the color of the clear liquid above the sand. A good idea of the quantity of the sand can be formed earlier than 24 hours, although this period is believed to give best results.

If the solution resulting from this treatment is colorless, or has a light yellowish color, the sand may be considered satisfactory in so far as organic impurities are concerned. On the other hand, a dark-colored solution the sand should not be used in high-grade work such as is required in roads and pavements, or in building construction. An unusually dirty sand or soil high in loam would make a sand unsuitable for use in concrete.

While it is not practicable to give exact values for the reduction in strength corresponding to the different colors of solution, the tests made thus far show this relation to be about as follows:

Color Plate Number	Reduction in Compressive Strength of 1-3 Mortar at 7 and 28 days—Per Cent
1	None
2	10- 20
3	15- 30
4	25- 50
5	50-100

Washing sands has the effect of greatly reducing the quantity of organic impurities present. However, even after washing, sands should be examined in order to determine whether the organic impurities have been reduced to harmless proportions.

APPARATUS

The following list includes sufficient apparatus for making five field tests at a time:

Five 12-ounce graduated prescription bottles.

Stock of 3 per cent solution of sodium hydroxide (dissolve 1 ounce of sodium hydroxide in enough water to make 32 ounces.)

This material can be purchased at a cost of about $1.

CONCLUSIONS

Experience and tests have shown that it is the presence of organic impurities of a humus nature that is responsible for most defective sands. The colorimetric test furnishes a simple and inexpensive method for detecting the presence of such impurities. The test is useful for:

1. Prospecting for sand supplies.
2. Checking the cleanness of sand received on the job.
3. Preliminary laboratory examination of sands.

This test is now being used by a large number of testing laboratories, engineers and contractors in passing on the suitability of sands for use in concrete.

In certain instances the test has been made the basis of specification requirement for sand.—*The Concrete Highway Magazine.*

The Electroculture of Crops*

A Review of Important Experiments in Plant Physiology

By Ingvar Jorgensen, Cand. Phil. (Copenhagen), D.I.C., and Walter Stiles, M.A. (Cambridge)

"The electrification of growing vegetables was first begun in Britain. Mr. Maimbray at Edinburgh electrified two myrtle trees during the whole month of October, 1746, when they put forth small branches and blossoms sooner than other shrubs of the same kind which had not been electrified. Mr. Nollet, hearing of this experiment, was encouraged to try it himself."

This quotation from Joseph Priestley's *History and Present State of Electricity*, published in 1767, records the simple experiment which formed the introduction of the subject of electroculture, and which was soon to be followed by numerous other experiments, which were repeated again and again at intervals, and are still being repeated in our time.

The subject is one the development of which through its various phases is particularly interesting to follow, not because of the achievements of the investigators therein, but on account of the light that is thrown on the factors making for success or failure in the solution of scientific problems.

It is instructive in the light of our present knowledge to take a survey of the field of investigation as it is presented by the works of Priestley and his contemporaries, who worked and wrote when experimental science was still in its infancy. It is interesting to note the variety of observations made by these "philosophers" who were engaged in this preliminary survey of natural phenomena. To us who live in the age of specialization in study this method of attack may appear strange and wasteful, but it is rather astonishing to realize the acuteness of the observational powers of these philosophers, by whose efforts were thus collected a large number of elementary observations, many of which formed the nucleus for enormous development. And the early method of investigation may not have been so wasteful after all, for we know how, today, fresh fields of investigation are opened out by new combinations of subjects.

In reviewing, then, the course of investigations in the time of Priestley, we find that the subject of electroculture was as favorite a one for examination as other branches of electrical science. We cannot avoid asking ourselves, therefore, how it is that while the study of electricity and its many industrial applications has developed into enormous importance, electroculture in the meantime has remained practically stationary for a century and a half, and this in spite of its obvious economic importance.

We probably find the answer to this question in the stagnation of the science of the living plant. The development of electroculture depends not only on the development of our knowledge of pure physics, but also on the development of our knowledge of the activities of the plant. While physics has developed so rapidly, the science of the living plant remains very much where it was when Woodward and Stephen Hales performed their experiments. While some excuse for this may be found in the political and economic conditions which have determined the position of agriculture, the main reason for this state of affairs must be that the science of the living plant has not attracted the genius which has been bestowed on electrical science for instance.

