III.—*On a Difficulty in the Theory of Rain.* By JAMES DALMAHOY, Esq.

(Read 7th April 1862.)

Nearly a hundred years ago,* Dr HEBERDEN of London made the following experiment :—Having prepared three exactly similar rain-gauges, he placed one of them on the roof of Westminster Abbey, another on the roof of a neighbouring house, but at a much lower level, and the third in the garden of the same house. At the end of twelve months he found that the gauge on the roof of the Abbey had received 12·099 inches of rain, the gauge on the neighbouring house-top 18·139 inches, and the gauge on the ground 22·608 inches.

This paradoxical result required, of course, to be confirmed by other observers, and in other localities ; and the similar results obtained by DOBSON, DALTON, HOWARD, and especially by ARAGO at the Paris Observatory, and by PHILLIPS at York, have amply supplied all that was wanting in this respect.

It may be noted in passing, as a curious fact, that in Dr JAMES HUTTON'S "Theory of Rain" there is no allusion to Dr HEBERDEN'S observations, though these were published in the "Philosophical Transactions"† fourteen years before Dr HUTTON'S Theory was read to the Royal Society of Edinburgh.‡

Of the attempts which have been made to reconcile Dr HEBERDEN'S observations with facts and principles already established, that of Dr FRANKLIN is the most plausible, and it has been very generally accepted as the true explanation. At first sight, indeed, it seems capable of explaining every difficulty ; and it is only when more carefully examined, and especially when tested quantitatively by actual results, that its inadequacy becomes apparent.

As, however, many may be disposed to question this conclusion, I am glad to be able to rest the proof of it on the following quotation from Sir JOHN HERSCHEL'S treatise on Meteorology.§

Having alluded to the fact that, during the year 1833–34, the quantity of rain received on the top of York Minster, at the height of 213 feet, bore to that received on the neighbouring ground the ratio of 1 : 1·706, the learned author proceeds as follows :—"The usual account given of this phenomenon (KOEMTZ) is, that rain falling from a high level, and therefore colder than air at the surface of the ground, arriving in an atmosphere nearly or quite saturated with moisture, condenses on itself, or causes the condensation, in the chilled air, of an additional

* 1766–67.

† 1770.

‡ 1784.

§ Encyclopædia Britannica, 8th edition, article *Meteorology*, par. 109.

quantity of vapour. But it is evident that this cause, though not uninfluential, is totally inadequate to account for so great a difference. Admitting a given weight of rain to arrive at 213 feet from the ground, with the temperature of the region at which it was formed unaltered, and supposing it to acquire, in the remaining 213 feet, the full temperature of the air (both of them extreme and even extravagant suppositions), admitting, too (though hardly less extravagant), the mean height of the formation of rain to be 12,000 feet, it would bring down with it a cold of 40° Fahr., which would condense (whether on the drops or in saturated air, if diffused through it) only $\frac{40}{960} = \frac{1}{24} = 0.42$ of its weight, = one-seventeenth of the quantity to be accounted for."

Although this demonstration does not really admit of being strengthened, yet, as showing how a similar conclusion was reached in a different manner, I beg to refer to a short paper in the 20th volume of the "Transactions of the Royal Society of Edinburgh," entitled, "On the Weight of Aqueous Vapour which is Condensed on a Cold Surface under given conditions," in which I have endeavoured to prove experimentally that, taking the observations at York during the three winter months of the years 1832-33, 1833-34, 1834-35, the increment which the rain received in falling between the level of the top of the Museum and the ground, a height of 44 feet, was above 600 times greater than it would have been if the condensation of aqueous vapour by cold had been the only cause in operation.

For these reasons, therefore, it seems necessary to reject this explanation, though one of such likelihood as to have suggested itself, independently, to Dr FRANKLIN, M. BOISGIRAUD, and Professor PHILLIPS.

