Correspondence.

A SUGGESTION FOR HOUSEHOLDERS.

To the Editor of the SCIENTIFIC AMERICAN:

Often the rooms just beneath a roof are almost unbearably warm in summer. We know how refreshing a shower is. I have often lowered the temperature ten degrees in lower rooms by turning the hose so the water would play against the side of the house.

If the water pipe were extended to the highest part of the roof, and then continued—as along a ridge pole—by a pipe with a row of tiny perforations on either side, the roof could be effectively showered and cooled. Of course, in case the roof were large, the pipes would have to be brought up from below in several places, so that the distribution of the water could be controlled from below. If desirable, the water could be caught at the eaves and run into a tank or miniature standpipe, and its elevation would be sufficient to carry it over a lawn or wherever it was wanted. Or it could be guided over the eaves by a strip of galvanized iron, and so be made to run down the side of the house. Or it could be caught at the eaves and run into another perforated pipe, to cool the side of the house under the gable. MRS. F. R. MILLER.

Montreal, Can., January 18, 1909.

THE PRACTICAL SIDE OF AERIAL LOCOMOTION.

To the Editor of the SCIENTIFIC AMERICAN:

At the risk of striking a discordant note, the writer, who has long followed with interest your aeronautical notes, would like to inquire, What is the practical value of all that has so far been learned from the many experiments with balloons and flying machines? Is there any sound principle underlying any of these air vessels (by whatever name called) that is capable of development to the point of commercial success? It should be self-evident that the measure of success in aerial navigation, as in the navigation of the land and of the sea, is the availability for commercial purposes. The value of any such vessel for war purposes is merely incidental. Yet even the inventors of the heavier-than-air machines do not claim to be able to carry substantial loads over definite courses and without regard to weather conditions; and a careful study of the principles on which such machines are based gives little ground for the belief that any such result can follow the methods employed. The balloon principle, even as modified by the genius of Count Zeppelin, has so many and such well-known elements of weakness, that it need not seriously be discussed.

It has occurred to the writer that inventors are inclined to keep their eyes so closely to their own work, that they fail to grasp the full meaning of the problem to be solved; and it might not be out of place to suggest that the airship of the future—the one that will finally solve the problem of aerial navigation—will be able to rise of itself to any altitude desired, to remain in the air if need be for not merely hours but days at a time, carrying a load equivalent to the weight of five hundred passengers or more, and to descend when and where desired. With this accomplished, the problem is solved; and it can be solved by the simple application of well-recognized mechanical laws.

New York, January 12, 1909. C. A. MCCREADY.

COMPARISON OF WRIGHT AND VOISIN AEROPLANES.

To the Editor of the SCIENTIFIC AMERICAN:

In the January 9, 1909, issue of the SCIENTIFIC AMERICAN is an editorial on Mr. Lanchester's paper comparing the Wright and Voisin aeroplanes. In this comparison there is a table which states that the skin friction on the wings of the Wright aeroplane is forty pounds, quite a considerable amount, and below this the editor entertains a doubt whether Mr. Wright has been correctly quoted as saying that he makes no allowance for skin friction, believing it to be negligible. Of course, the writer is not sure whether Mr. Wright made this statement or not; but there is good proof that it is correct. Prof. Langley proved by his experiments with the resultant pressure recorder that, as he himself says, "the pressure is normal to the inclined surface, and hence that the effects of skin friction, viscosity, and the like are negligible in such experiments." Prof. Langley's statements are all so exact and trustworthy, that we can hardly doubt this one. Also Mr. R. H. Thurston, in the Universal Encyclopedia, says that Mr. Maxim found the resistance due to friction of the surfaces of a plane was imperceptible, and might be neglected. Both of these results contradict the statement that there should be forty pounds of resistance due to skin friction in the Wright aeroplane, and uphold Mr. Wright's statement that this friction is negligible.

Farther on in the article Wilbur Wright is quoted as saying that "as far as the Wright aeroplane is concerned, stability depends entirely on the skill of the aeronaut"; and Mr. Lanchester compares this with the stable equilibrium of the Voisin aeroplane, both lateral and horizontal. The writer believes that Mr. Wright's statement refers only to the lateral stability, for evidently, with the tips of the wings turned down, his machine would quickly tip sideways if no aeronaut were guiding it, and thus the stability does entirely depend on the skill of the aeronaut, although, as the editor has well pointed out, when an aeronaut is in the machine, its stability is much surer than that of the Voisin type. Mr. Lanchester says, "The pressure is less per square foot on the tail (of the Voisin machine) than on the main aerofoil, so that the attitude of the aerodrome to its line of flight is one of stable equilibrium." In the Wright machine, however, the center of gravity is in front of the normal center of air pressure, and the front rudder is always turned upward at a slight angle, so that the attitude of this aerodrome to its line of flight is also one of stable equilibrium, and can be much more surely guided vertically than the fixed-tailed Voisin type. Moreover, it fulfills Mr. Lanchester's second condition of stability, in that the areas and disposition of the surfaces, the amount of inertia, the velocity of flight, and the natural gliding angle are related to comply as well with the equation of stability as that of the Voisin machine, so that any oscillation in the vertical plane of flight does not tend to any increase in amplitude.

