appears to me absolutely groundless. The only velocity that can possibly remain constant, or approximately constant, during wheeling, is the velocity with reference to the supporting medium; and as that velocity is, according to his previous assumption, not only small, but negative, there is no energy available to enable the bird to rise. Indeed, the bird, in passing from a negative velocity relative to the air, to a positive velocity relative to the air, must pass through the phase of no velocity relative to the air, in which he is practically helpless, being compelled to fall vertically in order to acquire sufficient speed to steer. Like Mr. Pickering, Professor MacGregor treats the subject as though the earth influenced the motions of a bird on the wing by some other means than gravitational attraction. He apparently fails to perceive, that, if the body of air in which the bird moves has no internal motion, its relation to his flight is precisely that of a calm.

Let me illustrate. A steamer propelled with uniform force on a calm ocean has its rudder turned constantly, and by the same amount, to the right, and consequently describes a circle. This circle is described *on the ocean:* it expresses a relation between the moving body and that by which it is supported. It has no reference to the bottom of the ocean. It makes no difference whether that part of the ocean is at rest or is part of a swift current. The relation of the boat to the water is not affected by the relative motion of water and bottom. Or consider a skater. Having acquired momentum, he is able to describe circles without propulsive effort until the stored-up energy is consumed by friction and by the resistance of the air. The ice on which he circles may be frozen to the shores of a pond, or it may float with uniform speed on a rapid river; but his relation to the ice is the same in either case, and his circles have the same pattern *as engraved on the ice.* The case of the soaring bird is closely analogous. His horizontal motions are related only to the air in which he moves, and by which he is supported, and they are not affected by the uniform horizontal motion of that air with reference to the ground.

A slight correction, and I have done. I assume, as Professor MacGregor says, that after wheeling, the bird's velocity relative to the medium in which he turns will be the same as before (discounting friction); but I do not admit the implication "that during the turn his velocity relative to the earth will change by an amount equal to twice the velocity, relative to the earth, of the medium in which the turn is made." His velocity relative to the earth will change by an amount equal to twice his velocity relative to the medium.

G. K. GILBERT.

Washington, D.C., Feb. 25.

IN two communications published in the last number of *Science* (p. 151) under the above title, Professor Pickering and Professor MacGregor have developed with considerable ingenuity a theory of the possibility of a bird soaring in a uniform horizontal wind; but it is certainly true that a bird cannot soar — that is, permanently sustain or elevate itself without expending energy — in such a wind, and it has therefore seemed to me to be important, in the interests of clear thinking, to show on dynamical grounds why soaring is impossible in this case.

Evidently the velocity of the wind relative to the earth has nothing to do with the question, as it is the relative movement of wind and bird that causes the re-actions between them, and therefore can alone come into consideration. Let the air, therefore, be supposed to be at rest relative to the earth, and it becomes at once obvious that the bird cannot soar: for, suppose the bird to have any imaginable initial velocity, and to wheel in the most artful manner, it is still a mass falling under the influence of gravity, and only resisted more or less by the fluid friction of the medium in which it is placed. This fluid friction of the air against its wings can only delay its fall, but can never prevent it, just as it delays the fall of a feather.

A theory of soaring must explain how energy is given to the bird by the wind; but it is clear, that, instead of the bird receiving energy, it is expending either its kinetic energy, as when in one of its whirls it sweeps upwards, or potential energy when it sweeps downwards. But the temporary increase of potential energy in a rise can never equal the corresponding loss of kinetic energy, because energy is being continually expended in frictional heating. There is thus a steady expenditure of energy, and none received from the medium, and the bird is therefore bound to come to the ground. The only effect of the medium is to resist the motion, in whatever direction it may take place, whether up or down.

As soon as it is clearly seen that the only thing we are concerned with is the relative motion of air and bird, and that the air may be at rest relative to the earth without affecting the question in the slightest, the futility of any attempts to explain soaring in a uniform horizontal wind is apparent.

If any one wishes to discover the particular fallacies in the theories above mentioned, let him attempt to follow out the reasoning as given in the communications referred to; assuming, however, that there is no wind, that the air is at rest relative to the earth; remembering that the mere fact of the earth's moving relative to the wind has no connection with the relation between the bird and the air.

The theory that soaring can be kept up by taking advantage of differentially moving layers of air is not open to the above criticism, and may be the true explanation: it is certainly not unreasonable on its face.

ARTHUR L. KIMBALL.

