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# XII.—On Dust, Fogs, and Clouds. By JOHN AITKEN.

(Read, Part I., December 20, 1880; Part II., February 7, 1881.)

# PART I.

Water is perhaps the most abundant and most universally distributed form of matter on the earth. It has to perform more varied functions and more important duties than any other kind of matter with which we are acquainted. From its close connection with all forms of life, it has been the subject of deepest interest in all ages. It is constantly changing from one of its states to At one time it is solid, now liquid, and then gaseous. another. These changes take place in regular succession, with every return of day and night, and every successive season; and these changes are constantly repeating themselves with every returning cycle. Of these changes, the one which perhaps has the greatest interest for us, and which has for long ages been the subject of special observation, is the change of water from its vaporous state, to its condensation into clouds, and descent as rain. Ever since man first "observed the winds" and "regarded the clouds," and discovered that "fair weather cometh out of the north," this has been the subject of intensest human interest, and at present forms one of the most important parts of the science of meteorology, a science in which perhaps more observations have been made and recorded than in all the other sciences together.

In the present paper I intend confining my remarks to this change of water from its gaseous or vaporous to its liquid state, with particular reference to that change when it takes place in the cloudy condensation of our atmosphere. Let us look briefly at the process as it goes on in As the heat of the sun increases, and the temperature of the earth nature. rises, more and more water becomes evaporated from its surface, and passes from its liquid form to its invisible gaseous condition; and so long as the temperature continues to increase, more and more vapour is added to the air. This increased amount of vapour in hot air compared to cold air is generally explained by saying that hot air dissolves more water than cold air. This, however, is not the case. Air has no solvent action whatever on water vapour. Water vapour rises into air to the same amount that it would do into a vacuum at the same temperature, only it rises into air more slowly than into a vacuum, and the amount of vapour which can remain in the air is independent

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of the amount of air present, that is, independent of the pressure of the air, and depends only on the temperature.

After air has become what is called "saturated" with vapour, that is, when the vapour tension is that due to the temperature, a momentary condition of stability is attained. Suppose the temperature to fall, a change must now take place. All the water cannot remain as invisible vapour; some of it must condense out into its visible form. It is this condensed water held in mechanical suspension in the air to which we give the names of fog, cloud, mist, and rain, phenomena having some resemblance to each other, yet possessing marked The particles composing a fog, for instance, are so fine they differences. scarcely fall through the air, a cloud is a little coarser in the grain, while a mist is coarser still in texture, and rain is any of these while falling, whether it be a wetting mist or a drenching rain. And the question now comes, Why this difference? Why should the water vapour condense out of the air in one case in particles so minute they seem to have no weight, and remain suspended in the air, while in another case they are large grained and fall rapidly?

As the key to the answer to this question is given by a very simple experiment, it will be well for us here to have a clear conception of the conditions of that experiment. Here are two large glass receivers, both connected to this boiler by means of pipes. If we now allow steam to pass into this receiver, which we shall call A, you will see the steam whenever it begins to enter. There it comes, rising in a dense cloud, and soon you see the receiver gets filled with the condensed vapour, forming a beautiful white foggy cloud, so dense that you cannot see through it. Let us now pass some steam into the other receiver, which we shall call B. Observe—nay, you may strain your eyes as much as you please, you cannot see when the steam begins to enter, and now it has been rushing in for some time, and yet you cannot see it. There is not the slightest appearance of cloudiness in the receiver, yet it is as full of water vapour as the receiver A, which still remains densely packed with fog.

Now, why this difference in the two cases ? Simply this. The receiver A, which is so full of fog, was at the beginning of the experiment full of ordinary air—the air of this room—while the other receiver B was also full of the air of this room, but before entering the receiver it was passed through a filter of cotton-wool, and all dust removed from it. The great difference, then, between the appearance of these two receivers is due to the dust in the air. Dusty air —that is, ordinary air, gives a dense white cloud of condensed vapour. Dustless air gives no fogging whatever.

But why should there be this difference in the two cases? Why should dust have this peculiar action? or rather, Why does not the water vapour condense into its visible form in air free from dust? The air is "supersaturated" in both cases, but in the one case it condenses out and forms a cloudiness, while in the other it remains in its invisible vaporous form. It will be necessary to diverge here a little from our immediate subject, to say a few words on the conditions under which water changes from one of its forms to another.

We have what are called the "freezing-point" and the "boiling-point" of water. These are, of course, the same as the melting-point and the condensingpoints of water. Water at 0° C. will freeze if cooled, or melt if heated. It will pass into vapour if heated above 100° C., and will pass from vapour to liquid if cooled below 100° C., that is, at standard pressure. But something more than mere temperature is required to bring about these changes. Before the change can take place, a "free surface" must be present, at which the change can take place. I may here say that what I mean by a "free surface" is a surface at which the water is free to change its condition. For instance, the surface of a piece of ice in water is a "free surface" at which the ice may change to water, or the water change to ice. Again, a surface of water bounded by its own vapour is a "free surface," at which the water may vaporize, or vapour condense. What are called the "freezing" and "boiling points" of water are the temperatures at which these changes take place at such "free surfaces." When there is no "free surface" in the water, we have at present no knowledge whatever as to the temperature at which these changes will take place.

It is well known that water may be cooled in the absence of "free surfaces" far below the "freezing-point" without becoming solid. Some years ago \* I showed reason for believing that ice in the absence of "free surfaces" could be heated to a temperature above the "freezing-point" without melting. Professor Carnelly has quite lately shown this to be possible, and has succeeded in raising the temperature of ice to  $180^{\circ}$  C.† Further, I have shown in the paper above referred to, that if water be deprived of all "free surfaces," it may be heated in metal vessels while under atmospheric pressure to a temperature far above the "boiling-point," when it passes into vapour with explosive violence.

From this we see that it requires a lower temperature to cause a molecule of water to adhere to another molecule of water to form ice, than for a molecule of water to adhere to a molecule of ice. Also that it requires a much higher temperature to cause a molecule of water surrounded on every side by other water molecules to pass into vapour, than for a water molecule bounded on one side by a gas or vapour molecule to pass into a state of vapour; and that a necessary condition for water changing its state is the presence of a "free surface" or "surfaces," at which the change can take place, if these changes are

<sup>\* &</sup>quot;Transactions Royal Scottish Society of Arts," 1874-75.

<sup>† &</sup>quot;Nature," vol. xxii. p. 435.

to take place at the "freezing" and "boiling points." At present we do not know at what temperatures these changes take place when no "free surfaces" are present. Indeed, we are not certain that it is possible for these changes to take place at all, save in the presence of a "free surface."

Returning now to the condensation of the water vapour, we see from the experiments given that precisely the same conditions are necessary for the condensation of a vapour as for its formation. Molecules of vapour do not combine with each other, and form a particle of fog or mist; but a "free surface" must be present for them to condense upon. The vapour accordingly condenses on the dust suspended in the air, because the dust particles form "free surfaces" at which the condensation can take place at a higher temperature than where they are not present. Where there is abundance of dust there is abundance of "free surfaces," and the visible condensed vapour forms a dense cloud; but where there are no dust particles present there are no "free surfaces," and no vapour is condensed into its visible form, but remains in a supersaturated vaporous condition till the circulation brings it in contact with the "free surfaces" of the sides of the receiver, where it is condensed.

We see, then, that each fog particle in the experiment was built on a dust particle. This indicates an enormous number of dust particles in the air. We must not, however, suppose that the particles of that dense fog we saw in the receiver A represented all the dust particles in the air experimented on. The experiment indicated an extremely foul state of the air indeed, but it does not tell the whole truth. Those fog particles only represent a small part of the That this is really the case is easily shown in the dust particles present. following way:-Let as much steam be blown in as will form a dense fog. Now allow this fog to settle, but do not allow any dusty air to enter. After the fog has settled blow in more steam. Again you will find a dense fog condensed on the dust which escaped the first condensation. Allow this again to settle, and repeat the process a number of times, when you will find, after many repetitions, that there is still fog forming. But it will also be noticed that after each condensation the fog becomes less and less dense, till at last it ceases to appear as fog; but on closely looking into the receiver the condensed vapour will be seen falling as fine rain. When the steam was blown in the first time the fog was very fine textured; each particle was so small it floated easily in the air. After each condensation the fog became less dense; it at the same time became more coarse-grained and heavier, and was seen falling slowly. Near the end, no fog was visible, and nothing but a fine rain to be seen falling. If the air was still further purified, even the rain seemed to cease.

