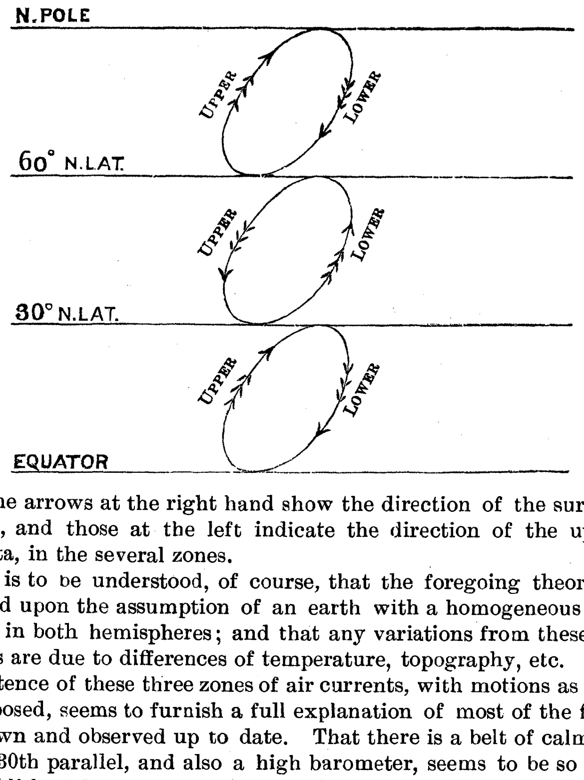
air, whither we follow it in an ascending current. It being perceived that its departure from the 30th parallel tended to produce a vacuum on that line, this current of air flows back again as upper strata in a south-west direction, obeying the same law which gives a western component of motion to the trades; when it reaches the parallel of 30° and then meets the other body from the equator, its further progress in that direction ceases, and it pours down to the surface and begins its circuit again. The northern limit of this motion is believed to be about the 60th parallel. So here we have another body of air, similar to that within the tropics, moving in a continuous circuit, but in opposite directions. For similar reasons, the atmosphere between latitude 60° and the pole will also move in a circuit between those points, only the direction of motion will be the reverse of that in the temperate zone. The coldest air in this northern zone being presumably at the pole, and therefore heaviest, will sink down to the surface and move southward with a western component. obeying the same laws heretofore given. When it reaches latitude 60°, it will meet the current from the south, rise to the upper strata, and flow back to the pole.

These several motions and the entire circulation in the northern hemisphere may be better understood by reference to the following figure: —



The arrows at the right hand show the direction of the surface flow, and those at the left indicate the direction of the upper strata, in the several zones.

It is to be understood, of course, that the foregoing theory is based upon the assumption of an earth with a homogeneous surface in both hemispheres; and that any variations from these results are due to differences of temperature, topography, etc. The existence of these three zones of air currents, with motions as here proposed, seems to furnish a full explanation of most of the facts known and observed up to date. That there is a belt of calms at the 30th parallel, and also a high barometer, seems to be so well established that no one can be found with the temerity to deny it. If there be an interchange of air between the poles and the equator, by a surface flow southward and upper strata flowing north, as proposed by Professor Ferrel and others, it seems impossible to explain the existence of this calm belt and high barometric pressure at parallel 30, or at any other place between the equator and the pole. The air flowing horizontally across any particular locality cannot produce a calm or a high pressure at that locality, whatever the velocity may be. A calm is produced by the meeting or parting of winds; a high pressure is produced by a downpour, and a low pressure by an up-pour of the air.

So, also, the prevailing winds in the north temperate latitudes, from the south-west to the north-east, are so well established, that it is deemed no evidence is required here to prove their existence. Their direction and motion cannot be explained on Professor Ferrel's theory of a southward tendency of the air in that zone at the surface. A calm at the pole might be reasonably deducible from his theory; but one at the 60th parallel is impossible.

T. A. BEREMAN.

Mount Pleasant, Ia, May, 1892.

Four-Fold Space.

IN the May 13 number of *Science*, I find a very interesting discussion of "The Possibility of a Realization of Four Fold Space," being a digest of a paper by Dr. T. Proctor Hall. As I have not had the pleasure of reading Dr. Hall's paper, and as I have not read any fourth-dimensional literature for quite a while, what I am about to say may be old. If not, and you find it worthy of publication, you may use it.

All modern thinkers about the Kantian philosophy of the fourth dimension of space, have, I suppose, dipped more or less into Professor Zöllner's Transcendental Physics. It looks as if Dr. Hall had done so, as his discussion of the knotted-string question and the "plane being" as distinguished from an ordinary three-dimensional mortal, is quite similar to certain illustrations used by Professor Zöllner.