For the sake of simplicity we shall only deal in this article with the form of electroculture in which electricity is discharged through the air to the plants from an overhead wire system, kept charged at a high potential by an electrical machine, or simply charged by atmospheric electricity collected at a higher altitude. This is the only form of apparatus for electroculture which has been employed on anything like a commercial scale, although very numerous experiments have been also made by passing currents through the soil in which the experimental plants are growing.

The subject, even with the restriction which we have indicated in the last paragraph, has such an enormous literature that we shall only attempt to cite the work of a few investigators typical of the various periods in which they made their observations. From these typical investigations we shall see how it appears to be manifest that electricity in certain cases exercises a remarkable influence on plant life, but that, on the other hand, many observers, with apparently as much justification, insist that electricity, if it has any influence at all on plant growth, has a harmful influence. The same discrepancy appears both in the case of small- and large-scale crop experiments. We shall attempt to correlate this discrepancy with other plant physiological investigations and quote parallel examples from recent electroculture research, and thus we hope to impress on the reader that this discrepancy is probably not due to a fault on one or other side of the authors of these discordant observations, but to a lack of realization on both sides of the exact position of the problem—a lack of knowledge of the life of the living plant, and a lack of knowledge of the experimental conditions of the electric discharge employed. This discrepancy, which has lasted for a century and a half, is not likely to be removed before a changed outlook is brought about, but as soon as this results it seems likely that out of all this apparently futile research on electroculture may arise a knowledge useful to mankind. This is likely to be the case not only in the particular example of electroculture, but in all that concerns stimulation of plants.

The first detailed description of experiments comes from the Abbé Nollet, the French court physicist, who, hearing of Maimbray's experiments, tried some experiments himself in the following year, 1747. Nollet filled two pewter vessels with similar samples of earth, and in the two vessels equal quantities of mustard seed were sown. After two days one of the vessels was subjected to the influence of the electric discharge for about ten hours—namely, from 7 a. m. to 12 noon, and 3 to 8 p. m. The other vessel was kept as control in the same room and at the same temperature, Nollet pointing out the usefulness of M. Réaumur's invention for measuring temperature. The next day both vessels were exposed to the sun. On the day following three seeds had germinated in the electrified vessel and produced seedlings three lines high; in the non-electrified control no seeds had germinated. The experimental vessel was again electrified in the evening, for three consecutive hours; the next morning it was found to contain nine seedlings seven to eight lines high. At this time still no germinations had taken place in the control vessel, although towards evening the first seedling appeared in it. In the afternoon of this same day the experimental vessel was again electrified for five hours, and electrification was continued up to the eighth day. At that time all the seeds in the electrified vessel had germinated and were fifteen to sixteen lines high, while in the control vessel only two or three seedlings had appeared, and these were no more than three to four lines high.

Similar observations on the stimulation by means of electricity of the plant in various stages of its life—germination, the growing period, opening of dormant buds—were made by numerous observers at this period, for instance Jallabert, Menon, and Nürnberg.

About this time atmospheric electricity had become a favorite subject of investigation, and we find suggestions to the effect that atmospheric electricity is an important environmental factor in the life of the plant. Thus Father Beccaria of the University of Turin, writing in 1775, says: "With regard to atmospheric electricity it appears manifest that nature makes an extensive use of it for promoting vegetation"; and again: "Besides, the mild electricity by excess (positive electric action of low tension), which, as I have observed for these many years past, constantly prevails when the weather is serene, certainly contributes to promote vegetation, in the same manner as experiments have shewn us that this is likewise the effect of artificial electricity *without sparks*. And is it not likely that the former kind of electricity promotes vegetation still better than the latter can do, since nature increases it and lessens it in such circumstances and at such times as particularly require it?" Similar views were put forward at the same time, and with perhaps even greater force, by the Abbé Bertholon, who designed an apparatus, the electro-vegetometer, for collecting atmospheric electricity and distributing it over growing crops.

There were, however, a few observers who concluded from their experiments that electricity was either harmful to vegetation, or at least did not stimulate it. The earliest of these appears to be Koestlin, who in 1775 reported that negative electricity is harmful to vegetation; but probably it was the testimony of the famous Dutch physicist Ingen-Housz which carried most weight, and who on account of his repute as a plant physiologist, has been often quoted in favor of the theory that stimulation of vegetation by electrical means is impossible. However, as Ingen-Housz connected his plants directly with the collectors, and he describes the plants as shrivelling up after treatment, his experiments simply indicate the possibility of killing plants by electricity.