This experiment may be made in another way. A large globular glass flask is provided, having a tight-fitting indiarubber stopper, through which pass two pipes. One of these pipes is connected to an air-pump, and the

other terminates in a stop-cock. To the other opening of the stop-cock is securely fixed a tube tightly packed with cotton-wool. Some water is placed in the flask to moisten the air. If now the stop-cock is closed, and one or two strokes are made with the pump, so as to cool the air by expansion, it will be noticed that a fog immediately appears in the flask. This fog is fine textured, close grained, and will scarcely settle. Now pump out a good deal of the air from the flask, and allow air, filtered through the cotton-wool, to enter in its After the temperature equilibrium is established, again make one place. or two strokes with the pump. The fog again appears, but is now opentextured and coarse-grained. Repeat the process, admitting more and more filtered air each time, and it will now be observed that the dense light fog which at first appeared gradually gives place to one coarser and coarser in texture, till at last no fog appears; but on looking closely a fine rain, as in the previous experiment, will be seen showering down inside the flask. If the process is continued still further the rain ceases, there being no more "free surfaces" to form nuclei for rain drops.

These two ways of experimenting, as might be expected, give exactly the same result, the conditions being so similar. In one the condensation is produced by the cold air mixing with the hot steam; in the other the "saturated" air is cooled by expansion in the flask. These experiments show clearly that when there is dust in the air the vapour condenses out in a visible form, but when no dust is present it remains in a supersaturated vaporous state. That the air, when no dust is present, is really supersaturated, is evident from the fact that when the dust particles become few, the fog particles are not only few, but are much heavier than when they were numerous, and also by their increasing in size as they fall through the air. Each falling particle becomes a "free surface," at which the supersaturated vapour can condense and increase the size of the drop. Another way of showing the supersaturated condition of the air is to allow unfiltered air to enter in place of filtered air. The unfiltered air will at once show itself by the vapour condensing on its dust. It will be seen rising from the jet into the pure air, falling over and spreading itself over the bottom like a fountain of some viscous cloudy fluid.

It was in the autumn of 1875, when studying the action of "free surfaces" in water when changing from one state to another, that I first observed the conditions necessary for cloudy condensation. I knew that water could be cooled below the freezing-point without freezing. I was almost certain ice could be heated above the freezing-point without melting. I had shown that water could be heated above the boiling-point, and that the nature of the vessel in which it was boiled had no influence on the boiling-point, and all that was necessary for cooling the water below the freezing-point and for superheating the ice, and the water, was an absence of "free surfaces" at which they might change their state. Arrived at this point, the presumption was very strong that water vapour could be cooled below the boiling-point for the pressure without condensing. It was on looking for some experimental illustration of the cooling of vapour in air below the temperature corresponding to the pressure that I thought that the dust in the air formed "free surfaces" on which the vapour condensed and prevented it getting supersaturated. Arrangements were at once made for passing the air experimented on through a cottonwool filter, and it was then that I first found that air which was free from dust gave no cloudy condensation when mixed with steam, and that the supersaturated air remained perfectly clear.

Shortly after this, the investigation had to be abandoned, and all that remained of it was a sketch of the apparatus in my notebook, together with a description of the experiments made with it, till about the middle of November last, when the investigation was continued. The apparatus with which the experiments were made before the Society is the same as when used in the first experiments.

The conclusions which may be drawn from these experiments are—1st, that when water vapour condenses in the atmosphere, it always does so on some solid nucleus; 2d, that the dust particles in the air form the nuclei on which it condenses; 3d, if there was no dust in the air there would be no fogs, no clouds, no mists, and probably no rain. As we do not at present know anything about the temperature of condensation of vapour where there are no free surfaces, we cannot tell whether the vapour in a perfectly pure atmosphere would ever condense to form rain; but if it did, the rain would fall from a nearly cloudless sky.

I have said that if there was no dust there would be no fogs, clouds, nor mists; but that is not all the change which would be wrought on the face of nature by the absence of dust. When the air got into the condition in which rain falls—that is, burdened with supersaturated vapour—it would convert everything on the surface of the earth into a condenser, on which it would deposit itself. Every blade of grass and every branch of tree would drip with moisture deposited by the passing air; our dresses would become wet and dripping, and umbrellas useless; but our miseries would not end here. The insides of our houses would become wet; the walls and every object in the room would run with moisture.

We have in this fine dust a most beautiful illustration of how the little things in this world work great effects in virtue of their numbers. The importance of the office, and the magnitude of the effects wrought by these less than microscopic dust particles, strike one with as great wonder, as the great depths and vast areas of rock which, the palæontologist tells us, is composed of the remains of microscopic animals.

Let us now look more closely into the action of dust in producing cloudiness. It is very evident that the results are not always alike. In one case the condensed vapour takes the form of a fog, so fine that it easily floats in the air and never seems to settle. In another case the cloudiness is coarser grained and settles down slowly, and in another case it is a very coarse-grained mist which falls quickly (of course I am not here speaking of the coarse grainedness produced by a number of small particles combining to form one). From the experiments described, it would appear that, when the dust is present in great quantities, the condensed vapour forms a fog, because as there are a great number of dust nuclei each nucleus only gets a very little vapour, and is not made much larger or heavier, so it continues to float in the air. As the number of dust nuclei diminish, the amount of vapour condensed on each particle increases, their size and weight therefore also increase. So that as the density of the cloudiness decreases the size of the particles increases, and their tendency to settle down also increases. Fogs will, therefore, only be produced when there is abundance of dust nuclei and plenty of vapour. There is probably also something due to the composition of the dust particles; some kinds of dust seem to form better nuclei than others.

We now come to the question of what forms this dust. What is its composition? Whence its source? I have been unable to get any trustworthy information as to the chemical composition of the dust. The only analysis I have seen is of dust collected in rooms. Now it is evident that as this dust has settled down, it will be, so to speak, winnowed dust, and will therefore contain too small a proportion of the finer particles.

As to where this dust comes from, it is evident it will have many sources. Everything in nature which tends to break up matter into minute parts will contribute its share. In all probability the spray from the ocean, after it is dried and nothing but a fine salt-dust left, is perhaps one of the most important sources of cloud-producing dust. It is well known that this form of dust is ever present in our atmosphere, and is constantly settling on every object, as evidenced by the yellow sodium flame seen when bodies are heated. There is also meteoric dust, and volcanic dust and condensed gases. At present, however, I wish to confine our attention to the action of heat as a producer of atmospheric dust, and more especially in relation to its fog-producing power.

Most of us on entering a darkened room, into which the sun is shining through a small opening in the shutters, have observed the very peculiar effect of the sun's rays when seen under these conditions, the path of the beam of light being distinctly visible, shining like a luminous bar amidst the surrounding darkness. On closely looking at it, it is seen that this peculiar effect is produced by the dust motes floating in the air of the room reflecting the

light, and becoming visible as they pass through the path of the beam. We are struck by the marvellous amount of dust thus revealed ever floating in our atmosphere, and which under ordinary conditions of light are not observed. It is known that when air containing this dust is highly heated or passed through a flame, all these motes are destroyed, and the path of the sun's rays becomes invisible.

Returning now to the question of fogs, one might naturally conclude from what we have said that air which had passed over or through a flame or through a fire, where the combustion was perfect, ought to be nearly dustless, and, therefore, ought not to be a good medium for fogs. Before, however, coming to any conclusion on this point, it was deemed necessary to make more direct experiments, and we shall presently see that, however natural our conclusion is, it is very far wrong. Heating the air may cause the dust motes to become invisible; but so far as my experiments go, they prove that the heating of the air by the flame does not remove the dust, but rather acts in the opposite way, and increases the number of the particles. The heat would seem to destroy the light-reflecting power of the dust, by breaking up the larger motes into smaller ones, and by carbonising or in some way changing their colour, and thus make them less light-reflecting.

Powerful as the sun's rays are as a dust revealer, I feel confident we have in the fog-producing power of the air a test far simpler, more powerful and delicate, than the most brilliant beam at our disposal. When steam escapes into the air it condenses on the dust particles, and thus by simply magnifying their size, makes their number evident to the eye. Every fog particle in the air was represented by a dust particle before the steam was added, but these were invisible to the eye till increased in size by the vapour. This would seem to indicate a condition of the atmosphere too impure to be true, yet I think we are justified in our conclusion, as it has been shown that when there is no dust there is no fogging. In the future, therefore, we will be compelled to look upon our "breath" as seen on a cold morning, as evidence of the dusty state of the And every puff of steam as it escapes into the atmosphere will remind air. us still more powerfully of the same disagreeable fact. If it was not for dust we would never see our "breath," nor would wreathes of steam be seen floating in the air, nor would our railway stations and tunnels be thick with its cloudiness. The only consolation we have is, this fine dust is not easily wetted. The air we breathe is not deprived of all its dust in its passage through the lungs. The air which we exhale is still active as a fogproducer. If, for instance, we inhale the air by the nostrils, and pass it by the mouth to the experimental receiver, we find it still full of dust and fogproducing. We might have expected, that after passing over so much wetted surface, the dust would have been all taken out of the air. This difficulty

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of wetting the dust in the air may be illustrated by passing air through "washing bottles," after which it will still be found to be full of dust. Further, during wet weather, after rain has fallen for a long time, all the dust is not washed out of the air. It is still active as a fog-producer, though in a less degree than during dry weather.

