To obtain the rapid evaporation and superheated state of ice, the U part of the tube was placed in a freezing-mixture of ice and salt till the condensed water coated the tube internally with a hollow sleeve of ice extending in the long and short parts of the U to a total length of about eight inches, and the flask itself was thereupon placed in a sawdust-bedded tin pail containing a large freezing-mixture of 5 lbs. of salt and 10 lbs. of pounded ice. Hydrochloric acid was at the same time added to the frigorific mixture round the U-tube, lowering the temperature of that part of the cryophorus to -29°C. (-20°F.), which had the effect of cracking the ice-sleeve (apparently by contraction) in all directions, giving rise at first, from its appearance, to the apprehension that the glass tube was completely splintered! Now came the critical operation. Would the hard frozen ice-coat bear the application of the heat ordeal without melting?

The freezing mixture round the U-tube was replaced by cold water in a water-bath, which was heated rapidly with a Bunsen-flame. About two inches of the ice-sleeve in the long part of the tube stood above the surface of the water; the rest soon melted, while the temperature of the water in the bath rose to 70°C. , and a thick rime of white frost from the condensed vapours of this water formed round the tube containing the projecting part of the ice-tongue, which must have been intensely cold, since the rime almost touched the surface of the warm water. Below the surface of the water the tube appeared always to contain some snowy-looking solid, along with what resembled water proceeding from its liquefaction. When the temperature of the bath reached 70°C. the projecting ice-tongue was still unmelted; it had replenished itself by condensing and freezing the vapours rising up to it from below, and formed a snowy plug in the tube an inch or more long.

The water-bath was then removed in order to heat this snow-plug with a Bunsen-flame, and to our extreme surprise a similar snow-lining was found still to remain coating, quite dry, the whole long part and much of the short part of the U-bend. The flame was applied, and the whole tube was heated violently without for some time appearing to have the least effect upon the white crust within, notwithstanding the tube was too hot to be touched! A small flake weighing at most a few tenths of a grain, at the bottom of the U, withstood the strongest heat there for several minutes, wasting insensibly away, and unchanged in shape, until it vanished with perhaps a moment's collection to a drop as it disappeared. As it slowly grew thinner the white coating in the tube seemed in general to be no more affected by the heat than white feathers would have been; but in particular parts there often occurred partial liquefaction or pasty fusion allowing pieces of the film to turn over by their weight and roll or slip down the glass while still adhering to it, quite in the manner of drops. That the liquid itself, completely freed from air, refuses to boil, and may be described in that respect as practically oily, was evident enough in the preparation of the flask, when except by sudden bumps the distillation of vapour, however much it was urged by heat, and seen to be passing copiously through the bent worm-tube, proceeded almost entirely from the surface of the water in the flask.

In these drops at least, adhering to the heated glass, it seems difficult to believe that the solid ice which makes them angular, jagged in form, and pasty, can be anything but superheated; and it seems also scarcely credible that the latent heat of sublimation of the insignificant weight of a few grains of ice-crust in the tube resisting its strong heat for many minutes can reach the large amount of its gain of heat in that time by conduction and radiation from the surrounding tube, if the ice-film is at no higher temperature than its natural melting-point. These are questions however which the calorimetric methods devised and pursued by Prof. Carnelley are best designed to answer, and to which replies without such positive determinations can only be urged at present as probable conjectures, or as clear preceptions and presentiments, on the other hand, of the action and operation of some hitherto undiscovered thermal laws.

Whether by direct sublimation, or by evaporation accompanied by liquefaction, the slender snowdrift with its enlargement at the top was gradually reduced in thickness, and subdivided into remaining parts along its length, which all adhered to the tube until they wasted away, and the largest top piece never fell by withdrawal of support below, but, like the rest, it clung like a thin scale to the last to its original place on one side of the tube. This power of adhesion of hermetically volatilised ice to hot bodies with which it is in contact forms, as Prof. Carnelley observes, one of its most marked, although not at all one of its most prognosticable, properties, and it certainly prompts, if it

does not unequivocally substantiate, the supposition that the ice in such close contact with extremely hot bodies must be superheated, or at a temperature sensibly higher than its natural melting-point.

The perfect success of the experiment and the beautiful appearance of the ineffaceable white snow-lining of the tube afforded us unqualified pleasure and surprise, and the simple preparation and preservation of the cryophoric apparatus needed for its exhibition will, I have no doubt, cause the experiment to be often reproduced and shown wherever freezing-mixtures and the cryophorus, and Boutigny's and Leidenfrost's phenomena, and Prof. Carnelley's theory and researches, are subjects of lecture-demonstrations.

A. S. HERSCHEL

College of Physical Science, Newcastle-on-Tyne, February 15

Dust, Fogs, and Clouds

I NEED not say that the information contained in M. H. J. H. Groneman's letter in *NATURE*, vol. xxiii. p. 337, was a most unexpected surprise. Nothing whatever seems to have been known in England about the results obtained by Messrs. Coulier and Mascart; and my astonishment was not diminished when I considered that their important investigation had borne no fruit, never having been practically applied by meteorologists or others.

