Correspondence.

The Utility of the Scientific American.

To the Editor of the SCIENTIFIC AMERICAN:

In regard to the item in your issue for October 24, page 291, "Utility of SCIENTIFIC AMERICAN," you suggest the pasting of clippings on Manila paper. I am sorry you did not advise use of light muslin instead of Manila paper. The muslin renders clippings almost indestructible by ordinary use, while the Manila paper will soon break. D. N. BYERLEE.

Hood River, Oregon, October 27, 1903.

A Suggested Wing Arrangement for Flying Machines.

To the Editor of the SCIENTIFIC AMERICAN:

I have taken more or less interest in aerial navigation and the Langley experiments reported in the SCIENTIFIC AMERICAN; perhaps some suggestions of mine would interest your readers.

With regard to the wings of the Langley flying machine: Would not the stability be greater if the convex sides of the wings were down, and of larger area, to compensate for the difference, or even perfectly flat?

The principle (in this respect alone), as I take it, is that the air should spill from the wings which support the machine, to give stability. Kites fly better when their convex side is toward the wind, or when their surfaces are flat, than when the concave side is toward the resisting medium. In the latter case they are sure to dive and be unstable, for they cannot spill the air and adjust themselves to steadiness.

If the wings were made larger and slightly convex, or perfectly flat, on a flying machine, with the usual rudder vane adjustment, it would be much more under control. The small area must be facing the wind or the air, in the forward movement, and the larger away from it, or in trying to get there it will cause disaster, as the adjustment is too fine for even a man to control. A bird in soaring spills the air through its feathers in adjusting for its stability. This is controlled by the bird's instinctive and unconscious habit—the same as a man in walking never thinks of falling down. This fine adjustment is impossible in a flying machine without something else being relied on.

Another and a better idea is to have substituted for wings or part of the wing area, one or more revolving aeroplanes, circular and *concave side down*. These would be perfectly smooth and revolved rapidly *to keep them (and the machine) in one plane*, and to spill the air, get stability, and reduce the wing area, giving greater strength. If a clay pigeon ejected from a trap did not revolve, it would not soar so beautifully. It is kept in its one plane on the gyroscope principle. It remains rigid in that plane, and as long as its momentum is not exhausted, it continues to soar. Attach a motor to the clay pigeon, and it would soar (fly) until the motor stopped. Again, any lack of symmetry in the circular aeroplanes is compensated for by all its sides being presented constantly to the resisting air. If a clay pigeon were thrown from an arrangement which did not send it spinning, it would not soar, and if it did, perhaps for a time, it could not keep on, for it would not have a *special and fixed plane* which the revolving motion gives, and which a bird uses as its principle of keeping right side up.

A flying machine constructed on the clay pigeon principle, with motors to revolve sufficient-sized, slightly concaved aeroplanes, with the double-vane rudder controlled by a man, and, of course, the regular propellers for the forward thrust, would, I suggest, be much more stable and easily managed during flight than the concave wing arrangement.

Toronto, Canada ARTHUR E. HAGARTY.

The Cause of Hay Fever.

To the Editor of the SCIENTIFIC AMERICAN:

My attention has been called to an article published in a recent number of your valuable journal entitled "The Cause and Cure of Hay Fever." In the interest of science I wish to enter a very emphatic protest against the doctrine there set forth. In this article it is claimed that the disease in question is caused by a toxin, which is introduced into the system by the application of pollen to the mucous membrane of the eyes and nose; that the effects of this toxin can be counteracted by a serum procured in the usual way, and hence that the disease may be cured by its use. It is also stated that there has been until this time great uncertainty as to the cause of this disease, and that hitherto medical treatment has been of little use. I believe that all of these statements are very questionable. In the first place, all diseases which are caused by the introduction of bacteria into the system and the subsequent development of the toxins have certain characteristics in common. One is a prodromal or stage of incubation. This is absolutely necessary, as a certain amount of time is required for the development of the toxin after the introduction of the bacteria. There is no stage of incubation in hay fever. In other toxic diseases this varies from one day to three weeks. In hay fever the disease follows immediately upon the application of the irritant, and disappears as soon as this ceases. In toxic diseases the acute stage follows the stage of incubation, which is characterized by a considerable rise in temperature, indicating a serious constitutional disturbance. There is no rise in temperature in hay fever.

