

XXV. *Letters of Henry Eeles, Esq; concerning the Cause of the Ascent of Vapour and Exhalation, and those of Winds; and of the general Phænomena of the Weather and Barometer.*

To the Rev. Tho. Birch, D. D. Secret. R. S.

L E T T E R I.

S I R,

Lifmore (in Ireland), Nov. 25, 1754.

Read Jan. 23,
1755.

I Received your letter of the 25th of November 1752, in answer to my letter to the Royal Society concerning the cause of thunder. As you tell me, that they honoured me with their desire of my thoughts on some subjects mentioned therein, I have ventured to send you a letter, for them, on one of those subjects, inclosed in two packets, by this post. I fear, that the Society will think my letter too hypothetical; our great modern philosopher having, in some measure, condemned the use of hypotheses. But I must boldly observe, that that objection is made in a part of his works, which is intirely hypothetical: I mean his queries at the end of his third book of optics. And I shall venture to shew (if the Society will have patience to hear me), that there is a great deal hypothetical in his second book of optics, where he thinks himself more ascertained. For he there speaks pretty positively of four causes of the refraction, &c. of light; three of which must be wrong; and the fourth (his æther),

æther), in the manner he has proposed it, can never be sufficient for that purpose. Not that I intend impudently to blame that truly great man, but to endeavour to investigate the cause of reflection and refraction of light, by means not made sensible to mankind in his time. I may also observe, that, if his hypothesis had not preceded his calculation, the world had never been obliged to him for his great doctrine of attraction: for at first he only supposed, that the power of gravity may extend to the moon; and that she may be retained in her orbit thereby: but when he made his calculation (having mistaken sixty English miles for the measure of a degree on the surface of our earth), he thought, that some other power must co-operate with that of gravity, to keep the moon in her orbit; and for that time laid his attempt aside; till Picart, in France, measured a degree, and found, that it contained about sixty-nine English miles and an half; and then, or some years after, our great author having resumed his computation, found, that the power of gravity was alone sufficient to keep the moon and planets in their orbits; and thereby proved his hypothesis.

Indeed, in works of invention, I cannot see how it is possible to proceed without something hypothetical; for the supposition must ever precede the proof. I am not for establishing hypotheses as principles; but if an hypothesis is rationally founded, I think it is worth the while to inquire into the truth of it; and thus invention may proceed.

I shall not say any thing in justification of my own, because I have not any vanity to gratify; for if my letter has not any thing worth the Society's or your acceptance,

ance, I can most readily and willingly ask pardon for the trouble, which I have given, and desist from giving any for the future. I am,

S I R,

Your most obedient humble servant,

Hen. Eeles.

To the Royal Society.

L E T T E R II.

Gentlemen,

Lifmore (in Ireland), Nov. 25, 1754.

Read Jan. 23, and Feb.
6, and 13, 1755.

THE great honour you have done me, in accepting my letter concerning the cause of thunder, has emboldened me to proceed in part of what I therein proposed, *viz.* to endeavour to shew, that the fire, which is made apparent by electrical experiments, is the principal cause of the ascent of vapour and exhalation; and that the lower part of our atmosphere is, by that means, kept more homogeneous than is generally supposed, and fitter for respiration, vision, &c. and that clouds of heterogeneous matter are kept suspended, at their usual height, merely by this fire. To which I have added something concerning the cause of the winds, and to explain the general phenomena of the weather and barometer.

But as I must now contradict some generally received opinions, it may be thought decent to make some apology for offering my own. But I shall not
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take up your time in so doing; for if they carry an appearance of truth, I need not make an apology for them; if they appear false, they cannot injure those opinions, which they contradict; and then I have but one apology to make, which is, to acquiesce, and sincerely ask your pardon for having troubled you with them.

It is agreed, that the ascent of vapour and exhalation through the air may be effected two ways; by impulse, and an alteration of their specific gravity.

That vapour does not generally ascend by impulse, may be proved by many familiar experiments, *viz.* Put boiling water into a vessel, and then empty it, and hold the vessel with the apperture downwards: the vapour, which is afterwards expelled from the vessel, must be in a direction downward; but we find, that as soon as it has got but a very little below the rim of the vessel, it has its direction altered, and ascends by the laws of specific gravity. The same thing may be observed in all boiling vessels, where the vapour is emitted in a direction downward; or, in cold weather, when the vapour of a man's breath may be seen, let him breath downward, and the direction of his breath will be presently altered, as in the former case. Since then vapour does ascend without any other impulse than that, which is incident on all bodies ascending by the laws of specific gravity; it is necessary to inquire, how the specific gravity of vapour is altered to cause its ascent. This is generally supposed to be done by filling vesicles of water with rarified air, until the diameter of the vesicle be ten or more times the diameter of a drop
of

of water, composed of the same constituent particles; and that the vesicle, by this means, becomes specifically lighter than air. But I think, that this cannot be done so easily, as it has been generally imagined; and when done, it will not be sufficient for the purpose; which will appear from the following considerations.

