THE Report of the Manchester Museum for 1904–1905 notes a deficiency in the finances of about $1,000, but causes one to wonder how so much good work as is accomplished by this institution can be done on an income of less than $15,000.

DISCUSSION AND CORRESPONDENCE.

CONTRIBUTIONS TO OUR KNOWLEDGE OF THE AERATION OF SOILS.

UNDER the above title Dr. Edgar Buckingham presents the results of a series of investigations relating to an important subject, in Bulletin No. 25 of the Bureau of Soils. As a practical problem in soil management the securing of those conditions which will insure a deep and ample ventilation is extremely needful; and hence any essential advance in our knowledge of the principles governing soil aeration is important. In the letter of submittal it is stated:

This paper presents for the first time definite information regarding the rate at which a gas escapes by diffusion from the soil into the atmosphere, or *vice versa*. It shows that the rate of diffusion varies approximately as the square of the porosity of the soil, and that this diffusion follows the laws for the free diffusion of gases. It thus becomes possible to calculate the rate of aeration in any particular soil from results obtained in experiments on free diffusion. Tables are given showing the rate of escape (and consequently, for a condition of equilibrium, the rate of formation as well) of carbon dioxide in the soil when the porosity of the soil and the concentration of the carbon dioxide at any given depth are known. The paper shows further that the aeration of soils is almost entirely due to diffusion phenomena changes in barometric pressure having very little influence in comparison.

The author in his 'Concluding Remarks' says:

1. We have measured the rate of flow of air under pressure by transpiration and of air and carbonic acid by diffusion, through four widely different soils, in varying states of structure, compactness and moisture content.

2. We have shown that the speed of diffusion of air and carbonic acid through these soils was not greatly dependent upon texture and structure, but was determined in the main by the porosity of the soil.

3. We have shown that the rate of diffusion was approximately proportional to the square of the porosity.

4. We have shown that when this relation is used to compute from our results the rate of free diffusion when no soil is present, it gives a result which is entirely consistent with what is already known from the work of other experimenters on the free diffusion of gases.

5. We have shown that when the porosity of a soil is reduced by compacting it, the ease with which air flows through it under the driving influence of a difference of pressure is greatly reduced, varying as the sixth or seventh power of the porosity.

6. We have investigated the depths to which free outside air might penetrate soils to different depths, under such barometric variations as are to be expected in average cases, if the outside air remained distinct from the soil air.

7. We have shown how to compute the rate of escape of carbonic acid from the soil by diffusion under given conditions of pressure, temperature, porosity and concentration of carbonic acid.

8. We have compared the linear velocities of diffusion and barometric transpiration, and hence—

9. We have shown that the escape of carbonic acid from the soil and its replacement by oxygen take place by diffusion and are determined by the conditions which affect diffusion, and are sensibly independent of the variations of the outside barometric pressure.

The foregoing remarks and conclusions are based on the mathematical treatment of a very limited series of laboratory experiments, which, however, have been executed with great care. The subject is one so complex and intricate that it can not be solved by so short and direct a cut and it is a matter for exceeding regret that this piece of work, admirable in itself so far as it goes, should be given out by the Department of Agriculture with so much of assurance of finality for its conclusions before they have been checked by even a single field observation or experiment. Almost infinite injury is done to the cause of agricultural science and to the growth of the Department of Agriculture along sound and enduring lines by prematurely exploiting results of investigation, striving to get them before the public eye of practical men—congressmen, farmers, merchants and manufacturers—but succeeding in getting them there in the form of untruths, or of partial truths which lead to errors of practice so soon as they are applied. Lamentable examples of these are furnished in 'Bulletin 22' and in the extended press promulgation regarding what may be expected through bacterial inoculation of the soil. Much expense was incurred in conducting the investigations referred to and in getting them before the public; very much more is being incurred by those who are giving them practical trial; but by far the greatest expense will accrue during the time required to outgrow the disappointment and the smart of defeat, of wasted effort. It is this condition of things, more than ignorance and more than conservatism, which maintains with terrible effectiveness, as a brake on agricultural progress, the dogma, 'Book farming don't pay.'

