polates in others crude statements which render them ridiculous, he does an injustice to the authors to whom he acknowledges his indebtedness, and he shirks responsibility by saying that "these notes do not lay claim to originality." Could anything be more misleading than the following description of sun-spots on p. 148? "They seem to rise suddenly to a great height, cool, and then sink back into the photosphere. They are due to uprushes of incandescent hydrogen, and are identical with the red flames seen during an eclipse." And the figure that accompanies this text cannot be a sun-spot at all, but must be something else inserted by mistake. Another blunder occurs on p. 59, where a section of an intermittent spring is shown upside down. The figures are mostly very coarse and poor, especially the moraines on p. 62, the section through a cinder cone on p. 89, and one of a volcano on p. 90; whilst the two figures of ocean bottoms on pp. 102 and 103 give a very wrong idea of their nature. There is, of course, a deal of information in the book, but no attempt is made to give it interest. In fact, although the author is a teacher of physiography, it is very evident from his work that he has not paid attention to the practical side of his science, or verified any of the phenomena he essays to describe. As a book of reference the work before us is untrustworthy; and as a work for students of elementary physiography it is useless and much to be condemned.

Thomas Sopwith, M.A., C.E., F.R.S.; with Excerpts from his Diary of Fifty-seven Years. By B. Ward Richardson, F.R.S. (London: Longmans, Green, and Co., 1891.)

MR. SOPWITH died in 1879 at the age of seventy-six. He was not eminent as an original scientific investigator, but he was a man of great vigour and freshness of mind, and had won the affection of a wide circle of friends by his genial and happy temper. For many years he resided at Newcastle as an engineer and railway surveyor. Afterwards he removed to Allenheads, where he served as the chief agent of Mr. T. W. Beaumont's lead-mines in Northumberland and Durham. Dr. Richardson's book will recall Mr. Sopwith vividly to the minds of his friends, and it contains many things which will be of interest even to readers who were not personally acquainted with him. During the long period of fifty-seven years he kept a diary regularly; and of this, of course, Dr. Richardson has made liberal use. The extracts show that Mr. Sopwith studied closely the currents of scientific opinion, and formed his own judgment about them in a shrewd and independent spirit.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Electric Transmission of Power.

YOUR article of the 1st inst. on the International Electrical Exhibition (p. 522), says: "In those days (before 1879) two wrong notions misled people—the one, that the maximum efficiency of a perfect electromotor could be only 50 per cent.; the other, quoting the remarks of Sir W. Siemens, 'in order to get the best effect out of a dynamo-electric machine, there should be an external resistance not exceeding the resistance of the wire in the machine.'"

These two notions are really one: the first follows by immediate inference from the second.

Your article says a little further on: "At the British Association in 1879, Prof. Ayrton exposed the fallacy of assuming that 50 per cent. was the maximum efficiency theoretically obtainable from an electromotor. . . . This was perhaps the first time

that it had ever been suggested that the efficiency in electric transmission of power could be more than 50 per cent."

This is a mistake as to historical fact. Many years ago, I am not sure of the date, but it was long before the dynamo was invented, I had some conversation with the late Prof. Joule about mechanical equivalents and motive power, in which he told me that an electromotor (worked, of course, by a voltaic battery) had shown a very high percentage of efficiency—I think he said 79 per cent., and I am sure it was far above 50. I said, "How is that compatible with Ohm's demonstration that the efficiency of an electric circuit is at a maximum when the resistance of the battery is equal to that of the rest of the circuit?" to which he replied, "The maximum effect, in Ohm's theorem, does not mean the maximum work done by the oxidation of a given quantity of zinc, but the maximum effect obtainable from a given surface of zinc plates." "I see," said I, "just as in the case of the steam-engine, the problem of getting the maximum of useful effect from a given weight of coals is a different one from that of getting the maximum of power from a given area of piston."

This appears to be an instance of a truth being grasped by one of the great masters of science long before it passed into general teaching. And it is also an instance of a truth being so mistaken as to mislead: Ohm's law was evidently understood to bear a significance that it did not really bear.

Belfast, October 13.

JOSEPH JOHN MURPHY.

[That Joule had clear and correct views regarding the efficiency of an electromotor driven by a voltaic battery was pointed out some years ago, being mentioned, for example, by Prof. S. P. Thompson in his book on "Dynamo-Electric Machinery." But in the paragraph quoted by Mr. Murphy from NATURE of October 1, the expression "electric transmission of power" had reference to the combination of apparatus exhibited at the lecture in question—had, in fact, the meaning usually attached to this expression, viz. the employment of a dynamo to convert mechanical energy into electric energy at one end of a pair of wires of some length, and the employment of a second dynamo at the other end of the wires to convert the electric energy back again into mechanical energy.