In 1789, the year following Ingen-Housz's more elaborate experiments, D'Ormoy found that the electric discharge stimulated the germination of mustard (and lettuce) seed. This, it will be observed, was Nollet's original experiment.

The few experiments we have cited of the many performed in this first period of half a century are not only typical of the eighteenth century, but we find the same repeated in the nineteenth century; the experiments differ only in the people who perform them, in the means of producing the discharge, and in their being usually conducted on a larger scale; but in all we find the same absence of any realization of the actual position of the problem.

We find that a lively interest in the action of atmospheric electricity on the plant had developed in this country before the end of the first half of the nineteenth century. Thus Forster in 1844 reported the results of some experiments with chevalier barley in which atmospheric electricity was collected by a horizontal wire and conducted to the soil by vertical wires at either end of the horizontal one. Forster found the electrified crop assumed a darker green color and grew more rapidly than the non-electrified control, while the yield of grain from the electrified plot was double and the yield of straw triple that of an average crop. In 1846 William Sturgeon, lecturer at the Manchester Institute of Natural and Experimental Science, went so far as to say that Forster's experiments "have commenced a new era in electro-cultural enquiries; and their flattering results have induced several persons, electricians and others, to try the same plan on crops of various kinds of the last year's growth." However, it was no new era that was inaugurated by Forster's experiments; it was simply a repetition of older experiments that resulted, the methods employed often being less refined than those of

*From *Science Progress.*

earlier workers. Nevertheless, Sturgeon's paper on the Electroculture of Farm Crops, published in the *Journal of the Highland and Agricultural Society* for March, 1846, contains some very trenchant remarks on the position of the subject. At that time the importance of manuring was coming into great prominence as a result of Liebig's researches and writings, yet Sturgeon realized that, in spite of the obvious benefits that were resulting from the application of pure chemistry to the questions of soil and plant a greater knowledge of the life-activities of the plant would be required before agriculture could develop to its fullest extent. As this writer has been completely neglected, we feel justified in giving one or two quotations from his paper. He says, for instance: "By what powers, or by what physical forces, do the organs of plants display, and keep in operation, their respective functions of vegetable life, is a problem of vast importance in the basement of agricultural science, and in every other branch of vegetable culture. This grand problem, the solution of which has not yet been accomplished, nor, indeed, scarcely attempted, presents the most formidable, and, at the same time, the most noble bulwark yet to be assailed in our inquiries respecting the functions of vitality in the vegetable kingdom." And again: "The rules of his art will always enable the practical chemist to be of much service in providing food for plants, although it may require a higher order of investigations than those he is in the habit of pursuing to discover the character and operations of those forces which stimulate the organs of vegetables to avail themselves of the food thus supplied for their use."

However, in spite of these laudable views put forward in 1846, the subsequent history of electroculture is little else than a repetition of the earlier. An enormous number of researches have been conducted on the subject. but the vast majority are on exactly the same lines as the older ones, and the results are similar, *i. e.*, the majority show favorable influence on germination, growth, and final yield resulting from electrification, a minority show no such improvement resulting. No leading principles are brought out and no contributions from other branches of science throw light on the subject.

It was during this period that the science of the physiology of plants was obtaining a certain amount of recognition, but unfortunately instead of developing along its own lines as the science of the living plant, and evolving its own guiding principles, it became subservient on the one hand to morphological botany, on the other to chemistry, with the result that although much has become known about individual processes in isolated organs over short periods of time, and still more conjectured, we know next to nothing about the interrelation of the various processes which make up the life of the plant and their variation in different phases of that life. It is no wonder, therefore, that the scientific agriculturist has always been very sceptical of the results obtained in plant physiology, and that there has resulted an apparent discrepancy between the results obtained in agricultural practice and in physiological experiments.

Hence it is not surprising that in regard to the contributions of plant physiology to electroculture there is very little to be said. In all the physiological researches conducted in order to solve problems of electroculture there is a lack of realization of the electrical problems involved and a neglect to inquire into the progress of pure physics, so that there is a general idea that it is unessential in these experiments to trouble about the conditions of discharge of electricity, and that results obtained, for instance, from experiments with currents of low E.M.F. through germinating seeds can be used as arguments in regard to experiments with electric discharge through air on actively growing and maturing plants.