In toxic diseases the acute stage lasts for a variable time, ending in death or gradual recovery, as time is required for the system to eliminate the poison. Treatment by the antitoxin method does not cause an immediate disappearance of the disease. It only lessens its severity and duration. Hay fever disappears as soon as the application of the irritant ceases. Again, the irritant is not necessarily the pollen of flowers. Any kind of dust may cause an attack. Dust from horses which have not been properly groomed is a very frequent cause, and many patients cannot ride behind a horse at any time of year without having trouble. Even the contact of a probe armed with absorbent cotton sterilized will cause sneezing and irritation of the mucous membrane of the eyes and nose, and if this be continued will cause a genuine attack of hay fever. Flour dust affects many. Further, if the toxic principle alone is the cause of this disease, all would suffer, as every one is exposed continuously to the same influences during the summer season. A continuous exposure of people generally to the poisons of the other toxic diseases, such as diphtheria, smallpox, etc., would result very disastrously. All of this points indubitably to the fact that the irritant acts mechanically and not chemically or vitally. Again, all of the toxic diseases are either contagious or infectious. Hay fever is neither. Usually one attack of a toxic disease renders the patient immune from further attacks. Hay fever recurs year after year, and usually increases in severity. In toxic diseases a single inoculation of the poison is sufficient to produce all of its different phases, while the application of the irritant must be contagious in hay fever. It would seem clear, then, that the toxic theory is not tenable. The statements that there is great uncertainty as to the cause of this disease, and that hitherto the medical treatment has been of little use, are not true. The cause is perfectly understood, and the cure absolutely certain if the treatment be properly carried out. My experience during the past two years has fully demonstrated the truth of my claims, and I am absolutely certain that any case of hay fever or spasmodic asthma can be permanently cured. The fact that the so-called toxin will cause an attack of hay fever when applied to the nasal mucous membrane proves nothing, as the application of any irritating solution, such as nitrate of silver, will do the same thing. If the sensitive tissue in the nose be thoroughly deadened by applying cocaine, the paroxysm will cease. Such an application can have no possible effect upon a toxin. FLOYD S. MUCKEY, M.D.

Minneapolis, Minn.

Unconsidered Facts in the Art of Flying.

To the Editor of the SCIENTIFIC AMERICAN:

In your issue of October 31 you printed an article called "Unconsidered Facts in the Art of Flying." In this article there are points which the layman would misunderstand unless made clearer, and I would be pleased if you would publish this letter upon the subject.

In the first place, the conclusion is drawn that because birds often cover long distances in a short space of time, at the rate of 80 miles per hour, and because they are not powerful, that therefore the power to fly is much overrated. It is also claimed that it takes less power to travel in the air than on land. It is evident that the fallacy here comes from failing to recognize that it is the speed relative to the air, not the earth, which determines the power spent, and there is no evidence in existence, so far as I know, that any bird can travel at 80 miles per hour relative to the air. Suppose a bird has a cross section area of 8 square inches with a coefficient of resistance of one-half; then to travel at the rate of 80 miles per hour relative to the air would require the expenditure of at least 0.132 horse power, thus

$$\frac{80^2 \times .0035 \times 4 \times 117}{144 \qquad 550} = 0.132 \text{ horse power.}$$

This is a power which could not by any possibility exist in such a small bird. The logical conclusion is that a bird traveling at this speed relative to the earth is taking advantage of a strong *wind* going in his direction. Birds rise to heights to find such a wind, not to get in a rarer atmosphere.

The power necessary can be found by experiment; but this, while it means that this power is necessary for flight, does not mean that the bird itself must expend this power. A vulture can fly for hours in the air when we know that the power to do this cannot possibly reside in his muscles; he extracts it from the wind.

