perature with a pull equivalent to a negative pressure of 25 atmospheres, by completely filling a bulb with alcohol, and then cooling it. The alcohol in contracting strains the bulb inwards; and finally, when the tension becomes very great, parts from the glass with a sharp "click."

To realize a portion of the other bend of the curve, an experiment has been devised by Mr. John Aitken. It is as follows:—If air—that is, space, for the air plays a secondary part—saturated with moisture be cooled, the moisture will not deposit unless there are dust-particles on which condensation can take place. It is not at first evident how this corresponds to the compressing of a gas without condensation. But a glance at the figure will render the matter plain. Consider the isothermal 175° for ether, at the point marked A. If it were possible to lower the temperature to 160°, without condensation, keeping volume constant, pressure would fall, and the gas would then be in the state represented on the isothermal line 160°, at G: that is, it would be in the same condition as if it had been compressed without condensation.

You saw that a gas, or a liquid, is heated by compression; a piece of tinder was set on fire by the heat evolved on compressing air. You saw that condensation of ether was brought about by diminution of pressure—that is, it was cooled. Now, if air be suddenly expanded, it will do work against atmospheric pressure, and will cool itself. This globe contains air; but the air has been filtered carefully through cotton-wool, with the object of excluding dust-particles. It is saturated with moisture. On taking a stroke of the pump, so as to exhaust the air in the globe, no change is evident; no condensation has occurred, although the air has been so cooled that the moisture should condense, were it possible. On repeating the operation with the same globe, after admitting dusty air—ordinary air from this room—a slight fog is produced, and, owing to the light behind, a circular rainbow is seen; a slight shower of rain has taken place. There are comparatively few dust particles, because only a little dusty air has been admitted. On again repeating, the fog is denser; there are more particles on which moisture may condense.

One point more, and I have done. Work is measured by the distance or height through which a weight can be raised against the force of gravity. The British unit of work is a foot-pound—that is, a pound raised through one foot; that of the metric system is one gram raised through one centimetre. If a pound be raised through two feet, twice as much work is done as that of raising a pound through one foot, and an amount equal to that of raising two pounds through one foot. The measure of work is therefore the weight multiplied by the distance through which it is raised. When a gas expands against pressure, it does work. The gas may be supposed to be confined in a vertical tube, and to propel a piston upwards, against the pressure of the atmosphere. If such a tube has a sectional area of one square centimetre, the gas in expanding a centimetre up the tube lifts a weight of nearly 1000 grams through one centimetre; for the pressure of the atmosphere on a square centimetre of surface is nearly 1000 grams—that is, it does 1000 units of work, or ergs. So the work done by a gas in expanding is measured by the change of volume multiplied by the pressure. On the figure, the change of volume is measured horizontally, the change of pressure vertically. Hence the work done is equivalent to the area ABCD on the figure.

If liquid, as it exists at A, change to gas as it exists at B, the substance changes its volume, and may be made to do work. This is familiar in the steam-engine, where work is done by water, expanding to steam and so increasing its volume. The pressure does not alter during this change of volume, if sufficient heat be supplied, hence the work done during such a change is given by the rectangular area.

Suppose that a man is conveying a trunk up to the first story of a house, he may do it in two (or, perhaps, a greater number of) ways. He may put a ladder up to the drawing-room window, shoulder his trunk, and deposit it directly on the first floor. Or he may go down the area stairs, pass through the kitchen, up the kitchen stairs, up the first flight, up the second flight, and down again to the first story. The end result is the same; and he does the same amount of work in both cases, so far as conveying the weight to a given height is concerned; because in going down-stairs he has actually allowed work to be done on him, by the descent of the weight.

Now, the liquid in expanding to gas begins at a definite volume; it evaporates gradually to gas without altering pressure, heat being, of course, communicated to it during the change, else it would cool itself; and it finally ends as gas. It increases its volume by a definite amount at a definite pressure, and so does a definite amount of work; this work might be utilized in driving an engine.

But if it pass continuously from liquid to gas, the starting-point and the end point are both the same as before. An equal amount of work has been done. But it has been done by going down the area stair, as it were, and over the round I described before.

It is clear that a less amount of work has been done on the left-hand side of the figure than was done before; and a greater amount on the right-hand side; and if I have made my meaning clear, you will see that as much less has been done on the one side as more has been done on the other—that is, that the area of the figure BEH must be equal to that of the figure AFH. Dr. Young and I have tried this experimentally—that is, by measuring the calculated areas; and we found them to be equal.

