

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 twenty metres in diameter, put in movement by a ten 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 aerostats 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 of a metre, a width 0.102 of a metre, a thickness two millimetres, and a density about 1,900 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 of a second.

Horizontal velocities...	0 m.,	5 m.,	10 m.,	15 m.,	20 m.
Time taken to fall					
from a constant					
height of 1.22 me-					
tres.....	0.53 s.,	0.61 s.,	0.75 s.,	1.05 s.,	2.00 s.

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 1,900 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 aerial 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 5,380 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 (α) 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 one horse-power engine ought to cause to advance horizontally with the velocity V and at the angle of inclination α .

α	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 endeavored to demonstrate in the memoir in question are —

(1) That the force requisite to sustain inclined planes in horizontal aerial 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 honor of submitting a more complete account of the experiments to the academy.

OLD STANDARDS.

By a curious accident it has just been discovered that the standard yard and certain other measures and weights which were supposed to have been lost when the Houses of Parliament were destroyed by fire in 1834 are still in existence. The following account of the matter, condensed from a statement in the *London Times*, is given in a recent issue of *Nature*.

A reference to the contemporary records shows that after the fire the standard bars of 1758 and 1760 were both found among the ruins, "but they were too much injured to indicate the measure of a yard which had been marked upon them." The principal

injury to both of the standards was the loss of the left-hand gold stud, but whether this was caused by the action of the flames or otherwise is not known. When the Palace of Westminster was rebuilt the bars were deposited in the Journal Office, and from that time until recently they seem to have been wholly lost sight of. Some time ago it happened to be stated in the lobby that one of the duties of the speaker was to inspect once in every twenty years the standards immured in the sill of the lower waiting hall. Inquiries at the standards department of the Board of Trade elicited the fact that, so far from any statutory requirement being imposed upon the speaker in the direction indicated, Section 35 of the Weights and Measures Act, 1878, which provides for the care and restoration of the parliamentary copies of the imperial standards, specially exempts the walled-up copy from periodical inspection and comparison. It was found, however, that in 1871 Speaker Denison took cognizance of the standards; and this fact was brought to the speaker's notice. While inquiries were being made as to Speaker Denison's inspection, an official in the Journal Office mentioned that when the contents of that office were recently being transferred to the new wing he had observed among the lumber some old weights and measures. These proved to be the missing standards. They were examined by Mr. Chaney, the superintendent of weights and measures.

The most important of the standards thus rescued from oblivion are the yard measures constructed by Bird in 1758 and 1760. The former was copied from a bar in the possession of the Royal Society, which was itself a copy of a standard preserved in the Tower; and the second was constructed under the directions of a committee of the House of Commons from the 1758 standard. "Each of these two standard yards consisted of a solid brass bar 1.05 inches square in section and 39.73 inches long. Near each end of the upper surface gold pins or studs 0.1 of an inch in diameter were inserted, and points or dots were marked upon the gold to determine the length of the yard." The other standards in the custody of the Journal Office are two brass rods answering the description of the old exchequer yard, and four weights supposed to be certain of the "copies, model, patterns, and multiples" ordered by the House on May 21, 1760, "to be locked up by the clerk and kept by him." The most important weight — the standard troy pound — is not among those now brought to light.

INHALATIONS IN THE TREATMENT OF PHTHISIS.¹

THE history of inhalations in the treatment of phthisis is not an encouraging one. They have been widely employed and hailed as the most rational and effective mode of assailing the disease at its seat. But the results of this method of medication have not been commensurate with the expectations excited by it, and we imagine that of late it has been gradually falling into disfavor and disuse. Several potent objections have been urged against the method. First, it has been denied that the various antiseptic and germicide agents applied by inhalation succeed even in reaching the seat of the disease — viz., the submucous tissue of the bronchial mucus membrane — but are arrested often in the pharynx, oftener still in the larynx. Then it has been shown that, even assuming that the medicated atmosphere produced by one of the ordinary inhalers really reaches the seat of the disease, its impregnation with carbolic acid, creosote, thymol, or other such agent, is so exceedingly feeble as to leave no solid ground for anticipating serious benefit from its use. Further, many authorities urge with much force that if the phthisical patient be taught to rely upon inhalations the inevitable result will be a life of indoor invalidism and constant tinkering with his inhalers, a mode of existence sure to effect an amount of mischief more than sufficient to counterbalance the problematical advantages of inhalation.

