

LETTERS TO THE EDITOR.

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The Stability and Efficiency of Kites.

IN NATURE of March 17 Mr. Dines suggests that the instability of kites may be due to changes in the relation of the weight to the velocity of the wind, or to deformation of the kite by excessive pressure, or that there is a critical velocity at which the forms of the stream-lines become so altered that instability results.

My own experience leads to the opinion that deformation by a strong wind is practically the only important cause of instability. At the Blue Hill Observatory, where the conditions for experimenting are unusually severe, the Clayton modification of the Hargrave kite is the only one that can be employed. In this form the longitudinal sticks are continuous from one cell to the other, and the lateral sticks form the front and rear edges of the cells which prevent the fluttering of the cloth unavoidable in kites the rigidity of which depends upon the tension of the covering. The Clayton-Hargrave kite is rigid, but there is no strain or tension anywhere except when flying, and it has proved itself to be sufficiently stable. Relief from sudden and excessive strain is necessary, and to secure this the bridle is made elastic, so that in strong winds the angle of inclination becomes smaller. Thus equipped, the kite is not uniformly efficient in strong winds, for, as the angle of inclination becomes smaller, the pressure of the wind upon the edges of the cells becomes relatively greater, and the altitude is reduced. A normal altitude of 55° to 60° may be lowered to 40° by an increase of wind of 15 to 25 metres per second. If the front cell of the kite is equipped with rigid curved lifting surfaces the efficiency is greatly increased, the mean altitude exceeding 60° , and the loss due to increase of wind is unimportant in velocities up to 25 metres per second.

Some of the lightest of these kites have flown in the strongest winds encountered while experimenting, the velocity in some instances having exceeded 30 metres per second. An interesting example of this kind occurred on April 14 during the international ascension. Two kites, weighing 600 and 850 grams per square metre, and having lifting surfaces of 11 and 7 square metres, respectively, were employed to lift the line. The outer section of the line was 1500 metres of wire having a tensile strength of 140 kilograms, and the next was 2500 metres long, having a tensile strength of 180 kilograms. The large light kite was placed at the outer end of the line, and the other at the junction of the two sections. At a height of 2000 metres, with 3500 metres of line out, two gusts of wind resembling thunder-squalls were encountered, the mean velocity for twenty minutes exceeding 30 metres per second, and the maximum reaching 33. The strain on the line at the ground did not exceed 90 kilograms, and, allowing for the weight of the line, probably did not exceed 110 at the second kite. The pull of the larger kite in a 10-metre wind is usually about 45 kilograms, and that of the smaller about 35, and, allowing for the pressure of the wind on the line, this, apparently, was not greatly exceeded. The large kite will fly in a wind of 5 metres per second, and was perfectly steady in a velocity of 33 metres per second. The pressures corresponding to these velocities are, respectively, 2 and 80 kilograms per square metre of surface exposed normally; hence it seems improbable that a well-made kite could become unstable through disproportionate weight or some unusual property of a high wind. It should be said that the velocities given are "true" velocities, and not to be compared with those from the large Robinson anemometers, in which the factor 3 is employed. The maximum velocity referred to, expressed in English units, becomes 74 miles per hour "true" velocity, 90 when reduced to the U.S. Weather Bureau standard, or about 100 miles per hour when reduced to

the same scale as the Kew pattern when the factor 3 is employed.

In 1900, while comparing different wires for use as kite-lines, I found that, theoretically, the larger wires were the more efficient, although slightly weaker, weight for weight, than the smaller. The reason for this is that the pressure of the wind is more effective upon the small wires than on the large. A No. 10 wire weighing 2.16 kilograms per 1000 metres usually breaks at 85 kilograms. Its diameter is 0.61 mm., and the surface presented to the wind is 1 square metre for each 1650 metres of length. If we wish to double the strength of our line we employ a wire 0.93 mm. in diameter, weighing 5 kilograms per 1000 metres of length. The cross-section, however, has increased only one-half, the surface presented to the wind being 1 square metre for each 1100 metres of length.

An opportunity to secure experimental data did not present itself until January, 1908. Since then, in conducting the monthly kite ascensions at Blue Hill, I have employed small kites flown with small wires, and large kites flown with large wires, to determine the relative efficiency of the two systems. The results show very conclusively that the system of large kites and large wires is the more efficient, not only for the sizes experimented with, but very probably for much larger sizes. The lifting surface of the kites employed has varied from 3 to 13 square metres, and the line has been made up of pieces of wire varying from No. 10, of 85 kilograms, to No. 21, of 235 kilograms, tensile strength.