This can be shown to you easily by a simple device—namely, taking them out and weighing them. As this diagram is an exact representation of the results of our experiments with ether, the device can be put in practice. We can detach these areas which are cut out in tin, and place one in each of this pair of scales, and they balance. The fact that a number of areas thus measured gave the theoretical results of itself furnishes a strong support of the justice of the conclusions we drew as regards the forms of these curves.

To attempt to explain the reasons of this behaviour would take more time than can be given to-night; moreover, to tell the truth, we do not know them. But we have at least partial knowledge; and we may hope that investigations at present being carried out by Prof. Tait may give us a clear idea of the nature of the matter, and of the forces which act on it, and with which it acts, during the continuous change from gas to liquid.

EXPERIMENTAL RESEARCHES ON MECHANICAL FLIGHT.

THE following is a translation of a communication made by Prof. S. P. Langley to the Paris Academy of Sciences on July 13:—

I have been carrying out some researches intimately connected with the subject of mechanical flight, the results of which appear to me to be worthy of attention. They will be published shortly in detail in a memoir. Meanwhile I wish to state the principal conclusions arrived at.

In this memoir I do not pretend to develop an art of mechanical flight; but I demonstrate that, with motors having the same weights as those actually constructed, we possess at present the necessary force for sustaining, with very rapid motion, heavy bodies in the air; for example, inclined planes more than a thousand times denser than the medium in which they move.

Further, from the point of view of these experiments and also of the theory underlying them, it appears to be demonstrated that if, in an aërial movement, we have a plane of determined dimensions and weight, inclined at such angles and moving with such velocities that it is always exactly sustained in horizontal flight, the more the velocity is augmented the greater is the force necessary to diminish the sustaining power. It follows that there will be increasing economy of force for each augmentation of velocity, up to a certain limit which the experiments have not yet determined. This assertion, which I make here with the brevity necessary in this *résumé*, calls for a more ample demonstration, and receives it in the memoir that I have mentioned.

The experiments which I have made during the last four years have been executed with an apparatus having revolving arms about 20 metres in diameter, put in movement by a 10 horse-power steam-engine. They are chiefly as follows:—

(1) To compare the movements of planes or systems of planes, the weights, surface, form, and variable arrangements, the whole being always in a horizontal position, but disposed in such a manner that it could fall freely.

(2) To determine the work necessary to move such planes or systems of planes, when they are inclined, and possess velocities sufficient for them to be sustained by the reaction of the air in all the conditions of free horizontal flight.

(3) To examine the motions of aërostats provided with their own motors, and various other analogous questions that I shall not mention here.

As a specific example of the first category of experiments which have been carried out, let us take a horizontal plane, loaded (by its own weight) with 464 grams, having a length 0·914 metre, a width 0·102 metre, a thickness 2 mm., and a density about 1900 times greater than that of the surrounding air, acted on in the direction of its length by a horizontal force, but able to fall freely.

The first line below gives the horizontal velocities in metres per second; the second the time that the body took to fall in air from a constant height of 1·22 metres, the time of fall in a vacuum being 0·50 second.

Horizontal velocities ...	0m.,	5m.,	10m.,	15m.,	20m.
Time taken to fall from a constant height of 1·22 metres	0·53s.,	0·61s.,	0·75s.,	1·05s.,	2·00s.

When the experiment is made under the best conditions it is striking, because, the plane having no inclination, there is no vertical component of apparent pressure to prolong the time of fall; and yet, although the specific gravity is in this more than 1900 times that of the air, and although the body is quite free to fall, it descends very slowly, as if its weight were diminished a great number of times. What is more, the increase in the time of fall is even greater than the acceleration of the lateral movement.

The same plane, under the same conditions, except that it was moved in the direction of its length, gave analogous but much more marked results; and some observations of the same kind have been made in numerous experiments with other planes, and under more varied conditions.

From that which precedes, the general conclusion may be deduced that the time of fall of a given body in air, whatever may be its weight, may be indefinitely prolonged by lateral motion, and this result indicates the account that ought to be taken of the inertia of air, in aërial locomotion, a property which, if it has not been neglected in this case, has certainly not received up to the present the attention that is due to it. By this (and also in consequence of that which follows) we have established the necessity of examining more attentively the practical possibility of an art very admissible in theory—that of causing heavy and conveniently disposed bodies to slide or, if I may say so, to travel in air.

