Correspondence.

A Letter from Leo Stevens.

To the Editor of the SCIENTIFIC AMERICAN:

I have decided not to enter the airship contest at St. Louis. The speed expected is too great. The man who enters this contest has everything to lose and nothing to gain.

The rules call for a speed of at least 20 miles per hour. This is impossible. The prize is perfectly safe with the Exposition Company.

I think the rules might have been modified just a little. For instance, the man making best time should be allowed to take first prize, second man second prize, and third man third prize. There would then be something in sight. Many Americans would certainly enter.

I will continue experimenting in this vicinity during this year and will prove what the American can do.

Aeronaut LEO STEVENS.

New York, March 5, 1904.

[The rules governing the airship competition at St. Louis have, we understand, recently been modified, so that the speed required is now 18¾ miles an hour, and the course to be covered 10 miles.—ED.]

Draper's Specula Test.

To the Editor of the SCIENTIFIC AMERICAN:

A paper by myself upon the above subject was published in the SUPPLEMENT, page 23232. A criticism by Mr. Edmund M. Tydeman appears in the SCIENTIFIC AMERICAN for November 14, page 348. Residence in Australia precluded earlier rejoinder.

In my paper, by a purely analytical method, developed step by step from first principles, and given *in extenso* to enable the results to be checked, the connection between variation of ordinate and variation of the position of the axial intersection of a ray after reflection from a parabolic surface is demonstrated. The results are compared with those computed for identical conditions by Draper's rule, and the latter is proved to be 50 per cent in error.

Mr. Tydeman in his strictures has wholly failed to grasp the significance of the third (III.) premise, clearly expressed, of the analysis, or to assimilate the subsequent treatment. Mr. Tydeman misquotes. In the second paragraph the factors represented by x and z are transposed; by this and the succeeding quotation a serious false issue has been raised. In the next instance the expression the "*constant H P*" (italicized in the letter) is substituted for the correct definition—"normal"—used by me. The position of the critic that the angle contained by an incident and reflected ray is *not* bisected by the correlated normal (not "*constant*" as wrongly quoted) *D H* to the reflecting surface is, of course, untenable. The matter, although a most essential point, is, optically, elementary; bisection is the corollary of Newton's second axiom in optics, hitherto unquestioned.

In the same paragraph Foucault's and Draper's methods are classed together as though identical; they differ broadly, as all who have used both know, and as recourse to the original memoirs will prove. In compilations they are not infrequently erroneously conjoined. My paper deals specifically with Draper's formula.

In the last paragraph Draper's formula is defended by attributing error to rules differing from it.

The errors and misapprehensions indicated wholly vitiate the criticism, and confirm the opinion that the reputation of the reflecting telescope, as an instrument of precision, has suffered by the unquestioning acceptance, without investigation as to their genesis, of "authoritative" formulæ.

It is assumed that the original will be compared by those interested. JAS. ALEX SMITH.

Melbourne, January 13, 1904.

Actinic Light.

To the Editor of the SCIENTIFIC AMERICAN:

An article on "Some Experiments with Actinic Light," by J. W. Kime, M.D., appeared in the SCIENTIFIC AMERICAN for June 20, 1903. There are in this article several very misleading statements and erroneous conclusions. Soon after its appearance, having occasion to prepare a paper for the Scientific Society of the Ohio University, with the help of Mr. J. O. Wright, assistant in the department of physics and electrical engineering, I prepared to demonstrate the fallacies of Mr. Kime, and to set forth the correct views of the subject. This was done by citing authorities, by a number of experiments, and by the use of lantern slides showing the effects of light under a number of different conditions.

In this communication I only desire to select from the paper above referred to a few points, which will sufficiently show forth Mr. Kime's mistaken notions about actinic light.