There is much weight in the above objections, and until they can be satisfactorily met, the place of inhalations in the therapeutics of phthisis can not be an important one. Professor Germain-Sée, in a recent paper presented to the French Academy of Medicine, has sought to overcome the first of the objections to which we have made allusion, viz., the failure of the medicated atmosphere to reach the actual seat of the disease. He recalls the

researches made at various times into the action of creosote upon tuberculosis. This drug has had a remarkable and somewhat checkered career. Discovered in 1833 by Reichenbach, its true composition was first made known in 1853 by Gorup-Besanez, who showed that it consisted mainly of two substances, Gaïacol and créosol. It was in 1877 that Bouchard and Gimbert conceived the idea of its possible utility in phthisis, and made trial of it both *per primam viam* and hypodermically. Later, Fraenzel and Sommerbrodt in Germany made extensive trial of creosote, and reported benefit from its use.

Guttmann, in his researches in the year 1889, found that the saturation of the system with creosote arrested the development of the bacilli, and suggested that the best means of bringing the system thoroughly under the influence of the drug would be to submit the patient to an atmosphere saturated with creosote under pressure. This is also the method recommended by Professor Germain-Sée. He describes it as follows: "The patient is shut up in a metal chamber, hermetically closed, and compressed air, passed through creosote and eucalyptol, is made to enter slowly. The air in passing through these liquids is saturated, and arrives charged with a large quantity of these medicaments. The pressure must be slowly increased, and should not exceed a half atmosphere. The speed of delivery of the air saturated with the medicated vapors is from fifteen to twenty cubic metres per hour for a space of five cubic metres of capacity. The length of time the patient remains in the chamber is usually two hours, sometimes three or more, and no inconvenience ensues as the result of this procedure. The inhalations are made daily or more frequently."

Professor Germain-Sée has tried this method in twelve cases, of which one was a case of apical bronchitis, a second was a case of foetid bronchitis, and the remaining ten were genuine cases of tubercular phthisis, all of which, with one exception, had arrived at the stage of softening. The results obtained appear to be surprising, a marked amelioration being in most cases observable, not only in the amount and character of the expectoration, but in the general constitutional condition, and in some cases, though not in all, a corresponding improvement in the physical signs. Contrary to what might seem probable, hæmoptysis was not only not excited, but seemed controlled by this mode of treatment, and the appetite and digestion were improved rather than otherwise. Hectic fever was also diminished. Naturally, the least improvement was manifested in the physical signs, but Professor Germain-Sée is inclined to hope that by this new method the disease, if not cured, may at least be arrested, and further progress prevented. Benefit was obtained in some cases in a fortnight, in others the treatment was kept up for three months. Very great benefit was obtained in some cases of scrofulous enlargement of the cervical glands.

At the present time it is hardly necessary to emphasize the necessity for extreme caution in admitting the claims of any new alleged remedy for tuberculosis, and the evidence before us in the present case, although interesting, falls immeasurably short of demonstration. Further trials will no doubt be made, and the results will be awaited with attention. One benefit, somewhat negative in character, may even now be derived from Professor Germain-Sée's researches, viz., the realization of the utter futility of the methods of inhalation so long adopted, and the uselessness of the inhalers now commonly employed. If inhalation is ever to become a valuable agent in therapeutics, it will probably be by the adoption of some plan analogous to that sketched above, and, according to our present knowledge, the most hopeful medicament with which to experiment would seem to be creosote.

LETTERS TO THE EDITOR.

**Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Beech Trees and Lightning.

THE question was raised in *Science* a short time ago as to whether beech trees were ever struck by lightning. During a severe thunder-storm which passed over here this afternoon a beech tree

¹ From the London Lancet.