I believe that at present some attempts are being made to collect and estimate the dust in the air. These observations deal with the weight and composition of the dust. I would here suggest that other observations be made by this fog-producing power of the air, to get not the weight or composition of the dust, but the relative multitude of the dust-specks in it at different times. There seems a possibility of there being some relation between dust and certain questions of climate, rainfall, &c.

The composition of the dust will also be of great importance in determining its power as a cloud-producer, as it is evident some kinds of dust will have a greater attraction for water vapour than others. Fine sodic chloride dust, for instance, we would expect would condense vapour, before it was cooled to the saturated point, on account of the great attraction that salt has for water. The instrument for these observations might be made to depend, either on the density of the fog produced by steam, or on its density when produced by reduction of pressure, as in the air-pump experiment.

Before making any experiment on the fog-producing powers of flames and combustion, it was necessary to test the effect of heat on the apparatus to be used, so as to be certain the effect was entirely due to the flame and nothing due to the heating of the apparatus used in collecting the hot gases. I accordingly experimented in the following manner —The cotton-wool filter was detached from the experimental receiver, and there was placed between it and the receiver a short length of glass tube, so arranged that the air after passing through the filter should pass through the tube on its way to the receiver. The tube was so arranged that it could easily be taken out to be cleaned, and opened for introducing into it any substance the effect of which we might wish to test. The receiver was connected to an aspirator, by means of which filtered air was drawn into the apparatus.

The glass tube was first carefully washed with soap and water, and then with sulphuric acid, the acid being carefully washed off before the tube was put in its place. Air was now drawn through the apparatus, the air being tested from time to time by the admission of steam into the receiver. At first the steam gave rise to cloudiness, but as the dust gradually got cleared out the clouding become less and less, till at last it disappeared, indicating a dustless state of the air in the receiver. After this condition was attained the glass tube, through which the filtered air was passing, was heated, to get the effect, if any, due to heating glass, and also to make sure that the

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effect produced by any substance placed in the tube was due to that substance alone. The result of heating the clean and empty tube was most remarkable, and very unexpected. A slight heating was sufficient to give rise to a very dense fog, on admission of steam to the receiver. We might have imagined that the careful washing the tube received was sufficient to make the glass Yet we see it was still so foul that heat drove off sufficient matter in a clean. fine state of division as to give rise to a dense fog. The glass tube was now highly heated, to see if heat would cleanse it. After cooling it was again heated to the same amount as at first. It was now found to be quite inactive. No fogging whatever appeared in the receiver. If, however, the tube was again highly heated fogging appeared. In testing different substances placed in the tube, it was therefore necessary to use only a low degree of heat, so that none of the effect might be due to the tube. After each experiment the tube was highly heated, to thoroughly cleanse it, before introducing the substance to be When this was done, and a lower degree of heat employed, I could tested. perfectly trust to the tube being inactive.

The next experiment was made with a small piece of brass wire placed in the testing tube. While it was cold there was of course no fogging, but when slightly heated, a dense clouding resulted. A piece of iron wire, and other substances, all gave a similar result. The wires were now highly heated in a BUNSEN flame before being put in the testing tube. On heating they were now found to be quite inactive, not the slightest fogging appeared. The high temperature had acted on them as it acted on the glass, and destroyed their dust-producing powers.

A piece of brass wire was now carefully filed bright, so as to remove all uncleanness from it, it was then placed in the experimental tube, care being taken that it was not touched with the hands. When heated it only gave rise to the faintest cloudiness. These experiments prove that the cloudiness was produced by some matter driven off by the heat from the outside of the metal. The slight cloudiness produced by the filed wire being due to the slight contamination got when being filed.

The amount of matter which is driven off these wires by heat is extremely small, and its result as a fog-producer so great, that this apparatus places in our hands a means of detecting in gases quantities of matter so small as almost to rival in delicacy the spectroscope. The following experiment will give an idea of the marvellous smallness of the amount of matter which may be detected in this way. If we take a small piece of fine iron wire,  $\frac{1}{100}$  of a grain in weight, and place it in the experimental tube, and apply heat, it will give rise to a very decided cloudiness. Now take the wire out, and if you so much as touch it with your fingers, on again returning it to the tube and heating, the fact of your having touched the  $\frac{1}{100}$  of a grain of iron wire will be declared by the fog which forms in the receiver. The effect seemed so great for so small a cause, that I repeated the experiment a great number of times, sometimes putting in the wire and getting the fog, and sometimes going through all the motions and changes necessary for, but not putting it in, and getting no fog, that I am compelled to come to the conclusion, that the fogging is really caused by the contamination due to the touch.

A great number of different substances were tested in this apparatus, and, as might have been expected, all were active fog-producers. Amongst other substances tried were different salts. One point noticed was that their activity did not depend on their power of evaporating or subliming. Camphor, though subliming and evaporating quickly, scarcely ever gave any fog, only a heavy coarse-grained fog which settled at once, while ammonic carbonate, sodic carbonate, and sodic chloride were very active, indeed the latter salt is one of the most active substances I have tried. If we place a crystal of sodic chloride  $\frac{1}{100}$  grain in weight in the tube, and apply heat, it will continue to give off nuclei sufficient to form a dense fog for a long time, without apparently losing in size.

We see from these experiments that when testing the fog-producing power of a flame, it will not do to collect the products of combustion and draw them into our experimental receiver, as the heat would raise a dust from the surface of the collecting tube sufficient to cause a dense fog; another method of experiment was therefore devised. It was, however, necessary before proceeding further, to test the effect of the gas to be burned, to see if it was active as a fog-producer. Gas from the gas pipes was accordingly passed into the experimental receiver, and tested with steam, and found to be perfectly inactive. No cloudiness appeared. Any effect then produced by the burning gas could not be due to dust carried in by the gas.

The apparatus was now arranged in the following manner to test the fog-producing powers of the products of combustion from a gas flame :—Two receivers were arranged alongside each other, and connected by means of a 'pipe. Gas was led into the first receiver by a pipe terminating a short distance inside the receiver in a glass tube, the end of which was drawn to a fine jet at which the gas was burned. The receiver used for this purpose was so large that the flame could not heat the glass sufficiently to make it active as a fogproducer. After the gas was lighted, a current of filtered air was drawn through the receiver to supply oxygen for the flame. The products of combustion were drawn into the second receiver through the connecting pipe. In this second receiver the products of combustion were tested from time to time with steam.

At first, of course, the air which came would be unfiltered dusty air; but as nothing but filtered air entered, this dusty air ought gradually to give place to pure air. It was found, however, that after filtered air had been

drawn through for a long time, there was not the slightest sign of the air becoming purer. To make sure the fogging was due to the flame, the gas was turned off, and combustion stopped, while the circulation was kept up. In a very short time after this was done, the air showed a marked decrease in cloudiness, and after a time became pure.

This method of testing the effect of combustion does not seem, at first sight, the best. The intention was to have, first, circulated the air till perfectly pure, and steam gave no cloudiness, and then to light the gas and see the effect. The difficulty of working in this way was that I could not light the gas without introducing a disturbing element. It was intended to have lit the gas by means of an incandescent platinum wire, but on testing the effect of the hot wire alone, it was found to make the air active, and powerfully fogproducing. By highly heating the wire, it was possible to make it less active at lower temperatures, but the temperature produced by igniting the gas would again make it active.

I have great hesitation in coming to any conclusion from this experiment. At first sight it would look as if the small flame is very far from being a dust destroyer, and is on the contrary a very active producer of it. It will be remembered that the flame was fed with filtered air, and the result of the combustion of filtered air and dustless gas is an intensely fog-producing atmosphere, and that the fogging is due to dust cannot, I think, be doubted, as the products of combustion, when filtered, give no cloudiness when steam is added. Yet the question may be asked, Was the dust produced by the combustion ? It seems almost possible it might be the result of soda driven off by the heat from the glass jet.

On the 8th and 12th of January this experiment was repeated. The glass jet at which the gas was burned being removed, and a platinum one put in its place. Platinum was selected because it was thought in the highest degree improbable that any nuclei could be driven off the platinum by the heat of the gas flame. After the jet was fixed in its place it was highly heated to thoroughly cleanse and make it inactive at the lower temperature produced by the flame. The gas was lit, and the receiver then put in its place, and the supply of filtered air drawn through the apparatus. The result was the same as before. Increase of fogging on the gas being lighted, and the fogging continued so long as the gas was kept burning, and only stopped when the flame was put out.

There seemed a possibility that the fogging might be due to some residual motes still remaining in the receiver getting into the flame and being broken up by the heat into a great number of parts. The experiment was accordingly varied to meet this. A fine platinum wire, which could be heated by a battery, was arranged so that the gas might be lit by it without opening the receiver, the platinum wire being previously highly heated to cleanse it as much as possible. The receivers being closed, and the gas not lit, air was drawn through the apparatus till the air in the receivers was purified; and no cloudy condensation took place on admitting steam. Contact with the battery was now made, and the gas lit. At once a densely fogging atmosphere was produced.

