tube of the funnel was bent upwards and had an orifice about 1-20th of an inch in diameter. Close above this orifice was placed a glass slide, held by springs and bearing a drop of glycerine on its lower surface. The tube and glass slide were protected by a roof and two cheek-boards, which formed the vane of the weather-cock. The glass slide was so placed that the current of air issuing from the narrow orifice of the bent tube, under pressure of the wind on the wide mouth of the funnel, impinged on the centre of the drop of glycerine, and a large proportion of any solid matters carried by the air was caught on the glycerine. After a day or two, according to the weather, the slide was removed, a thin disk of glass was placed on the glycerine-drop, and the contents were then examined under the microscope, a duplicate slide being left in the aeroscope for the next observation.

This instrument depended for its function on the wind. If there was no wind, there was no current through the tube, and nothing was caught on the glycerine; but in general there was wind enough, and the captures were ample, often embarrassing by their multitude. The observations were mostly made in the neighbourhood of London, at the Greenwich Observatory. The nature of the captures varied according to the direction and velocity of the wind, the state of the weather, and the season of the year. A north-west wind, blowing over London, brought soot and globules of coal-tar, textile fibres, nondescript *débris*, a few vegetable spores, now and then an epithelial scale or two, always a number of half-cooked starch-grains (identified by their reaction with iodine and by traces of concentric lamination giving a black cross with the polariscope), and sometimes microscopic bread-crumbs (half-cooked starch-grains in meshes of gluten). The starch-grains were the most constant capture of all, in all seasons and for all directions of wind. They seem to be very durable. (If I remember rightly, M. Pouchet found starch-grains in all specimens of dust, even the most ancient, obtained from the neighbourhood of human dwellings.) A southerly wind, blowing from the country, brought a great variety of vegetable spores and pollen-grains and *débris*, with a smaller proportion of matters characteristic of town air. The size and quantity of the captures depended mainly on the velocity of the wind. Once or twice a strong wind swept a living acarus, or an entomostracan, or the shell of a diatom into the glycerine. In dry windy weather a quantity of siliceous sand was caught, which gave trouble by tilting the disk when in preparation for the microscope, and the larger grains had to be removed with the point of a needle.

The most interesting variation in the character of the organic captures was that which depended on the season of the year. In January and February scarcely anything was found (besides *débris* and inorganic matter) but a few fragments of mycelium of some fungus; but with the first fine weather in March the glycerine began to yield good returns. Spherical grains of poplar pollen were caught in large numbers, thirty or forty in a single drop, though the nearest poplar tree was a quarter of a mile distant. These were soon followed by the triangular pollen of birch and hazel—trees depending, like poplar, on the agency of the wind for fertilisation. From this time onward, through spring and summer, a great number and variety of pollen grains were caught. Cryptogamic cells increased in number through the summer, and reached their maximum in the autumn, when brown septate spores and others of various kinds, which my imperfect knowledge did not enable me to identify, appeared in abundance. If left for some days, they began to germinate. Towards winter their number diminished, the latest being minute dark brown oval spores of some species of agaricus (?). The winter months were comparatively barren.

I did not find any Bacteria, but there were numbers of excessively minute particles, of which I could not tell the nature. Once, after leaving the aeroscope for several days, I found the glycerine swarming with a minute *torula* which had evidently multiplied in that pabulum. In fact the glycerine was fermenting.

Among these facts, the only one which seems to have any bearing on the question of the propagation of infectious diseases is the great prevalence of cryptogamic spores in the air in autumn, when those diseases are especially rife.

To avoid fallacy in the results obtained, I used to place two drops of glycerine on the same slide, but only directed the air-current against one of them; both were examined under the microscope, and the difference credited to the air. By using glycerine that had been boiled with carmine, many of the organic captures were made more distinct: the nuclei of epithelial scales and of many other cells were brightly stained by the carmine. Glycerine had the disadvantage of absorbing moisture in damp weather and swelling to an inconvenient bulk. At such times I used oil instead, with good effect. My plan of examination was to sweep the whole disk in successive parallel zones, by the aid of mechanical stage-movement, and make record of every organic body that could be recognised. Such as I had not seen before were sketched with pen and ink and coloured chalk in a book devoted to that purpose.

It will be seen that my observations entirely support Mr Cunningham's, as to the abundant presence of living spores in the air. I was satisfied that this branch of research, in the hands of one thoroughly familiar with these microscopic forms, would lead to results of great interest, and I heartily congratulate Mr. Cunningham on the valuable work which he has produced.

HUBERT AIRY

Animal Locomotion

MR. WALLACE'S last letter seems to call for a word of explanation from me. I did not refer to the up stroke of the bird's wing because this was not the point in dispute. But in reply to Mr. Wallace's latest stricture—that I appear "to ignore the great downward reaction, added to gravitation, during every up stroke"—I would say (1) that the downward reaction is not great, (*a*) because, as Mr. Wallace has himself observed, of the valvular action of the feathers; (*b*) because of the convex form of the upper surface of the wing; and (*c*) in some cases, because the wing is less expanded in the up stroke. (2) As to the effect of gravitation, this was already allowed for in determining the resultant motion consequent on the down stroke, and must not be reckoned twice. Just as with an arrow shot from a bow, so with the bird; the motion consequent on the down stroke lasts long enough for the wings to be raised before it is spent. Mr. Wallace is certainly right in saying that the down stroke should counteract the downward reaction of the up stroke, but this downward reaction being slight cannot require "a highly-inclined upward motion," and what is more, it cannot require that the under surface of the wing should be directed forwards as Dr. Pettigrew asserts.

Again, I do not say the movement of the wing as a whole is downward and backward, but that the action of its surface is in that direction. The Duke of Argyll is no doubt correct in maintaining that the wing as a whole moves in a perpendicular line, or perhaps with a slight forward overlap.

I cordially agree with Mr. Wallace that the matter is not to be settled by "discussing theoretically, but by observation and experiment;" still the elementary principles of mechanics may surely be heard in evidence without disadvantage even at the outset of the inquiry.

JAMES WARD

Trinity College, Cambridge, March 30

Rudimentary Organs

IN a former communication (NATURE, vol. ix. p. 361) I promised to advance what seems to me a probable cause—additional to those already known—of the reduction of useless structures. As before stated, it was suggested to me by the penetrating theory proposed by Mr. Darwin (NATURE, vol. viii. pp. 432 and 505), to which, indeed, it is but a supplement. Epitomising Mr. Darwin's conception as to the dwarfing influence of impoverished conditions, progressively reducing the average size of a useless structure, by means of free intercrossing; the present cause may be defined as the mere cessation of the selective influence from changed condition of life.

Suppose a structure to have been raised by natural selection from 0 to average size 100, and then to have become wholly useless. The selective influence would now not only be withdrawn, but reversed; for, through Economy of Growth—understanding by this term both the direct and the indirect influence of natural selection *—it would rigidly eliminate the variations 101, 102, 103, &c., and favour the variations 99, 98, 97, &c. For the sake of definition we shall neglect the influence of Economy acting below 100, and so isolate the effects due to the mere withdrawal of Selection. By the condition of our assumption, all variations above 100 are eliminated, while below 100 indiscrimi-

* See former communication, NATURE, vol. ix. p. 361.