A favorite subject of investigation by the physiologists was the effect of electricity on *protoplasmic movement*. In 1837 Amici and in 1838 Becquerel and Dutrochet studied the influence of an electric current on the movement of protoplasm of *Chara vulgaris*. Similar experiments were conducted at a later date by Heidenhain (1863), Kuhn (1864), and others, on protoplasmic movements in the leaves of *Vallisneria*. Similar experiments have been performed up to our own day, but nothing fruitful for our subject has resulted from them.

A few investigators have attacked the problem from the point of view of determining whether the beneficial effect of electroculture is due to an acceleration of the *assimilatory process*. For this purpose a current was passed through a piece of a water plant, as by Thouvenin (1896), or through an aerial leaf, as by Pollacci (1907), and in each case an increased rate of assimilation was recorded as a result of the passage of the current. But these experiments are open to so much criticism that it is impossible to draw any conclusions from them. This holds also for arguments derived from *in vitro* experiments, such as those of W. Lob (1905), who argues from the supposed formation of formaldehyde from carbon dioxide and water under the action of the silent discharge, to the increase in crop production resulting from the electric discharge.

It was proposed by Gassmer in 1909 that the beneficial effects so often observed as a result of the electric discharge are chiefly due to an influence on the *transpiration* rate. This writer observed that more than twice as much water was transpired by the plants subjected to the electric discharge than the non-electrified control plants. He suggests that this is simply brought about by the formation of air currents by the silent electric discharge, and these alone would be sufficient to explain the increase in evaporation. However, the general criticism we have levelled against all the physiological experiments dealing with electroculture hold for these as for all the others. But that the water relations of the plant are influenced by the electric discharge is an opinion fairly generally held. Thus Nollet was struck with the more rapid rate of evaporation from electrified liquids than from non-electrified.

The *respiration* of plants under various electrical conditions has formed the subject of an investigation by Knight and Priestley published in *Annals of Botany* for 1913. However, these various electrical conditions are not those of the actual electrocultural experiments in the field, and the results of many of the experiments of these authors, as they themselves admit, are of dubious interpretation on account of the experimental arrangement.

Recently a paper appeared by R. Stoppel in which the cause of the *movements* of the leaflets of *Phaseolus multiflorus* was traced down to atmospheric electrical changes, and the author from this observation proceeds to far-reaching generalizations as to the importance of atmospheric electricity in the life of the plant.

We see, therefore, that the physiological investigations in reference to electroculture have dealt with numerous plant processes: assimilation, transpiration, respiration, irritability, protoplasmic movement. In no one case have the experiments been conducted in such a way that they give us any information as to the influence of any definite electrical conditions on any one of these processes at any definite stage in the history of any plant or any plant organ. How much farther off are they therefore from even suggesting a solution of the problems of electroculture?

Exactly similar criticism can be levelled against those who regard the benefit resulting to growing crops from the electric discharge as due to changes brought about in the soil; with this school it is therefore not necessary for us to deal further.

It is strange that the greatest and most remarkable contribution to electroculture should have come from a physicist, namely S. Lemström, who was Professor of Physics in the University of Helsingfors. His work is remarkable not only because it was the first in which comparatively large areas of land under crops were subjected to treatment, but because he pursued the subject with great energy through his lifetime right up to his death in 1905. He carried out experiments in many countries under a great variety of different conditions, and so collected a great deal of empirical information as to the conditions under which the discharge affected the life of the plant. Further, he made many experiments with the object of discovering the manner in which the discharge affected the plant, but in this matter his lack of knowledge regarding the life of the plant prevented him from expressing himself in a way which would appeal to plant physiologists or scientific agriculturists. Nevertheless, it is likely that some of his observations and ideas may prove to be sound when correlated with later experiments. His researches make it clear that the overhead electric discharge will affect the life of the plant in all its phases: germination, vegetative growth, and maturation. Lemström in his experiments used the undirectional discharge from an influence machine, but he obtained favorable results when either the positive pole or the negative pole of the influence machine was connected to the overhead network. Similarly he was able to obtain favorable results whether the discharge was applied in the daytime or at night. He sums up his experience, however, by stating that the best results are obtained (1) with the network positively charged; (2) by applying the discharge in the morning and the evening; and (3) by having the general conditions favorable for plant growth.