First, the great difficulty in forming those vesicles, especially of the particles of dry bodies carried off by exhalation, and filling them with rarified air, while the exterior air remains condensed. Secondly, That there is not any allowance made for the weight of the included air. Thirdly, The constituent particles of water are but very little (if at all) altered in their specific gravity. Fourthly, That this thin vesicle can never be a sufficient boundary between the exterior condensed air and the interior air, so exceedingly rarified. And therefore as the density of air is proportional to the force compressing it, it follows, that the interior air must be suddenly compressed (and the vesicle with it), until it be of equal density with the exterior air; in which state the vesicle is not more fitted for ascending, than if the same constituent watry particles were formed into a spherical drop.

For here I must beg leave to set aside a false reasoning, which I have found in many approved writers on this subject, who assert, that the specific gravity of bodies is altered by a mere increase of surface, upon their being divided into minute parts. Which is wrong; for the specific gravity of any body is only to be altered by making that body occupy a greater or less proportion of space. It is evident, that upon
dividing

dividing a body into parts, the surface increases according to the number of parts, but the proportion of space occupied (and consequently the specific gravity) remains the same. Take a cube, whose side is ten inches, the surface is six hundred inches, the space occupied one thousand cubic inches; divide that cube into a thousand cubes; the surface will be increased to six thousand square inches, but the space occupied will remain a thousand cubic inches, and therefore the specific gravity will remain the same: and so on, divide it into as many parts as you please. Indeed bodies by an increase of surface meet a greater resistance in passing through any medium; but, I think, resistance and specific gravity are here carefully to be distinguished: for that resistance, which prevents the sinking of the minute parts of a body, in a fluid specifically lighter, must equally retard their ascent in the same fluid; and therefore can never be the cause of their ascending.

But let us suppose, that this vesicle is formed and filled with rarefied air, and ascending through the atmosphere; it is plain, from another cause, that it cannot remain so during a few seconds of time: for no solar or culinary heat can expand air so as to make it permanent in that state, in a vesicle of vapour ascending through the open atmosphere, for the space of one minute. For it is evident, that metallic bodies, heated many degrees hotter than boiling water, and exceedingly larger than the ascending vesicle, cool in a few seconds when exposed to the open atmosphere. For instance, mechanic operators, in tempering of small drills, only heat them red hot in the flame of a lamp, and then waving them three

or four times through the air, find them not only cold, but hardened by their sudden cooling, so as to be fit for cutting other parts of the same steel, which has been cooled more gradually. Now the vesicles being exceedingly smaller than the points of such drills, must cool much quicker, and the included air be condensed equal to the circumambient air almost instantaneously; in which state the vesicle is not fitter for ascending than that portion of water was before the vesicle was formed: for though the surface of the water is vastly increased by the form of the vesicle, which may retard the descent of it through the air, yet that form must equally obstruct its ascent; and the specific gravity of the constituent particles remaining the same, I cannot see how the increase of surface can aid its ascent. And here I must observe, that it has not yet been explained, how water can be dilated so, as to occupy eight hundred or a thousand times the space, which it naturally does. For I think the greatest expansion it suffers by boiling (the greatest heat we can give it) is not more than a nineteenth or twentieth part of what it was before.

But supposing these vesicles formed and ascending, there is still a very necessary power wanting, I mean repulsion, to prevent their running into contact (by their natural attraction), and forming larger masses; in which state they would immediately descend. Now let us suppose, that they ascend by impulse; it will be very difficult to explain their motions, which are evident to sense. For if they ascend by impulse, their motion must be continually retarded by their gravity, and the obstructions of the air, until they

arrive at their greatest height, which must always be in proportion to their first velocity; and then they must descend by the laws of specific gravity, as all other bodies do. The velocity with which we generally see vapour ascend, is not sufficient to carry it a few yards in height; and therefore, if there were no other cause of ascent but impulse, the vapours and exhalations must be pretty equably diffused over the surface of the earth, ascending to a small height, and immediately descending again; or if it were possible for them to float at that height, the consequence must be a perpetual fog. How far the great uses of vision would be obstructed or lost in this case, I need not mention; nor need I say, how detrimental it would prove to respiration, since mankind have agreed to call fogs unwholesome. However, when I come to speak of the uses of electrical fire in animal life, I shall say something on this subject.

However necessary an impulsive power may be in the first emission of vapour and exhalation from their masses, I think it evident, from the slowness of their ascent, that it cannot carry them to that height, where clouds are usually formed; much less is it possible, that it should be the cause of their floating there for many days, as we see they do. This can only be done by an alteration of their specific gravity, till they are brought into an æquilibrium with part of the atmosphere where they float.