The conclusions in the above-quoted article are false, for they assume that since birds with a small amount of muscular power do fly, therefore but a small amount of power is *necessary* for *flight*. This is as absurd as to say that because there is no source of power in a sailboat, therefore it requires no power to drive a sailboat. As a matter of fact, it probably takes one hundred times more power to sustain a vulture in the air than the vulture exerts with his muscles. This extra power the bird extracts from the wind, by utilizing its variations.

To say that flight is accomplished with the expenditure of very little power by the bird is one thing; but to say that the phenomena of flight *require* the expenditure of but little power is similar to saying that it requires the expenditure of no power to drive a cable car because, forsooth, there is no motor in the car. The power to sustain any body in the air can be accurately figured; and to fly without that body expending that amount of energy simply means that the body must in some way extract the difference from the wind. If one is to depend entirely upon internal power, the internal power required is great; but if one is skillful enough to draw power from the wind, the internal power required may be reduced to any amount, depending entirely upon skill and local conditions. That there is a large source of power in the wind cannot be doubted since the publication of Prof. Langley's pamphlet on "The Internal Work of the Wind," which work is due to the fact that no wind is ever absolutely horizontal or uniform. Our dynamics of flight are perfectly sound, but our observations lead us astray, for we are never in a position to know exactly just how much power the bird is extracting from the wind.

The observation of birds will never tell us how much power is necessary for flight; from them all we can get is a knowledge of how much they extract from the wind, which of course is the difference between what they can exert, computed from the size of their muscles, and what we know is necessary for support, computed from the lift and drift of these birds, as found by experiment. The reason there is so much dispute over this question is because the ability to extract this power is entirely dependent upon local conditions, and local conditions vary for different observers. The power necessary for flight can be computed from experiments, but the question of how much of this power it is necessary to *carry with us* will depend upon our skill in guiding the machine and the local conditions. It takes much more power to travel in the air than on land, although that power need not reside in the thing traveling. It takes more power to travel in the water than on land, although in the case of the sailboat *no* power need reside in the boat.

The lesson to be learned is that skill is the first thing to be gained, for with this the amount of power that must be carried in the machine can be greatly reduced; but this does not in the least affect the fact that the phenomena of flight do require the expenditure of more power, regardless of the source from which it is drawn, than either travel in water or on land. A. A. MERRILL.

Boston, Mass., October 30, 1903.

The Current Supplement.

The current SUPPLEMENT, No. 1455, contains a variety of instructive articles. Among these may be mentioned the front-page article on the Tunis gas plant, illustrated by two clear engravings; Major Baden-Powell's *resumé* of recent aeronautical progress and deductions to be drawn therefrom regarding the future of aerial navigation; and the continuation of Mr. George J. Henry, Jr.'s paper on tangential water-wheel efficiencies, admirably illustrated with instantaneous photographs of the effect of a stream of water on Pelton buckets. Prof. Meldola's paper on the relations between scientific research and chemical industry is concluded. Dr. S. G. Tracy tells of the use of radium in medicine. Dr. Horace C. Hovey presents a very fully illustrated account of the Colossal Cavern of Kentucky. Mr. A. Frederick Collins describes the camphor industry of Formosa—a counterpart to the article on artificial camphor appearing elsewhere in this issue. A hydraulic coal hoist for discharging coal from railway cars directly into a ship's hold is described and illustrated.

Height of the Sea Breeze.

Observations as to the height of the diurnal sea breeze are few in number, albeit of considerable importance. By means of a captive balloon, sent up from Coney Island a number of years ago, it was found that the average height at which the cool inflow from the ocean was replaced by the upper warm outflow from the land was from 500 to 600 feet. At Toulon, in 1893, the height of the sea breeze was found to be about 1,300 feet, and a distinct off-shore current was found between 1,900 and 2,000 feet. More recently (1902), on the west coast of Scotland, Dines, using kites, has noted that the kites would not rise above 1,500 feet on sunny afternoons, when the on-shore breeze was blowing.—Quart. Journ. Roy. Met. Soc.