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.*

LETTER I.

SIR, Lismore (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,

SIR,

Your moſt obedient humble ſervant,

Hen. Eeles.

To the Royal Society.

LETTER II.

Gentlemen, Liſmore (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 homogeneal 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 phænomena 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 take

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 unwholſome. 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

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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 phænomena 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 equa-

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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 toward

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 phænomena 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 wherever two or more particles are forced to approach within their repulſive powers, they will run into contact and

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 imping 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 phænomena 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 ſeperate 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 ſail from the extremity toward the centre, if they luff up, they will be taken aback, and run the hazard of being diſmaſted: and this I always found true in practice.

Why high winds are ſeldom found in a ſerene ſky without clouds, is alſo pretty evident; for clouds occaſion theſe high winds in a double manner; firſt, when they are large and united, and upon the deſcent, as they occupy a great ſpace in the atmoſphere, they muſt preſs the ſubjacent air into a great velocity; and ſecondly, when they coaleſce, and come down in heavy rains, they make room for the air to flow in with violence, to reſtore the equilibrium of the whole air.

To know how the vapours coaleſce in warm ſeaſons, to form thoſe diſtinct denſe clouds, which produce thunder and heavy ſhowers, we muſt firſt conſider the ſtate of the vapour in its aſcent, which muſt be ſtrongly endued with the electrical fire to enable it to aſcend to the great height it then does in the atmoſphere: Secondly, The great quantity carried up, and kept afloat at that great height, during a ſeries of hot dry weather. The quantity may be proved from the height of the mercury in the barometer (as I ſhall ſhew preſently), and the great height and diſſipation of the vapour; from the ſerene appearance of the ſky; and alſo by the great height, where even thunder-clouds are formed in its deſcent; which may be eaſily meaſured by the length of time between the light and noiſe of thunder. In this ſtate the upper air is in a quietude, and whatever part of this vapour begins to coaleſce and ſubſide firſt, will carry down with it part of the ſubja-

cent

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 ſubſtract 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 phænomenon, 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 phænomenon, 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 croud 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.

LETTER III.

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

SIR, Liſmore (in Ireland), March 31, 1755.

Read April 2, I Received your obliging letter of the
.1 inſtant, of which I am highly
ſenſi .nall always be ſubſervient to your
com

Y me the honour to tell me, that the Royal
Soci deſire to know the experiments, by which I
foun 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 waſte-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,

SIR,

Your moſt humble and

moſt obedient ſervant,

Hen. Eeles.

P. S. I electrified the down between each experiment.

XXVI.