No doubt part of this fogging was due to nuclei driven off the heated platinum wire, but as the wire was previously cleansed, and only heated for a short time, and quickly removed from the flame, there would be but little due to this cause, and what dust it did give off would be so fine that the heat of the flame would not be likely to break it up any further, and it would be gradually removed by the circulation, and its place filled with filtered air. It was, however, found that though the supply of air was kept up, and the flame kept burning for some time, the fogging showed no signs of decreasing. On shutting off the gas, the fogging at once began to diminish, and soon cleared away, showing that the fogging was due to the products of combustion.

These experiments seem to indicate that the combustion of dustless gas and dustless air do of themselves give rise to condensation nuclei, and do not act by simply breaking up larger dust motes into smaller ones. These nuclei produced by the combustion of gas must be extremely small, as a very small flame so loads a considerable current of dustless air as to cause it to become full of a very fine and closely packed form of fog when mixed with steam.

The question may here be put, Is it really dust which is driven off by the heat from the surface of glass, from the brass and iron wires, and from the other substances? It is extremely difficult to get a direct answer to this question, but I think that, reasoning from the known conditions necessary for the condensation of vapour, it is extremely probable that it really is an extremely fine form of solid matter which is produced under these circumstances. Further, they have all been put to the test of the cotton-wool filter, and all of them have been filtered out and the air made non-cloud-producing. If it was some gas or vapour which was produced by the heat, we see no reason why the cotton-wool should have kept them so completely back.

Another set of experiments was now made to test the fog-producing power of air and gases from different sources. The air to be tested was introduced into the experimental receiver, and steam blown in and mixed with it. Its fogproducing power was tested by the density of the cloudiness produced, and also by the time the fogging took to settle. It was always found that the air of the laboratory when gas was burning gave a denser fog than the air outside, sometimes two or three times as dense. The products of combustion from a BUNSEN flame and from a smoky flame were compared. They were found to be about equally bad, and both much worse than the air in which they were burned. These products were collected by holding the open end of the receiver over the

flame, taking care not to heat the glass. Products of combustion from a clear part and from a smoky part of a fire were tested, and found to be about equally foggy, and both much worse than the air of the room.

From these experiments it would appear that combustion under all conditions is bad as a fog-producer ; bad, whether the combustion be perfect, as in a BUNSEN flame and a clear fire, or imperfect as in a smoky flame and smoky fire. It is therefore hopeless to expect that by adopting fires having a perfect combustion, such as the gas ones now so much advocated, we would thereby diminish the fogs which at present, under certain conditions, envelop our towns, and give rise to so much that is both disagreeable and detrimental. All fires, however perfect the combustion, are fog-producers when accompanied by certain conditions of moisture and temperature. From this it will be observed that it is not the visible dust motes seen in the air that form the nuclei of fog and cloud particles, as these may be all destroyed by combustion, and yet the air remain fog-producing. No doubt these motes also play their part in the condensation, but their number is too small to be of importance. The fog and cloud nuclei are a much finer form of dust, are quite invisible, and though ever present in enormous quantities in our atmosphere, their effects are almost unobserved.

A number of experiments have been made by burning and highly heating different substances to test their fog-producing powers, and I have found that highly heated sodic chloride, as, for instance, when burned in an alcohol flame, or salt water spray heated in a BUNSEN flame, gives rise to an extremely dense fog when tested with steam. But perhaps the most active of all substances I have yet tried is burning sulphur. The fog produced when steam has been blown into air in which a very little sulphur has been burned is so dense that if ever fog was "cut" it might or should be. So dense is it that it is impossible to see through a depth of more than 5 centimetres of it. The sulphides when burned also give similar results.

These experiments evidently introduce a new element into the investigation. We have here not only to do with the attraction of the different molecules of the same kind, but the gaseous molecules in this case have also chemical affinities for each other. It is very difficult to understand this marvellous fog-producing power of burned sulphur. Sulphur in burning gives rise to sulphurous acid. Now from experiment I have made with sulphurous acid prepared from sulphite of soda and sulphuric acid, and also from copper and sulphuric acid, the sulphurous acid being carefully dried with sulphuric acid, I do not find it active as a fog-producer. It gives rise to no fumes, it does not increase the fogging of dusty supersaturated air, and produces no fog in filtered supersaturated air.

Sulphuric acid vapour, it is well known, gives rise to dense fumes by combining with the moisture of the air, and I find, under certain conditions, it also

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gives rise to a dense fog with steam, but I also find that these fumes and fog owe their formation to dust. This is illustrated by the following experiment.

In a retort was placed a quantity of sulphuric acid. The stopper of the retort was removed, and in its place was put a tube connecting the retort with a cotton-wool filter. The neck of the retort was connected to a wash-bottle by means of a glass tube. An aspirator drew the air out of the wash-bottle, and thus kept up a current of air from the filter through the retort to the wash-bottle, the air bringing the sulphuric acid vapour along with it. At first, when unfiltered air passed, dense fumes filled the retort and wash-bottle, but when the filter was introduced the cloudiness gradually disappeared. The absence of dust entirely prevented any foggy condensation, even though there were chemical affinities. After the experiment had been continued for some time, slight fumes began to appear, even when filtered air was passing, but this only happened when the acid became very concentrated, and much acid evaporated, and the fumes with filtered air were very slight, while unfiltered air gave very dense fumes.

It is not necessary to suppose the want of dust prevented the chemical affinities from acting, it only prevented the new compound from condensing When the acid was weak its vapour would combine with the in cloud form. moisture in the air, but would remain as vapour when there was no dust for it to condense upon. But when the acid became highly concentrated, the molecular strain would be greatly increased on account of the vapour tension being greatly in excess of that due to the temperature, and it would then seem to be able to condense without the presence of a "free surface." There is, of course, the possibility that the filtering of the air was not perfect. I may remark here that the fumes of highly concentrated sulphuric acid are found to be an excellent fog-producer. If we dip a glass rod in the acid, and heat it highly, and allow a little of the fumes to pass into the experimental receiver, steam will now give a very dense fog indeed.

The effect of dust in producing the cloudy form of condensation of other vapours than water was tried. With all the vapours experimented on, which included alcohol, benzol, and paraffin oil, it was found that pure air gave no clouding whatever, while unfiltered air gave more or less cloudiness with all of them.

The cause of the blue colour of the sky has long afforded interesting matter for speculation. The theory which seems most satisfactorily to explain its blue colour depends upon the property which very small particles of matter have of scattering only the rays of the blue end of the spectrum, and the question is, What are these very small particles composed of ? It has been suggested that they are very small particles of condensed water vapour. Now, we have shown the high improbability of water vapour ever condensing out in a visible form in pure air, and that if it did condense in those circumstances, the particles would be large. From the all-pervading presence of the infinitesimal atmospheric dust, the idea naturally suggests itself, that the blue sky may be caused by the light reflected by this dust. What seems to support this theory is that, as we ascend to high elevations, the sky becomes deeper blue, this being caused by fewer and only the finer of the dust particles being able to keep floating in the thin air at these elevations. Further, after rain the sky is darker blue, this deepening of the colour being caused by much of the dust being washed out by the falling rain.

I wish now to apply the result of these experiments to the great fog question, which Dr Alfred CARPENTER opened at the last Social Science meeting, and to which at present so much attention is being directed. The increased frequency and density of our town fogs are now becoming so great as to call for immediate action. But before doing anything, a much clearer knowledge of the conditions which produce a fog is necessary, or much time will be lost and expense uselessly incurred. I wish, therefore, to call attention to the teaching of the experiments described, so far as they bear on this important question. What I have to say on this point must, however, be received with reservation. The conditions of a laboratory experiment are so different and on so small a scale, that it is not safe to carry their teaching to the utmost limit, and apply them to the processes which go on in nature. We may, however, look to these experiments for facts from which to reason. and for processes which will enable us to understand the grander workings of nature.

We have seen that fogs and clouds are produced by the condensation of vapour on the dust particles floating in the air. The condensation is produced by cold, the result of radiation or expansion of the air, either by reduction of barometric pressure or by the elevation of the air into higher regions. A fog, therefore, before it appears, is every particle of it represented by a particle of very fine invisible dust; the thick visible fog was previously represented by an invisible dust cloud. Now, it is very evident that if there is an enormous number of these dust particles in the air, so that they are very close to each other, then each particle will only get a very small amount of vapour condensed upon it. It will therefore become but little heavier. and will float easily in the air. To this light and dense form of condensation we give the name of fog. If there are fewer dust particles, then each particle gets more vapour, and each particle is heavier and settles sooner. It must not be supposed, from this, that rain only falls when these dust particles are few, and the vapour particles very large, because there seems to be always enough dust in the air to make the cloud particles small enough to keep suspended. Their union and fall as rain is determined by certain conditions

on which the present inquiry throws no light. But of clouds there are vast degrees of texture, the fog being the finest grained, most dense and persistent, almost never settling down.