The eminent meteorologist LUKE HOWARD proposed* an explanation which, however, seems to differ from the one just considered chiefly in that it supposes the cold, on which the condensation of vapour depends, to originate, in some unexplained way, in the atmosphere itself, instead of being brought down by the rain from the upper regions,

There is another mode of accounting for the difficulty under consideration, which has been advocated by Mr JEVONST† and other writers.‡ According to this theory, the deficiency of rain in the upper gauge is produced by wind—the gauge itself, or the building on which it stands, giving rise to eddies which partially obstruct the entrance of the rain into the mouth of the gauge.

That wind may affect the indications of a rain-gauge, has been proved by the interesting experiments of Professor A. D. BACHE of Philadelphia.§ Having placed gauges at the four angles of a high square tower, at a height of ten inches

* Report of British Association for 1834, p. 563.

† Philosophical Magazine for December 1861, p. 421.

‡ Dr STARK in Transactions of the Royal Scottish Society of Arts, vol. v. p. 66.

§ Report of British Association for 1838, Transactions of the Sections, p. 25.

above the parapet, he found that the gauges at the lee side of the tower received more rain than those at the windward side; but that, the same arrangement continuing, when gauges were also placed on poles at the height of six feet above the parapet, there was observed scarcely any difference between their indications.

The bearing which these results have on the present question will afterwards be noticed; but neither these, nor any similar facts which writers have adduced, seem to meet the special difficulty to be explained—namely, that of three gauges, at three different levels, and each variously placed as respects surrounding objects, the lowest gauge always receives more rain than the middle one, and the middle gauge more than the upper one.

But that which seems an unanswerable objection to this mode of explanation, is the fact that, when the upper and lower gauges have been inspected after a perfect calm, and when the rain fell perpendicularly, the upper gauge was still found to contain less rain than the lower one. This fact is recorded very expressly by ARAGO,* and also by PHILLIPS.†

While, therefore, it is denied, for the reasons now adduced, that the difficulty under discussion can be accounted for by the effect of wind, it is not disputed that, under certain circumstances, wind does modify the indications of a rain-gauge.

Sir JOHN HERSCHEL concludes his notice of the attempts which have been made to account for the phenomenon, in these words:‡—"The real cause is yet to seek, and there is no more interesting problem which can fix the attention of the meteorologist. Visible cloud rests on the soil at low altitudes above the sea-level but rarely, and from such cloud only would it seem possible that so large an accession of rain should arise."

I was first led to think of this puzzling question a good many years ago, and the result of my repeated attempts to find a solution of it is contained in the following hypothesis, which, in spite of its many defects, I venture humbly to submit to the consideration of those who take an interest in meteorology.

The hypothesis begins by taking for granted the truth of Dr HUTTON's "Theory of Rain."§ It assumes that the spherules of water composing the clouds from which rain proceeds are, at their first formation, so small, that the terminal velocity of their descent is almost insensible. It further assumes that these minute globules coalesce, at innumerable points, into drops of sensible magnitude, and fall in the shape of rain; while portions of the cloud, which do not thus coalesce, are floated downward in a current of air, and fill the whole space between the clouds and the earth with minute particles of water.

This medium, consisting of cloud carded out, as it were, by the downward

* Œuvres, tome xii. pp. 409, 416.

† Report of British Association for 1833. See Transactions of Sections, pp. 403, 404. Report for 1834, p. 561.

‡ Article *Meteorology*, par. 109.

§ Transactions of Royal Society of Edinburgh, vol. i. p. 41.

current of air, and dispersed through a very large space, is assumed to be so rare as not to affect the transparency of the atmosphere; and it is the constant floating down of this medium in a current of air which, according to the hypothesis, is the principal and almost sole cause of the phenomenon to be accounted for.

But it will be necessary, at this point, to answer a question which may naturally be anticipated,—namely, whether there be any proof, from theory or observation, that rain is actually accompanied by a downward current of air and floating moisture.