It now remains to inquire, by what means this may be done; since neither impulse, rarefaction of the air, or any formation of their parts by expansion, (which I know of) seem sufficient for the purpose.

There appears to me but one way of altering the specific gravity of the particles of vapour and exhalation, to render them lighter than air : which is by adding to each particle a sufficient quantity of some fluid, whose elasticity and rarity are exceedingly greater than that of the air. That the fluid or fire of electricity is such, I believe will be easily granted me : but how far it is adapted to this purpose, we must inquire from experiments.

But first, let me not be thought absurd in saying, that the Creator might have thought proper to adapt this fluid to this particular purpose. For the purpose is great ; no less than all vegetation and animal life depending on the ascent and descent of vapour and exhalation.

I need not run into a prolix detail of electrical experiments, to prove the properties of this fire, which are much better known to you ; although I have made some experiments, which, perhaps, have not yet come before you ; by which it appears, that all fumes arising from fire, whether blazing or otherwise, and all steams rising from boiling or warm waters, and from all other fluids, and the breath of man, and of all other animals, and all the effluvia thrown off by perspiration, are strongly electrified. But I shall not trouble you with these, till I come to speak of the uses of this fire in animal life. I shall now only mention a few, which are well known, and which are to my purpose. First, That desultory motion, by which it flies off from an electrified body to any number of non-electrics, which are brought within the sphere of its activity and affection, until it be equably diffused through all.

Secondly,

Secondly, That the sphere of its activity is increased by heat. Thirdly, That this fire does not mix with air. Fourthly, That it intimately pervades water, and many other bodies, covering their superficies to a certain distance; which distance is not in proportion to the bulk of the body electrified, but in proportion to the state of activity of the electrical fluid. Fifthly, This electrical fluid readily joins with any fire, which fumes, or rather with the blaze or fumes of any fire; but will not mix or fly off with the fire of red-hot iron, or any other metal, which does not fume. I have not met with this in any writer on electricity, but I have proved it by experiments.

Now, to shew, that this electrical fire or fluid is the principal cause of the ascent of vapour and exhalation, we need only prove, that it attends all vapour and exhalation, and that in such quantity, as is necessary to render them specifically lighter than the lower part of the atmosphere.

I shall not undertake to determine, by what cause vapour and exhalation are detached from their masses, whether by the solar or culinary fire, or by the vibrations of the electrical fluid rendered more active by those fires; though I am led to think the latter. But it is evident, that they are emitted in exceeding minute distinct particles, and that these particles must pass through that electrical fluid, which surrounds the surface of the mass; and that, by that means, they must be equally electrified with the mass; that is, they must be covered with the electrical fluid to as great a distance from their superficies as the mass is covered; which must always be in proportion to the state of activity of the electrical fluid. In which
state,

state, when they have passed the surrounding fluid, they must be repelled by it; and also repel each other; and if each particle of vapour, and its surrounding fluid, occupy a greater space than the same weight of air, they must be fitted to ascend till they come in æquilibrium with the upper and rarer part of the atmosphere; where they must float, until their specific gravity is altered. As it is very difficult to assign the magnitude of each particle of vapour and exhalation, and that of the surrounding fluid; and to shew, that both, taken together, occupy a greater portion of space than the same weight of air; we can only apply to experiment, to shew, that it is possible that it may be so; and that will shew, that in all probability it is so; since it is evident, that every particle must be endued with a portion of this electrical fire or fluid, and that there is not any other sufficient cause assigned for their ascending.

It is evident, that, upon electrifying any light matter, such as down, or the downy parts of feathers, their specific gravity is much lessened; and that, by holding another electrified body under them, they may be driven upwards at pleasure. It is also evident, from experiment, that the more you divide the parts of such bodies, the more of their specific gravity they will loose by being electrified; and by dividing them into very minute parts, I have found, that they ascended to a considerable height after they were electrified. From whence I think it highly probable, that the exceeding small particles of vapour and exhalation may be, and are, sufficiently electrified to render them specifically lighter than the lower air; and that they do ascend by that means.

And

And that they will ascend proportionally higher, as the surrounding fluid is proportionally greater than the particle, which is carried up.

It may be now thought necessary to shew what becomes of our vapour and exhalation above; or how they are to come down again. But I must here beg your patience, while I endeavour to shew, that the ascent and descent of vapour and exhalation, attended by this fire, is the principal cause of all our winds. In doing of which, I hope to bring down the vapour and exhalations, and to explain the general phenomena of the weather and barometer.