Although Lemström used an influence machine giving a very high potential, the overhead network in his experiments was only charged to a few thousand volts or even less. That neither he nor subsequent investigators should have derived advantage from this observation for the construction of more efficient apparatus is at least surprising. All experimenters appear to have been obsessed with the idea that extremely high voltages are necessary, while at the same time in the construction of apparatus they neglect arranging for sufficient output of current.

It is unfortunate that none of the physicists who associate their names with electroculture investigations should have taken the trouble to work out the physical questions involved. It was left to a pure physicist, working from quite another point of view, to put forward the considerations which enable us to formulate the conditions of the discharge (see J. S. Townsend, *Phil., Mag.* 1914).

That the publication early in this century of Lemström's book on "Electricity in Agriculture and Horticulture," translated into several languages, should have given rise to a large number of fresh experiments is not surprising. It is unfortunate that neither plant physiology nor scientific agriculture should have been sufficiently advanced at this time to give their contribution to the subject. As a result recent development has consisted almost entirely in the application of all possible devices for the production of high-tension current which electrical industry has evolved for other purposes. However, a certain amount of empirical information has been collected, for instance, in this country by Mr. J. E. Newman, Mr. William Low, and Miss E. C. Dudgeon, but our knowledge of the subject is not greater than when Lemström left it, in spite of improvements in apparatus.

We hope we have made it clear in the foregoing that the history of electroculture presents an ever-recurring cycle of experiments having as their object the proof or disproof that the electric discharge has a beneficial effect on vegetation. That both these results should be obtained regularly in the cycle should be sufficient to inform us that there must be something fundamentally wrong in the method of inquiry. This, in our opinion, is to be found in the neglect of quantitative measurement of the discharge, and in the lack of knowledge of the science of the living plant. This may perhaps be more easily understood if we give a few details of a parallel series of investigations by H. Molisch, published in 1912.

Molisch was concerned with the influence of radium emanation on plant life. We find in Molisch's work a realization of the principles of the application of stimuli, that the effect depends on the intensity of the agent and the length of time during which it is applied; further, that the effect of the stimulus may first appear a considerable time after the application. Molisch gives evidence that he possesses a good knowledge of the living plant; thus he realizes that the same agents react quite differently on a plant in its resting period, in its period of germination, and during its active growth.

Molisch, therefore, from his first empirical experiments which showed that radium emanations would induce growth in a resting organ (flower-buds) in winter time, does not conclude that radium emanation will always have a beneficial influence on plant life. He determines by a carefully planned series of experiments that a definite quantity of radium emanation applied for a definite period will induce the opening of flower-buds; but if the organ is not in its resting condition, application of the emanation makes no difference in the rate of development. Further series of experiments deal with the action of various quantities of emanation applied over definite times, to germinating seeds or growing seedlings. It was generally found in these cases that a much smaller quantity of emanation than that applied to resting organs has a distinctly harmful effect and hinders development, but in the case of certain species, with extremely small quantities of emanation a slightly increased rate of growth was observed.

It is not surprising that such a series of experiments should have given rise all over the world to experiments on the value of radio-active manure, but it is remarkable that some of the investigators, in whose experiments an arbitrary and unknown quantity of agent was used and which was allowed to act throughout the whole season, should argue from the negative results obtained by them that radium emanation is without influence on plant life, and Molisch's experiments are consequently disproved.

There thus seems to be the possibility of the production of a cycle of experiments, dealing with the use of radium emanation, similar to that which already obtains in regard to electroculture, and this in spite of Molisch's original well-planned work.

If we now take in review the electroculture experiments and consider them in the light of the work on radium emanation with which we have just dealt, we find that all investigators show the same lack of realization (1) of the necessity for quantitative measurement of the electric discharge, (2) that a stimulus may act differently on the plant at different stages of its life, (3) that the effect of the stimulus depends on its intensity, (4) that the effect of the stimulus depends on the time for which it is applied, and (5) that the effect of the stimulus may appear a considerable time after it is applied. Numerous examples of this can be quoted.