Theoretically, two causes may be assigned for such a current. The first is the continual displacement of the air by the downward motion of the drops of rain; for, the effect of bodies in rapid motion to draw after them any light matter in their neighbourhood, such as air or smoke, is familiar to everybody; and that the drops of rain should have this effect will not seem improbable, when it is remembered that the largest of them may attain a velocity of nearly 400 inches per second.

The second cause of the downward current of air which theory suggests, is the cold which rain brings with it from the upper regions. This must render the air inside the limits within which the rain is falling heavier than the air outside those limits, and thus co-operate, in an obvious way, with the former cause.

But in order to prove that theory is in this case borne out by observation, I adduce the two following quotations from writers of unquestionable authority.

Professor PHILLIPS, in his first Report* on the observations at York, makes the following remark:—"I have noticed in several instances the fact, that the wind which accompanies the fall of rain takes the line of the rain-drops themselves; and on the Minster, in particular, this was very strikingly illustrated when, with my friends Mr JONATHAN GRAY and Mr WILLIAM GRAY, junior, I watched the progress of a storm for thirty miles down the vale of York. The wind was insensible, except during the fall of rain, and then it came downward *with* the drops."

This testimony establishes the fact, that rain is sometimes observed to be accompanied by a downward current of air so strong as to be described as a "wind." It also records two other facts, which have a bearing on the hypothesis; the first of these is, that on the occasion on which the downward wind particularly attracted notice, the observers were not standing on the ground, but on the top of the Minster, at the height of 213 feet; and the second fact is, that the rain and downward wind began and ceased simultaneously, which affords a strong presumption in favour of their being connected as cause and effect.

The other quotation which I have to adduce is from EMERSON TENNENT'S "Ceylon,"† and is as follows:—"The first fall of rain was preceded by a down-

* Report of British Association for 1833, p. 404.

† Vol. i. p. 69.

ward blast of cold air, accompanied by hailstones, which outstript the rain in its descent."

In this case the coldness of the hailstones and the velocity of their fall produced so rapid a current as to be described as a "downward blast;" and it is probably only when the hail or rain falls with unusual violence, as in this case, that the vertical direction of the wind is perceptible to an observer at the level of the ground.

But if, for the reasons which have been adduced, it be granted that rain is always accompanied by a downward current of air, of greater or less velocity, it seems necessarily to follow that this current, originating, as it must do, in the very region of cloud, will descend charged with minute particles of water.

Having thus proved, as I hope, both by theory and observation, that rain is accompanied by a downward current of air, mingled with minute globules of water, I shall now endeavour to explain the twofold agency of such a current, in causing the indications of equal rain-gauges to vary with their height above ground.

The first and most obvious way in which the downward current produces this effect, is by filling the entire space through which the rain falls with a constantly renewed supply of very minute globules of water. The rain-drops must, of course, absorb as much of this watery medium as they come in contact with, each drop growing in size during the whole time of its descent; and the necessary result must be, that if two equal gauges receive each an equal number of rain-drops, the gauge nearest the ground will indicate the most rain.

But, with reference to the process thus described, it will naturally be asked, what becomes of the multitude of minute globules of water which are not absorbed by the rain-drops? The answer to this will, it is hoped, be found in the following explanation of the second and less obvious way in which the downward current affects the indications of rain-gauges placed at unequal heights above the ground.

Taking for granted, then, that during rain a slow current of air carrying minute globules of water is continually descending from the region of cloud, I now assume that these minute globules of water, in the course of their descent, often come into contact with one another, and coalesce into drops of sensible magnitude; and this again leads to the inference that globules which, at a higher level, descended chiefly by participating in the motion of the downward current of air, acquire, after their coalescence, a velocity and momentum which enable them not only to outstrip the current, but also to resist being carried out of their downward direction by any lateral motion which may happen to be impressed upon the current.