It being admitted, that wind is only air put into motion, many have been the conjectures how that motion is caused. Among which, the motion of the earth, and the air's being rarefied by the sun, seem to stand first. The trade winds being most regular, and occupying a considerable part of the globe, it has been thought proper first to account for them, from the afore-mentioned causes.

But I think, that these causes by themselves are not sufficient for the motion of those winds, and much less so for the irregular motion of all the other winds. If the apparent motion of the air was occasioned by the diurnal revolution of the earth from west to east (by the air's being left behind), the motion must be found more regular, and very different from what it is; for in that case the greatest motion must be at the equator, and from thence lessen by regular degrees to the poles; and must be continued always equally one way, both day and night, and at all seasons. But we find quite the contrary; the most gentle gales blowing at the equator

tor and between the tropics pretty steadily, one way all day long, and dying away at night; while high winds and storms, blowing all manner of ways, are found in the higher latitudes. I shall not detain you to shew, that the sun's rarefying air cannot simply be the cause of all the regular and irregular motions, which we find in the atmosphere; but I shall proceed to what, I think, is the cause, *viz.* the ascent and descent of vapour and exhalation, attended by the electrical fire, or fluid.

You are well acquainted with the calculations (by sundry authors) of the quantity of vapours raised in a day; but I will venture to say, that all those, which I have met with, fall exceedingly short of the quantity raised in the torrid zone. Now, all this vapour and exhalation, being buoy'd up by the electrical fire, must add a column to the air (though of a different matter) at least a thousand times greater than the vapour and exhalation taken up; which column must necessarily force the adjacent part of the incumbent air upwards; and must as necessarily be reacted upon by the incumbent air, to restore the æquilibrium of the whole air. And as it cannot be readily forced down again, it must float off, at that altitude, toward those parts, where little or no addition has been made to the atmosphere; and by that means must propel the air on the horizontal level with it, and that below it, as it is itself propelled by the weight of the incumbent air. And that motion must be from the equator (where the greatest quantity of vapour, &c. is raised), toward the poles, and partly to the west; as the column of vapour is always rising from east to west, as the earth turns to-
ward

ward the sun. For here we must confess, that the sun is the great agent in detaching vapour and exhalation from their masses; whether he acts immediately by himself, or by his rendering the electric fire more active in its vibrations: but their subsequent ascent I attribute intirely to their being render'd specifically lighter than the lower air, by their conjunction with this electrical fire. The fire, which surrounds the vapour, beginning to condense, and the vapour to subside, in passing the tropics, becomes a greater pressure on the air beneath, and by that means forces some part back into the tropics, in the place of that air protruded by the ascent of the vapour, &c. and the remainder in a direction toward the poles. The common rotation of the air in coming in below, to supply the place of that part carried up by any fire, may explain this motion. To shew, how this motion must tend to the west, we must consider, that the column of air, raised by the ascending vapour, &c. is at its greatest altitude to the east; and therefore must press that air to the westward, which is continually protruded by the vapours, &c. beginning to ascend from east to west; and the compressed air at the tropics must tend to the westward, till their forces meeting make the motion intirely to the west. The air itself being rarefied, and carried up by the reflection of the intense heat of the sun (for heat, in flying off from all bodies, does ascend through the air with great velocity), may be a considerable additional cause of these trade winds; but never can be the sole cause of all the erratic winds. As I am not sufficiently inform'd of the particular formation of the globe, to account

for all the irregular winds within the tropics, I shall only venture to say, that, where such happen, it must be by means of some tracts of land, which rise to a greater height above the horizontal level, than vapours generally do; by which the motion of the vapours is stopped, and the vapour accumulated by succeeding vapour, and the air, upon which they float, is of consequence pressed into a new direction. And from hence may also be explained the cause of the rains, particularly so called in the sea language.

I must now consider what becomes of the vapour, &c. floating from over the tropics toward the poles; which being less affected by the heat of the sun, reflected from the surface of the globe, the surrounding electrical fire begins to condense more and more as it moves toward the poles, and the vapours of course to descend; and that part most, which is most remote from, or is farthest left behind by the sun; and of consequence the higher column of air must tend that way to restore the equilibrium; which motion, at this side the equator, must be to the north-east; and as the vapour, &c. fall again to the earth, the motion must be more to the east. From whence our south-west and westerly winds, which blow a considerable part of the year.

But as this system is too regular to account for the phænomena of the erratic winds, we must consider whence they arise. I have before observed, that tracts of land rising into the atmosphere will stop the regular motion of the vapour, &c. and that the vapour being accumulated by succeeding vapour, the subjacent air must be pressed into new directions. Now this cause added to the daily dilatation of the
electrical

electrical fire, and the contraction at night, and the coalition of the vapours, to occasion their total descent, will be sufficient to produce a very great variety of winds on this side the tropic. But if I were to set down all that has occurred upon this subject, about upper currents, under currents, and intermediate transverse currents, &c. occasioned by the various motions of the atmosphere, to restore an equilibrium, and to produce my proofs, I must send you a book, instead of a letter. But I write not to common minds.