ARTHUR HOLLY COMPTON.

Wooster, Ohio, January 19, 1909.

MORE CURIOUS FACTS ABOUT NUMBERS.

To the Editor of the SCIENTIFIC AMERICAN:

Mr. Springer's articles on "Curious Facts About Numbers" have been interesting me much. Though not a profound mathematician, like Mr. Springer, I have given some attention to arithmetic, and among other work I was fortunate enough to hit upon a general method of forming right-angled triangles, in whole numbers, and have often tested its usefulness in teaching junior arithmetic and mensuration.

1. My first formula was $\frac{n^2}{2} \pm \frac{1}{2}$, which gives a right-angled triangle, with $n\left(\frac{n}{2}+\frac{1}{2}\right)$ and $\left(\frac{n}{2}-\frac{1}{2}\right)$ as sides. Thus $3^2 = 9$ and $\frac{9}{2} \pm \frac{1}{2} = 4$ and 5. Again, $5^2 = 25$ and $\frac{25}{2} \pm \frac{1}{2} = 12$ and 13. And $7^2 = 49$ and $\frac{49}{2} \pm \frac{1}{2} = 24$ and 25.

It is easy to continue this series, which gives a right-angled triangle, in whole numbers with every odd number as the short side, the difference of the two longer sides being always 1: 3 gives 3, 4, 5; 5 gives 5, 12, 13; 7 gives 7, 24, 25; 9 gives 9, 40, 41; 11 gives 11, 60, 61; 13 gives 13, 64, 65; etc.

2. My next formula is $n, \frac{n^2}{4} \pm 1$ or $\frac{n^2}{4} + 1$ and $\frac{n^2}{4} - 1$, for even numbers only.

Thus 4 gives $\frac{16}{4} \pm 1$ or 4, 5, and 3.

6 gives $\frac{36}{4} \pm 1$ making 6, 8, and 10.

8 gives $\frac{64}{4} \pm 1$ producing 8, 15, 17.

12 gives $\frac{144}{4} \pm 1$ producing 12, 35, 37.

16 gives $\frac{256}{4} \pm 1$ producing 16, 63, 65.

20 gives $\frac{400}{4} \pm 1$ producing 20, 99, 101.

Some of these results are multiples of those produced by the first formula, but the difference between the two longer sides is always 2.

3. Another formula of the same series gives n with $\frac{n^2}{8} + 2$ and $\frac{n^2}{8} \pm 2$ as three sides, n being divisible by 4, and the two longer sides having a difference of 2. But these sets frequently turn out to be multiples of sets already found, because any multiple of 4 contains the factor 16, when squared, and hence $\frac{n^2}{8} \pm 2$ gives numbers always divisible by 2, and thus the three numbers are often capable of division by 2 or some multiple of 2.

4. Let n be any odd number, and s the number of formula in the series, and we get the general formula for any number of the series:

$$\frac{(2^{s-1}n)^2}{2^s} \pm 2^{s-1}$$

Victoria, B. C. J. G. HANDS, M.D.

THE AURORA BOREALIS AND MOREHOUSE'S COMET.

To the Editor of the SCIENTIFIC AMERICAN:

Prof. S. A. Mitchell's article on the peculiar behavior of Morehouse's comet is very interesting. We might well hope that a study of this comet will give us a much more definite and comprehensive conception of the significance of cometary and allied phenomena. Apparently the greatest change occurred during the night of September 30. Now it was on the evening of September 29 that there occurred such a display of the aurora borealis that you have but to inquire of any man in the northern part of the United States (who happened to be outdoors that evening) from Maine to Washington, to be assured of its wonderful activity, and to realize that it is very rarely that we witness displays that can compare with this.

In a letter from Mr. Sidgraves (Stonyhurst College Observatory) which appeared in Nature October 29, he tells of a magnetic disturbance which coincides in time with this aurora. It seems to be universally granted that the aurora and magnetic disturbances are closely related, also that both may be referred to solar influence. Mr. Sidgraves has shown that the coincidence in the time of happening of the aurora and magnetic disturbance was almost exact. It seems that the violent change in the comet and the terrestrial maanifestations can also be made to coincide in time of happening after suitable deductions have been made, and therefore prove that they were acting under the same influence. Now it was suspected a long time ago that there was a strong affinity between the aurora and comet's tails; this is a unique opportunity for confirming that suspicion.

It has been claimed that there is an 11-hour interval between a solar disturbance and the terrestrial response. Now, then, the distance of the Morehouse comet from the sun on September 30 was about 1.7 times the distance of the earth from the sun. Applying this correction, we should expect to see the comet responding about thirty hours after the same influence had reached the earth.