This process of coalescence, it is conceived, becomes more and more rapid as the resistance of the ground begins to tell on the velocity and downward direc-

tion of the current ; and, judging from facts of observation, it seems to be at some point less than 40 feet from the ground that a great and sudden coalescence of the small globules of water takes place,—the effect, probably, of the current being first retarded and then forced into a lateral direction, and of the irregular mingling together of the particles of water to which this gives rise.

The hypothesis next assumes as true the following important proposition,—namely, that while a rain-gauge, placed at some height above the ground, receives all drops having a sensible magnitude, it receives none of those globules which are so minute that the velocity of their descent, though partly the effect of gravity, is chiefly due to the motion of the downward current of air on which they float. These very minute globules, it assumes, are borne by the current past the mouth of the gauge, and continue to descend until, by the coalescence of a great number of them, a drop is formed large enough to fall into a lower gauge if placed in its path.

This essential point of the hypothesis may be illustrated by what is observed when wind blows through a room having two windows opposite to each other. In such a case the leeward window is no sooner closed, than the breeze, if gentle, is scarcely felt within the room, though the other window remain open ; and the obvious reason is, that the air inside the room, being now supported at all points, resists the entrance of any more air by the open window. In a similar manner, it is conceived, the slow downward current of air and floating moisture fails, as the hypothesis assumes, to find entrance into the mouth of a rain-gauge, and is forced to turn aside and to continue its descent towards the ground.

But here the question may occur, what will be the result if this downward current be combined with a current of wind at right angles to it? One effect doubtless will be, to accelerate that process of coalescence which, it has been assumed, takes place among the minute globules of water in the downward current. And this more rapid coalescence would evidently tend to increase the indication of the upper rain-gauge, and thereby to equalise it with that of the lower gauge, were it not that this tendency is more than counterbalanced by the wind uniting with the downward current of air, to turn aside from the mouth of the gauge, not only the very smallest of the globules of water, as during a calm, but also the smaller of those globules which, under ordinary circumstances, would have fallen into the gauge. It appears therefore that, as respects its ultimate effect, the wind increases the difference between the indications of the upper and lower rain-gauges, and this is in accordance with observation.

But besides the wind, which, it has been shown, increases the ordinary effect of the downward current, there are a few causes which seem to lessen or mask its influence. Thus, it is well known that, on some rare occasions, the quantity of rain in the upper gauge equals, and even exceeds, the quantity in the

lower gauge; and one or two circumstances which might tend to produce such an equality may be mentioned. If, for example, the upper gauge were placed rather close to the top surface of a tower or building, and nearer to one side of it than to the other, then, in accordance with Professor BACHE's experiments, when the wind blew from the direction of the more distant side, the indication of the gauge might be expected to be greater than if the wind blew from the near side; and thus it would not be surprising if, in certain directions of the wind, the indication of the upper gauge became nearly or quite equal to that of the lower one. Again, there are two other causes, opposite to each other in character, which, it might be expected, would tend to equalise the indications of the upper and lower gauges. The first of these is when the rain falls, more or less plentifully, but in extremely small drops; and the second is when the rain falls in large drops and very copiously. In the former case, the very small terminal velocity of the drops would give rise to a downward current of air so slow as scarcely at all to hinder the entrance of the minute globules of water into the upper gauge, or accelerate their coalescence into drops as they approached the ground; and the ultimate effect would be, a tendency to equalise the indications of the two gauges, by allowing *more* than the usual quantity of water to enter the *upper* gauge. In the latter case, again, the downward current of air might be so strong as to reach the level of the ground, and this also would tend to equalise the indications of the two gauges; but, on this occasion, it would do so by allowing *less* than the usual quantity of water to enter the *lower* gauge.

Having explained what I conceive to be the twofold agency of the downward current in producing the paradoxical results under consideration; and having also noticed some of the causes which may serve occasionally to modify these results; it would now have been desirable to test the hypothesis quantitatively; but unfortunately the want of data renders it impossible to do so in a satisfactory manner. There is, however, one point on which a numerical estimate, even if it were only a probable one, is essential. I mean respecting the quantity of water, in the shape of minute globules, which a given volume of the atmosphere must be assumed to contain, in order to account for some of the more remarkable results recorded at York or elsewhere. I shall, therefore, now endeavour to make a rough approximation to this quantity, the chief object in view being to determine whether the quantity of moisture would be so great as to render necessary the supposition of visible cloud throughout the space where rain is falling.