It now remains to shew, how the general phenomena of the weather and barometer arise from this system. First, Why it generally rains in winter, while the wind is south, south-west, and westerly. Secondly, Why north-west winds are generally attended by showers in the beginning, and become more dry, as they are of longer continuance. Thirdly, Why north and north-east winds are generally dry. Fourthly, Why the east wind continues dry and dark for a considerable time together. Fifthly, Why squalls precede heavy and distinct showers; and why a calm ensues for some little time after they are pass'd. Sixthly, Why storms and high winds seldom happen in a serene sky without clouds. Seventhly, Why the vapours, in warm seasons, coalesce to form those distinct dense clouds, which produce thunder and heavy showers. Eighthly, Why the barometer falls lowest in long continued rains, attended by winds; and why it rises highest in long continued fair weather; and why the intermediate changes happen. Ninthly, Of land-breezes and sea-breezes, and water-spouts.

First, The vapours passing the tropics into colder regions, have their surrounding fire condensed by degrees ; which must increase their specific gravity, and lessen their repulsive power ; by which means they must both descend, and approach each other, till at last they form dense visible clouds ; and these clouds are also accumulated by other succeeding vapours, of like specific gravity, till they form clouds, which are often several hundred yards in depth ; which I have often seen, in passing through them up the sides of very high mountains. In clouds of such depth, I think, the coalition of their particles, to form drops, may arise from their motion, and the order of specific gravity. For the air being a great deal denser, and of greater specific gravity, at the bottom of such a cloud than it is at the top ; and the particles of vapour, with their fire, being pretty near an equal specific gravity ; those particles, which are below, must necessarily be forced upwards, and those above must as necessarily descend, till they all meet at that altitude, whereat their specific gravity would make them float, if they were not prevented merely by their repulsive power : but this repulsive power must be greatly acted upon, if not quite overcome, in some places by this pressure of the air and of the vapours. But supposing the particles brought into a very near approach by this pressure, it will be scarcely possible for them, upon any motion of the clouds, to keep their stated distances without impinging upon one another. And whenever this happens, it is easy to shew how rain is formed. For where-ever two or more particles are forced to approach within their repulsive powers, they will run into contact and
sphericity

sphericity (if fluid) by their attractive power, and by that means become covered with the electrical fluid to a greater distance from their superficies, than the other particles are, or than they were before their conjunction (as I have shewn in my letter concerning the cause of thunder); that is, will be more electrified: and consequently (as appears by electrical experiments) the excess of electrical fluid will run off among the other particles; by which means the enlarged particles have their specific gravity increased, and are enabled to descend to a lower region of the air. And the more particles they impinge upon, in their descent, the more their specific gravity and velocity be increased; and the more their velocity is increased, the more particles will they impinge upon, till they fall from the clouds in drops; whose bigness will be according to the depth and density of the cloud they have passed through.

It may be necessary to shew, why I assert, that the increase of velocity in the enlarged particle will make it impinge upon a greater number of the smaller particles, in its passage through them. And this will appear by electrifying a feather, which may be driven upwards, by the electric body, with a certain velocity; but if you add a much greater velocity to the electric body, it will overtake the feather, and get within their repulsive powers; and the feather will stick to it by their attractive powers. And the less distance the feather keeps above the electric body, the less increase of velocity is necessary to make them join. You will pardon my prolixity in this particular, because, I think, much depends upon it in the formation of rain.

Having

Having shewn how I think vapour may coalesce into rain, I shall now endeavour to explain the phenomena of the weather, &c.

First, Why our south, south-west, and westerly winds are wet in winter. I have before shewn, that the wind, and the vapour with it, must tend from the tropic toward the pole, and how it may be varied to the north-east and to the east. In the cold winter season the vapour, in this course, must coalesce more and more, as it comes into the colder regions, and at last come down in mists and rain; and the more the column of air is decreased by the fall of such mists and rain, the more room is there for succeeding vapours to be pressed into the same course, and to fall in like manner; and so on; which may occasion the continued course of these winds, and wet, which generally attends our winters. But sometimes, at the fall of these rains, the wind comes in from the north-west, to restore the balance of the atmosphere, and roll the vapours (still floating in the air), by altering their motion, into heavy dense clouds, which, upon their sudden coalition, let fall heavy showers. But by that means the remaining particles of vapour in the cloud have a greater quantity of electric fluid distributed among them; which enables them to ascend, and form what the sailors call a hard dry sky; and as they approach farther into a warmer climate, which increases the power of the electrical fluid, they rise, dissipate, and vanish out of sight; and this is the general consequence of north-west winds. As to the north and north-east winds, whatever vapour they bring with them, has its repulsive and ascending power (that is the electric fluid) so increased

creased by approaching into warmer and warmer climates, that it scarce ever comes down in rain, except from the effects of some upper current of the air.