In the very first paragraph we read, "the light of the sun is composed of *three distinct kinds of rays*, luminous, heat, and chemical, or actinic." Italics are mine. Now, the fact is, the only inherent physical difference in light waves so called, is one of *wave length*. Moreover, the very same rays may produce all three of the effects named, though in varying degrees depending on the wave length. Electro-magnetic waves of the magnitude of sound-producing air waves are now utilized in the transmission of wireless messages; when they are much shorter, and approach the infrared, but still too long to affect the sense of sight, they begin to produce heat. Still shorter, produce more intense heat effect. When we come into the region of $\lambda = 0.8\ \mu$ (micron), we have the first visible portion of the spectrum. As the waves become shorter and shorter, the color changes through orange, yellow, green, blue, indigo, to violet, $\lambda = 0.36\ \mu$, the heat effect in the meantime decreasing and the actinic effect increasing. Passing out into the ultra-violet invisible rays, the chemical action continues to increase; indeed, it is those radiations whose wave lengths are below 0.36 micron which produce the greatest part of the total chemical effect of a given beam of light. Continuing down the scale, we reach the extremely short waves, which exhibit the properties attributed to the X-rays.

It is at least misleading to speak of "those bands of the spectrum which are rich" and "those which are poor" in certain rays. It leaves the impression all the time that a mixture of rays of very different properties, regardless of wave length, is to be understood. Color itself is subjective, the physiological and psychological effect of wave length. Therefore it is absurd to speak of a color being rich or poor in heat or actinism.

In Mr. Kime's fifth paragraph he explains that colored strips of glass corresponding to the spectrum colors were used. "In this manner," he says, "we obtained a *true photograph of actinic light*," etc. In his sixth paragraph we read, "we are unable to recognize any difference whatever between the open space (no colored glass) and the blue glass." Also, he says less light passed through clear glass (one thickness) than either the clear space or the blue glass. These statements will be examined further down. In the next paragraph he concludes from his observations just quoted that "blue glass cuts off no chemical light," and that "the ultra-violet rays are either not markedly actinic, or that blue glass does not retard their passage. It is very evident one hundred per cent of actinic light has reached the plate through the blue glass." He also finds no regularity of actinic effect. "The yellow," he informs us, "transmits an *appreciable* amount, and the green *just enough* to be seen. From this point we jump from almost zero in the green to *one hundred per cent* in the blue." "In the violet," again, "we drop back to almost the same percentage as in the yellow." And now to the profound and revolutionary conclusion that "*wave length has nothing to do with determining the chemical activity of light.*" From this list of insufficient and erroneous data he further generalizes: "It is apparent from our photographs that color, *independently of wave length, influences the chemical action of light.*" In all cases the italics are mine.

In the paragraph headed "Experiment No. 2," Mr. Kime either does not say what he intends, or else he does not know exactly what his photographer did. He informs his readers that No. 2 is a positive, yet immediately says it was taken on sensitized paper, just as No. 1 was taken on a dry plate. If this last is true, then No. 2 is a negative, just as No. 1. But anyone with the most rudimentary knowledge of photography knows that No. 2 *is* a positive, as stated at first. If so, then it was never printed through the glass strips as was No. 1. Moreover, a glance at once shows that No. 2 was obtained as a positive print through the negative corresponding to No. 1. The No. 1 may also have been made on paper as well as on glass. I do not see how we can do better than count as worthless all conclusions and inferences about relative actinic values when based upon experiments made, apparently, in darkness of the most elementary laws of light. We are also unable to make any extenuation, however conclusive the author of these experiments would have his results appear, in that they were "confirmed by repeatedly going over the experiments, and always with like findings."

Now to illustrate by a few notes from our own experiments the results obtained therefrom. In timing the exposures a seconds pendulum was used, being arranged so as to indicate magnetically its successive passages. In the development exactly the same kind of plates and developer were used throughout, and subjected to the same length of development where results in any way depended upon these conditions.