That in the electric discharge we possess a means which can be used as stimulus at most periods of plant life seems clear from the greater number of experiments, but how indifferent, or even hostile, plant physiologists have been to the general conception of stimuli is perhaps best illustrated from some experiments of Gassner on electroculture published in 1907. First of all he finds that with *Pisum* and *Helianthus* the electric discharge has no influence on germination and growth, the electrical treatment lasting eight to fourteen days, and being used for fourteen hours daily. The electric discharge was so strong that light phenomena appeared round the plants and the plants were black on account of precipitated dust. But with barley he concludes that the discharge has a favorable influence. We may quote in detail one of his experiments on this plant. The electrified series consisted of three pots where the distances between the discharging points and the plants were respectively 10 cms., 21 cms., and 35 cms. On March 12 thirty barley grains were sown in each pot. On March 16 the seedlings began to appear, and on the same day the discharge was started. On March 17 all the seedlings had appeared; no difference was visible between any of the experimental pots, nor between experimental and control. On March 18 the electrified were visibly in advance of the control and more in advance the smaller the distance between the discharging point and the plant. On that day the first leaf had appeared in 16 plants in the pots 10 cms. from the discharging point, 12 in the pot 21 cms. from the point, and 4 in the pot 35 cms. away; the numbers for the two controls were 1 and 3. In the further course of the experiment the electrified developed quicker than the control, but soon the plants farther away from the discharging point were ahead of the others. On March 27 the experiment was stopped.

Consideration of Molisch's experiments will render these observations readily understandable. It is clear that Gassner was dealing with a phenomenon of the same type as Molisch, in which the intensity of the agent and duration of its action determine the result. It is regrettable that such a view should not have presented itself to Gassner and that he should simply have attempted to explain his experimental results in the light of a single process, namely, evaporation of water.

Limitation of space has prevented us in this article from going into detail in regard to the very numerous experiments which have been performed on electroculture. We hope, nevertheless, we have made clear our main object, which is to emphasize that the condition for development of electroculture is a sounder outlook in plant physiology.

It is true that in the past the contributions of plant physiology to our subject have been negligible, and what advances have been made have come from the physicist. Nevertheless, although a knowledge of physical methods is essential for intelligent research in electroculture, the problems involved are essentially problems of plant physiology. With a sounder outlook in plant physiology, the apparent discrepancy between plant physiology and agriculture should disappear, and great developments in the methods of crop production should take place. It is only when research, based on this new outlook in plant physiology, has become established, that we shall be able to judge how far the application of stimuli, including the electric discharge, may be of economic importance in the production of crops.

The Standardized Infant

Dogmatic teaching is recognized as a useful method of introducing students to a subject which is new to them, and represents an economy of effort on the part of both teacher and pupil. It has its place even in regard to subjects in a state of flux, where any standard set up must be an artificial and temporary one. Such standardization is more proper on the teaching than on the research side of any study, and it may, if unchecked, lead in the long run to stagnation and to want of progress in directions where change is the inevitable accompaniment of growth. In the campaign on behalf of the health of women and children, successfully waged by the Royal New Zealand Health Society for many years past, standardization of method has proved fruitful of great results. Initiated by Dr. F. Truby King at the time when Lord Plunket was governor of the Dominion, and carried through with infinite patience over a period of discouragement, the scheme now includes 35 nurses whose general training of three years or more has been supplemented in some cases by a further year of maternity work and in all cases by three months or more of special study at the Karitane-Harris Baby Hospital. These Plunket nurses carry out over the whole area of New Zealand a series of health recommendations which in course of time have crystallized into three pamphlets, written by Dr. Truby King and issued by the Public Health Department, on the expectant mother, the baby in the first month, and the feeding and care of infants in general. Personal work of this kind has been assisted by wholesale cooperation on the part of the New Zealand press which prints some 200,000 copies weekly, under the heading of "Our Babies' Column," of instructive matter supplied to it by the Health Society. Coming from such a successful and comprehensive scheme of dogmatic teaching, Dr. Truby King is now paying a welcome visit to this country in the hope of contributing to a similar health campaign, especially in the direction of forming a general consensus of opinion in regard to elementary matters of physiology and pathology of infant life. He has undertaken the charge of a clinic placed at his disposal by St. Thomas's Hospital. It is true that the National League for Health, Maternity and Child-Welfare has issued from its office a booklet in pamphlet form dealing with matters of maternal and infant hygiene, but the recommendations contained therein are of a general character, arrived at in the spirit of compromise, or in much the same way as the highest common factor is obtained from a group of large numbers. Dr. Truby King uses a percentage whey mixture which has become the standard method of artificial infant feeding at the Antipodes, and involves the use of lactose, which in this country is now both scarce and costly. Here and in America there is still an almost infinite divergence of practice, which may, of course, be evidence that the one ideal method of artificial infant feeding has not yet been found, but it is, at all events, a testimony to the infant's power of adaptation to his environment amounting to little less than genius. To take a single example, in London the infant whose parents cannot afford to give him fat in any of the usually approved forms has shown his capacity to digest and assimilate linseed oil in suitable emulsion, although the Bradford baby is stated to rebel at the substitute. The necessity of adding lactose to the milk mixture in artificial feeding has long been disproved by the experience that any form of sugar is equally well assimilated by the London infant if given a little time for the necessary adaptation. To adopt in London the humanization of artificial milk on the New Zealand plan would therefore, appear to be retrograde and costly, a fact to set against the advantages of a simple dogmatic scheme of teaching infant welfare workers. The successful breeding of calves and pigs on a large scale is undoubtedly expedited by the adoption of routine methods, but we have seen during the last few months many changes dictated by war conditions to which the youthful cattle have accommodated themselves with a good grace. It is, at all events, arguable whether the human infant should be standardized, and the more the important points involved are examined the better for our knowledge, assuming, of course, that only those speak who have personal experience of the subject.—*The Lancet.*