Johns Hopkins University, Baltimore, Feb. 23.

To keep Water-Mounts Moist.

IN biological work with the microscope it is frequently desirable to preserve water-mounts for several days, that growth, development, etc., may be observed from time to time. Water lost by evaporation can be very successfully replaced to the glass slips from a beaker beneath by means of capillary tubes. To make these, hard-glass tubing of about three millimetres bore is softened in a Bunsen flame, and then drawn out to a diameter of from two-tenths to three-tenths of a millimetre. This is then divided up into lengths of five centimetres, and each piece bent at an angle of about 80° one centimetre from an end by holding it over a very small flame for an instant, when, of its own weight, the end falls to the proper angle. One tube is sufficient for a slip, and is applied by first touching the longer limb in water, when instantly the liquid will rise and fill the tube, which may now be suspended by the shorter portion from the glass slip, allowing one end to just touch the edge of the cover-glass, and the other to dip beneath the surface of the water in the beaker. A thin film of water will run along the shorter limb, and hold it securely in place. The whole is then covered with a suitable bell-jar. In this way mountings in water or nourishing solutions may be kept an indefinite time, and are always ready for examination without disturbing them in the least. Should it be desired to supply more fresh water or nourishing solution to the mount than would ordinarily arise, a bit of filter-paper applied to the side of the cover-glass opposite the capillary tube will accomplish this.

E. B. KNERR.

Parsons College, Fairfield, Io., Feb. 18.

Color-Blindness a Product of Civilization.

THE following is a summary of a paper read before the Kansas Academy of Science at Leavenworth, Nov. 1, 1888: —

The fact that blindness to certain colors exists among civilized people is well established; also the percentage of cases to be found among males has been determined with considerable probability for the races of Europe and America. There has been much diversity in methods of testing, and the results of many reported determinations might well be called into question. Still it is probably not far from the truth that about four out of every hundred males are more or less deficient in color-sense. Of females there have been reported (B. J. JEFFRIES, M.D., *Color-Blindness*, p. 74) as examined in Europe and America 39,828; and of these, only 60 were color-blind, or 2 per cent. Of both males and females, 156,732 have been tested; and of these, 6,721, or 4.27 per cent, are color-blind. These statistical facts have naturally excited interest and discussion. If so large a number as four out of every hundred are unable to distinguish colors, there arises, of course, a practical question important to the railroads, marine, etc.

The gravity of this fact is already recognized more or less in all countries by the test examinations for color-blindness among employees. But there is in these statistics also much of interest to scientists.

Most cases of color-blindness are found to be congenital, and are incurable. Many have been produced by disease, some by violent concussions in accidents, and some by excessive use of tobacco and alcohol. Temporary blindness to violet may be induced by santonine. From these facts several interesting questions have suggested themselves to us. If color-blindness follows the law of heredity, is it on the increase, or decrease? Further, is it a product of civilization? The first of these queries can be answered only by statistical data extending over long periods of time. The second naturally suggests a comparison, first, of the color-sense of civilized nations among themselves; and, second, of civilized with uncivilized peoples. Of tests in native tribes, we can find but two recorded, — those of Dr. Favre on some tribes in Algiers, and those of a Dr. Fox on 150 American Indians, but where we do not know.

First, for the comparison of civilized tribes among themselves, we have calculated the following percentages from tables reported by Dr. Jeffries: —

	No. Examined.	Per Cent Color-blind.
Austria	5250	3 79
Denmark	5840	3.74
Belgium	8106	4.13
Holland	2300	1.43
Finland	1200	5.00
Norway	205	4.88
Sweden	32504	3 73
Switzerland	3024	5.36
Germany	6344	4 12
Russia	12830	3.30
Italy	2065	2.32
England	16431	3.75
United States	44844	3.64
Average per cent		3.76

No great reliance can be placed upon these results. The numbers examined are too small, the methods of testing not uniform, not equally reliable. However, the probabilities of error are about equally distributed; so that the conclusion is fairly well established, even without great accuracy of data, that among civilized nations color-blindness is at present almost equally common.