In selecting a case for such a purpose, recourse cannot be had to those rare instances in which 30 inches or more of rain fell in the course of twenty-four hours; for in none of these does it appear that the observations were made at more than one level.

Perhaps the winter observations at York will furnish as severe a test of the hypothesis as can be found, at least as regards the ratios of the quantities of rain

received at different levels. It is true that the maximum quantity of rain which fell *continuously* in a given time, cannot be determined by means of this series of observations, for the intervals between the consecutive observations were too long to admit of this; but still it is possible to assume a value which may be supposed, on good grounds, rather to exceed than fall short of the actual maximum rate. The following table* contains an abstract of the winter observations at York to which allusion has just been made. It exhibits the depth of rain which fell into gauges, at three different levels, during a period of 270 days, comprising the months of December, January, and February, of the years 1832-33, 1833-34, 1834-35:—

	Height of the Gauge above the ground, in feet.	Depth of Rain in inches.	Ratios of the Depths, that on the ground being 1.
Ground gauge,	0	17·32	1·
Museum gauge,	44	12·17	0·7
Minster gauge,	213	8·65	0·5

The remarkable fact to be learnt from this table is, that the Ground gauge received 30 per cent. more rain than the Museum gauge, the difference of level being only 44 feet; and 50 per cent. more than the Minster gauge, the difference of level being 213 feet.

As respects the rate at which the rain fell, the table shows that the Ground gauge received 17·32 inches in the course of 270 days,—that is, on an average, 0·064 inches in twenty-four hours. This, of course, is the mean rate for the whole period, including fair and rainy weather; but what is wanted is the largest quantity which fell continuously in a given time. Sir JOHN HERSCHEL states,† that “it is considered, in the greater part of England, a heavy rain if an inch fall in the course of twenty-four hours.” Therefore, guided by this, it is proposed to assume, that on one of the 270 days included in the York observations it rained continuously for twenty-four hours; and that, during this period, the Ground gauge received one inch of rain, the Museum gauge 0·7 of an inch, and the Minster gauge 0·5 of an inch,—these quantities bearing to each other the same ratios which, as the fourth column of the above table shows, the whole quantities bear to each other.

Adopting, then, these data, the hypothesis assumes that, on the occasion in

* Report of Brit. Assoc. for 1835, p. 173. *N.B.*—The error in the Minster column is corrected.

† Art. *Meteorology*, par. 115.

question, and within the period of twenty-four hours, a current of very minute globules of water—equivalent, in all, to a depth of half an inch of rain—was floated downward past the level of the Minster gauge, without giving any indication of its passage; and that of this half inch, a portion, equivalent to 0·2 of an inch, changed its form while descending between the levels of the Minster and Museum gauges,—the larger part of this quantity combining to form drops of a size sufficient to admit of their falling into the lower of these two gauges, and the remainder coalescing with drops of rain already formed, and thereby rendering each drop larger than when it passed the level of the Minster gauge. The effect of this coalescence of the minute globules of water with each other, and with the larger rain-drops was, according to the hypothesis, to raise the indication of the Museum gauge to 0·7 of an inch,—that is, 0·2 of an inch above the indication of the Minster gauge.

Again, it is assumed that the current, now containing a quantity of water equivalent only to a depth of 0·3 of an inch, was carried past the level of the Museum gauge without leaving any trace of its passage, and was by a similar process of coalescence, but much more rapid than what took place at the higher level, converted into drops which, owing to the cessation of the downward current as it approached the ground, were now no longer, even the smallest of them, carried past the mouth of the gauge placed there, but entering it, raised its indication to one inch,—that is, 0·3 of an inch above the indication of the Museum gauge.

