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detached from the molecule, and also the residue of the acid, but whilst with the sulphur compounds these two simply combine (forming a compound ether), and leave a hydrocarbon sulphide, in the case of the phosphonium salt the acid residue is reduced by the tertiary phosphine, and the group thus left combines with the hydrocarbon radical, forming a ketone.

Thus :—

A result of this kind is in perfect harmony with the views already expressed by one of the authors* regarding the analogies and differences existing between phosphorus and sulphur and their compounds.

As regards the action of heat on the hydracid salts of tetraethylphosphonium, the chloride and cyanide alone afforded interesting results. In the case of the cyanide these, however, were complicated by the presence of water, which attacked the compound and converted it into hydrocyanic acid, phosphine oxide, and ethane. A small quantity of it, however, appeared to behave like a sulphur compound, as it was apparently converted into cyanide of ethyl and triethylphosphine. The action of heat on the chloride seems to be anomalous, and as it is the only method by which the tertiary phosphine can be obtained in any quantity from a phosphonium salt, the decomposition of phosphonium chlorides containing various hydrocarbon radicals has been the subject of a separate research, the results of which one of the authors hopes soon to be able to publish.

XXIII. On Dew. By JOHN AITKEN.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE read the "Remarks on a new Theory of Dew," by Mr. Charles Tomlinson, F.R.S., in your Journal for June; I have also read the paper there referred to, and written by the same author in the 'Edinburgh New Philosophical Journal,' "On the Claim of Dr. Wells to be regarded as the Author of the 'Theory of Dew.'" In certain respects and up to a certain point, these two papers bear a strong resemblance to

* E. A. Letts, Trans. Roy. Soc. Edin. vol. xxx. part 1, p. 285.

each other. The aim and object of the greater part of the first of these two papers appears to be to detract from the merit of Dr. Wells's great work, by endeavouring to show that almost all the facts observed by him had been previously noted by other investigators. As Mr. Tomlinson, however, just at the end of his paper, reinstates Dr. Wells in the position he previously occupied, perhaps nothing further need be said on that part of the subject; but as he has not been equally generous to me, I must undertake the uncongenial task of trying to put myself right with your readers. I will take Mr. Tomlinson's remarks in the order of his paper, and as your space is valuable, will make my reply as short as possible.

At the very outset Mr. Tomlinson, by raising a false contention, attempts to place me in opposition to recognized authorities, by entitling his paper "Remarks on a new Theory of Dew." The results of my investigation are in no sense entitled to be called, and never have been called by me, a "*New Theory*;" nor was this so-called "new Theory of Dew" "promulgated in opposition to that of Dr. Wells." My work has not resulted in a new theory; it is only an extension of the work the foundations of which were laid by Dr. Wells. The great fundamental principles established by that authority are unaffected by anything contained in my paper.

When Dr. Wells wrote his celebrated "Essay on Dew," he must have been quite aware of the uncertainty as to the source of the vapour. Although he appears to have thought that but little of it could rise from the ground while dew was forming, yet with admirable caution he adds, "He was not acquainted with any means of determining the proportion of this part to the whole," thus clearly recognizing the soil as a possible source of the vapour. Wells's theory, however, has principally to do with the condensation of the vapour after it is in the air, and but little with its source.

While quite agreeing with the author as to the difference between "literary work" and "scientific work," and while he in his comparison would extol the literary, I would rather be inclined to consider the difference between literary and scientific work to be so great that they cannot be compared. Both are useful in their way. Mr. Tomlinson no doubt has had sufficient experience as a thinker, a writer, and an investigator to have seen many a fair theory, apparently perfect in all its parts, and consistently thinkable all through, vanish, under the light of experimental test, like the baseless fabric of a dream at the first touch of day.

At pages 485-6 Mr. Tomlinson clearly states the chief points, established by Dr. Wells, under six heads, and then

with equal clearness gives under eight heads what he calls Mr. Aitken's theory of dew. If he had entitled these last as eight points contended for in my contribution to the theory of dew, no fault could be found with this part of his paper, as he states the points with clearness and precision. But unfortunately he still looks on these eight points as rivals to the previous six of Dr. Wells, and so continues hostile to them. "Such," he says, "is the new theory of dew, which, if accepted, must go far to render nugatory the results obtained by some of the most celebrated observers." After reading Mr. Tomlinson's paper, however, I do not find that he adduces any results of previous observers that are in any way rendered nugatory by the results set forth in my paper. It has already been stated that the "new theory" is not in opposition nor are the results contrary to the teaching of Dr. Wells; nor are they, so far as I know, contrary to those of any of the "celebrated observers."