As near as I can ascertain from the available data, this is precisely what happened. It is almost half a century since Proctor prophetically said that "as surely as the brilliant planets which deck the nocturnal skies are illuminated by the same orb which gives us our days and seasons, so are they subject to the same mysterious influence which causes the northern banners to wave resplendently over the star-lit depths of heaven. Nay, it is even probable that every flicker and coruscation of our auroral displays corresponds with similar manifestations upon every planet which travels around the sun." There is very little doubt of this to-day. What remains is the greater problem, namely, its physical interpretation.

Prof. Mitchell has made an inevitable comparison between Morehouse's comet and Daniel's comet of 1907. Daniel's comet was an "orderly, well-behaved body," while Prof. Barnard regards the Morehouse comet as "the most startling comet since the application of the sensitive photographic plate." There is something else about these two comets in which they differ as widely as possible.

The orbit of Daniel's comet is approximately in the plane of the sun's equator, while the orbit of Morehouse's comet is at right angles to the sun's equator.

Their difference in behavior is just what we should expect from our knowledge of solar activity. It is well known that solar activity varies with the solar latitude; therefore any body that revolved around the sun at right angles to the sun's equator would necessarily experience all the possible range of solar influence that can be ascribed to latitude. It would seem that the influence due to the inclination of a comet's orbit upon the comet is anything other than unimportant.

An examination of all the apparitions of which we possess an adequate record may result in a classification that would help to solve the riddle of the comet.

WILFRID GRIFFIN.

Pittsfield, Mass., January 14, 1909.

MAGNETISM AND THE AURORA.

To the Editor of the SCIENTIFIC AMERICAN:

I take the liberty of presenting for the consideration of your readers some suggestions which seem to me to afford material upon which to found new hypotheses explanatory of the causes of the magnetic streams of the earth, of the aurora, and of the observed coincidence between magnetic and auroral disturbances and the occurrence of sunspots.

If we suppose the earth with its content of iron to be an armature cutting in its rotation the field of force projected from the sun, do we not suggest a valid explanation of the magnetic streams of the earth, which flow from pole to pole, and in the current of which lies the magnetic needle like a ship moored in running water?

Is it not, therefore, a fair inference that the earth, being magnetically energized by its rotation in the field of the sun, projects from one pole, to be received at the other, a stream of one or more of energy's interconvertible forms? The pattern assumed by iron filings sprinkled upon a sheet of paper laid upon a bar magnet suggests the possible direction of the paths taken by this stream.

Is it not fair, also, to assume that the branches of this stream, which radiate from one pole and seem to converge upon the other, in their passage through the rarefied gases of the upper atmosphere, produce the aurora—perhaps by the kind of action suggested by Arrhenius in his theory of the pressure of light?

If the magnetic currents of the earth depend upon the earth's rotation in the sun's field of force, it is permissible to believe that any variation of the intensity of the force of that field must instantly result in a corresponding variation of intensity in the earth's magnetic currents. Such a change might exhibit itself as a change of direction as well as of intensity, owing to the change in the established relationship existing between the earth's currents and the earth's diversely located magnetic content which a change of the intensity of those currents would entail.

Assuming therefore that a change of the intensity of the sun's field entails a corresponding alteration of the intensity (and perhaps direction) of the earth's currents, it follows that their polar off-world streams would likewise be varied in intensity. This being so, the visible expression of these streams, found in the aurora, should vary, and the visibility of the aurora be altered thereby.

It has been observed that during the occurrence of sunspots there are marked magnetic and auroral disturbances upon the earth. The line of reasoning thus far pursued would suggest that at such times there is a correspondingly violent alteration of the intensity of the sun's field in which we rotate. Is it of greater, or of less intensity?

If it be greater, the earth's currents and their various expressions should be of greater intensity; if it be less, they should be correspondingly of less intensity. But should a lessening of intensity in the earth's currents involve only such a change as is without violence in its expressions? Not necessarily; for a curtailment of the received supply of the sun's energy would seem as likely to upset the earth's magnetic equilibrium as would an accession of energy, while in either case violent forms of expression conceivably could result.

Nor is the fact that an amplified auroral display during the existence of sunspots seems to indicate that the play of forces is then of greater intensity, conclusive that such is the case. For it is conceivable that a lesser stream pressure than is normally maintained might be better adapted to render visible the atoms of the rarefied atmospheric gases which, when played upon by the polar streams, become the aurora borealis.

The writer is inclined to believe that a sunspot diminishes the intensity of the sun's field of force, because it lessens the amount of what seems to be its area of greatest activity to which the earth is exposed.

Other phenomena, such as the gradual shift of the magnetic poles and diurnal magnetic variations, may be explained, the writer believes, by hypotheses in harmony with the foregoing.

HENRY A. WISE WOOD.

New York, January 30, 1909.