From this view it will be seen that the vapour condenses on the solid matter floating in the air, whether that matter be fine dust or condensed smoke. This view I am aware is different from the one generally received, namely, that cloud particles are hollow vesicles, hollow to enable them to float, and that smoke, &c., attaches itself to the outside of these vesicles.

Since, then, fogs are produced by an over-abundance of fine atmospheric dust in a moist atmosphere, and as we have but little control over the moisture in the air, our attention must be directed principally to the diminution of the atmospheric dust, if we wish to reduce the density of fogs. We have seen that all forms of combustion, however perfect, are great producers of this less than microscopic dust. The brilliant flame, the transparent flame, and the smoky flame are all alike fog-producers. Perhaps there may be some form of combustion which is not a dust-producer, or some form of combustion which may give a coarse-grained dust. If there is, it ought to be more generally known. As a correction of the present form of combustion, perhaps something could be done to arrest the dust before it escapes into the atmosphere. But any plan which at present suggests itself is too troublesome and expensive ever to be put into general use. To prevent mistakes I may here remark, that when speaking of the dust produced by combustion, I do not mean the dust usually spoken of in connection with fires, as it is comparatively heavy, and soon settles to the ground, nor do I refer to smoke or soot. The dust I refer to is the invisible dust, so fine that it scarcely settles out of the air. If we put air into the experimental receiver and leave it for days without any communication with the outer air, we will still find it fog-producing, though in a very marked degree less than at first.

All our present forms of combustion not only increase the number and density of our town fogs, but add to them evils unknown in the fogs which veil our hills and overhang our rivers. In the country the fogs are white and pure, while in towns they are loaded with smoke and other products of imperfect combustion, making the air unwholesome to breathe and filthy to live in. But why should these two miseries always come together? Either the fog or the smoke is bad enough alone ; why should the smoke which usually rises and is carried away by the winds fall to the ground when we have fogs? I think that the conditions which account for the fog also account for the smoke falling. When we have fogs, the atmosphere is nearly saturated with vapour, and the smoke particles, being good radiators, are soon cooled, and form nuclei on which the vapour condenses. The smoke particles thus become loaded with moisture, which prevents them rising, and by sinking into our

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streets add their murky thickness to the foggy air. This seems to explain the well-known sign of falling smoke being an indication of coming rain. That the colour or blackness of what is called a pea-soup fog is due to smoke, is, I think, evident from the fact that a town fog enters our houses and carries its murky thickness into our rooms, and will not be induced to make itself invisible however warmly we treat it. It will on no account dissolve into thin air, however warm our rooms, for the simple reason that heat only dissolves the moisture and leaves the smoke, which constitutes a room fog, to settle slowly, and soil and destroy the furniture. If the fog was pure, that is to say, was a true fog and nothing but a fog, such as one sees in the country, it would dissolve when heated, as every well-conditioned country fog does—at least I never remember meeting a fog in a country house.

But while admitting the bad effects of a fog aggravated by smoke, yet we must not forget the probable good effects of the smoke. It has been elsewhere pointed out that the suspended smoke or soot may exercise the wellknown disinfectant properties possessed by the different forms of carbon. Before utterly condemning smoke it will be necessary fully to consider its value as a deodoriser. And further, we must remind those who are crying for more perfect combustion in our furnaces and grates, that combustion, however perfect, will not remove or diminish fogs. It will, however, make them cleaner, take away their pea-soupy character, but will not make them less frequent, less sulphurous, less persistent, or less dense.

We have shown that sulphur in its different forms when burned is most active as a fog-producer. Now, almost all our coals contain sulphur, which is burned along with the coal, and it is certainly worth considering whether some restriction ought not to be put on the amount of sulphur in the coal used in towns. The quantity of burned sulphur that escapes from our chimneys is Suppose we put the amount of coal annually consumed in the verv great. London district at a little over 7,400,000 tons. Now, the average amount of sulphur in English coal is more than 1.2 per cent. Suppose that it is 1 per cent., so as to be within the mark, that would give 74,000 tons of sulphur burned every year in London fires, or at the rate of about 200 tons in an average day, and the amount will be greater in a winter day-a quantity somewhat alarming, and quite sufficient to account for the density of our fogs. Its presence and effects during our fogs is very evident in the discoloured metal on our street door and in our houses.

But, like smoky fires, burnt sulphur is not an unmitigated evil. During fogs the air is still and stagnant; there is no current to clear away the foul smells and deadly germs that float in the air, and which might possibly be more deadly than they are if it were not for the powerful antiseptic properties of the sulphurous acid formed by the burning sulphur. Before condemning the smoke and fog-producing sulphur, it would be well for us thoroughly to investigate their saving properties and weigh their advantages, lest we substitute a great and hidden danger for an evident but less evil.

While we look upon fires and all forms of combustion as fog-producers, yet we must remember there is ever present plenty of dust in the air to form clouds and even fogs; fires simply increase the amount of the dust. Now it is evident that as the rain is constantly washing the dust out of the air, fresh supplies must therefore be constantly added.

We have every reason for supposing that there are immense quantities of very fine salt-dust ever floating in the air. This is evidenced by the everpresent sodium lime that at one time so troubled spectroscopists. One source of the supply of this salt-dust is evidently the ocean, and it affords us another example of how very closely the phenomena of nature are interlinked. The ocean, which under a tropical sun quietly yields up its waters to be carried away by the passing air, almost looks as if he repented the gift, when tossed and angry under tempestuous winds, as he sends forth his spray, which dried and disguised as fine dust becomes his messenger to cause the waters to cease from their vaporous wanderings, descend in fertilising showers, and again return to their liquid home.

## PART II.

Since making my first communication to this Society on Dust, Fogs, and Clouds, many of the experiments have been repeated under different conditions and with improved arrangements of apparatus. I shall first give a short description of the changes made in this direction, which seem to fill up some points wanting in the first paper, and shall then describe some experiments made in a department of the subject which I have only touched upon.

We have seen that when steam is blown into dustless air there is no cloudy condensation, and that the vapour remained supersaturated till it came in contact with the sides of the receiver, on which it deposited itself. My next experiments were to determine to what extent dustless air can be supersaturated without the vapour condensing into drops—to determine whether vapour molecules can combine with one another to form a liquid, or whether they must have a nucleus to condense upon even when the vapour is very highly supersaturated. It is evidently very difficult to get a definite answer to this question, and I shall only describe the direction in which I sought to get an answer, the experiments not being sufficiently conclusive to settle the point.

The first thing to be done was evidently to get quit of all "free surfaces" of all nuclei of condensation, and the experiments have resolved themselves very much into questions of filtration, as I have not yet arranged any experi-

ment in which I have been certain there might not have been some nuclei present. The first step in this direction was to test the action of the filter through which the air passed. All the cotton-wool was removed from the filter and a fresh quantity put in. At first only a thin layer was used, and its effect tested, noting the degree of cloudy condensation produced. More cotton-wool was then put over the first layer, and the improvement noted. Fresh quantities were added till no improvement was observed. Then double the total quantity was put in, and the filter was now considered to be doing all that cotton-wool could do to purify the air of the receiver from dust.

The result was—when a small quantity of steam was blown into the receiver there was no cloudy condensation whatever; the receiver remained perfectly clear. But when the steam valve was opened wider and more steam allowed to enter, although no effect was noticed at first, yet after a time the vapour became so supersaturated that it condensed and fell as fine rain. If a still greater amount of steam was blown in, then it was seen condensing on entering the receiver, and the falling rainy condensation was seen tossed about by the rush of the entering steam.

Attention was now directed to the steam. It seemed possible that nuclei might be given off from the hot sides of the boiler, and from the hot parts from which the steam was rising. To prevent any nuclei which might be formed in this way from entering the receiver, the end of the steam pipe inside the receiver was covered with a cotton-wool filter. The result was, however, as before, with little steam, no condensation, with much steam, rainy condensa-On account of the tendency of the cotton-wool to get wetted by the tion. steam, the action of the filter did not seem satisfactory, some parts getting wet and stopping the passage of the steam, and throwing all the duty on the weak parts. The experiment was accordingly arranged in the following way :-- The steam was generated in a glass flask. This flask, filled with water, was placed in a vessel full of water, kept boiling during the experiment. In order to make the water in the glass flask boil, or rather evaporate, under these conditions, a stream of filtered air was blown through it, and the mixture of air and vapour blown into the receiver. Again the result was as before-rainy condensation when highly supersaturated. By this last arrangement it seems impossible any nuclei could be given off from the vessel in which the water was boiled, and the fine drops given off by the bubbling of the air and the vapour in the flask are probably all caught on the sides of the pipes, because if they did enter they would form nuclei in very slightly supersaturated, as well as in highly supersaturated vapour. We may therefore conclude from these experiments that the nuclei of the rainy condensation in highly supersaturated vapour are either some fine form of dust which the cotton-wool cannot keep back, or are produced by the vapour molecules combining together without a nucleus.