Having traced the agency of the downward current thus far, the next step ought to be to ascertain the velocity of the current. But it is difficult to find any data for making such an estimate; for though it may be inferred, that the velocity of the downward current of air which accompanies rain will have some direct relation to the quantity of rain which falls in a given time, and to the degree of cold which it brings down with it, yet, unfortunately, from not knowing what that relation is in some actual instances, no use can be made of the general principle. Since, however, it is necessary to arrive at some estimate on this point, there are one or two considerations, connected with the hypothesis itself, which seem to suggest a lower limit, at least, to the velocity of the downward current.

In explanation of this, let it be assumed that (it being during winter) 3000 feet was the height of the clouds from which the one inch of rain fell in twenty-four hours. Also let it be assumed that the diameter of the drops of rain was uniformly one-tenth of an inch, which, as respects bulk, is greatly nearer the lower than the higher of the two limits to their size which Professor LESLIE has assigned. Then, one inch of rain being supposed to fall in twenty-four hours, it follows that the number of drops which would fall on any particular spot in the same time would be fifteen,* and the interval of time between the consecutive drops would be ninety-six minutes. In order, therefore, to keep the space between the clouds and the earth replenished with the minute globules of water, the

* Playfair's Geometry, Supp., B. III. Prop. xxi.

downward current ought to have a velocity of at least 3000 feet in 96 minutes,—that is, about 7 inches per second, or $\frac{7}{3}$ ths of a mile per hour.

In a table given by Dr THOMAS YOUNG,* it is stated that a wind blowing at the rate of two miles an hour is just perceptible; and as the downward wind recorded by Professor PHILLIPS was distinctly felt, it may be concluded that its velocity exceeded two miles an hour. Therefore, in adopting 7 inches per second, or $\frac{7}{3}$ ths of a mile per hour, as the estimated velocity of the downward current in the present case, there is, at least, the certainty that it is considerably less than a velocity which observation has proved to be possible.

And now, let it be imagined that a hollow prism reaches vertically from the level of the Minster gauge to the ground, and that the area of its base is equal to one square inch; also let attention be directed only to that portion of the current which may be supposed to descend through the prism;—it is evident that in the course of twenty-four hours, or 86,400 seconds, its volume will amount to $7 \times 86,400 = 604,800$ cubic inches. This volume consists of air and minute globules of water, the latter being, by supposition, equal in weight to half a cubic inch of water = 126.23 grains. Hence, according to this estimate, each cubic inch of the atmosphere between the levels of the Minster and Ground gauges, and within the limits of the raining space, would contain, besides the aqueous vapour due to its temperature, only $\frac{126.23}{604800} = 0.00021$ gr. of condensed vapour,—*i.e.*, less than half the quantity which would be requisite to saturate it, if dry, at zero of Fahr.

If, therefore, this estimate be at all near the truth, it seems to follow that even such remarkable results as those of the winter observations at York may be accounted for by the presence in the atmosphere of a quantity of condensed vapour too small to give rise to the appearance of visible cloud.

Before bringing the paper to a close, there remains to be noticed what, at first sight, might seem to be a new source of difficulty, namely, the fact that when the elevation on which a rain-gauge is placed, instead of standing detached like a house, or tower of a cathedral, forms part of a mountainous country, the ordinary effect of elevation, in appearing to diminish the quantity of rain, is no longer observed. Such a result, however, presents no real difficulty; for when the current of air carrying minute globules of water which accompanies rain descends on an elevated, but, at the same time, extended and uneven surface, the resistance offered to its downward progress ought, according to the hypothesis, to produce nearly the same effects as at the level of the ground.

In concluding this attempt to explain a long standing difficulty in the theory of rain, I do so with an unfeigned sense of the very imperfect manner in which it has been executed, but, at the same time, with a good hope that it will be found to be based on a true principle.

* Lectures, vol. ii. p. 457.