precipitation of the organic material in the nutritive medium rather than to any direct action upon the living organisms, which, as we have seen, are not killed by a far greater quantity of the reagent.

The conclusions at which Dr. Sternberg arrives, are, that the vital resistance of bacterial organisms to chemical reagents differs, within certain limits, for different species. And certain species show special susceptibility to the germicide action of particular reagents; e.g., the septic micrococcus to alcohol, and B. termo to boracic acid.

There is, therefore, reason for supposing that different pathogenic organisms may differ in like manner, as to susceptibility to the action of various reagents administered medicinally with a view to their destruction. Nevertheless, the comparative germicide value of the reagents tested is the same for the several test-organisms, and, allowing certain limits for specific peculiarities, it is safe to generalize from the experimental data obtained in the practical use of these reagents as disinfectants. But it must be remembered that the resisting power of reproductive spores is far greater than that of bacterial organisms in active growth (multiplication by fission), and the data obtained for the latter cannot be extended to include the former.

The antiseptic value of the reagents tested depends upon their power to prevent the multiplication of putrefactive bacteria; and this is not necessarily connected with germicide potency, for some reagents which fail to kill these micro-organisms are, nevertheless, valuable antiseptics, e.g., ferric sulphate and boracic acid.

Clinical experience has demonstrated the value of all the potent germicide reagents tested in one or more of the diseases which there is the most reason to believe are due to the presence of pathogenic micro-organisms in the *primae viae*, in the blood, or in the tissues; e.g., intermittent-fever, typhoid-fever, dysentery, erysipelas, syphilis, etc. The 'germ-theory' as to the causation of these diseases receives, therefore, very strong support from modern therapeutics; but the experiments do not justify the belief that any one of the reagents tested can be administered as a specific in germ-diseases generally. This also accords with the results of clinical experience, and makes it possible to believe that the specific, self-limited diseases are also 'germ' diseases.

LETTERS TO THE EDITOR.

The practical value of soil-analysis.

IN *Bulletin* lvi. of the New York agricultural experiment-station, Dr. Sturtevant gives the reasons for which the station declines to make soil-analyses 'for the purposes of the individual farmer;' summarizing them in the statement that such ayalyses "can offer no solution of the problem of what fertilizer, and how much, to apply."

Were this statement made in a somewhat less general and absolute manner, I should have no fault to find with it; for in the case of the long-cultivated fields of the state of New York, which have been subject to indefinitely varied culture-conditions and the use of fertilizers, the cases in which chemical analysis alone would point with any degree of certainty to the true cause of failure to produce profitable crops would be exceptional; and the station would be likely to be overrun with requests for an indefinite amount of comparatively useless routine work.

But when Dr. Sturtevant broadly adds his denial "that analyses of soils can give us definite information concerning their productiveness," he seems to go beyond the limits justified by the record, and beyond what the context following would appear to show he intended to say. If the clause above quoted were to read, instead, "while denying that analyses of *cultivated* soils can give us definite information regarding their *present* productiveness," I should agree with him so far as the great majority of cases is concerned, — so much so, that it is only exceptionally that I undertake the analysis of a cultivated soil, but usually go back to its virgin ancestor for information as to its general character; and from this, and the usually simple history of its cultivation, pretty definite inferences as to the prominent wants even of a cultivated soil can in very many cases be deduced, as is proved by the practical results. Dr. Sturtevant's own statement as to the frequency and consequent practical importance of such inquiries would seem to justify the taking of some pains to approach its solution, before proclaiming an absolute *non possumus*.

As for virgin soils, which over wide areas have been subject to uniform or uniformly variable conditions, it is *a priori* reasonably presumable, and I think experience confirms the inference, that, other things being equal, the amount of available plant-food, and therefore the durability of a given soil under the usual culture, without replacement, is sensibly proportional to the plant-food percentages shown by the usual method of analysis. Whether or not other things are really equal can only be ascertained by intelligent examination in the field as well as in the laboratory; and soil-specimens taken by non-experts rarely fulfil this condition.

While, therefore, believing that Dr. Sturtevant's action in this matter is well advised under the circumstances, I nevertheless believe that my contrary practice in regions but sparsely or recently settled is at least equally well justified, and that the importance of affording the settler at least an approximate insight into the present and ultimate durability of his soil, and its general character and adaptations, is so great as to justify a considerable public expenditure, upon a well-considered plan carefully carried out by competent persons both in the field and in the laboratory, even with our present limited knowledge of the chemistry of soils — which, I cannot but remark, is not likely to be increased very rapidly if the composition of soils serving for culture-experiments continues to be ignored, as has so largely been the case heretofore. The prime importance of the presence of a certain minimum percentage of lime, for example, is manifestly so great, that no experimenter can afford to be ignorant of the presence or absence of a proper supply of that substance in his soil; and the cases in which analysis shows the extreme scarcity or extreme abundance of lime, phosphates, or potash, in virgin soils and subsoils, are far more frequent than the contemners of soil-analysis suppose. In the former case the practical value of the indication is too obvious to be overlooked, and is amply attested by the results following the application, e.g., of phosphate fertilizers in such cases. We might not be able to detect the addition thus made to the phosphates of the soil by the most careful analysis; but the fact that the soil is naturally poor in phosphates will remain a fruitful truth forever after.

I trust that the record which will be shown in the census report of cotton production, now in press, will form a convincing illustration of the legitimate uses of soil-analysis. E. W. HILGARD.

University of California, Sept. 1, 1883.

Do humming-birds fly backwards?