I have just seen the article by M. Coulier in the *Journal de Pharmacie et de Chimie*, and will (with your permission) make a few remarks on his paper and on my own. M. Coulier was led to the discovery of the important part played by dust in the condensation of vapour, by making experiments on the well-known cloudy condensation produced by expanding saturated air. Instead of the ordinary air-pump arrangement M. Coulier's apparatus consisted of a glass flask, in which was placed some water. This flask was connected by a tube to a hollow india-rubber ball. He first compressed the india-rubber ball, thus compressing the air in the flask. The pressure was then removed from the ball, when the air in the flask expanded, and the condensation became visible.

In making repeated experiments with this apparatus M. Coulier noticed that the action of the air was capricious. After the air had remained in the flask some days he found it quite inactive. He also found it inactive after shaking it up with the water in the flask, and that on expelling some of the inactive air and replacing it with the air of the room the cloudiness again appeared. He then found that if he filtered the air before admitting it to the flask it did not give any cloudiness when expanded, and he explains with perfect clearness that the dust in the air formed the nuclei on which the vapour condensed.

Having explained the manner in which M. Coulier approached the subject I shall now give the history of the corresponding part in my work and show the direction from which I approached it. I had been studying the action of "free surfaces" in water and other forms of matter, when changing from one state to another. I knew that water could be cooled below the freezing-point; I was almost certain ice could be heated above the melting-point, and I had shown that water could be heated far above the boiling point; that the material of which the vessel holding the water was composed had no influence on the result—and all that was necessary to prevent the change of state taking place, at the freezing and boiling points, was an absence of "free surfaces" at which the change could begin. Arrived at this point, the presumption was very strong that vapour could be cooled below the "condensing point" without changing to water, if no "free surfaces" were present. I first intended to experiment with steam at the pressure of the atmosphere, but found difficulties in operating at so high a temperature. I then made arrangements to conduct the experiments at a lower temperature, and for convenience experimented on steam mixed with air. I then saw that dust in the air would form "free surfaces" on which the vapour would condense. I therefore attached a cotton-wool filter to the apparatus, and filtered the air before it entered the receiver. When this was done I found that the steam on entering the receiver was perfectly invisible, and gave rise to no cloudy condensation, the air remaining supersaturated. The experiment was immediately afterwards repeated somewhat in the same way as was done by M. Coulier, the air being supersaturated by expanding it by means of an air-pump.

Though the two investigations approached the subject from very different points, they seem afterwards to have flowed in almost parallel directions. Starting from his first experiments,

M. Coulier made experiments with the products of combustion from flames in which the combustion was as perfect as possible. He found these gases much more active than the air of the room. This he attributed to particles of unconsumed carbon. He also found the air after rain and storms to be less active, and the air in summer less active than in winter. After extending the experiments to alcohol and benzine, the paper concludes with some remarks on the peculiar action of ozone.

Up to this point the two investigations run perfectly parallel, and the strange likeness between the two sets of experiments is not the least interesting point connected with them. After going over this first paper by M. Coulier, I found he had communicated a second paper, which will also be found in the same volume of the *Journal de Pharmacie et de Chimie*, at page 254. This second paper is almost entirely occupied with a description of some experiments in which inactive air was heated and rendered active.

In the first experiment described in M. Coulier's second paper a platinum wire was heated in the purified air of the flask, after which the air was active. In the second experiment pure air in which hydrogen was burned became active. In the third experiment pure air which was passed through a glass tube surrounded with tinsel ("cliquant"), and moderately heated, was made active. Fourth experiment, oxygen, nitrogen, and hydrogen became active after they had been heated in a tube. After describing some effects in ventilation when *highly* heated air is used, he says, "In the preceding note (the first paper) I believed I could attribute the activity of the air to the presence of solid bodies, and it seemed to me that the only solid body that could escape from a carbon flame could be nothing but carbon itself. It was the remarkable experiment, so easily made, of filtering air through cotton-wool, that led me to form this hypothesis, which the experiments above related invalidate (à faire cette hypothèse, que les expériences relatées plus haut infirment)." He concludes by saying, "The explanation of these phenomena remains still to be found."

Experiments exactly corresponding to some of those described in M. Coulier's second paper will be found in mine. Wishing to test the effect of combustion on air, I first made experiments to test the effect of heat on the apparatus to be used in collecting the hot gases. For this purpose I passed filtered air through a heated glass tube, after which I found it was remarkably active. It was found however that this activity is not due, as M. Coulier seemed to suppose, to the heating of the air, but to impurities driven off the surface of the tube by the heat. This was proved by showing that the air remained inactive when the hot tube through which it was passed was thoroughly cleansed.