In order to indicate by another specific example the nature of the data obtained in the second category of my experiments, I will cite the results found with the same plane, but carrying a weight of 500 grams, that is 5380 grams per square metre, inclined at different angles, and moving in the direction of its length. It is entirely free to rise under the pressure of the air, as in the first example it was free to fall; but when it has left its support, the velocity is regulated in such a manner that it will always be subjected to a horizontal motion.

The first column of the following table gives the angle (a) with the horizon; the second the corresponding velocity (V) of *planement*—that is, the velocity which is exactly sufficient to sustain the plane in horizontal movement, when the reaction of the air causes it to rise from its support; the third column indicates in grams the resistances to the movement forward for the corresponding velocities—a resistance that is shown by a dynamometer. These three columns only contain the data of the same experiment. The fourth column shows the product of the values indicated in the second and third—that is to say, the work T, in kilogram-metres per second, which has overcome the resistance. Finally, the fifth column, P, designates the weight in kilograms of a system of such planes that a 1 horse-power engine ought to cause to advance horizontally with the velocity V and at the angle of inclination a.

	V	R	$T = \frac{VR}{1000}$	$P = \frac{500 \times 4554}{T \times 60 \times 1000}$
45 ...	11·2 ...	500 ...	5·6	6·8
30 ...	10·6 ..	275 ...	2·9	13·0
15 ...	11·2 ...	128 ...	1·4	26·5
10 ...	12·4 ...	88 ...	1·1	34·8
5 ...	15·2 ...	45 ...	0·7	55·5
2 ...	20·0 ...	20 ...	0·4	95·0

As to the values given in the last column, it is necessary to add that my experiments demonstrate that, in rapid flight, one may suppose such planes to have very small interstices, without diminishing sensibly the power of support of any of them.

It is also necessary to remark that the considerable weights given here to the planes have only the object of facilitating the quantitative experiments. I have found that surfaces approximately plane, and weighing ten times less, are sufficiently strong to be employed in flight, such as has been actually obtained, so that in the last case more than 85 kilograms are disposable for motors and other accessories. As a matter of fact, complete motors weighing less than five kilograms per horse-power have recently been constructed.

Although I have made use of planes for my quantitative experiments, I do not regard this form of surface as that which gives the best results. I think, therefore, that the weights I have given in the last column may be considered as less than those that could be transported with the corresponding velocities, if in free flight one is able to guide the movement in such a manner as to assure horizontal locomotion—an essential condition to the economical employment of the power at our disposal.

The execution of these conditions, as of those that impose the practical necessity of ascending and descending with safety, belongs more to the art of which I have spoken than to my subject.

The points that I have endeavoured to demonstrate in the memoir in question are:—

(1) That the force requisite to sustain inclined planes in horizontal aërial locomotion diminishes, instead of increasing, when the velocity is augmented; and that up to very high velocities—a proposition the complete experimental demonstration of which will be given in my memoir; but I hope that its apparent improbability

will be diminished by the examination of the preceding examples.

(2) That the work necessary to sustain in high velocity the weights of an apparatus composed of planes and a motor may be produced by motors so light as those that have actually been constructed, provided that care is taken to conveniently direct the apparatus in free flight; with other conclusions of an analogous character.

I hope soon to have the honour of submitting a more complete account of the experiments to the Academy.

ON THE SOLID AND LIQUID PARTICLES IN CLOUDS.[1]

IN this paper are given the results of some observations made while on the Rigi in May last, on the solid and liquid particles in clouds. It was noticed, when making observations on the number of dust particles in the atmosphere, that when the top of the mountain was in cloud, the number of particles varied greatly in short intervals; while previous experience had shown that at elevated stations the number was fairly constant for long periods. In order to investigate the case of this want of uniformity in the impurity of clouded air, extreme conditions were selected, and the air tested in cloud and in the clear air outside of it. When this was done the clouded air was found to have always more dust in it than the air outside. Its humidity was of course also greater. The relative amount of dust in pure and in clouded air varied greatly. Some parts of the cloud had only about double the number of particles there were in the clear air, while in other parts the proportion was much greater. The best example tested occurred on the 25th of the month, when there were observed 700 particles per c.c. in the clear air, while the number in cloud went up to over 3000, and in one cloud to 4200 particles per c.c. These observations were taken on the top of the mountain while the clouds were passing over it; the readings being taken in the cloud and again when it had passed and was replaced by clear air.