Two sets of negatives were made with 1.5 seconds exposure and 2 minutes development, under colored films, arranged to give six of the seven spectrum colors. The one set resulted from exposure to sunlight, diffused, and no glass intervening; the other set was exposed through four thicknesses of glass. In the positives the colors, beginning at the left in each, were red, orange, yellow, green, blue, and violet, the indigo being omitted. There was observed a slight actinic effect through the red; orange, yellow, and green, practically none; blue and violet are alike strongly actinic. This photograph differed from Mr. Kime's result, as well it might. The effects in both sets are equally strong, both in the negative and in the print. The intervening glass, even four thicknesses, caused no difference capable of detection by this means when the visible portion of the spectrum, or that which apparently represents it, is employed. It is well known, however, that glass does have a powerful absorptive effect on those waves lying in the ultra-violet section, so that prisms and lenses made of quartz should be used when studying this portion. For convenience the above colors are 5, 7, 20, 30, 23, 28.

Another positive was made from a negative taken through six films arranged to represent the spectrum, and exposed six seconds, developed five minutes. The colors were 6, 17, 8, 10, 11, 27. Here the red, orange, yellow, and green cut off all actinic rays. The blue and violet only slightly absorb them.

Still another positive was made through seven films, 16, 7, 21, 30, 22, 12, 29. In this the red showed slight effect, orange none, yellow strong, green none, blue and indigo same as the yellow, none of them being 100 per cent. The violet absorbs a large part of the actinic waves. This supposed spectrum would be considered quite anomalous, did we not bear in mind that we are using colored films instead of the true spectrum.

Other photographs were made which showed the actinic effects of the wave lengths in the true spectrum of the voltaic arc. The spectrum was produced by projecting the light from the crater of the electric arc through a glass prism. The visible portion of the spectrum was about three feet long and one foot deep, so that ample room was given for studying the effects in the various bands, and indeed for several positions in the same color. Reference marks were made on the screen, so that the same positions could be used with accuracy any number of times. Mr. Wright constructed an apparatus for exposing successive parts of the same plate in successive parts of the spectrum. His arrangement permitted as many as ten exposures on the same plate.

We made a photograph to show the results in infrared, red, orange, yellow, green, blue, indigo, two positions in the violet, ultra-violet. The negative and print showed nothing for infra-red and red, orange very faint, yellow slightly stronger, still more intense in the green; neither plate nor print shows any difference for the blue, indigo, violet, and ultra-violet. It is known, however, that the effect *does actually* increase into the ultra-violet before beginning to decrease again.

A photograph from red, orange, yellow, green, blue, indigo, two in violet, ultra-violet, and ultra-violet taken far out, perhaps 12 inches beyond the last of the visible violet, showed a perceptible diminution of actinic intensity in the last band.

The conclusions from these data are obvious. It is certainly altogether unreliable to use colored glass or colored films to represent the spectrum. Their coloring matter may absorb some of the waves corresponding to their own apparent color, or let pass waves not corresponding to their color, or both. The experiments cited show that different films of apparently like color produced quite diverse photographic results. Further, it is certainly clear that there *is no irregularity* in the actinic effect, but rather that it increases progressively even to $\lambda = 0.36\ \mu$, or less, from which point it begins to decrease. *Wave length, and not color,* determines the actinic effect, as it does every other property of electro-magnetic radiation. A. A. ATKINSON.

Physical Laboratory, Ohio University, Athens, Ohio.

A Union Building for New York Engineers.

With the present of $1,500,000 made recently to the Mechanical, Mining and Electrical Engineers of this city, and the Engineers' Club, steps were immediately taken toward the completion of plans for a union building, which the gift provides for. The building will have a 125-foot frontage on West Thirty-ninth Street, and will be backed by the new Engineers' Club building—a separate structure opposite the new Public Library on West Fortieth Street. It will probably be twelve stories high, and besides spacious headquarters for the three national engineering societies, there will be several auditoriums of various sizes, one of which will seat 1,200 to 1,500 people; an engineering museum; and a library having at the start 50,000 volumes, and which, in co-operation with the Public Library, will be the finest technical library in the world.

The three societies, each of course maintaining its identity and autonomy, will need considerable room in the new building, which it is hoped to have completed by 1906. These societies have to-day a combined membership of over 9,000, and they are growing at the rate of from 10 to 15 per cent. annually. Besides these societies, there are other technical societies having some 5,000 members engaged in all branches of civil, mechanical, electrical and municipal engineering, whom it is desired to accommodate.