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If all nuclei are absent, water may be cooled below the "freezing-point" or heated above the "boiling-point" without any change taking place; but there seems to be a limit to the amount it may be cooled or heated under these conditions without the water freezing or boiling. However carefully we may make the experiments after the water has been cooled to a certain amount, it always freezes without the presence of a free surface, and it also boils without the presence of a free surface when heated much above its "boiling-point." In these cases there always, however, appears to be some want of continuity or uniformity produced by the presence of some substance which exercises an influence on the water, and determines a weak point at which the change begins, and when once begun progress is of course rapid. In water we can easily understand how the sides of the vessel and the surfaces of foreign matter, &c., will form weak points, from which "free surfaces" are developed, extending into the mass of the liquid; but it is much more difficult to understand how weak points can be formed in gases, and even when started they have no power of These considerations would seem to suggest that the propagating themselves. rainy condensation in filtered air may be produced by some form of nuclei which passes the cotton-wool filter, and which are perhaps very small, and do not become active as nuclei till a considerable degree of supersaturation is attained.

There are, however, certain considerations which show that if the degree of supersaturation is sufficiently great, then condensation will probably take place without nuclei. Professor JAMES THOMSON \* has shown that the isothermal curves obtained by Dr ANDREWS from his experiments on carbonic acid at temperatures below the critical temperature of that substance may not be really so discontinuous as they appear, and that there may be a condition of that substance which would be represented by a continuation of the vapour part of the curve beyond the "boiling" or "condensing line." To test this point Professor THOMSON suggested an experiment in which saturated steam, surrounded by a heated vessel, was to be expanded till it was cooled below its condensing point for its pressure, and the effect on the volume and pressure noted. This experiment, I believe, has never been made. We, however, see from the experiments described, that the theoretical extension of the curve discovered by Professor Thomson has a real existence. This curve of Professor THOMSON'S shows that the degree of supersaturation possible has a perfectly definite limit, beyond which supersaturation is impossible. Further, if we examine these curves of Dr ANDREWS, which we may extend to water, they show us that it is only for temperatures below the critical temperature of the substance that supersaturation is possible. At temperatures above the critical

\* "Proceedings of the Royal Society," No. 130, 1871.

temperature there is no boiling and condensation, the change being perfectly continuous from the one state to the other, if under those conditions we can say there are two states.

All the previously described experiments have been made at temperatures at which the condensed water was in a liquid state. It was now desirable that they should be made at lower temperatures, to see if the same conditions are necessary when the vapour condenses at temperatures below the "freezingpoint," and passes from the gaseous to the solid state. The experiments were made with the air-pump arrangement of apparatus, the condensation being effected by the cooling produced by expansion in the receiver. In the first experiments the receiver was placed in a freezing mixture. They were, however, repeated under more favourable conditions during the severe cold of January last. The apparatus was removed to the open air and experiments made with it. The temperature at the time was 8° Fahr. The results were the same as at higher temperatures-cloudy condensation with unfiltered air, and no condensation when filtered air was used. The amount of cloudiness produced was not so great as at higher temperatures. This is due to the smaller amount of vapour in the air at the lower temperature.

I did not succeed in observing any of the optical phenomena produced by small crystals of ice in our atmosphere. This was probably due to the conditions under which the crystals in the experiment were produced. As the crystals were rapidly formed, there would not be time for the vapour molecules to arrange themselves in the simpler forms of crystallisation, but by being forcibly compelled to solidify, would form complicated shapes, which do not give rise to any peculiar optical phenomena.

In the first part of this paper I have referred to the detection of small quantities of matter driven off by heat from pieces of iron, brass, and other kinds of matter. By the arrangement of apparatus then described, it was shown to be possible to detect the dust drawn off so small a piece of iron wire as the  $\frac{1}{100}$  of a grain. In later experiments in this direction, the apparatus has been entirely changed. In place of using the supersaturation produced by mixing steam and cold air, the air-pump arrangement of apparatus has been employed, and is found to work much more satisfactorily than the other. The impurities drawn off so small a piece of iron wire as the  $\frac{1}{2000}$  of a grain can with ease be detected with it.

The arrangement of the apparatus for this purpose is as follows — A glass flask provided with a tight-fitting stopper, through which pass two tubes, which rise to a short distance into the interior of the flask. One tube is connected to an air-pump, the other terminates in a stop-cock, to which is attached a cotton-wool filter. A piece of glass tube is introduced about the middle of the length of this pipe. Some water being placed in the flask, the apparatus is complete. The glass tube must now be thoroughly cleansed. This is done by highly heating it in a BUNSEN flame, while air is being drawn through it. The end of the glass tube next the filter is now opened, and three or four small pieces of iron wire introduced into it. The pieces of wire are placed some distance from each other, and near one end of the tube. The tube is now closed, and the BUNSEN flame placed under the other end of the tube, and far enough away from the pieces of iron so as not to heat them. The air in the apparatus is now thoroughly cleansed by pumping out the air and admitting filtered air, till no cloudiness appears. During this process the height of the flame has been reduced, so as the temperature may not be high enough to drive anything off the glass tube. When the air is quite pure, and all rainy condensation ceased, the flame is reduced to about one-half, so as to leave a good margin of safety. After this is done, one of the small pieces of iron wire is drawn from the cold part of the tube by means of a magnet, and dropped in the hot part, and two or three strokes of the pump are made, to cause a current of air to pass through the tube and bring whatever impurities are driven off the iron into the flask. The stop-cock at the filter is now closed, and a slight vacuum made. The amount of nuclei given off by the wire is indicated by the amount of cloudy condensation which now takes place.

To make further certain that the impurities came from the wire, the piece of iron is now removed by means of the magnet, when the filtered air is now found to come into the flask without any nuclei, the air remaining cloudless on expansion. To make still further certain of the result, another of the pieces of wire is drawn into the hot part of the tube, when the cloudiness again appears, and again disappears after its removal, or after it has been highly heated.

The pieces of iron wire experimented on weighed from  $\frac{1}{1000}$  to  $\frac{1}{2000}$  of a grain. With pieces so small as this, so abundant and evident is the cloudiness produced, that I feel certain that if I could have manipulated, say the  $\frac{1}{100,000}$ of a grain, the effect would have been perfectly definite and decided. Thousands of particles driven off the  $\frac{1}{2000}$  of a grain, and the wire not perceptibly lighter afterwards, indicates almost molecular dimensions. It seems probable that some of the nuclei in these experiments are driven off as gases or vapours. These gases and vapours will afterwards condense when cooled in the receiver. It is not necessary that these gases should have nuclei on which to condense, as they will be highly supersaturated when cooled to the temperature of the receiver, and we know that it is only when supersaturation is slight that nuclei are necessary. These gases will, according to their composition, condense either to solid or liquid nuclei, on which the water vapour will condense.

In the first part of this paper attention has been called to the importance of the composition of the atmospheric dust. It was pointed out that some kinds

of dust will have a greater attraction for water vapour than others, and that chloride of sodium dust would probably condense vapour and cause fogging in an atmosphere which was not saturated.

There are evidently two ways in which dust may exert an attraction for water vapour, and determine its condensation while still unsaturated. The first is the attraction which the surface of some kinds of matter has for vapour, a power which they have of condensing a film of water on their surface. This power they possess at all degrees of saturation, but the amount they condense depends on the degree of saturation. Glass might be taken as an example of a substance whose surface has a strong affinity for water, a fact which disagreeably demonstrates itself in the conducting power of glass insulators of electrical apparatus in damp weather. The dust nuclei are so small that the condensing power of fine pores is not likely to have any influence. The other form of attraction which may exist between the dust and water vapour, is the chemical affinity which exists between the two. This will evidently depend on the composition of the dust or nuclei. As an example of this form of attraction, it will be sufficient here to mention the well-known affinity which chloride of sodium and other salts have for water, causing them to become wet when the air is moist.

We shall presently see that besides these two ways in which nuclei may condense vapour in unsaturated air, there is another way in which the condensation may be produced in unsaturated as well as in saturated air without nuclei. This happens when there are gases or vapours present which have an affinity for each other, and the resulting compound is in a highly supersaturated condition. These new compounds under these conditions condense and form nuclei, which may be solid or liquid, and may or may not have affinity for water.

Now it is evident that if there are any kinds of matter in the form of dust in the air which have an affinity for water vapour, they will determine condensation in unsaturated air. Some experiments were made to see to what extent cloudy condensation could be produced under these conditions. My first experiments were made by burning sulphur, and vapourising chloride of sodium. A small quantity of sulphur was lighted, and an open-mouthed receiver held over it for a few seconds, and then placed on the table. At first scarcely anything was visible, but after a time a decided haze made its appearance, and the density of this haze or fog was always in proportion to the moisture present in the air. The damper the air the thicker the fogging, and if the air was nearly saturated, the result was very remarkable. If the inside of the receiver was wetted so as to moisten the air, the sulphur products were a little more evident, and on placing the receiver on the table, a thin haze could be seen. After a time, however, this haze grew denser and denser, and

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after fifteen or twenty minutes the receiver was full of a dense white fog, which remained for a long time.