These observations at once showed the cause of the variability in the number of dust-particles in the clouds. The dust acted as a kind of ear-mark, and showed that the air forming the clouds was impure valley air, which had forced its way up into the purer air above. This impure air had become more or less mixed with the purer upper air. Where little of the impure air had mixed with the upper air, the number of particles was not large, and the clouding slight; but where the valley air was greatly in excess, the number of particles was great, and the clouding dense. It should be noted here that all the clouds tested were cumulus. It is quite probable that the conditions in stratus and other clouds may be different.

During this visit to the Rigi there were a number of opportunities of investigating the water particles in clouds. The apparatus used was the small instrument described to the Society in May last. With this instrument the water particles in clouds can be easily seen, and the number falling on a given area counted. The results are similar to those already communicated to the Society from observations made in fogs during last winter. On observing with this instrument in clouds, the water particles were distinctly seen showering down, and the number falling on the micrometer easily counted. The number of drops falling was observed to vary greatly from time to time. At times so quickly did they fall that it was impossible to count the number that fell on only 1 sq. mm. The greatest rate actually counted was 60 drops per sq. mm. in 30 seconds, but for a few seconds the rate was much quicker. Though the quick falls seldom lasted long, yet 30 drops per sq. mm. per minute were frequently observed for a considerable time. The maximum rate of 60 per sq. mm. per half minute gives 12,000 drops per square centimetre per minute, or 77,400 drops per square inch per minute. This does seem to be an enormous number of drops to fall on so small an area in the time. These drops, however, are so extremely small they rapidly evaporate, more than two or three being seldom visible at the same time on one square of the micrometer. The denser the cloud the quicker was the rate of fall, and as the cloud thinned away the drops fell at longer intervals, and they diminished in size at the same time.

It was frequently observed when the mountain-top was in clouds, particularly if they were not very dense overhead, that the surfaces of all exposed objects were quite dry; not only the stones on the ground, which might have received heat from the earth, but also wooden seats, posts, &c., were all perfectly dry, and if wetted they soon dried. While everything was dry, the fog-counter showed that fine rain-drops were falling in immense numbers. From the fact that the air was packed full of these small drops of water, it might have been assumed that the air was saturated, and tests with properly-protected wet and dry bulb thermometers showed that it was saturated. A few observations were therefore made to explain this apparent contradiction of surfaces remaining dry while exposed to a continued shower of fine rain and surrounded by saturated air. The explanation was found to be, simply, radiant heat. Though the cloud may be so dense, it is impossible to see the sun or even a preponderance of light in one direction to indicate its position; yet, as a good deal of light penetrates under these conditions, it therefore seemed possible some heat might do so also. A thermometer with black bulb *in vacuo* showed that a considerable amount of heat penetrated the clouds under the conditions, as it rose 40° to 50° above the temperature of the air while the observations were being made. This radiant heat is absorbed by all exposed surfaces and heats them, while they in turn heat the air in contact with them, and the fine drops of water are either evaporated in this hot layer of air or after they come in contact with the heated surfaces. Other observations made on Pilatus pointed to the same conclusion. All large objects, such as seats, posts, &c., were quite dry in cloud when there was any radiation; while small objects, such as pins, fine threads, &c., were covered with beads of water. The large surfaces being more heated by radiation than small ones, when surrounded by air, these surfaces evaporate the drops falling on them, while the small ones, being kept cool by the passing air, are unable to keep themselves dry.

The observations made with the fog-counter point to the conclusion that the density or thickness of a cloud depends more on the number of water particles than on the number of dust particles in it. The number of the dust particles in the clouds varied too much and too quickly to enable any conclusion to be drawn from observations made in clouds themselves. However, on comparing the thickness of a cloud on the Rigi and a fog at low level, when the number of water-drops was about the same, it is found that the fog, though thicker, was not greatly so, although there were only a few thousand dust-particles per c.c. in the cloud, while there were about 50,000 in the fog.

The observations with the fog-counter show that, whenever a cloud is formed, it at once begins to rain, and the small drops fall into the drier air underneath, where they are evaporated, the distance to which they will fall depending on their size and the dryness of the air. It is thought that much of the dissolving of clouds is brought about in this way.

[1] Abstract of Paper read before the Royal Society, Edinburgh, on July 6, by John Aitken, F.R.S. Communicated by permission of the Council of the Society.