I think Dr. Hall's idea of trying to get a clear concept of fourth-dimensional space, by initial projections from three-dimensional space, and then modifying those projections as best we can, is very ingenious, and may become a very useful factor in the study of the possibilities of four-dimensional space and four-dimensional beings; and I think he is entitled to great credit for his clear and effective start made in that direction.

I have only one criticism to make about it, which is that such a process would be exceedingly slow, as slow as the building up of the science of mathematics, or chemistry, or any other science which had to start with wholly unknown premises. I do not think that the study of four-fold space absolutely requires treatment of this elementary character. This opinion is based upon the following thoughts and inferences, which I have from time to time drawn with regard to this fourth dimension, and made use of in private conversation with regard thereto.

The so-called universe of matter, as has been repeatedly said, is known to us only because there is an unknown x (whether force or substance we cannot tell), which successfully resists our attempts to penetrate it, whether the attempt be made by the sight, the touch, or such power of projectile force as we think we have succeeded in bringing under our control. Outside of this resistance there is absolutely nothing but inference, an inference which some philosophers regard as amounting to conviction, and others, not.

When we say a block of granite is impervious or impenetrable, we simply announce an inference mentally drawn from impressions received by our various organs; and the point which I am now raising is simply this: that the same impressions might be received, and hence the same inference drawn, under a totally different state of affairs, provided we assume — and we have no reason for not assuming — that our standards, such as a foot of twelve inches, an inch of three barley-corns, etc., are simply relative, and compared with the infinite universe mean absolutely nothing, in other words, are not standards at all. Not to make this too long, but to illustrate hurriedly where I have thought, for some years, a starting point for the practical demonstration of four-dimensional space may be found, let me use an illustration.

Let us call our granite block a ten-foot cube. Standing in front of it we can only see one side; at a certain angle we can see two sides. From an elevated point we can see two sides and the top; but we can never see, except by the aid of reflectors, more than three of the six sides at once. We can easily walk around and under it, and see the other sides. In other words, and this is the key of the whole situation, we can see the whole of the cube successively but never simultaneously; and this applies to the inside as well as the outside. If this granite block were magnified so that each dimension was a thousand times what we have assumed it to be, it might be a very porous and loosely-jointed structure; yet if our eye were placed with increased faculties at a proper distance, the phenomenon presented to that eye would be exactly that which now shines forth in the ten-foot block of granite, and our inference as to its size and structure would be identical with our first assumption.

As we have no difficulty in believing that, owing to the revolution of the earth combined with its motion around the sun, we have been carried many miles through space in the fraction of a second which elapses, as we think, between dropping a coin and picking it up again, why should we regard it as an incredibly extravagant assumption that a correspondingly large space is unconsciously travelled over when we walk from one side to the other of our granite block? As the glimpse which we get of some of the fixed stars is merely a ray of light which has taken many hundred years to reach us, why should it be an altogether unreasonable assumption that the light-ray from our granite block may take a good deal longer to reach us than we are aware of? As we know, from experiments with birds, that there are sounds too high-pitched for our ear to detect, is it not in every way natural to expect that there are dimensions which the eye cannot detect?

To sum up: As our inferences with regard to the material world are rather the result of the limitations of our faculties than limitations of so-called matter itself, are we not likely to get ahead faster in the effort to broaden our concepts, and with them our ability to form concepts, by modifying our inferences than by trying to project our inferences into an unknown dimension?

W. P. PREBLE.

New York, May 23.

H. Carvill Lewis's Work on the Glacial Phenomena.

THE following communication from the wife of the late Professor Lewis seems to me worthy of publication, both out of respect to the writer and for the considerable amount of valuable information which it contains upon a subject that is now uppermost in the minds of a considerable portion of the geological world. I have no doubt that a large circle of your readers will read it with great interest.

G. F. WRIGHT.

Oberlin, Ohio, May 23.

PROFESSOR G. FREDERICK WRIGHT, LL.D.

Dear Sir: — YOUR valuable reprint from the *Journal of Science* for January, 1892, on "The Theory of an Inter-Glacial Submergence in England" was duly received this morning, and after a careful perusal of its contents I hasten to thank you for your courtesy in sending it.

