

valuable series of papers on the widespread occurrence of barium and strontium in the silicate rocks, with methods for the determination of these elements in small amounts. Mr. Hillebrand also deplored the laxity existing in rock analysis and brought forward a strong appeal for greater completeness in the future. The papers are of extreme interest and hardly permit of abstract but can be found in full in the February Journal of the Chemical Society.

THE EXAMINATION OF BEESWAX.

MR. LYMAN F. KEBLER has recently published the results of an investigation of commercial beeswax, his general conclusions being as follows: He finds the beeswax upon the market to be adulterated to the extent of 50 per cent, while in the English markets this adulteration may even reach 66 per cent. The melting point varies from 62°–74°C. It is raised by adding carnaüba wax, stearic acid, certain mineral waxes and paraffin, and is lowered by China wax, Japan wax, cacao butter, resin, tallow, spermaceti, vegetable wax, etc. On the other hand the melting point is apparently unaltered when suint wax and certain of the mineral waxes are used. The specific gravity of the pure wax varies from 0.960 to 0.973, and this appears to be greatly influenced only by resin, carnaüba wax and certain mineral waxes, which raise it, and by paraffin, which lowers it. The "acid number" ranges from 19–21 mg. of potash per gramme of beeswax. Stearic acid resin and suint wax increase, while carnaüba wax, mineral wax, cacao butter, paraffin and spermaceti decrease the acid number. The "ether number" varies from 73–76 mg. of potash per gramme of beeswax and is increased by China wax, Japan wax, cacao butter, tallow and vegetable wax. It is unaffected by addition of carnaüba wax but is lowered by mineral wax, paraffin, resin, stearic acid, etc. It must be noted in this connection that wax bleached by certain chemical agents may have on either number as high as 84 and yet be pure. The percentage of iodine varies from 8–11, but here also certain bleaching agents, such as chlorine, destroy the value of this test. Paraffin, mineral wax and stearic acid lower the percentage, while cacao butter, resin, suint wax and tallow increase it. The volume of hydrogen (53–57.5cc) evolved from one gramme of beeswax and the percentage (12.5–14.5 per cent) of hydrocarbons evidently are the most reliable data securable, the former being vitiated by all adulterants excepting tallow, and the latter by all except suint wax. The author gives a resumé of the best methods employed with many references to the literature.

BUTTER NOSTRUMS.

VARIOUS nostrums for increasing the yield of butter have long been more or less secretly upon the market, and notwithstanding their frequent exposure, they no doubt meet with considerable sale. Professor H. W. Wiley has again called attention to these frauds in Farmers' Bulletin No. 12, United States Department of Agriculture. It is claimed that from one pound of butter, one quart of milk and a little of the magic butter compound, two pounds of butter can be made! Analysis shows this compound to consist of common salt, coloring matter and a little pepsin or rennet. Such a mixture churned with the butter and milk merely coagulates the latter and allows of its incorporation, together with considerable water, into the resulting "butter" mass. Such outrageous adulteration can be easily detected by melting a sample in a test tube and comparing with a similar sample of genuine butter, the latter showing the fat in a clear limpid mass with only a small amount of water and a little curd, while with the former almost half of the whole volume will be a mixture of water, curd, and foreign substance. These compounds are retailed at from \$2.00 to \$2.50 a box of about two ounces.

LETTERS TO THE EDITOR.

* * * Correspondents are requested to be as brief as possible. The writer's name is in all cases required as a proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The Editor will be glad to publish any queries consonant with the character of the journal.

Comment on "A New Thermo-Electric Phenomenon."

AN article has recently appeared in *Science*,¹ entitled "A New Thermo-electric Phenomenon." It is from the pen of W. Huey Steele, and has been copied and abstracted in several journals. Previous to seeing Mr. Steele's article, but not previous to its writing, I had made a few rough experiments along the same lines and had looked up the subject, historically, to see what had been already done.