In calling attention to the results of the author's investigations it is important to point out that rates of transpiration, as measured in the laboratory trials, are quite inapplicable for use in giving a measure of the rate of flow of air through soils under field conditions. It must be noted that in preparing the soils for the measurements of rates of transpiration they were 'first broken up fine in a mill.' This condition is very wide from what is found in the field and represents more nearly a puddled soil which is always a condition of sterility, and we believe that one of the chief causes of this sterility is the inadequate aeration possible under such conditions. In considering the results of the author therefore it must be borne in mind that he has measured the rates of transpiration—and of diffusion also—through a thin layer whose field structure had previously been altered by what may be designated dry-puddling. In illustration of the effect of dry-puddling we shall cite two series of observations made by Mr. Nelson and Mr. Hogenson, under our direction, while connected with the Bureau of Soils. They are taken from the records in the office of the bureau, which contain several hundreds of measurements covering many types of soil which have been examined as to the rates of transpiration in the first, second, third and fourth feet. In the particular cases cited we select two of the soils which were under investigation in 1903 upon which corn and potatoes were grown, as reported in Bulletin 26 of the Bureau of Soils. The rates of transpiration through these soils were measured under five different conditions, as indicated in the table. By 'field condition' is to be understood the granulation into which the soil falls naturally when plowed in good condition of moisture, but using only such portions of it as readily pass a one millimeter screen in the air dry condition and without rubbing. This soil was firmly packed in the transpiration tube and the rate of flow of air through it measured, after which it was returned to a mortar and pulverized by gently working it under a rubber pestle. When in this condition the transpiration was again measured, after which it was pestled a second time, the process being repeated until the rates of transpiration were obtained for the five different conditions.

MEAN RELATIVE RATES OF FLOW OF AIR THROUGH AIR DRY SOILS MORE OR LESS FINELY PULVERIZED.

	Norfolk Sandy Soil.		Janesville Loam.	
	Pore Space.	Seconds.	Pore Space.	Seconds.
Field condition,	37.0	69	51.6	83
Pestled once,	31.8	1,050	48.5	600
Pestled twice,	29.9	1,724	48.5	800
Pestled three times,	29.1	2,025	47.9	1,200
Pestled four times,	28.5	2,550	46.8	1,350

It is clear from this table that a very profound change in the permeability of the two soils has been effected by the dry-puddling; the rate of flow of air through the Norfolk sand being finally reduced to only about one fortieth of what it was at first, and that of the Janesville loam to about one seventeenth. It will be observed also, if computations are made, that the rates have not varied as the sixth or seventh power of the porosity.

Under undisturbed field conditions the rate of transpiration would in all probability be very different from what is given in the first line of the table and for the surface foot; in air dry condition it is quite certain to be larger than there found. In actual field conditions the body of the soil is ramified by channels and passageways which are often larger than capillary and through which the air moves more nearly in accordance with the laws controlling the flow through pipes. These passageways often cross-divide the soil itself into endlessly irregular and varying blocks, so that even deep in the ground both air and water flow in more or less open channels, percolation and transpiration taking place into these so that much of the mass movement of either air or water may occur without passing through capillary spaces. We have called attention to this fact in 'Movements of Ground Water'[1] and have there shown how far computations based upon laboratory trials may be from what occurs under natural conditions. These remarks apply with especial force to the surface one to four feet of field soils, the movements through which are of greatest agricultural importance. In this zone shrinkage cracks and passageways left by the decay of roots or formed by burrowing animals, it appears to the writer, influence in a very profound way the interchange of air as effected through changes of atmospheric pressure and cause the estimate of the author to be, in our judgment, very much below the true value.

The particular mode of action of atmospheric pressure which, it appears to us, must be most potent in causing an interchange of air in the surface soil has not been considered in the Bulletin under review. We refer to the pressure and suctional effects which result from, or are associated with, changes in wind velocity and the turbulency of the air movement at the earth's surface. The fluctuations of pressure to which we refer are of too short duration to be recorded by ordinary barographs, but they are nevertheless of sufficient length to be transmitted into the soil and their magnitude often exceeds some of those which the author has considered, while their frequency is very great. The agency which it appears to us is likely to be found most influential in the aeration of the surface soil is the wind itself, as it is the chief factor which effects a change of air in a house. As the air passes over the surface of a field, there must be maintained an excess of pressure on the windward side of obstructions to flow large and small of whatever kind, while on the leeward side there will be maintained a deficiency of pressure, so that on the whole air will be flowing into the soil in some places, traveling more or less horizontally and then rising to come out at places where the air pressure is less. And we do not see how it is possible that this influence can be limited to so small a depth as the author estimates for barometric 'rinsing.' Besides this, when the wind is blowing strong and is gusty in character there is a turbulency of flow analogous to that which occurs in a stream flowing down a rapid, giving to the air a downward thrust upon the surface, from which it rebounds, driving the air into the soil in some places and sucking it out in others.