In continuation, my critic then, in place of sustaining his accusation by giving examples in which my teaching is at variance with recognized authorities, gives an example in which my observations agree with recognized authority. At the foot of p. 486 he objects to the statement in my paper that "the ground at a short distance below the surface is always hotter than the air over it," because it is not a new observation, Pictet in 1779 having previously observed the fact. I am, however, puzzled to understand what bearing this has on the subject, or what conclusion he wishes to be deduced from this statement. To compare small things with great: more than one person had seen an apple fall to the ground before Newton made such good use of the observation. Pictet, when he made this observation, was working at another subject, and did not draw any conclusion which would be rendered nugatory by the acceptance of my conclusions.

Mr. Tomlinson proceeds to say, "that dew rose out of the ground is a very old notion." I was well aware that the idea was an old one, and it is distinctly stated in my paper. It is difficult to understand what impression my critic wishes to convey by this remark. If it is that the idea does not possess the merit of novelty, having been already expressed, my reply is that it has been already said that dew was caused by the moon-beams, that it descended from the stars, &c.; and each and all of these ideas were supported by about the same amount of evidence, namely the *ipse dixit* of the theorist.

The same answer applies to the footnote, p. 486, regarding the exudation of moisture by plants. Though "Muschenbroeck regarded dew as a real perspiration of plants," yet

beyond the bare statement I am not aware of anything he advanced to support his idea. His theory thus fails to explain the difference between the dew-drop on plants and true dew. Nor does it account for dew on dead matter. On these historical matters I am, however, open to conviction, as it is extremely difficult to get access to all that has been written on this very popular subject.

As I nowhere say that vapour may not be condensed out of air under conditions other than those where dew is forming, the two paragraphs at the foot of page 487 have no bearing on the subject.

I am unable to see the relation between the observations made by Wells and Six and myself, which Mr. Tomlinson brings together by the footnote at page 488. In the investigations made by the two former observers, the difference remarked was between the readings of thermometers placed on the grass and other substances, and thermometers hung above them; while in my experiments some thermometers were placed *on* and others *under* the grass. The only likeness in the two sets of observations is a similarity in the amount of the difference in the readings of the respective sets of thermometers.

It is unnecessary to follow Mr. Tomlinson through the natural processes as described in pages 489, 490, further than to say that the cooling he there describes as taking place on grass at night is not correct. At the middle of page 490 he says:—"We may imagine three layers of air—one in contact with the points of the grass; the second immediately below it, where the blades are more numerous, and more or less exposed to the zenith; and the third entangled in the matted portion, which is entirely sheltered from the sky." He then describes the manner in which he supposes these layers to get cooled, one after the other, beginning at the upper, till "in the end the lower stratum will be colder than the first, so that the blades and stems which are least exposed to the aspect of the sky will be colder than the points of the blades, and the thermometer buried in the grass will mark a lower temperature than one in contact with the surface." If Mr. Tomlinson had read my paper carefully, or practically put the question to nature, he would not have ventured such an opinion, as he would not, I expect, ever have found the "thermometer buried in the grass mark a lower temperature than the one in contact with the surface." On the contrary, all through the night the lower thermometer remains warmer than the upper one. Mr. Tomlinson, in describing the progress of cooling, appears to have forgot to take into account the supply of heat given

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off by the earth. This supply is quite sufficient under all conditions, so far as I have observed, to maintain the soil and the thermometer over it at a temperature much above that at the top of the grass; and while the thermometer at the top of the grass falls quickly at night and falls a good deal, the one "buried in the grass" falls comparatively little, the upper one remaining much the colder throughout the night. It is not till day arrives and the temperature rises that the upper thermometer gets heated and rises to the temperature of the lower one.

With regard to the remarks about dew in "Persia" and "the African desert," I have nothing to say, as they do not bear on my paper, it being distinctly stated there that my remarks apply only to this climate. I may, however, add here that I wait for further information before forming any opinion as to what takes place in other and unknown conditions.

At page 493, Mr. Tomlinson says, "There is such a vast consensus of scientific opinion in favour of the received theory of dew, that any attempt to set it aside in favour of another must be supported by the strongest experimental evidence." Now, as I have already said, I have never made any attempt to set aside Wells's theory; and as all my work has tended to confirm and extend his, I hope Mr. Tomlinson will, on reconsideration, give my contribution to this subject a more favourable reception; and might I also ask a more careful consideration of that part referred to by him in this same paragraph, where he says, "Mr. Aitken exposes a turf six inches square to the air in a scale-pan"!! This misconception of the essential conditions of the experiment has given rise in his mind to the objection to the conclusion I have drawn from the experiment, and has also given rise to the confusion Mr. Tomlinson has made in comparing my experiment with other previous ones.

