Correspondence.

Strange Growth of a Tree.

To the Editor of the SCIENTIFIC AMERICAN:

I would like to inquire, through the columns of your paper, if any of the readers of your most valuable publication has ever seen or heard of a branch of any kind of tree starting and growing up through the hollow of the mother tree. If so, I would like to hear from them, either through the columns of your paper or otherwise. I get the paper here. I have a piece of redwood, that I secured at Boulder Creek last summer, that started and grew right in and through the hollow of the mother redwood tree. The piece is about four feet long, and dropped out of the hollow of the log at the mill when the saw cut it free, the remainder being split by the saw and spoiled. JOHN DOUGLAS.

Watsonville, Cal., January 17, 1906.

[While it seems not at all impossible that a shoot of a tree should come up through the hollow trunk of the same tree, it might also be possible that the shoot originated from a seed dropped into the moist, hollow trunk and germinated there, forming a second tree growing within the first. It is very common that a small tree starts from another tree; it may be a different kind. The origin of the new tree is a seed from some fruit which was lodged in a decayed place in the old tree.—ED.]

Should Railroad Speeds be Decreased?

To the Editor of the SCIENTIFIC AMERICAN:

The contribution of Willard P. Gerrish in the issue of January 20, on "Safety on Railroads," has attracted my attention. I agree with Mr. Gerrish that the space interval for train operation is the only way to provide absolute safety, and it lies with the traveling public to determine whether they will railroad safely, or on chances, the latter choice being due to the present craze for fast riding—a craze that is far-reaching, affecting all classes of travel, from the boy on the bicycle to the Twentieth Century Limited. The former works hard to get there as fast as possible; the latter, the same. The officers of the company running this train, will tell you the public demands fast time and they have to meet competition, and the traveler has to stand in the breach. If he gets there safely, all well and good; if not, the company which is trying to supply his demand for fast time is called on to heal his wounds with greenbacks. Now if the public is responsible for this craze and its attending evils, why cannot we have, along with rate regulation, rate-of-speed regulation? We shall live just as long, and make as much money if we do not go from New York to Chicago in eighteen hours. The railroads will make more money in the long run, there will be less loss of life and valuable property. Now, Mr. Gerrish wants to make the long-suffering railroad use up its hard-earned surplus on signals, which he says are not capable of producing infallibility of operation, and as a further protection he wants automatic stops. My experience with the latter is that it is all right in ordinary conditions—for instance, they may be all right in the Subway where ice and snow are unknown; also, in "the good old summer time." But just at the time they are most needed, in a blinding snow storm, they are not capable of doing business, and the engineman, with his bunch of frailties, is the one to fill the gap. Some years since, a prominent road running out of Chicago tried a stop that would even shut off the steam, besides applying the brakes, if the engineman ran signals; but they either did not have a good thing, or did not know a good thing when they saw it. I would again say, let us have speed-rate regulation, backed by law, allowing no train to run above forty miles per hour, and I will show you reasonably safe railroading.

J. V. N. CHENEY,
Division 40, B. of L. E.

So. Portland, Me., January 26, 1906.

Teaching of Science in Schools.

To the Editor of the SCIENTIFIC AMERICAN:

The letters that have recently appeared in your paper regarding the teaching of science in the schools are not only interesting, but the discussion is timely and important. The condition described in Mr. Perkins's first letter (October 21)—that of several men presenting themselves for examination for which they were hopelessly ill fitted—seems to harmonize but too well with what we might easily expect as the result of many elementary science courses in high schools. We could pass this by perhaps with a smile, were it not for the deep conviction that the student has not only failed to correctly estimate the purpose of his course, but has gained from it very little of anything that will compensate for the time and energy spent.