Similar results were got by vapourising chloride of sodium. The salt was in some cases vapourised by a BUNSEN flame. It was also vapourised by placing it on a piece of hot iron, and the receiver held over it to collect the vapour, which condensed and formed nuclei, which determined the condensation of the water in unsaturated air. In some experiments the salt was vaporised in a heated platinum tube and drawn along with air through a coil of pipe to cool it, before admitting it into the receiver. In these experiments the density of the fogging was in proportion to the vapour present, and if the experiment was made in a wetted receiver, the fog took some time to attain its maximum density.

The condensing power of sulphur products and salt can be illustrated in another way. The air with either of these substances in suspension, is drawn through a coil of pipe to cool it. If now this stream of air is made to strike any wetted surface, the wetted surface looks as if it had suddenly become heated—a stream of condensed vapour flows away from it. This vapour is, of course, invisible if ordinary air is used, and without the powerfully condensing nuclei.

Experiments on a larger scale were also made with these two substances. A little sulphur was burned in a cellar, the air of which was damp, but not saturated. The temperature was about  $43^{\circ}$  Fahr., and the wet and dry bulb thermometers showed a difference of from  $\frac{1}{2}^{\circ}$  to 1° during the experiments. After the sulphur was burned a fogginess was evident, but, on returning half an hour afterwards, the fogging was found to have increased very greatly in density, the air was very thick, and not the slightest smell of sulphurous acid perceptible. This fog hung about the cellar for many hours. The experiment was repeated with chloride of sodium, the salt being sprinkled over an alcohol flame. The result was similar to the sulphur products, a fogging which gradually increased in density, and very slowly cleared away.

Experiments have also been made by burning sulphur in the open air. When the air is dry the fumes can only be traced a short distance, but as the amount of moisture increases the cloudiness becomes more and more evident, and in certain conditions of the atmosphere the cloudiness can be distinctly seen flowing away in the passing air, leaving the sulphur in a pale thin stream of vapour, which gradually increases in size and density, and rolls away in a horizontal cloudy column, ten or fifteen feet in diameter, clearly marked out from the surrounding air.

There may be a certain amount of doubt as to the action of the heated salt in these experiments. When heated in the BUNSEN flame it is probable decomposition of some of the salt takes place, and part of the result may be due to

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the hydrochloric acid set free. In order to prevent this decomposition as much as possible, I have made some experiments at as low temperatures as possible, and the results are the same as when higher temperatures are used, allowance being made for the smaller amount of salt volatilised.

The action of the products of combustion of sulphur would appear to be something like the following :---When the sulphur combines with the oxygen of the air, sulphurous acid is formed. I have shown in the first part of this paper that sulphurous acid has but little condensing power; we must therefore look to the change which takes place in the sulphurous acid for the explanation of the wonderful condensing power of the sulphur products. The sulphurous acid becomes further oxidised in the air, and sulphuric acid is produced, and it is the great affinity which this sulphuric acid has for water which enables it to rob the air of its moisture and condense it in visible form. It does not seem to take long for the sulphurous to change to sulphuric acid in the air. A short time after the sulphur was burned in the cellar all smell of sulphurous acid was gone, and I am informed by Dr Wallace that he has found that all traces of sulphurous acid cease at a short distance from calcining ironstone bings in which much sulphur is being burned. The gradual thickening of the sulphur fog will probably be in part due to this gradual change of sulphurous to sulphuric acid. The gradual thickening of these fogs is also in part due to the slow evaporation of the water from the sides of the receiver, and subsequent condensation on the absorbing nuclei.

I find that the fumes from highly concentrated sulphuric acid have a fogproducing power similar to the products of combustion of sulphur. If we highly heat a glass rod wetted with sulphuric acid, or heat the acid in a platinum cup, and admit a little of the fumes into the receiver, they are found to have a very strong fog-producing power.

The above represents something like what the action of sulphuric acid is in moist air, in which there are no other vapours or gases with which this acid tends to combine. Before considering these more complicated effects I shall describe some experiments made to test the action of acid vapours on moist filtered air. The apparatus consisted of the air-pump arrangement, with test receiver or flask, one pipe as before being connected with the air-pump, and the other with the filter. Between the receiver and the filter was placed a test tube, in which was placed the acid to be experimented upon. The filtered air was caused to bubble through the acid on its way to the moist air in the receiver, the acid being generally kept at the temperature of the room.

When nitric acid is put in the test tube and filtered air passed through it, it is found that its vapour always gives rise to fumes when mixed with the moist air in the receiver. These fumes—as cloudy condensation in unsaturated air may be called—may therefore be produced without nuclei when nitric acid is used. When the air in the receiver is expanded and cooled, this cloudy condensation becomes thicker.

When commercial hydrochloric acid is put in the test tube, its vapour does not give rise to fumes on mixing with the moist air in the receiver, and on expanding and cooling the air, no fumes appear, only the rainy form of condensation is produced. A quantity of very strong hydrochloric acid was prepared by keeping the solution in which the acid was condensed in a freezing mixture. This acid fumed abundantly in the air, but gave no fumes in filtered air, and only rainy condensation when the pressure was reduced.

These two acids act very differently, the first condensing freely at many centres, and without nuclei, and giving a foggy condensation in pure and unsaturated air, while the hydrochloric acid only condenses with difficulty, and at few centres, and only gives the rainy form of condensation when supersaturated.

The next experiments were made with commercial sulphuric acid, and also with some of the acid concentrated by boiling in a glass vessel. The air which had passed through this acid gave no fumes, but on making the slightest expansion a fog appeared. This fog is quite characteristic of sulphuric acid, and is quite different from any artificial fog I have seen. The particles are extremely small, and the display of colour remarkably brilliant, and when properly lighted rivalling in distinctness the colours of the soap bubble. This beautiful fog is only got when the acid is strong, and I think is best produced when the entering air is dry. This point, however, requires confirmation, though the result might be expected, as the surface of the acid will then be less weakened by moisture abstracted from the air. After the acid has absorbed much vapour, or if water has been added to it, the fogging decreases and gives place to the rainy form of condensation when expansion is made. This rainv condensation also disappears when the acid is very weak. If we heat the strong acid to a temperature of about 60° or 70° C., the vapour condenses and forms fumes in pure air without nuclei, and without being expanded.

These experiments show that water vapour may be condensed without nuclei being present. The affinities which the vapours of the acids have for the water, causing the formation of new compounds, and these compounds being highly supersaturated, condense easily without nuclei, and in certain circumstances this condensation may be determined in even unsaturated air. These water-acid nuclei once formed, continue to act as centres of condensation. In these cases the manufactured nuclei are liquid, but solid nuclei may be formed in a similar manner. This may be shown by the following experiment. Place hydrochloric acid in the receiver or flask, and pump out all the air and replace it with filtered air. If, after this is done, and the acid shows no sign of cloudiness, and nothing but rainy condensation on expansion, we take the stopper out of a bottle of ammonia and hold it near the filter, so that the escaping

gaseous ammonia may pass along with the air through the filter, the ammonia on arriving in the flask will combine with the hydrochloric acid and form a dense cloud of sal-ammoniac. When the ammonia and the hydrochloric acid combine in the filtered air, the tension of the sal-ammoniac vapour so formed is enormously greater than that due to the temperature, and it easily condenses without nuclei. This experiment suggests that part of the rainy condensation given by hydrochloric acid may be due to the ammonia in the air combining with the acid and forming sal-ammoniac nuclei on which the vapour condenses.

These experiments show how nuclei may be formed from gases in the air, and these nuclei may have so great an affinity for water vapour as to cause it to condense on them from an unsaturated atmosphere.

Returning again to the action of the products of combustion of sulphur in air, we have seen that these products alone can determine the condensation of water vapour from unsaturated air. There are, however, many substances in the air with which this acid will tend to combine. It would be impossible to go over all the substances in the air which have affinities for this acid, and consider the effects of these new compounds, in moist air. I have, however, selected one, which from the magnitude of its effects deserves special That substance is ammonia, another of the products of combustion of notice. If we take an open-mouthed receiver wetted on the inside, our coal fires. and hold it over a little burning sulphur for a few seconds, as in the previous experiment, we will get a thin haze, which we know tends to thicken. But if on placing the receiver on its tray, we put a drop of ammonia on a piece of glass and introduce it into the receiver, the result is very striking. Dense fumes will be seen to rise from the ammonia, and in a few minutes the receiver will be full of a fog so thick it will be impossible to see an object in the middle of the receiver. In this case there are evidently formed solid nuclei, composed of sulphite and sulphate of ammonium, in a very fine state of subdivision. The intense cloudiness is only in part due to this solid, the greater part is due to the condensation of water vapour. If the experiment is made in dry air the fogging is not nearly so intense as in moist air. By burning a larger amount of sulphur in the moist air of the receiver, we can easily make a fog so very intense that it is impossible to see through an inch of it. This fog is found to be very suitable for experiments on vortex rings, as it is easily prepared, and the "dead" rings dissolve, and do not thicken the air of the room to the same extent as the usual sal-ammoniac rings.