The opinion, held by many, that large kites are inferior to small kites in meteorological work is not sustained by these experiments. The Clayton-Hargrave kite when built with three sections can be made stronger for the same weight than when made with two or four sections. The increase of weight as the size increases is unimportant in meteorological experiments, for kites with lifting surfaces exceeding 15 square metres need not weigh more than 650 grams per square metre. The ability of these larger kites to withstand high winds apparently is greater than that of small kites, for the large and heavy sticks necessary in the framework, like the large wires, present relatively a smaller cross-section to the wind for a proportionate weight and strength.

Increased stability may be secured by placing two diverging vertical planes in the rear cell of a kite. If these planes are adjustable, the kite may be caused to fly on either side of the mean direction of the wind, or any errors of flight may be corrected.

The entire question of stability appears to be one of eliminating unequal strains and unnecessary resistances.

F. P. FERGUSON.

Hyde Park, Mass., U.S.A., April 20.

I AM much interested in Mr. Fergusson's letter, and his long experience with kites, about double my own, makes me very diffident about expressing an opinion contrary to his.

Doubtless deformation is a very fruitful source of instability, but after carrying out some thousand kite ascents from a steamer and on land, I am of opinion that it is not the only cause. However, my position is that we do not know with certainty the cause of instability, and it is very desirable at the present time that we should know beyond dispute.

I agree with Mr. Fergusson as to the advantages of large kites; they are more stable than small ones, and, as he has shown, since the wind resistance on the wire is the one serious obstacle to reaching great heights, it is obviously desirable to make that resistance small in proportion to the other forces. But there are practical objections. Large kites and thick wire require a stronger and more expensive outfit, and more assistance at starting and landing; also, should an accident occur, the risk of its being serious is far greater.

I do not agree with Mr. Fergusson that the Clayton-Hargrave kite is the only one that can be used when the wind is strong. The conditions in England in the winter are probably more severe than at Blue Hill, and have been particularly severe during the last winter. Nevertheless, the strength of the wind has on no occasion prevented our

flying a kite with non-rigid edges at Pyrton Hill, and we have been fortunate in breaking only one kite in landing it, and in not failing once since last October to bring back the kite to the starting point without accident; also Mr. Cody's kite, which has non-rigid edges, will certainly fly in a strong wind.

Mr. Fergusson states that a kite of 7 square metres surface will exert a pull of 35 kilograms in a wind of 10 metres per second. In English units this is equivalent to 1 lb. per sq. foot of sail area in a wind of $22\frac{1}{2}$ miles per hour. If the whole area were exposed normally to the wind, the pressure or pull would be 53 kilograms, and hence, remembering that the back sails are partially sheltered by the front, and that the angle of incidence is only about 15 degrees, 35 km. seems a very high value. The pull of a diamond-shaped box kite of 7 metres sail area in the same wind is certainly below 15 km. It would be interesting if Mr. Fergusson would tell us how the wind at the kite is measured at Blue Hill. I do not think any anemometer placed in the kite can be trustworthy—one might as well place one close to the roof of a house amongst a set of chimney stacks—and if an anemometer is placed on the wire there is the difficulty of avoiding oscillation and of correct orientation.

I am glad to be able to state that we do not now officially publish in England values of wind velocity based on the factor 3 for the Kew pattern Robinson cups, but on the factor 2.2. This reduces what would have been called 100 to 73; but the values quoted by me in my letter of March 17 are entirely independent of the Robinson anemometer. For reasons fully given in a recent publication of the Meteorological Office (M.O. 202) those values are doubtful, but the evidence is in favour of their being too low rather than too high.

I should like to take this opportunity of replying to Mr. Gold's criticism of the method of measuring wind velocities on a kite in England, namely, by measuring the tension of a piece of cotton carrying a light sphere at the far end, away from, and out of the influence of, the kite (NATURE, April 21). It is true that the surface of the cotton exposed to the wind is comparable with that of the sphere, but the force is for all practical purposes a normal one, since the tangential component is admitted by all to be very small. It follows that the tension of the cotton, neglecting its weight, is the same throughout, just as in the case of the string stretched on a smooth curve given in text-books on mechanics, and hence the tension measured is the tension of the cotton where it is tied to the sphere, and is independent of the length.

W. H. DINES.

A Difference in the Photoelectric Effect caused by Incident and Divergent Light.

RECENT investigations have shown that the ionisation produced by the secondary rays arising from a thin metal plate traversed normally by a primary beam of Röntgen or γ rays is greater on the emergent than on the incident side. The present experiments were made to see if a similar effect could be detected with ultra-violet light.