The general impression received from a survey of courses and results in secondary school work in physics is that there is a great deal of hit-or-miss work being done. This seems to be due not to any lack of care on the part of the instructors, who as a class are earnest workers, but rather to a desire to accomplish too much—and that coupled with a vague grasp on methods not always well understood. For example, the physics course is often given complete in one year. The pupil is perhaps getting his first insight into even the most elementary phases of the subject; yet he is given apparatus and set experiments to perform, often with purposely little explanation, and then expected to be ready for college work at the end of the year. We are not astonished that the average pupil gets so little out of his year's course in physics. The wonder is rather that this sort of procedure has so strong a hold upon secondary school work. In a few of our better schools the problem of science teaching is being solved in a manner which seems to be distinctly a step forward. Physics is made the chief (sometimes the only) science subject, and it is offered in two years. During the first year a popular plan is followed, while the work for the second year is college preparatory. Each course is complete in itself, though the first is generally required, whereas the second is optional. The pupil gets his general survey of the subject the first year, and comes to his college preparatory course with the single purpose, and hence the more time, to master fundamental principles—not in the vague hope of escaping some of his college physics, but with the definite object of getting ready for it. Thus the student who looks ahead to college work and the one who wants simply a clearer understanding of common phenomena are served, each to his needs. In an increasing number of schools physical science is being taught in the grammar and sub-grammar grades, not playfully, but so well that the pupil may come to his high school course with a good foundation for work of a college preparatory nature.

There seems to be a growing appreciation of the value of science study as a training in accurate thinking. Moreover, in these days when the startling results of investigation are so closely woven into our daily activities, it is deplorable that the principles underlying these phenomena are not taught in the schools in a manner which shall arouse interest and make the study profitable. This can hardly be done in a course of one year, with pupils who lack previous instruction in the subjects, if the work is governed by the necessity of fitting for college examinations at the same time. In whatever school the subject of physics is first taken up seriously, the study could be profitably directed toward an understanding of those principles that are applied to so many things right at hand, and the hope of passing college examinations at the end of one year should not be allowed to confound this good work. LOTHROP D. HIGGINS.

Danbury, Conn., January 17, 1906.

The Lessons Taught by Parachute Descents.

To the Editor of the SCIENTIFIC AMERICAN:

In 1892 I became dissatisfied with the parachute then in general use, because of its unpleasant habit of diving, plunging, and oscillating. I attempted to do away with these objectionable features, and commenced a series of experiments. I have made in all two hundred and fifty-four parachute descents. Some of the results of these experiments follow. I first tried various alterations in a parachute which I had been using for some time, and which would come down slowly and steadily one day, permitting me to land with the ease of a bird, only to be followed the next day by such violent pitching and swinging (under apparently the same weather conditions) that to land without injury became a matter of difficulty.

I raised the center of gravity from a point 20½ feet below the supporting surface, to 19 feet, then to 18, 17, 16, and lastly to 15 feet, before any decided results were noticed. Here the speed of descent, which had been gradually increasing, became very rapid, due, I believe, to the shortness of the ropes and to a consequent prevention of a wider spread of cloth, thus reducing the diameter of the supporting surface. One would naturally expect oscillations to become more frequent with the center of gravity at this short distance below the supporting surface, yet I found them occurring less frequently than they had been doing at 20½ feet. I then lowered the center of gravity to a point 22 feet below the supporting surface, when the speed of descent again became normal (decreased), oscillations remaining. However, I persisted in dropping the center of gravity to 23 feet, 24, and finally to 25 feet. At this last point no increase in the speed of descent occurred, and the oscillations were fewer in number, but followed by so great an increase in their violence, that the difficulty of landing reached the danger point, and induced me to return the center of gravity to a point 20½ feet below the supporting surface, where I left it. This series of experiments convinced me that the mere raising or lowering of the center of gravity (alone) will not produce stability. I now reduced the area of the cloth (supporting surface) and first removed 68 square feet. This failed to produce an increase in speed of descent, surprising as it may seem. I continued to make reductions of area until I had removed 105 square feet of cloth. The last cut was made after some hesitation, as I feared the speed of descent would increase very much. It did so, to an alarming extent, but this fault was partially compensated for by the ease with which the parachute could be guided, a fact that made it possible to avoid all obstacles in landing. I had sacrificed 105 square feet of supporting surface, leaving but 288 square feet. Yet I (who weighed 144 pounds) was able to land without any great inconvenience to myself. This feat would have been impossible had the oscillations continued, but they had disappeared, due no doubt to the increase of speed of descent. I now rebuilt this parachute by replacing the same amount of cloth that had been removed. I next tried what effect enlarging the opening in the top of the parachute would have. This opening is said to "prevent oscillations," by permitting the escapement of compressed air accumulating beneath the sustaining surface of the parachute. That air does accumulate under a parachute can be seen by the following experiment: Should the aeronaut fail to let go of the parachute immediately on landing, the expansion or other effect of this air accumulated in descent will cause the parachute to jump to one side or the other, jerking him off his feet. I never knew this experiment to fail. That the phenomenon is not due to a breeze is shown by its occurring when no perceptible wind is blowing. In my parachute this top opening was 8 inches in diameter. I increased the size to 9 inches, then to 10, 12, 13, 14, 15, and lastly to 16 inches, at which point oscillations disappeared entirely, and the speed of descent, which had been gradually increasing—as each additional inch was added to the size of opening—now reached a point beyond which I felt I could not go with safety.