Experiments were also made in the cellar with this fog-producer. The wet and dry bulb thermometers at the time showed a difference of fully one degree. Yet by burning a few grains of sulphur, and dropping on a piece of paper a little ammonia, the cellar became filled with a most intense fog, many times more intense than would be produced by the sulphur alone.

Using the same apparatus as was used for determining the fuming power of the different acids in filtered air, it is found that when experimenting on sulphuric acid and vapour of ammonia, that sulphate fumes are formed in the receiver if the acid is slightly heated, thus showing that this sulphate dust can form without nuclei. It, however, seems in the highest degree probable that when dust is present the dust particles will form the centres on which the sulphate will condense.

Almost all salts when heated in a BUNSEN flame produce nuclei which determine condensation in unsaturated air. The condensing power of the different products, however, differ greatly. The bicarbonate of soda gives but little effect, while chloride of calcium and bromide of potassium are much more powerful. But by far the most powerful artificial fog-producing substance when used in this way is the chloride of magnesium. If we put a small quantity of this salt on a piece of wire-cloth, and heat it with the BUNSEN flame, and collect the products in a wetted receiver, the fog will be seen rapidly forming and showering down the sides of the receiver. As rapidly as the water is evaporated from the sides of the receiver it is condensed by the active nuclei in the gases. After the receiver has been placed on the table for a few minutes it will be found full of a fog so dense it is only possible to see through a depth of five centimeters of it. When a little of this chloride was heated in an alcohol flame in the cellar the result was a fog many times more dense than that produced by sulphur alone. The fog-producing power of the heated chloride of magnesium would appear to be due to the salt being decomposed by the heat, and free hydrochloric acid being driven off in a highly concentrated The amount of hydrochloric acid is, however, small considering the state. The density of this fog is very much greater than density of the resulting fog. the fog produced by hydrochloric acid prepared from chloride of sodium and sulphuric acid.

In all these cases the reactions are excessively difficult to trace. Other experiments in which the action is much simpler were made by burning a little sodium in the receiver. The combustion of this substance gives rise to its oxides in a fine state of division. This fine soda-dust when mixed with dry air gives but little cloudiness, but when mixed with damp air a dense fogging results. Potassium when burned gives a similar effect, but the fog is not so intense.

We may conclude from these experiments—1. That as regards cloudy condensation of vapour in our atmosphere there is dust and dust. Some kinds of dust have such an affinity for water that they determine the condensation of vapour in unsaturated air, while other kinds of dust only form nuclei when the air is supersaturated, that is, they only form free surfaces on which the vapour may condense and prevent supersaturation. In many of the experiments it

was noticed that when the air was nearly purified, when all the dust which had an affinity for vapour had received its burden of water and settled down, that there remained to near the end of the experiment some particles which seemed to require a certain degree of supersaturation before they became active. In highly supersaturated air all kinds of dust will form nuclei and determine condensation, but in unsaturated air only those kinds of dust which have an affinity for water will be active. We have precisely corresponding phenonema to this in freezing, melting, and boiling. We have water in a solid state at a temperature above the "melting-point," when it is combined with some other substance, as in the water of crystallisation of salts. Water may be liquid at a temperature below the "freezing-point" when mixed with some salts. Water boils at a temperature above its "boiling-point" when it holds some salts in solution, and boils below its "boiling-point" when mixed with some substance having a lower "boiling-point" than water.

2. This affinity which some kinds of dust have for vapour explains why it is that our breath and escaping steam dissolve even in foggy air. The large cloudy particles in our breath and in condensed steam tend to evaporate in the same air in which condensation is taking place, because the dust particles on which the breath has condensed have had their affinities more than satisfied, they therefore tend to part with their surplus by evaporisation in the same air as those particles which have not had their affinities satisfied tend to condense it.

3. Dry fogs are produced by the affinity which the dust particles have for water vapour, in virtue of which they are enabled to condense vapour in unsaturated air. From the experiments with chloride of sodium, from the known affinity of that salt for water, and from the fact that great quantities of salt-dust are ever present in the air, it is evident that if it is not *the* cause of dry fogs in the country it must play some part in those phenomena. There will doubtless be other kinds of nuclei having affinities for water which will cause dry fogs. The nature and composition of these other nuclei will probably be best arrived at by collecting the fog particles by washing or otherwise, and analysing them.

4. That as the products of combustion of the sulphur in our coals, especially when mixed with the other products of combustion, such as ammonia, have the power of determining the condensation of water vapour in unsaturated air, and give rise to a very fine-textured dry fog, they are probably one of the chief causes of our town fogs, as they have a greater condensing power than the products of combustion of pure coal.

Though there may seem to be but little doubt that products of combustion when mixed with the sulphur compounds are most active producers of town fogs, yet we must not rest satisfied that they explain everything. There may be other causes at work, and conditions yet requiring explanation, but as these involve intricate chemical reactions, it will be advisable that the matter be now handed over to the consideration of the chemist.

These chemical nuclei, as they might be called, though found in far greatest abundance in the air of our towns, will no doubt be also found in the air of the country. We know that sulphuric acid and ammonia are constantly being produced by decomposing animal and vegetable matter, and we know that these substances, along with nitric acid and other gases and vapours, are always present in the air.

Again, we have the gases given off from volcanoes, and the amount from this source must be considerable. There are about two hundred active volcanoes constantly discharging their gases into our atmosphere, and it has been roughly calculated that volcanoes evolve ten times more carbonic acid than is given off by the combustion of all kinds of carbonised material. With this carbonic acid there is given off great quantities of sulphurous and other gases which will condense and form nuclei.

Vegetation, both when alive and when dead, gives off vast quantities of small organic particles, and microscopic life, which almost seem to populate the air we breath, and will of course add much to the dust in our atmosphere.

Professor TYNDALL has shown that light decomposes certain gases and vapours, and that this decomposition is greatly aided by the presence of other gases or vapours. It seems therefore probable that the sun's rays will decompose some of the gases and vapours in the air, and if these decomposed substances have a lower vapour tension than the substance from which they are formed, they condense into very fine particles. These particles may be solid or liquid, and will form nuclei for the condensation of water vapour.

We know that there are ever present in our atmosphere great quantities of chloride of sodium and other kinds of dust which have affinities for water. These dust particles by their affinities for water vapour cause condensation to take place in unsaturated air, and if present in great quantities give rise to dry fogs. Let us look briefly at the effect of this affinity between the dust and the vapour. If there was no affinity between the two, then condensation would only begin when supersaturation began, and those dust particles which permitted the vapour to condense on them easiest would get most vapour, and would tend to grow largest. This would evidently tend to inequality in the size of the cloud particles which would determine the fall of some of them through the others. But if there is an affinity between the dust and the vapour, then each particle of dust tends to take the same amount of vapour, and if one particle gets more than its proportion, the others tend to rob it of its surplus. This evidently tends to equality in the size of the cloud particles, and tends also to prevent any of them falling through the others, and thus prevents it beginning to rain, that is, if rain drops are formed by the collision and union of

the quickly falling particles with those falling more slowly. It would thus seem that while on one side if we have no dust we would have no clouds and probably no rain, as we don't know whether the air would ever become sufficiently supersaturated to condense without nuclei. On the other hand, an over-abundance of dust having affinities for water vapour also prevents the vapour falling as rain, as the vapour under these conditions condenses into minute particles which all tend to be of equal size, and none of them are able to fall quickly enough amongst the others to cause collisions. The result is the condensed vapour cloud instead of falling in minute parts as rain, tends to fall as a whole. The air becomes so loaded with the water held in mechanical suspension that it is dragged downwards by its weight. If we make artificial fogs with sulphur fumes and ammonia, or by heating chloride of magnesium, the fog is so heavy it can be poured from one vessel to another.

After the affinities of the dust particles are satisfied, this tendency to stability no longer exists. After this stage the growth of the particles becomes unequal, and, as has been shown by Professor CLERK MAXWELL,\* the larger drops or particles in a cloud tend to rob the smaller ones, or rather, from what we now know, will tend to prevent them growing after the affinities of the nuclei are satisfied.

It would appear, then, that condensation will always begin in our atmosphere before the air is saturated. There is, however, still much to be done in this department of our subject to determine whether the amount of cloudy condensation is always the same for the same degree of saturation, or if it varies; and if it varies, to find the composition and source of the nuclei which cause the variations.

I feel that these two papers only start this inquiry. Much, very much, still remains to be done. Like a traveller who has landed in an unknown country, I am conscious my faltering steps have extended but little beyond the startingpoint. All around extends the unknown, and the distance is closed in by many an Alpine peak, whose slopes will require more vigorous steps than mine to surmount. It is with reluctance I am compelled for the present to abandon the investigation. It is, however, to be hoped it will be taken up by those better fitted for the work, and that soon the roughness of the way will be levelled, the difficulties bridged, the country mapped, and its resources developed.

\* "Theory of Heat," Professor CLERK MAXWELL, p. 270.