It sometimes happens, while the wind blows very gently at east, that the vapours coalesce at a considerable height in the atmosphere, so as to form one even uniform cloud, sufficient to shut out the bright shining light of the sun, which is therefore called a dark sky: Which cloud being uniform, and of no great thickness, and carried on by a very gentle motion, and that neither approaching to a warmer or a colder climate, and seldom descending so low as to be disturbed by the tops of mountains, the vapours may keep the order of their specific gravity for a considerable time; as they generally do, till some cross or opposite wind forces them to separate into denser clouds, and leave apertures for the sun to shine through; and this is generally the case for a day or two before any wet descends.

Why squalls precede heavy distinct showers, and a calm ensues for some little time after they are passed, is pretty evident; for the descending rain, attended by the electrical fluid, forces the subjacent air out of its place, and that which is driven forward must add a proportional velocity to the motion of the wind that way, and that which is driven backward must also obstruct the motion of the wind advancing after the cloud. The wind preceding the cloud is also pressed off obliquely toward each extreme of the shower. Which may be of good use for sailors to know and observe; for if they sail upon a wind from the center of the shower toward the extremity, they may safely venture to keep their luff; but

but if they fail from the extremity toward the centre, if they luff up, they will be taken aback, and run the hazard of being difmasted: and this I always found true in praëtice.

Why high winds are feldom found in a ferene fky without clouds, is alfo pretty evident; for clouds occasion thefe high winds in a double manner; firft, when they are large and united, and upon the defcent, as they occupy a great fpace in the atmofphere, they muft prefs the fubjacent air into a great velocity; and fecondly, when they coalefce, and come down in heavy rains, they make room for the air to flow in with violence, to reftore the equilibrium of the whole air.

To know how the vapours coalefce in warm feafons, to form thofe diftinct dense clouds, which produce thunder and heavy fhowers, we muft firft confider the ftate of the vapour in its afcent, which muft be ftrongly endued with the electrical fire to enable it to afcend to the great height it then does in the atmofphere: Secondly, The great quantity carried up, and kept afloat at that great height, during a feries of hot dry weather. The quantity may be proved from the height of the mercury in the barometer (as I fhew prefently), and the great height and diffipation of the vapour; from the ferene appearance of the fky; and alfo by the great height, where even thunder-clouds are formed in its defcent; which may be eafily meafured by the length of time between the light and noife of thunder. In this ftate the upper air is in a quietude, and whatever part of this vapour begins to coalefce and fubfide firft, will carry down with it part of the fubjacent

cent vapour, and make room at the top for other vapours to flow in from all sides, which, by their meeting, may also coalesce and subside to form this central descending cloud, and so also of like succeeding vapours. And thus, I think, clouds of any density may be formed in a quiet air (and thunder generally happens in calm weather), till the density is too great for the buoyant electrical fire to sustain the weight, which flying off, in part, from the largest drops, lets them fall almost in spouts.

Why the mercury falls and rises in the barometer, will appear from what I have been saying; for in long continued fair dry weather, there is a great quantity of vapour raised, and kept afloat in the air; which must necessarily add a column to the air at least a thousand times greater than the vapour taken up; and often much greater than that; which must as necessarily increase the weight of the air, and make the mercury rise proportional to the additional column; and when rains descend, they subtract a column from the air, near a thousand times as great as the water fallen, which must necessarily lessen the weight of the air, and make the mercury fall proportional to such subtraction. As to the winds, they affect not the mercury, but as they are generally, in these climates, a consequence of descending clouds and rains, blowing into those parts, where the column of air has been lessened, and while they blow, the mercury will continue low in the barometer, till the equilibrium of the atmosphere be restored. But, I believe, if it were inquired into, whether the mercury descends, when the wind begins to blow every morning between the tropics, and the

vapours to ascend, that it will be found, that the mercury rather ascends.

There is a phenomenon, which has puzzled many; that is, Why the mercury falls before the rain falls? This may be explained by the column of air's being lessened gradually, by the condensation of the electrical fluid from the first descent of the vapours till they fall in rain.