A reference to all of the foregoing experiments will show that, to secure the stability and control of the parachute (aeroplane) I was compelled to sacrifice safety, by adding to the speed of descent. (Notice the heavier than air principle here.) The greater the speed the fewer the oscillations, and the less effect any prevailing breeze had to cause a horizontal drifting, and the easier the parachute was to guide.

Now, apply these facts to an aeroplane, and we see at once that a flying machine constructed on the aeroplane system is an exceedingly difficult thing to control. Add to this the desire of the inexperienced operator to avoid (what he calls) great heights, and his usual attempts to glide near the surface of the earth (where the wind always comes in puffs) and we arrive at the real cause of failures and accidents. With the aeroplane so close to the ground, the slightest dive or other variation in its course is very apt to result seriously, because the operator will be unable to control the machine quickly enough to avoid disaster. A greater height would have added to the safety by giving him more time to control its movements. What does a man care how much a parachute (aeroplane) tosses about at a height of five hundred feet or more above the earth? At no time need he feel any anxiety, nor is it necessary to make an attempt to control it until he is much nearer the ground. The faults of the aeroplane flying machine may be many, but why add to them by placing it in the hands of an inexperienced person for operation? More than one good machine has been discarded or remodeled, when the operator was the failure and not the aeroplane. That an intelligent attempt to guide or control some one or more of the various movements of a parachute (aeroplane) is productive of good results cannot be denied; and on nearing the earth, and just before landing, I always try to stop oscillations if possible. If not, I still have another method to try. For instance, we will say the wind is blowing from the west; my parachute would then be drifting east. Now, just before landing, I pull down and hold down the west side of parachute. This will cause it to swerve or dive west, or right against the wind, and it will continue this westward movement until the wind pressure stops further progress in that direction, when of course the eastward drifting would again commence. If, however, I have timed my pulling-down movement just right (and practice has enabled me to do so with some degree of certainty) my parachute will permit me to strike the ground just before the eastward drifting again takes place, or at that instant of time when the parachute has no horizontal motion. I also wish to say that, contrary to prevailing opinion, parachutes have always given me the most trouble by oscillating, diving, and pitching on a day when little or no wind was perceptible. With a fresh breeze prevailing I have experienced no difficulty from these causes. J. J. COUGHLIN.

Versailles, Ohio, January 18, 1906.

Preparation of Mercurial Water. This is prepared with 10 parts of quicksilver, and 11 parts of nitric acid of the specific gravity 1.33 poured on it with the necessary precaution. It is allowed to repose until all the mercury is dissolved, then shaken vigorously, and 540 parts of water added.—Journal de l'Orfeverie.