Now, not only would it have been somewhat difficult to foretell what would be the combined efficiency attainable by the employment of two dynamos as generator and motor, at a period "long before the dynamo was invented," but even down to 1879 no one had succeeded in practically transmitting power by means of this combination with an efficiency of as much as 50 per cent. over a distance of even one mile.

The only direct-current dynamo in common use at that date was the series-dynamo, and that machine, as is well known, differs radically in its behaviour from a voltaic battery. For while it is when a voltaic battery is developing a very small current that it gives power most economically to the outside circuit, the series-dynamo, when only a very small current is passing through it, develops practically no electromotive force, no power, and therefore has a very low efficiency. Hence, although electricians were undoubtedly mistaken in fancying that there was a theoretical limit of 50 per cent. in the efficiency when two dynamos were employed in the transmission of power, neither the error, nor its correction, were of that obvious character in 1879 that one might imagine from reading Mr. Murphy's letter.—W. E. A.]

Rain-making.

IN 1883 I published in NATURE (vol. xxviii., p. 83) an account of some experiments which I made to explain the curious phenomenon commonly seen at the Bocca of the Solfatara of Pozzuoli: paper or brushwood is kindled near the fumarole, and the action of the flame, even when its duration has been very brief, is observed for some time after in the relatively great increase of cloudy vapour that appears to roll out of the Bocca and to rise from the surrounding minor fumaroles. According to Prof. Arcangelo Scacchi, this increased condensation of vapour is due to the carbon dioxide produced in the combustion; this gas causing condensation from the highly saturated medium in the same way as fumes become visible when concentrated hydrochloric acid is exposed to ordinary air. My experiments of 1883 tend to show that not only carbon dioxide, but (in accordance with the views of Dr. Aitken on the formation of cloud or mist) the increase of solid corpuscles made to float in

the vapour-laden air inside or near the fumarole, might be the cause of a rapid and continuous condensing of the invisible vapour. I noticed that the "powdering" of the air with any kind of dust increased the cloudy column issuing from the Bocca of the Solfatara. I am therefore led to believe that the action of a paper- or faggot-flame in causing the increase of visible vapour from the Bocca of the Solfatara is due both to the production of carbon dioxide and to the increase of solid particles of soot and of light unburnt fragments made to rise and float in the air.

These experiments may help in explaining the action of explosives in causing a downfall of rain. Not only does the explosion produce a certain amount of carbon dioxide, but dust is widely scattered in the air, and carried upwards by the hot gases produced in the explosion. If the results of the experiments in Texas and Kansas by General Dyrenfurth and Prof. Curtis be confirmed, it would be interesting to see if the condensation of vapour in the atmosphere could be better insured by purposely increasing the quantity of dust produced in each explosion. The effect would perhaps be enhanced if the dust were of a markedly hygroscopical nature: the scattering in high air of very minute particles of calcium chloride should help in the making of cloud and rain. ITALO GIGLIOLI.

Laboratory of Agricultural Chemistry,
Royal Agricultural College, Portici, near Naples,
October 12.

Weather Cycles and Severe Winters.

THE following view of the relations of severe winters is one which I do not remember to have seen stated.

Consider the 79 years 1812-90 (at Greenwich), and let us take, as a measure of winter cold, the mean temperature of the three months December, January, and February. Divide the series of years at 1860; giving a first series of 48 years (1812-59), and a second of 31 years (1860-90).

Now consider the first series. The coldest winter in it is 1813 (meaning, by that, 1813-14). The coldest of the following winters is 1829; the coldest of the following, 1840; then come (reckoning similarly) 1844 and 1846 (equal), 1854, 1859. The absolute order of decreasing severity is to some extent the same, but at certain points the order of time is reversed.

Next take the second series. The coldest winter in this is 1890 (*i.e.* 1890-91); the coldest of those preceding, 1878; the coldest of those preceding, 1870; then come (similarly) 1864 and 1860.

Thus we have a succession of severe winters of decreasing severity, and another, after it, of growing severity.