I shall now say something of land-breezes and sea-breezes, a phenomenon, which sometimes happens in fair settled weather, that the wind blows out from the land at night, and in from the sea at day-time. The land-breeze is occasioned by the descent of the clouds, and the particular formation of the land; for if the land rise into a hilly country from the sea, when the clouds and vapours descend at night, which they often do by the electrical fluids being condensed, they must press the air down the land toward the sea in their fall; as may appear from the smoke of any fire running down the side of a hill, in the evening of a damp day, when the clouds are upon the descent. And the sea-breeze is occasioned by the clouds ascending in the day-time, which must impel the incumbent air upwards, and make room for the sea-breeze to flow in: but, beside the mere ascent of clouds, there is an exceeding greater quantity of vapour raised from the land than from the sea. For the same extent of land has an exceeding greater surface than the same extent of sea; which may appear from the various forms of vegetables and animals, &c. and the greater the surface, the greater will the evaporation be. Beside, the more irregular these surfaces are, the greater will be the

the reflection and refraction of the sun's beams, which will increase their power. And it is also necessary, that the evaporation should be much greater from vegetable and animal fluids, than from fluids in a quiescent state, to carry on a circulation for the great work of nutrition : but of this when I come to speak of vegetation and animal life. Now, the ascent of these vapours must beget a circulation of the air inward from the sea ; in the same manner as the ascent of vapours from any fire brings in the air below to that fire.

As to water-spouts, I have found them oddly described by the learned, as being great columns of water sucked up from the sea by the clouds. But I never saw any such ; nor could I find, upon inquiry, from many honest men, who have sailed almost all our known seas, that they ever met any such ; and therefore I do not believe that there are any such. There is indeed an appearance something like their description, which may have given rise to their conjectures ; but this is no more than a very heavy shower from a very dense cloud, which is drawn into a conical form, and a very narrow compass at bottom, before it arrives at the sea ; which it dashes with great violence in its fall. It may be worth the while to inquire how the shower comes into this conic form ; which I think is caused by the general attraction. For while the vapours floated in the air, being of like specific gravity, they were equally attracted by the air as by one another ; but when their specific gravity was greatly increased by their being formed into larger drops, and letting go great part of that electrical fluid, which buoy'd them up,

and the greatest quantity of water falling in the center of the shower, the extremes may be greatly attracted to the center, in their passage downward.

There is one objection, which may be made to all I have been saying of the winds and weather ; that is, the great distance of the tropic, and that the wind must be a great time in coming to us from thence. But this objection will lessen, when the velocity of the wind is considered, which in a fresh gale will move a degree in two hours ; which will soon bring it from the tropic to us. And this velocity, and a much greater, I proved by a machine, which I invented, and made, in the year 1733, to go upon wheels, by the force of wind. For I have been carried in it at the rate of more than a degree in two hours, when the wind was upon the beam ; that is, when I could sail forward or backward upon the same tract, with equal facility ; so that my motion going before the wind, must have been much greater, which I then neglected to estimate.

And now, Gentlemen, I fear two things ; that I have said too much, and that I have said too little. For if this letter appears with the same face of truth to you as it does to me, I have omitted many things, for fear of being too prolix, which I ought to have added. But, if I have written without a foundation in truth, I must, long before you come to this part of my letter, appear prolix and impertinent ; and therefore it may be time to conclude. Before I do which, I will assert one thing ; that is, that if any thing in this letter is found worth your acceptance, it is intirely my own ; for I have not borrowed a single hint. No more I did in my letter concerning the
cause

cause of thunder. For when I wrote that letter (which was on the 20th of September 1751), I did not know, that any one had the least conjecture, that the electrical fire was the cause of thunder. And I told many gentlemen in this kingdom, in the beginning of the year 1751, that I thought I had discovered the real cause of thunder. Indeed, immediately after my sending that letter to you, on the 18th of June 1752, I met some accounts in our news-papers, that iron bars, some how or other set up, during the time of thunder, were found to be electrified; but from that time to this I know nothing particular of those, or any other experiments on thunder-clouds. I waited the publishing of your volume of Transactions, for the year 1752, to inform myself of these matters; but I do not find, that it is yet come to our kingdom.

As I am not a plagiary, so I would not willingly be thought one; nor should I venture to trouble you with what I thought you may have better from other hands; but as the matter of this letter appears new to me, so I venture to lay it before you, without any other motive than to add my mite in the investigating truth. I am,

Gentlemen,

Your most humble

and most obedient servant,

Hen. Eeles.

My

My fear of trespassing upon your patience had almost made me forget to explain, how far the wind assists vapours to rise in greater quantities, and in less time, than they would in a quiet air. And this omission would not be excusable, because both the learned and unlearned have observed, that a greater quantity of vapour is carried off in dry windy weather, than when the weather is calm. When vapours are emitted in great plenty from the surface of fluids, such as from a pot before it begins to boil, or from the surface of a bowl of hot punch, &c. it often happens that they crowd each other in such manner, that the surrounding electrical fluid of each particle is compressed, and cannot occupy that portion of space, which is necessary to make it buoy up the particle: and thus the particles float in a steam just above the surface of the liquor, and they are prevented from falling back into the liquor by the electrical fluid on the surface of the liquor; in which state none but the uppermost particles can ascend; which they do by the elasticity of the electrical fluid's forcing them upwards, till they have room for the electrical fluid thoroughly to expand itself, so as to be able to buoy up the inclosed particle of vapour.