The many questions relative to the causes and extent of the great glacial epoch have, with its accompanying phenomena, occupied a large share of my thoughts during the past twelve years, first, because of its surpassing interest and close connection with the solution of some of the most important physical and astronomical problems of the day; and, second, because I had the pleasure of sharing all my husband's ideas and plans and much of his field-work, from the day when he first made your acquaintance at the Boston meeting of the American Association, in August, 1880, to July 17, 1888, when, knowing the precarious nature of the malady which had attacked him, he gave all his unfinished manuscripts into my care, with the request, that, as I knew his inmost wishes in regard to them, I would see that they were all completed and published as they ought to be. The MS. for my husband's "Observations on the Glacial Phenomena of Great Britain"— so ably edited by the Rev. Dr. Crosskey of Birmingham, and covering 1,100 pages of foolscap, has been in Washington since July last awaiting publication. Had it been printed before the paper which you have so kindly sent me was written, I think you would have obtained a slightly different impression of my husband's later views from that expressed in the closing paragraphs.

As the importance of clear definition in scientific work of all kinds can hardly be overestimated, and as my husband's one wish was to learn the truth irrespective of theories, which he regarded merely as tentative hypotheses, to be thrown aside when they no longer served the purpose for which they had been constructed, I think that the term, "Correction of some of Professor Lewis's earlier working hypotheses," would give a clearer impression of the real state of the case than the phrase, "Correction of Professor Lewis's personal equation," which to the world in general implies a constant and known element of error in all that an observer sees or does, and which must be strictly accounted for in the sum-total of his work.

As no one could be more anxious than I am (except my husband himself) that all errors of whatever sort shall be promptly eliminated from his life work, and as I have only too good reason for knowing the endless and varied misconceptions with regard to his views, which have naturally arisen from the fragmentary reports of his European observations that have hitherto been published, I think that it may aid not only yourself but the scientific world generally if I send you a short synopsis of his later opinions. These are briefly as follows: —

With regard to the terminal moraine in Pennsylvania, over the last third of which he enjoyed the great pleasure and advantage of your companionship, his opinion remained unchanged, that a well-defined moraine had throughout the State defined the line of the solid ice-front.

The varying line of bowlders, scattered about as plums over a pudding, found considerably south of the moraine at different points in the western portion of the State, and which you both decided to name "The Fringe," he at first suggested (see Report Z) had been caused by a projection of the upper layers of ice — which move more rapidly than those beneath them — over the lower layers, which, as the ice rose hundreds of feet higher than the moraine at its base, would naturally and in accordance with its proper motion project the bowlders on the surface lying beyond the moraine line.

This view, however, was merely a tentative one, as he himself confessed (see Report Z), and he abandoned it in 1886, as his investigation of the English glacial deposits drew toward a close.

From many similar instances of "fringe" observed in Great Britain, and also in Switzerland and northern Italy, he was thoroughly convinced that the phenomena in each case that he himself examined had been caused by the damming back of streams flowing toward the ice-front and forming bodies of water of varying size and depth, which he called "extra-moraine lakes."

Full details and diagrams relating to his studies of these will be found in the forthcoming volume, and also his application of them to the phenomena observed in western Pennsylvania, where like features occur. The deposit of bowlders over the beds and along the edges of these extra-moraine lakes he held to be largely due to the drifting and melting of detached bergs, or cakes of ice, from the foot of the glacier, in which the *débris* had been frozen, or on whose surfaces the bowlders had been perched.

I do not remember my husband at any time thinking that "the fringe was the remnant of an earlier and distinct glacial period," though in the Old World he found in many places very clear evidence of there having been an advance or retreat, and a second advance of the isolated or coalescing streams, which together gave rise to the phenomena of the great glacial period.

I do, however, recall his frequent statement that never in any of his personal observations in America, Ireland, Great Britain, Switzerland, or Italy had he found a single instance of a glacier, ancient or modern, which had not at the time of its greatest extension been marked by a moraine at the foot of the solid ice, though these moraines often showed the greatest variety of form, from a low, flat deposit of gravel, sand, or till, from a few feet to a mile in width, and from a tiny ridge over which a man could easily step to the gigantic drift hills of northern Italy.

Exceptions to these observations occurred in cases where the ice moved from the land into the sea, as on the south side of the Killarney ice-centre, on the west side of the Clare Mountains, and in other instances, of which he himself has left a full description. The moraine in some portions of western England was much disturbed by the alternate elevation, depression, and re-elevation of that section of the country during the period of maximum glaciation, which caused a mingling and interbedding of morainic and marine deposits. Special stress should here be laid upon my husband's qualifying expression, "in my own experience," for he never at any time denied that a glacier ever had existed, did now exist, or could exist in the future without being bounded by a terminal moraine; he simply said, "I, personally, have been unable to find one."

With regard to your own admirable work in the State of Ohio, and beyond it toward the Mississippi valley, where the ice-front had not been marked by any definable moraine,— owing to its having gradually lost momentum and become very much attenuated in passing over a long, wide, and gently sloping plain till practi-