But to these statements the author will doubtless reply that the writer is merely naming possible factors and doing so without testing their probable efficiency even mathematically. This is quite true, but both these and his own views can and should be checked by field observations and he is aware that we had begun a series of observations on the composition of soil air collected simultaneously at different depths down to four feet and that a considerable amount of the data so obtained are unpublished among the records of the bureau. He is well aware too that my object in having him called to the bureau was that he might make investigations along exactly the lines presented in the Bulletin, with many others, but to have him do so in conjunction with simultaneous field studies so that each line of work would supplement and check the other and be definitely related to observed crop and soil conditions. My criticism now is that the language of the Bulletin conveys the impression that such laboratory and mathematical treatment as he has presented have been sufficient to solve the method of soil aeration and to give a measure of the rate at which it occurs under field conditions, without making a field check on the results.

In regard to the longer period atmospheric waves, which the author has specifically considered, attention should be called to the fact that these, even when they are as short as

[1] 'Principles and Movements of Ground Water,' XIX. Annual Report, U. S. Geol. Survey, Part II., p. 249.

fifteen or twenty minutes, exert an influence which is great enough to very materially influence the discharge of water into wells, field drains, springs and river channels. It is well known too, in the case of breathing or blowing wells, that there is for days together a continuous flow of air out of and into the ground, the currents being strong enough, in a case which we have personally observed, to rattle loose two-inch planks lying over the well, itself nearly a hundred feet deep and four feet in diameter. In this particular case we were called to examine the well because it was impossible to prevent the suction pipe in the well freezing and bursting during the winter, caused by the large volume of cold air sinking into the well at times of high pressure when the thermometer was very low. The owner informed me that in digging the well, after a depth of eighty feet had been reached, work was stopped for the Christmas holidays and that after taking up the work again the gravel was found frozen so that a pick was necessary to loosen it before beginning digging.

We have observed fluctuations in the discharge of water from tile drains, associated with and apparently caused by changes of barometric pressure, amounting to fifteen per cent., and in the case of a deep well, discharging through a six-inch pipe, where the rate of flow was measured in a reservoir on ten consecutive days, the discharge per minute was found to vary between the wide limits of 15.441 and 13.947 cubic feet per minute,—a variation of fully ten per cent. We have also secured autographic records on the Wisconsin and Fox Rivers and from Lake Mendota which seem to indicate that the general seepage over wide areas changes its rate with changes in barometric pressure to such an extent that when the discharge is collected into channels the differences in depth are measurable, and when we have such changes as these it is difficult to believe that the inflow and outflow of air are not greater than is suggested by the conclusions of this Bulletin.

