression, and ambiguities and inconsistencies of style are eliminated, insofar as possible. Some changes are no doubt trivial, but the over-all effect, it is hoped, is to improve without fundamentally changing the style of each author. During the past eight years, throughout which the present associate editor has edited the manuscripts of contributed articles, no contributor other than Dr. Key has made any complaint about the final wording or form of his paper. Many

have expressed appreciation. In the case of Dr. Key's manuscript the editor assumed that, like so many of us, Dr. Key was rather lax and inconsistent in the use of verb tenses. He now realizes, sadly, that Dr. Key is never lax or inconsistent, but always says precisely what he means.

It is our general policy, in handling manuscripts from foreign contributors, not to return the edited manuscript to the author prior to the printing of galley proof. The danger of lengthy delay in the mails or of the loss of a unique copy that embodies many days of editorial labor, is too great. Instead, galleys are printed on thin paper, and these are sent to the contributor by air mail, with a note that he is privileged to reinstate the original wording wherever the correct meaning has been altered or the change appears undesirable. This practice has in other cases always given mutual satisfaction to authors and editors, both before and since the present instance.

It would be most desirable if all manuscripts could be edited soon after receipt. Unfortunately, the editors of the *Quarterly Review of Biology* must do all their editorial work as a spare-time service to science without monetary compensation, and over and above their other duties. It is generally impossible to commence work on one issue until the preceding issue is in press. As Dr. Key's article was scheduled for publication in the issue of December 1950, the editing of his manuscript was begun in late August. It was due at the printer's on Oct. 5, and the galleys were sent to him on Oct. 27. By the time Dr. Key's returned galleys reached us, the issue in which his review was to appear was already in page proof. Since he had written repeatedly to urge more speedy publication of his paper, the editors preferred not to hold up its chance of publication in the issue for which it had been scheduled. The returned galleys asked for reinstatement of the original wording in 287 instances, and an accompanying letter justified many of these. In 131 instances (45.6%), including virtually all those justified by the author, the original wording was reinstated. Where the change in meaning appeared trivial and would at the same time require resetting of lengthy passages or entire paragraphs, the original wording was not reinstated. Alterations in proof must be made by hand, and are not only costly but also hazardous because errors commonly are introduced by the printer in making the corrections. In the case of page proof these errors, unfortunately, cannot be corrected. It may be pointed out, for example, that in making one correction of original wording desired by the author, the printer jumbled an entire line. One must decide whether making a change is worth the risk of further error, and decisions must often be made quickly to meet rigid printing schedules. The December number was scheduled to be issued on Dec. 22, 1950. When Dr. Key's air mail letter of Dec. 18 arrived, with the first intimation that he would withdraw the paper if his demands were not met, the issue was already printed and could not have been withdrawn either by him or by us. That we did not learn until too late of his adamant determination to insist on every item does not warrant or support his charge of hypocrisy.

Considering the present communication from Dr. Key to SCIENCE, the editors of the *Quarterly Review of Biology* feel that it is most strange that 13 months elapsed after the article was published without any protest to them from the author in the meantime. They had supposed he was quite satisfied by the acclaim with which the review was greeted. Since he evidently is not, they are willing to publish in the next issue of the *Quarterly Review of Biology* either of the following corrections, whichever one the author may prefer, although they feel that such corrections would more properly have been printed in the immediately succeeding issue of the journal. If, on the one hand, Dr. Key really feels that the paper is unrecognizable as his own, in spite of the opinion of the professor of English at the Canberra University College that the changes were not very important one way or the other, the following announcement can be printed:

> Dr. K. H. L. Key, to whom authorship of the review article "A Critique on the Phase Theory of Locusts" in the December 1950 issue of this journal was attributed, wishes it to be known that, because of editorial changes in the article, many of the views expressed are not his own, and he disclaims responsibility for them.

The alternative would be to publish, as is customary, a page of errata, reinstating each of the 156 disputed words—e.g.:

> P. 364, col. 1, l. 12: For "With these physical differences are correlated . . ." read "With these physical differences were correlated. . . ."
>
> P. 364, col. 1, l. 25: For ". . . ph. *migratoria* can be transformed into . . ." read ". . . ph. *migratoria* could be transformed into. . . ."
>
> P. 364, col. 2, l. 5: For ". . . which Uvarov quoted . . ." read ". . . which Uvarov quotes. . . ."

In conclusion, the editors of the *Quarterly Review of Biology* would like to indicate their complete agreement with the principles of good author-editor relationships outlined by Ralph W. Gerard in SCIENCE, and referred to by Dr. Key. In practice, the pressure of printing schedules often creates difficulties, as every editor knows. The author will sometimes have to choose between compromise or postponement of publication. For contributors abroad, as the present dispute reveals, a satisfactory agreement within the normal schedule may be impossible, and a more leisurely schedule should be adopted. In the more recent publication by the *Quarterly Review of Biology* of contributions from abroad, no difficulty has been experienced.

BENTLEY GLASS
CARL P. SWANSON
B. H. WILLIER

Department of Biology
The Johns Hopkins University