Second, among uncivilized people, Dr. Favre's results from Algiers, already alluded to, show 414 examined, and only 2.6 per cent color-blind. Dr. Fox reports 161 young Indians tested, and only 1.81 per cent color-blind. These percentages, so low compared with those for civilized people, suggested to us that color-blindness may be a product of civilization, and have led to our tests here reported. At the Haskell Institute at Lawrence, Kan., are several hundred Indians, representing many tribes. These we have recently examined by Holmgren's method with Berlin worsteds. Out of 418 tested, — 285 males and 133 females, — only three cases of color-blindness exist, or only .7 of one per cent. These three are full blooded Indians of the Pottawattamie, Pawnee, and Crow tribes. Of these, two have defective color-sense for red, and one for green.

The Indians of the school are about equally divided as full-bloods and half-breeds. It seemed to us that the half-breeds showed more instances of blunted color-sense than the full-bloods. This was evidenced in more frequent and prolonged hesitation among them in comparing the colors than among the full-bloods. If this be confirmed by more extended examinations, it would, in conjunction with the low percentages obtained as above, be an argument in the theory proposed by us that defective color-vision is in some way the product of civilization.

The use of tobacco suggests itself as a possible cause. This would explain also the low percentage among females. It leads also to the thought of increase of color-blindness in males in future generations. But the data are at present too meagre to more than suggest this explanation.

It is certainly not accidental that nearly every case of color-blindness is for red, few for green, and seldom one for violet. Why are the defects thus limited, at present at least, to the longer wave-lengths of light?

The Young-Helmholtz theory of color-perception will locate the affection in that layer of the retina corresponding to the first of the three primary sensations of color. But why this special layer, with few exceptions, is the only one affected, has at present no explanation.

The law of heredity indicates increasing sensitiveness in those nerves which are subjected to special use through many generations. It seems reasonable to look for an explanation of the more perfect color-sense in females to this fact; but among males there will probably be an increase, in future generations, of the number of cases of defective color-sense.

L. I. BLAKE.
W. S. FRANKLIN.

Lawrence, Kan., Feb. 19.

Note on the Wind-Pressure Constant.

THERE is a very old formula in use among English and American engineers and meteorologists for obtaining the force of the wind from its velocity. The product of the square of the wind's velocity in miles per hour into the factor .005 is taken as the pressure in pounds upon each square foot. It is used alike at sea-level and on the tops of high mountains, and in the extreme temperatures of winter and of summer, notwithstanding the pressure must vary as the density of the air. This is the value of the factor determined by Rouse from experiment about one hundred and fifty years ago, and of all the crude experiments which have been made from that time to this, and before. It seems to be an extreme value. Of a number of the older determinations of this factor, it is stated, in Gehler's "Physicalische Wörterbuch," that this is the worst, while those of Hutton and Woltman are perhaps not much in error. It is astonishing to see, therefore, with what tenacity engineers and meteorologists still hold on to this factor. It has been maintained by the writer for several years that this factor is much too large, — first in *Van Nostrand's Journal*, 1881; then in "Recent Advances in Meteorology;" and in the *American Meteorological Journal*, 1887. It was shown that the theoretical value of the factor, not considering friction, is .0027; and it was thought that this could not possibly be increased to .005 by the friction of the air. And it was shown that this view of the matter is confirmed by Loomis's results obtained from the discussion of experiments made by the request of Newton in St. Paul's Cathedral, London, upon the velocity of the falling of hollow glass globes and of bladders, and of Hutton's experiments with a whirling-machine. From all these researches, and also the somewhat recent experiments of Hazen with a whirling-machine, it was concluded that the theoretical constant above could not be increased by friction more than one-tenth, or, at most, one-eighth part. But the old factor has been in use so long, that conservatives think it must be correct, and so are unwilling to give it up.

A further confirmation of the erroneousness of the factor is now found in the last number of the *Quarterly Journal of the Royal Meteorological Society*, which contains a report from the wind-force committee appointed by that society. The committee, as yet, have made only a few preliminary experiments in this part of their work; but the average value of the factor from these is .003, which is about one-tenth part greater than the theoretical value given above. This will, no doubt, be changed a little in their final report, after more experiments shall have been made; but as it agrees nearly with the factor obtained by Hazen, and with what is to be inferred from other experiments, it is not probable that the final result will vary much from this.

This is a factor in which engineers are especially interested, and its value ought to have been accurately determined by them long ago; but, as the Royal Meteorological Society has now taken it in hand, it is to be hoped that its committee will do the work thoroughly, as they apparently intend to, and determine the value of this factor accurately, not only for plates of different sizes and shapes, but also for different temperatures and barometric pressures.

WM. FERREL.

Kansas City, Mo., Feb. 20.