We may tabulate the data:—

Severe winters with lessening severity.	Mean temperature.	Severe winters with growing severity.	Mean temperature.
1813 ...	31.9	1860 ...	37.4
1829 ...	33.2	1864 ...	37.1
1840 ...	33.9	1870 ...	36.4
1844 ...	34.9	1878 ...	34.6
1846 ...	34.9	1890 ...	34.1
1854 ...	35.6		
1859 ...	37.4		

These data, put into the form of a graphic diagram, give a wave whose crest (mildest of the severe winters) we seem to have passed in the sixties. And it would appear judging by the past, that we have not yet reached the bottom of the hollow; but that after some years' interval we may have a winter even more severe than last, possibly we may have more than one, of growing severity.

It is right to state that, as far as 1856, the values of mean temperature used are those of Mr. Belleville, reduced to sea-level, as given in a paper by Mr. Eaton to the Royal Meteorological Society (Quarterly Journal, January 1888); after that date, those of Greenwich Observatory, published annually. The slight difference in kind does not materially affect the result.

In the *Meteorologische Zeitschrift* for September, M. Woeikoff considers the question whether winters in Russia have been growing warmer, and his examination of the St. Petersburg records, from 1744 to 1890 (noting the number of cold days), leads to an affirmative answer. The number of very cold days has, on the whole, fallen off considerably in the later sixty-three

years compared with the earlier, and in the second half of our century, as compared with the eighteenth and the earlier half of the nineteenth.

This, he finds, corresponds with popular opinion for Northern and Central Russia, according to which intense frosts have become more rare; but in the south, in the Crimea, the Caucasus, and Turkestan, there have been complaints of colder winters of late.

Mr. Glaisher some time ago expressed the view that our winters had been becoming milder. I have seen a criticism of this view, to the effect that the proximity of Greenwich to such a rapidly growing city as London might have to do with such a result. If the facts are as I have suggested above, a growing severity has taken the place of growing mildness, and the criticism referred to would fail to apply. A. B. M.

A Lunar Rainbow.

ON the evening of Saturday, October 17, at about 6.30 p.m., the rare and interesting phenomenon of a lunar rainbow was observed from Patterdale, Westmoreland. On the south-east, the moon, which had just risen, brightened the sky behind the mountains, while on the north-west there hung a uniformly dark and unbroken screen of haze or rain-cloud, which lightened off somewhat and was more scattered on the extreme west. With its highest point lying almost exactly north-west, a semi-circle of pale whitish light was projected against this vapoury curtain. The bow was quite complete, but much brighter and sharper on its northern arc than on that falling south. The brighter portion fell over weird and clear into Glenridding (a favourite haunt of sun-painted rainbows), and as seen striped against the dark hill-sides of that valley, appeared to emit a pale-blue phosphorescent glare. At one time a shred of the dark smoky haze scudded over, but did not completely obscure the highest reaches of the spectral light. The radius appeared smaller than in the case of an ordinary solar rainbow, and the breadth of beam was about one-half thereof, or perhaps rather less. The spectacle having lasted for about eight minutes, light rain began to fall, and then the sky in a very short time became quite clear and star-lit, and all was over.

P. Q. KEEGAN.

Patterdale, Westmoreland, October 17.

The Destruction of Mosquitoes.

THE recent mention of this subject in your pages reminds me that I was told a few years ago by an English gentleman who has a most beautiful place on the Riviera that he had freed his property from this pest.

The property in question is a peninsula, and for that reason is exceptionally open to separate treatment. On the Riviera, as many of your readers will know, fresh water is a somewhat rare commodity, and all of it that the inhabitants can lay hold of is stored for future use in tanks or small receptacles.

The larva of the mosquito lives, as I understand, only in fresh water. Consequently, on the Riviera he is found in the tanks I have named.

The carp is, I am told, passionately fond of the larva of the mosquito, and the Englishman I refer to had extirpated the insect by putting a pair of the fish in every tank.

The plan is not one that could be adopted everywhere, but it is worth bringing under the notice of those whose circumstances are like those of the Riviera. S. A. M.

Law of Tensions.

POSSIBLY many science teachers find some little difficulty in satisfactorily demonstrating to a class the "law of tensions" for vibrating strings. In practice, unless the sonometer is fixed vertically, the error introduced by friction at the pulley (especially with heavy weights) is so great that the real tension is very different from that represented by the weight attached. Even if the apparatus be thus fixed, the changing of the weights occupies time, and a comparison wire is necessary, which must first be tuned to exact unison. The following admirable and very simple method was suggested to me by one of my students, and possibly there are some teachers to whom the idea is new.

Instead of applying tension by attaching weights, the result may be effected much more readily by means of an ordinary spring suspension-balance, such as is often used for weighing