Thin films of platinum were prepared by sputtering from a platinum kathode on to quartz plates 1 mm. thick. These could be mounted in the centre of two similar brass cylinders so that their planes were perpendicular to the axes of the cylinders. A narrow beam of ultra-violet light from an arc passed down the axis of the two cylinders normally to the plates. The saturation current from the illuminated plates to the cylinders could be measured. The plates could also be turned so that the film side was either away from (position A) or towards the light (position B).

In every experiment two similar plates were used; one was used as a standard to determine the strength of the ultra-violet light, and its position, whether A or B, was unchanged. The other plate was compared with this for each of the two positions alternately. By referring each measurement to the standard plate, the otherwise troublesome variations of the arc were rendered harmless. Unless the films were very thick it was always found that position A gave rise to a relatively greater photoelectric current

than position B, although it was penalised by having to pass through the thickness of the quartz plate.

When no allowance is made for the absorption by the quartz, a very thin film gives 12 per cent. more photoelectric current for the emergent than for the incident light. When the absorption of the quartz is allowed for the difference is increased to 16 per cent.

These results have been confirmed by reversing the direction of the light without altering the position of the plates, and other experiments have been made to ensure that they do not arise from scattered light or other defects in the apparatus. The ratio of the emergent to the incident effect has been determined for a series of films of varying thickness.

This investigation was suggested to me by Prof. O. W. Richardson, and the experiments have been carried out under his direction.

OTTO STUHLMANN, JUN.

Palmer Laboratory, Princeton, N.J., April 26.

A Link in the Evolution of the Bees.

THE ligula or "tongue" of the bees presents two main types, one broad, obtuse, and often emarginate, the other pointed, acute, frequently much elongated. The obtuse-tongued bees have been considered to be the more primitive, and there is no doubt that the most advanced types are long-tongued. The difference between the two groups has seemed so important that at one time (Trans. American Entomological Soc., xxix., p. 185) I entertained the idea that they had no common bee-ancestry, but were derived from different groups of wasps.

Frederick Smith, in 1853, described a new genus of bees from Australia under the name *Meroglossa*. This was based on a male from Port Essington, which had many of the characters of the obtuse-tongued *Prosopis*, but had a pointed, dagger-like tongue. Ashmead, in 1899, placed it in the same group as *Prosopis*, in spite of the tongue; in 1905 (Trans. Amer. Ent. Soc., xxxi., p. 318) I gave an account of Smith's type, remarking that it was "not unlike some *Prosopis*." In 1905 I described a number of Australian species supposed to belong to *Prosopis*, but remarked of one of them (*P. turneriana*) that the mouth-parts did not seem to agree with the genus. I had at that time no material for dissection, but Dr. R. C. L. Perkins had such material, and discovered that several had acute tongues. In Proc. Hawaiian Entom. Soc., October, 1908, he founded the genus *Palaeorhiza* for my *P. perviridis*, with the following interesting remarks:—

"*Palaeorhiza* is evidently represented by many species in Australia. Several have been described as belonging to the genus *Prosopis*, in spite of the fact that the most superficial examination shows that these insects have an acute lanceolate tongue. Hitherto no connecting-link between the blunt-tongued and acute-tongued bees has been recorded, but in *Palaeorhiza* we have a form which, except for the structure of the tongue, would be assigned to the section of *Obtusilingues*. It will therefore be obvious that this section and the *Acutilingues* can no longer be maintained as of great importance, since *Palaeorhiza* must always be associated with *Prosopis*, as the male genital characters, and all other ones, save the lingual, clearly show."

Nevertheless, he proposes for *Palaeorhiza* a distinct family, *Palaeorhizidae*, at the same time suggesting that it should be *Meroglossidae* if *Meroglossa* is allied.

In the course of going over the splendid collection of Australian bees formed by Mr. Rowland E. Turner, now the property of the British Museum, I have been able to examine the structure of a number of species of *Palaeorhiza*. In the first place, I find that *Palaeorhiza* and *Meroglossa* are substantially the same genus; but the truly astonishing thing is that the females have broad, obtuse tongues like *Prosopis*, while the males have sharp, dagger-like tongues! I first discovered this in *P. penetrata*, subsp. *percrassa* (properly *Meroglossa penetrata percrassa*), a black insect with the face of the male canaliculate, much in the manner of the original type of *Meroglossa*. My natural thought was that there must be two species, in spite of every appearance to the contrary. I next took