If Mr. Tomlinson had delayed till he had seen the complete paper, he probably would not have found the difficulty he has experienced in reconciling the two sets of observations referred to in the first paragraph, page 494, of his paper. The paper containing the account of my experiments, on which Mr. Tomlinson founds his criticism, being in abstract was made as short as possible, and does not contain sufficient information to make the conditions of the experiments clear to Mr. Tomlinson. The edges of the trays were, as he supposes, in contact with the ground; one slate and one weight were also in contact with the ground. But, in referring to the latter, they were simply described as they appeared without

touching them, that is without lifting them from the ground; whereas it is the condition of the undersides or insides of the trays that are specially referred to. The slate and weight resting on the ground were of course quite wet on their undersides, which were invisible except when moved, while their exposed parts were dry; whereas the elevated slate and weight were wet all over. With this explanation I have no doubt Mr. Tomlinson will see the importance of the contact with the ground in keeping the temperature of the slate and weight above the dew-point; while the wetness of their undersides, which are invisible, brings these experiments into harmony with the experiments with the trays.

In the second last paragraph of Mr. Tomlinson's paper he seems to object to that part of the "new theory" which relates to the excretion of liquid by plants, principally because it is "startling" and not in keeping with preconceived opinions, and he asks, "for what purpose are plants endowed with such high radiating powers?" I scarcely think he can expect me to answer that question, as it forms no part of my theory. If any one, surely he is the proper person to give the reply. But are plants really such good radiators? Take even the figures given by Mr. Tomlinson for the different substances named. In none of these is the difference of any importance; indeed the differences are not more than might easily arise from errors of observation by the method used by Melloni, except in the case of vegetable mould. So far as my own measurements go, by the method used by me, I have not found any difference in the radiating powers of grass and soil at night. If there is any difference, it must be very small. One result of recent investigations is to show that plants with leaves like those of grass are very badly adapted for collecting dew, the blades being little narrow strips placed at right angles to the direction of the air-currents; the slightest movement of the air prevents the surfaces of the blades being cooled by radiation much below the temperature of the air. In illustration of this I may mention that *narrow strips* of glass or other material exposed at night will sometimes collect no dew, while a *sheet* of the same material will be running with water, the windward edges being of course dry. Plants with large leaves have quite an advantage over grass in dew-collecting powers; but, on the other hand, grass has the advantage during sunny days, on account of its narrow blades being but little heated by the sun. This may be one reason, though not the only one, why grass so seldom flags under a scorching heat.

If I might be allowed to make a slight explanation here,

perhaps it would help to remove a difficulty some have felt in accepting some of the conclusions contained in my paper. One of the results contended for is, that dew on grass and on bodies near the ground is formed from vapour rising from the ground during night; the reason given for this conclusion being that grass-land is always in a condition to give off vapour during dewy nights. The vapour that rises from the ground after sunset will thus displace the vapour that rose during the day, and the latter will diffuse itself into the drier air over the grass. The stems and blades of the grass during night will thus be surrounded by the vapour that has risen most recently from the ground. Another reason given is, that at night the temperature of the air among the stems is much higher than that of the air at the tips of the blades, and being in contact with moist soil is nearly saturated. The vapour-tension of this hot air, rising among the stems, is thus much higher than that of the air over them, and is thus in a much more favourable condition for forming dew than the air higher up.

These are the reasons given for concluding that dew on grass is formed from vapour rising at the time from the ground and not from that which rose during the day. The principal difficulty experienced by some in accepting this conclusion, seems to have arisen from extending this conclusion to the source of the dew deposited on bodies placed some distance from the ground. Now, when we consider what takes place in these higher positions, it is easy to see that the conditions are much more complicated, and we can now say very little about the vapour, either as to the place of its evaporation or as to the time when it changed its state. Whenever the vapour coming from the ground rises above the grass and mixes with the drier air over it, we get an entirely different and much more complicated condition of matters. No doubt some of the vapour molecules in this upper air will have risen from the ground only a short time before, but some of them will certainly, if there is the slightest wind, be molecules which have risen during the day, and no doubt some of them will have risen into the air many days previously; and while some will have risen from the ground immediately underneath, others will have come from lands and oceans far away. But while this may be so, it in no way affects the conclusion that vapour is almost constantly, night and day, given off by the soil, and that dew on grass is part of this rising vapour trapped by the cold blades.

Darroch, Falkirk,
June 14, 1886.

Yours truly,
JOHN AITKEN.