The Duke of Argyll, in his Reign of law (p. 145), lays it down in italics, that '*no bird can ever fly backwards.*' He mentions the humming-bird as appearing to do so, but maintains, that, in reality, the bird falls, rather than flies, when, for instance, he comes out of a tubular flower. But this morning, while watching the motions of a humming-bird (Trochilus colubris), it occurred to me to test this *dictum* of the duke; and, unless my eyes were altogether at fault, the bird did actually fly backwards. He was probing one after another the blossoms of a Petunia-bed, and more than once, when the flower happened to be low down, he plainly rose, rather than fell, as he backed out of and away from it. I stood within a yard or two of him, and do not believe that I was deceived.

It may not be amiss to add that the Duke of Argyll's objections seem to be purely theoretical, since the 'Reign of law' was published in 1866, and it was not till 1879 that the author came to America and saw his first living humming-bird.

BRADFORD TORREY.

Boston, Sept. 14, 1883.

Wright's ice-dam at Cincinnati.

I notice on p. 320 of SCIENCE, vol. ii. no. 31, an inaccurate report of what I said at the Minneapolis meeting, which does injustice both to Mr. Wright and to myself, and which I would beg to have corrected.

The reporter makes me speak slightingly of Mr. Wright's discovery of the ice-dam at Cincinnati, as not sufficing to explain our Pennsylvania terraces. On the contrary, I expressed my admiration for the discovery as furnishing precisely the explanation we need for the *local-drift* terraces of the Monongahela, and the *rolled-northern-drift* terraces of the lower Alleghany, Beaver, and upper Ohio rivers.

The reporter probably mixed this up with what I said afterwards respecting the *rolled-drift* terraces of eastern Pennsylvania, which only reach a height of 800′ A. T., in Northumberland county, and require some explanation, perhaps, quite unconnected with that which Mr. Wright certainly furnishes in a most satisfactory manner for the 800′ to 1,100′ A. T. terraces of the Ohio River basin. J. P. LESLEY.

Second geological survey of Pennsylvania,
Philadelphia, Sept. 15, 1883.

Erratic pebbles in the Licking valley.

While engaged in tracing the outcrop of 'Clinton ore' in eastern Kentucky, in the fall of 1882, I became interested in the pebbles, which in certain localities, and up to a certain height, were very abundant in the surface-soil.

Most abundant were rounded quartz pebbles, probably from the millstone grit. Somewhat less abundant were fragments of chert, showing little or no wear derived from the sub-carboniferous limestone. Still less abundant, though by no means rare, were some from the carboniferous, often containing characteristic fossils. They were confined, so far as I could determine, to the valley of the Licking and its larger tributaries. Vertically, they range from the river-bottoms to the top of the table, formed by the upper Silurian rocks, which borders on the Devonian escarpment; so that these tables are quite uniformly covered with the material.

The distribution of the material is such as could only have been made while the valley was temporarily occupied by a lake. I was therefore led, though with some hesitation, to suppose that the glacier must have crossed the Ohio at Cincinnati, damming the river. I was not at the time aware of the labors of Mr. Wright in tracing the glacier across the Ohio.

Having now the certainty that there was a dam at the required point, I think I may have no hesitation in saying, that, during a portion of the glacial period, the valley of the Licking was occupied by a lake which overflowed laterally, and whose bottom became littered with materials brought from the mountains of eastern Kentucky by floating ice. They are most abundant where the ice may be supposed to have had freest access.

Terraces which might have been expected are wanting in the region in which my observations were made. Possibly they may be found in other parts of the valley, especially above; their absence in the region in question being due to the fact that only small portions of the region would have reached above the lake-level, which, by their disintegration, could furnish the material for terraces.

The overflow was probably to southward, but I could not search for it. Could it be traced, the amount of erosion might give some data for an estimate of time. G. H. SQUIER.

Trempealeau, Wis., Sept. 14, 1883.

Depth of ice during the glacial age.

In the issue of SCIENCE for Sept. 7, reporting my paper at Minneapolis, I am made to say, that, during the glacial period, the ice was indeed "600 feet over New England, and very likely of equal depth over the area to the west." I said 6,000 feet over New England. The evidences of glaciation are distinct upon the Green Mountains to a height of nearly 5,000 feet. The lower summits of the White Mountains, like Carrigain (which is 4,300 feet above the sea), are covered with transported bowlders; and there can be little question that some found by Professor Charles Hitchcock, within a few hundred feet of the summit of Mount Washington, were transported thither by glacial agency. Such is the evidence for New England.

For the region north of Pennsylvania and the Ohio River, direct evidence of such a great depth of ice is naturally wanting; but, according to Ramsay, glacial scratches are numerous upon the summit of Catskill Mountains in New York, at an elevation of 2,850 feet above the sea. In southern Ohio there are numerous places where the ice, within a mile or two of its farthest extension, surmounted elevations which are about 500 feet higher than the plains to the north of them. I see no reason why it should not have been as deep over the bed of Lake Erie as over the region to the north of the White Mountains, though there are there no glaciometers like Mount Washington to measure the height of the frozen mass.

G. FREDERICK WRIGHT.

Oberlin, O., Sept. 13, 1883.

The 'stony girdle' of the earth.

In your issue of Sept. 7, just received, you are kind enough to insert a synopsis of the two abstracts of papers which I sent to the Minneapolis meeting. Allow me the space necessary to make a correction and some brief explanations. We are required to furnish these 'abstracts' to suit a printed form of small note size, which is apt to lead to small chirography: hence I suppose the mistake in reading and printing the title.