In this state the assistance of the wind will be easily understood; for the wind blowing the vapours from the surface of the liquor, dissipates them, and gives them room to ascend, and clears the surface of the liquor, to make room for the succeeding vapours to ascend, and so on; which in this case will greatly aid the ascent of vapour. And thus far I think that the wind assists in the ascent of vapour, but no farther.

I have

I have mentioned hot liquors only, because the steam is more visible in them: not but the same case often happens in evaporation, where we cannot so easily perceive it.

L E T T E R III.

To the Reverend Thomas Birch, D.D.
Secret. R. S.

S I R, Lismore (in Ireland), March 31, 1755.
Read April 2, I Received your obliging letter of the
 a instant, of which I am highly
sensible shall always be subservient to your
command

Y me the honour to tell me, that the Royal
Society desire to know the experiments, by which I
found all ascending vapours and exhalations to be
electrified. At first I only supposed they must be
so, according to the reasonings in my letter; but
upon trial, with a very simple apparatus, I had the
pleasure to convince myself that they were so.

I extended a fine string of silk eight feet horizontally, and from the middle suspended two pieces of such down as grows upon our turf-bogs, by two pieces of fine silk, about twelve inches each in length; and then, by rubbing a piece of sealing-wax on my waist-coat, over my side, I electrified the pieces of down; and then brought sundry burning things under them, so as to let the smoke pass in great plenty through and about them, to try whether the electric fluid would run off with the smoke; but
I had

I had the pleasure to see that the down was but a little affected by the passage of the smoke, and still remained electrified. I then brought sundry steams from the spout of a boiling tea-kettle, and otherwise, in the same manner, and still found, that the down remained electrified. I then breathed on them in great plenty, but found that the down still remained electrified. I then joined the palms of my hands together, with the fingers extended perpendicularly under the down, which still remained electrified; although the subtile effluvia, thrown off by perspiration, passed in great plenty through the down; as may appear by holding one or both the hands in the same manner under any light matter floating in the air, which will be driven upwards thereby, with as great velocity as an electrified feather is by any electrified body held under it. In short, I tried all the vapours and exhalations I could think of, in the same manner, and with the same success.

I then warmed a wine-glass, and with the skirt of my coat held inside and outside the glass between my fingers and thumb: I rubbed the glass briskly about, and electrified the down, and found all experiments answer in the same manner as they did with the wax. I mention this particular, because some writers on electricity have said, that there were two kinds of electrical fire, the one resinous, and the other vitreous; because light bodies electrified by glass are attracted by electrified wax, &c. and those electrified by resins are attracted by glass. But I think these different effects must arise from some differing qualities in the resin and glass, which have power to actuate this fire differently. For if there
were

were really two distinct species of this fire, opposite in their nature; the afore-mentioned experiments would have a very different consequence from what appears. For if the vapours were impregnated by the vitreous fire, they must absorb, or some way disturb, the resinous fire, which electrifies the down, and so *vice versa*: but we find, that the same vapour, with its electric fire, passes through the electrified down in the same manner, whether it be electrified by glass or resin. But I will not detain you on this subject.

The electricity remaining in the electrified down after these experiments made it appear, that the smoke and steams must be either electrics, or non-electrics electrified. It was easy to suppose them non-electrics, as they arise from non-electric bodies; and the more, because the highest electrics by a discontinuity and comminution of their parts (long before they come to be as minute as the particles of ascending vapour), become non-electrics, or conductors of electricity. For glass, resin, wax, &c. all become non-electric, even in fusion. But to try whether the steams, &c. were non-electrics, I only bedew'd the wax and glass with my breath, steams, &c. from my hand to the end of the wax and glass; and then touching the electrified down with the end of the wax or glass, I found, that the electrical fire immediately passed from the down into my hand, through the steams, &c. which rested upon the wax and glass. Which, I think, sufficiently proves the steams, &c. to be non-electric; and I think, that it as plainly appears, that they are all electrified while ascending, because the electrical fire in the down

does not join with them in their passage through it ;
which otherwise it would do with them, or any
non-electric not electrified.

I made some other experiments to this purpose ;
but these principally persuaded me to say, that all
ascending vapours and exhalations were electrified.
But how far they will weigh with the Royal Society,
I must leave to their better judgment. I am,

S I R,

Your most humble and

most obedient servant,

Hen. Eeles.

P. S. I electrified the down between each experi-
ment.