From what I have been able to find out, in the course of my reading, it seems to me that Mr. Steele's phenomenon is not "new" at all, but is, perhaps, a slightly different phase of an old phenomenon first discovered by Becquerel in 1829.² Becquerel found that in a platinum wire, strained by a spiral and heated at one side of the strained portion, there is an electric current set up, and he thought that this was due to a difference in the propagation or movement of the heat to right and left of the heated portion. However, Magnus and Matteucci have shown that in a homogeneous metal the differences of temperature and of section are not sufficient to produce a current, and Becquerel's phenomenon should be attributed to a difference in the molecular state.

In addition, Sir William Thomson³ has shown that thermo-electric currents were set up between the strained and unstrained portions of a single metal constituting a circuit; the effects in copper and iron being opposite, and the residual effect in each case being the reverse of the effect when the metals are temporarily strained. He also found the relations between hammered and unhammered iron and between brittle and soft iron.

To my mind these experiments are almost identical with those of Mr. Steele; practically no wire can be obtained that has not been through its own history of various strains. (The process of wire-drawing leaves the metal in a permanent state of elongation, and the residual thermo-electric effect, in that case, is the reverse of the effect which is induced by the force applied during the wire drawing.) As for the metals which he completely melted, I think there can be no doubt (from the very fact that "the effects are not always steady; in fact, they very seldom keep steady" and the *direction* of the currents keep shifting) that the melted metal was continually undergoing strains brought about by currents and strata of the metal shearing past one another, or, in other words, by a difference in the molecular state at different times. In addition there is probably, in the case of the melted metals, a large effect due to the thermo-electric action between the melted and solid metal that leads to the galvanometer, those at the opposite ends of the "clay tubes" being in different and continually changing molecular relations.

Becquerel must have obtained a considerable E. M. F. when he heated his platinum wire to a *white heat* and strongly affected a galvanometer of 1829. From these considerations I do not think that Mr. Steele's is a "new" phenomenon, and I think that everybody that reads his article and compares it with those of Becquerel and Thomson (which are far too extensive to even outline in this note) will agree with me.

In the opening lines of his paper Mr. Steele says that it has been "generally known that electric currents may be produced by heating a single metal, if there be any

¹Nov. 10, 1893.

²Becquerel's "Traité d'Elec. et Mag.," Vol. I., p. 155.

³Phil. Trans., 1856, p. 711 et seq.

variation in *temper*, or if the distribution of heat be very irregular and the changes of temperature abrupt." However, this, to me, is only partial and does not touch the root of the matter—a molecular dissymmetry.

Perhaps this comment is totally uncalled for, but as there are already so many apparently different phenomena and "effects," in physics, it is well, when we can, to ascribe all modifications of any effect, that are manifestly due to the same principle, to that effect, instead of setting it down as a *new* phenomenon. W. R. TURNBULL.

Ithaca, N. Y.

Birds Singing on Their Nests.

It is one of the encouraging hopes in natural science that taking anything for granted is giving way to facts. It is but a few years ago when one of our popular ornithologists said that birds were silent on their nests, else it would betray their nests. I had never seen anything in print to the contrary, up to 1886, when I discovered the nest of a white-eyed vireo from hearing the song proceed from one direction. I cautiously followed it, to find the bird on the nest. And here I may say I saw the male and female change places on the nest about noon every fifteen or twenty minutes; the male singing all the time on the nest as heartily as when off it. In February, 1892, I communicated these facts to the *Ornithologist and Oölogist*. It came out in "Bird Notes" in March, 1892, after which several correspondents from widely differing localities testified to hearing other species of vireos and the black-headed grosbeak of California singing on the nest.

In the April number of the *Ornithologist and Oölogist*, 1892, Clyde L. Keller writes: "It seems to be a trait peculiar to that family (vireos). I have observed both cassius and the western warbling vireo singing on their nests." In the next month, May, Mr. S. R. Ingersoll writes: "Let me add both the red-eyed and warbling vireos singing on their nests, especially the latter variety" (speaking of the eastern warbling vireo). This takes in so many of the vireos that it is probable all the family have this peculiarity. One may at first wonder that it was so long undiscovered. I think the solution lies in the fact that all these birds I hear of not being silent on the nests are all birds that have their nests well concealed. I had looked many years in vain for a goldfinch's nest before I could find one, till my ears helped me. Passing by a tree several times a day, I heard the voice of a female in the tree answering the call of a passing male. After searching some time I discovered her sitting on her nest, as well concealed as a goldfinch knows how to do it, so that the risk of betraying the nest is not great, with such birds as we now know to sing on their nests.