High Flying in Modern Aeroplanes

The most marked development in the modern machine is its extraordinary capacity for climbing to a great height in a short time. At the beginning of the war the average height flown on active service was 4,000-5,000 feet, simply because few of the machines then in use with the impedimenta carried could get much higher. Today a height of 20,000 feet is, I believe, on certain occasions reached, and it is fairly certain that if progress continues at its present rate, heights a great deal beyond this figure will be reached as a usual thing. These great altitudes bring forward many difficulties which will have to be seriously considered. The first trouble in the winter will be the extreme cold to which the occupants will be subjected unless they are protected by special cowling. This, to a certain extent, is the natural advantage obtained in the tractor. The question of the difference in the comfort of machines in this respect was shown to me in a very marked manner last winter. I was testing the fall-off of engine power at a height on a tractor two-seater in which it was specially arranged that the warm air from the radiator and engine passed along the fuselage to the pilot and then to the passenger, and although at a height of over 21,000 feet with the thermometer below freezing at ground level, I did not suffer in the least from the cold, neither did my passenger who sat behind complain until we shut off to descend. As a contrast to this, a few days later I was on a single-seater scout at an altitude of 17,000 feet, and although it was a tractor with a rotary motor, I suffered intensely from the cold, and became so numbed that my vitality must have been something akin to a dormouse under the snow, and, in spite of being well gloved, I had frostbitten finger tips which pained for many days afterwards. Surely this is a very inefficient state for a pilot at the front to have to take on an air fight or other exacting work? Put two pilots up to a great altitude, one kept well warmed and comfortable, the other half dead with the cold, and it would be easy to surmise which would be most likely to do the best work.

I really believe it is more by accident than design that the pilot or passenger has benefited at all in the past from the heat of the engine, with the exception perhaps of the late S. F. Cody's machine. He purposely placed the radiator of his pusher in front of the pilot to keep him warm. I know from my experience when flying in France in the cold weather that the discomfort owing to the extreme cold became intense when flying only at 6,000 feet on a two or three hours' reconnaissance flight. This is a point to which designers should give attention, as machines are now capable of reaching great heights.

Cold also affects the motor pretty seriously. This is more noticeable with the water-cooled type. Unless some provision is made for blanketing the radiator surface at heights, it becomes far too cold for efficient running. Cases are known of the freezing of the water system on a descent from a great height, with pretty serious results to the motor, as well as the difficulty of getting the engine to run again efficiently through being too cold to effect a landing. In the future war machine the pilot must have a very wide range of control over the water-cooling system.—An abstract from a paper before the Aeronautical Society of Great Britain, by Capt. B. C. Hucks.

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