In making experiments on the effect of burning gas I arranged a platinum wire, connected with a battery, to enable me to light the gas in the pure air of the receiver. On testing the action of the heated wire alone, it was found that simply heating the wire gave rise to cloudiness. It was however found that by highly heating the wire its activity was destroyed, all impurities being driven off.

These experiments explain M. Coulier's first and third experiments. The fourth experiment is also to be explained by the nuclei driven off the tube by the heat. These nuclei may be driven off in the solid state, or as gases which condense without nuclei when *highly* supersaturated on being cooled to the temperature of the flask. The nuclei are in some cases formed by chemical union of the gases driven off by the heat, and in other ways unnecessary to enter upon here. As to the second experiment, more information is required as to arrangement of apparatus, &c., before any opinion can be formed as to the origin of the nuclei.

It now appears to me that this second paper explains why the first results of M. Coulier, though repeated and confirmed by M. Mascart, have not received that general acceptance we should have expected. In his second paper he describes a number of results which he did not succeed in fitting into his hypothesis. They even seemed to him to shake his first conclusions, and the uncertain sound given by his second paper seems to have blighted any fruit his first paper was likely to have produced. There can however be no doubt that M. Coulier was the first to show the important part played by dust in the cloudy condensation of the vapour in air, and his first paper clearly explains its action. It seems highly probable that if it had not been followed by his second paper, or if he had succeeded in getting the key to the explanation of his experiments, and his conclusions had con-

firmed instead of weakening the teaching of his first paper, his result would long ere now have been applied to explain the different causes and the different forms of cloudy condensation in our atmosphere, as well as other physical phenomena.

Darroch, Falkirk, February 15

JOHN AITKEN

Geological Climates

I DESIRE to express my thanks to Dr. John Rae for the valuable contribution of "facts" which he has added to this interesting question, of which I hope to make use in due time.

I wish also to answer the question asked by Prof. Woeikoff in his letter of February 17. My authority for January, July, and mean temperatures in the northern hemisphere and in the southern is the most recent and accurate available, viz., United States Coast Survey, "Meteorological Researches for the use of the Coast Pilot," Part I, by William Ferrel (Washington, 1877). Mr. Ferrel gives the January and July temperatures for every ten degrees of longitude and latitude, up to 80° N. and 60° S. as follows, so far as regards the annual means:—

Lat. N.	Annual.	Lat. S.	Annual.
0°	80°·1 F.	0°	80°·1 F.
10	81°·0 "	10	78°·7 "
20	77°·6 "	20	74°·7 "
30	67°·6 "	30	66°·7 "
40	56°·5 "	40	57°·9 "
50	43°·4 "	50	47°·8 "
60	29°·3 "	60	35°·3 "
70	14°·4 "	70	—
80	4°·5 "	80	—

This table fully justifies what I said of the southern hemisphere as compared with the northern, and is, of course, explained by the existence of three great gulf streams in the south, which raise the mean temperature, producing insular climates with a small range from July to January.

Mr. Ferrel adds, at the close of his discussion (p. 22):—

"From Dove's Charts of Isothermal Lines, which do not extend beyond the middle latitudes in the southern hemisphere, it has been inferred that the southern hemisphere is colder than the northern, and this has been the accepted view ever since his charts were first published, in the year 1852; but from the results obtained above it is seen that the mean temperature of the southern hemisphere is the greater of the two."

I was well aware that the east coast of Asia is colder, latitude for latitude, than the east coast of North America, but this has nothing to do with reducing the temperatures of the east coast of America, by means of alterations in the ocean currents of the North Atlantic, which I deny to be possible.

SAMUEL HAUGHTON

Trinity College, Dublin, February 19

Climate of Vancouver Island

As questions connected with the climate of Vancouver Island and the influence on it of ocean currents have lately been the subject of several communications in the pages of NATURE, it may be worth while to draw attention to the fact that Esquimalt, at the southern extremity of the island, together with several places on the mainland of British Columbia, have now been for a number of years occupied as regular stations of the Canadian Meteorological Service, and that trustworthy meteorological results are to be found in the annual reports to Government.

When writing a report on British Columbia for the Canadian Pacific Railway Survey in 1877, I applied to Prof. Kingston, then in charge of the Meteorological Department, for some information on climate, and received from him an abstract, which was published at the time ("Can. Pacific Ry. Report, 1877," p. 246), by which it appears that the mean summer temperature of Esquimalt is 57°·82 F., mean winter temperature 34°·45, mean annual temperature 47°·97. This does not include however the additional results of the last few years.

Much information on the climate of the northern part of the north-west coast may also be found in the *Alaska Coast Pilot*, 1869, and the *U.S. Pacific Coast Pilot*, Appendix I, 1879. In the latter, series of monthly and mean annual isothermal lines are given for the air and sea surface, which—though the observations at command are by no means complete—are doubtless nearly correct. A partial abstract of these, with some discussion