HENRY HALES.

Ridgewood, N. Y.

Effects of Weather on Scientific Work.

VERY few persons recognize the sources of error that come directly from atmospheric conditions on experimenters and observers and others. In my own case I have been amazed at the faulty deductions and misconceptions which were made in damp, foggy weather, or on days in which the air was charged with electricity and thunder storms were impending. What seemed clear to me at these times appeared later to be filled with error. An actuary in a large insurance company is obliged to stop work at such times, finding that he makes so many mistakes which he is only conscious of later that his work is useless. In a large factory from ten to twenty per cent less work is brought out on damp days and days of threatening storm. The superintendent in receiving orders to be delivered at a certain time takes this factor into calculation. There is a theory among many persons in the fire

insurance business that in states of depressing atmosphere greater carelessness exists and more fires follow. Engineers of railway locomotives have some curious theories of trouble, accidents and increased dangers in such periods, attributing it to the machinery. These are common illustrations and can be confirmed in the experience of all thoughtful observers. If some one would gather up reliable facts and tabulate them in this field, no doubt some laws of mental activity would be found. In an inquiry among active brain workers in my circle I find a settled conviction that many very powerful forces coming from what is popularly called the weather control the work and its success of each one. The psychology of the weather should be a most pregnant new land for study, and I would be pleased to hear from any one who may have some personal experience on this topic, for the purpose of making some future studies for the readers of *Science*.

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A Freak of Inflorescence.

AMONG several hycinths blooming in the window there is one which reverses the usual order of inflorescence. Its first spike of flowers was normal, that is, indeterminate, but the second began to bloom at the top, its lowest blossoms opening about the same time as the upper ones on the first spike. A spike on another plant began near the middle to open its flowers, and continued the process in both directions.

My observation of "late-blooming trees" has been that generally the second blossoming occurs only when the normal action of the tree has been in some way thwarted. A number of Duchesse pear trees blooming so early as frequently to get touched by frost are almost sure to bear clusters of blossoms the following August. A Siberian crab, in one of its off years, bore in July several clusters of flowers. These were larger than the ordinary flowers, and nearly as double as a Baltimore Belle rose. The summer flowers of the pear trees, on the contrary, are generally not so large, nor so many in a cluster as the spring flowers.

LUCY A. OSBAND.

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Maya Hieroglyphs.—A Correction.

In my first article on the "Interpretation of Maya Hieroglyphs by their Phonetic Elements" (*Science*, Dec. 15, 1893), p. 325, 2d col., 3d line from bottom, for 162 read 102; p. 327, 2d col., 5th line, for 123 read 125; 43d line, for 136 read 128, 129; p. 328, 1st col., 32d line, for mout read mouth; 4th line from bottom, for 84 read 86; 20th line from bottom, for 166 read 167; 21st line from bottom, for 165 read 166; fig. 35 should have been given in the illustration, but was omitted from the drawing sent. It is composed of three squares, similar to those given in fig. 128, and has the phonetic value xa v/s, sha v/s, cha v/s.

H. T. CRESSON.

The Native Calendar of Central America and Mexico.

In *Science*, Feb. 2, and also in the *American Anthropologist* for January, Dr. Cyrus Thomas publishes some observations on the above subject, bearing upon my recent work, "The Native Calendar of Central America and Mexico" (Philadelphia, 1893). As Dr. Thomas is evidently under some misapprehensions as to my statements, I beg to place them in a somewhat clearer light.

In the *Anthropologist* he undertakes to correct some of my quotations from the writings of Dr. Ed. Seler; but from his own words, it is plain that Dr. Thomas is very