In regard to the influence of simple diffusion, in effecting soil aeration, it appears to the writer that the author has obtained values which must be much too large for field conditions. In carefully measuring the rates of diffusion, under the conditions of rigid control, which he did, the author has done exactly the right thing; but what is lacking is supporting field checks which are greatly needed in verifying the conclusions reached, particularly when the results are used so precisely as to compute the amount of carbonic acid escaping from a given field surface from the per cent. of carbonic acid found in the soil air at a given distance below the surface, where the porosity of the soil is known. Referring specifically to some of the author's data: If it is true, as indicated on page 39, that carbonic acid was escaping from soil in the flower bed in front of the building of the Bureau of Soils at the time of observation at the rate of .04 of a cubic foot per day and that it was being produced at this rate in the soil below the depth of six inches throughout the growing season—let us assume of 120 days—this would mean a production of carbonic acid, through the oxidizing of organic matter, at the rate of 209,088 cubic feet per acre; and, taking the weight of a cubic foot of carbonic acid at .12323 pounds, there would have been a loss from the soil of 7,026 pounds of carbon per acre. This amount of carbon represents, using an analysis of Hall's, 13,970 pounds of water-free grass per acre, or eight tons of hay containing the usual 15 per cent. of moisture. If we take Ebermayer's observations on the amount of carbonic acid in soil air, extending over a full year, except that August, September and October are not included, as given in the Bulletin, we shall find by the method of the author a still larger loss of carbonic acid. We use for this computation the mean amounts found for the year under the five conditions reported upon. At a depth of 15 centimeters (5.9 inches) the mean amount of carbonic acid found in the soil air was 1.09 per cent., the smallest amount in any single observation being .02 per cent., the next smaller .13 per cent. and the next .27 per cent., while the highest amount found was 4.61 per cent. Taking 120 days, as in the former case, and calculating from the table the amount of carbon carried out of the soil during this period, expressing it again as dry grass on the basis of Hall's analysis, the amount required would be 27,420 pounds per acre or, expressed as hay containing 15 per cent. water, 15.7 tons. Again, using Ebermayer's determinations for the depth of 70 centimeters (27.6 inches) and 120 days, the computed loss of carbonic acid from the soil below this depth would be represented by that carried by 31,960 pounds of dry grass or 17.3 tons of hay per acre. In speaking of the first instance cited the author says: "We may say, then, that, in this case, carbonic acid is escaping from the soil at the rate of about 0.04 cubic foot per day per square foot and therefore that this was the rate of production of carbonic acid in the soil at this place below the depth of six inches." The amount of carbon thus carried out of the soil, according to the assumption and calculation, would be greater than the amount we have calculated above by whatever was produced in the surface six inches. It is clear, however, that no such losses of carbonic acid, resulting from the decomposition of organic matter, could be maintained year after year, as the amount of organic matter in the root system of a crop is not equal to that produced above ground, at least usually, and the amounts produced above ground are only rarely equal to the amounts computed; indeed they are seldom more than one third of those quantities. It must be concluded, therefore, that the laboratory observations and methods of computation give a rate of diffusion of carbonic acid from the soil of a field much greater than actually occurs as a seasonal average. It should be noted that in getting these enormous losses of carbonic acid from the soil we have included only one third of the year, while Ebermayer's observations show that the amounts present in the soil at all seasons, including even winter, are large.

In view of the relations to which we have called attention it is clear that the generalizations cited require critical field trials to be made, bringing them to suitable tests before they should be accepted with full confidence.

F. H. KING.

MADISON, WIS.,
September 16, 1905.

THE QUESTION AS TO WHETHER FALCONS WHEN SOARING INTERLOCK THEIR PRIMARY WING FEATHERS.

THE observations of Mr. Trowbridge upon the habit of hawks when soaring to overlap their primaries (*i. e.*, on the upper side of the wing) have several times been commented upon adversely. And a well-known ornithologist has objected that this behavior of feathers has not been previously observed, in spite of the voluminous field notes as to the habits of hawks, and that no one has been able to confirm the observation of interlocking feathers. Accordingly, I am led to jot down the following notes in favor of Mr. Trowbridge's results, —for my observations are at first hand and were made, I believe, under quite favorable conditions.

It so happened that we were coming up the narrow canal from Sakai to Matsue in the face of a strong wind, so strong, indeed, that our small steamer labored to make headway against it. At one point we disturbed a kite, *Milvus melanotus*—a very common bird, by the way, along Japanese waterways—which rose slowly in the face of the wind and after making several circles followed the margin of the canal, flying and soaring, almost opposite the boat and making about equal headway. It did not occur to me at the moment that the opportunity was a favorable one for watching the wing feathers (for the bird was sometimes as near as a hundred feet), when my eye was caught by the behavior of the primaries. The hawk was flying low, about the height of the eye, and when the wing passed through the plane of the horizon I could see as the wing flapped that several primaries stood out sharply, finger-like, *dorsal* to the plane of the descending wing. This was so conspicuous, indeed, that it seemed difficult to conclude that these feathers could fold *under* one another when, in face of a strong wind, the wings became passive in soaring. Nevertheless, the distance of the bird was so great that I could not convince myself that the interlocking actually took place; I was only sure that the primaries were bowed, so that in soaring this part of the wing must have