asks how I manage to direct that energy along a definite path so as to produce a willed result. The only answer I know is, "By a nervous impulse liberated from brain centres." But what is it that is thus liberated? and what pulls the trigger to liberate it?

By mechanical analogy one would say that energy can only be guided by force, and that force must therefore be exerted in the brain cells; but, if so, the relation between force, which is a mechanical thing, and will or life, or whatever it is, which is a psychological thing, demands investigation.

I trust that Mr. Lloyd Morgan will help me to get my ideas on these subjects straighter, and will point out if I have made any assertions which are obviously erroneous or grotesque. The borderland of psychology and physics is the last place in which I would like to dogmatize; and in a letter like this I see no harm in airing confessedly immature and groping notions, in the hope that ventilation may clear the air. So far as physics only is concerned, I have stated how I regard the phrase "expenditure of energy" in the *Philosophical Magazine* for June 1885.

With regard to the crux raised in Mr. Dixon's last paragraph, that nothing but matter can exert force, because the acting matter must receive an equal opposite momentum, it may perhaps be just worth noticing that an infinite mass can absorb any amount of momentum without receiving a trace of energy or being itself in any way affected.

OLIVER J. LODGE.

Liquid Prisms.

I OBSERVE in NATURE of July 2 (p. 207), that it is stated Herr Wolter has recently recommended α -monobromnaphthalene as a substance peculiarly fitted for study of the ultra-violet part of the spectrum, by reason of its high dispersive power and transparency for the ultra-violet rays.

Perhaps I may be permitted to state that Mr. Madan published an account of its dispersion and refractive power in the *Phil. Mag.*, and recommended its use in liquid prisms. Having made use of many other substances, including methyl salicylate, I gave this a trial. For ordinary work it would be excellent if colourless; but unfortunately, no matter how free from colour it may be when freshly prepared, long-continued use causes it to become yellow, and in considerable thicknesses even dark brown. For the ultra-violet rays it is undoubtedly better than carbon disulphide, but nevertheless practically useless, as the line N, which it is said to transmit, has a wave-length of 3580, so that only about one-half of the ultra-violet solar rays are observable with it. In metallic spectra almost all lines of interest lie between 3580 and 2000. A liquid which I considered to possess much superior optical properties is *mercuric methide*; it is perfectly colourless, and of such density that flint glass will float upon it. When the glass is immersed it becomes invisible, consequently the refraction and dispersion of the liquid are probably exceptionally high. As far as I can recollect, being without access to my notes, a thickness of 50 millimetres freely transmitted all rays to about λ 2900—that is to say, the entire solar spectrum. Unfortunately, it has its drawbacks, in being somewhat volatile, and its vapour highly poisonous.

Stonehaven, N.B.

W. N. HARTLEY.

The Identification of Templeton's British Earthworms.

BETWEEN the years 1829 and 1836 the first series of Loudon's *Magazine of Natural History* appeared in nine volumes. In the last volume we find some notes on earthworms by Templeton, which have proved somewhat puzzling to students of more recent times. I have been fortunate enough to follow Templeton in some of his researches, and am able to correct and verify certain of his statements.

The *Lumbricus xanthurus*, Temp. (*op. cit.*, ix. 235), is the angler's gilt tail, and as such is synonymous with *Lumbricus puter*, Hoffm., and *Dendrobana Boeckii*, Eisen. *Lumbricus gordianus*, Temp. (*loc. cit.*), is undoubtedly the mucous worm (*Allolobophora mucosa*, Eisen), or one of its near allies, all of which are to be found of a pale rosy colour coiled up into a knot at certain times of the year.

It is to *Lumbricus omilurus* (= *Omilurus rubescens*, Temp., *loc. cit.*), however, that I wish to direct special attention. Grube, in 1851 ("Familien der Anneliden," p. 101), placed it, with Templeton's other worms, in a list of species which were insufficiently characterized for systematic purposes. Vejdovsky, in 1884 ("System und Morph. der Oligochæten," p. 62), places it among

the questionable species without note or comment, and, so far as I can gather, no one has been able to throw light upon it since.

Templeton says the worm is never larger than half the size of *L. terrestris*, L., is of a bright reddish-brown, with the tail very flat, and the body unfurnished with a belt at the position of the sexual organs. It would be very easy to suppose from this somewhat vague account that the writer had only seen immature specimens; but a little careful study of his words shows that he knew what he was writing, and that his worms were mature. Now a mature species of *Lumbricus* without a clitellum is certainly an anomaly, and needs investigation.

While collecting Annelids recently, I came across half-a-dozen specimens which at first sight exactly resembled *Lumbricus rubellus*, Hoffm. I took them home for verification, and immediately observed the difference. I had obtained with them typical specimens of *rubellus*, which enabled me to make a careful comparison of the two species in a living state.

The following is a description of the worm as I wrote it down before observing Templeton's account.

Colour dark brown, iridescent on the dorsal surface anteriorly, becoming lighter towards the posterior extremity, which is flesh-coloured or light red, pink ventrally. Prostomium dovetailing completely into the peristomium, and possessing a transverse groove in the middle, as shown in the accompanying sketch.

Lumbricus rubescens. Segments 1 to 3 with prostomium entirely cutting the first segment or peristomium.

Segments not annulated (or divided by transverse rings). Length about 3 inches, total number of segments about 120. Setæ in couples as in typical *Lumbricus*. Male or spermiducal pores on segment 15 with papillæ, which, however, do not extend over the neighbouring segments. Body cylindrical in front, flattened posteriorly. The dorsal pore between 5 and 6. It appeared at first between 7 and 8, but by using polarized light on the cuticle when spread on a glass slip the whole series of pores in one or two specimens became clearly visible from the fifth segment backwards.

On the ventral surface prominent papillæ appeared on segments 28 and 29, such as are often seen on typical *L. agricola*, Hoffm. Now came the crucial question, Is there no clitellum? By studying all the examples carefully, I found that they agreed in one particular. The segments 34 to 39 differed in structure from the rest on the dorsal surface. On the under surface from 33 to 40 were differentiated, and showed a glandular structure, while the band representing the tubercula pubertatis extended distinctly along the ventral surface of 35, 36, 37, 38.

This description of the external characters shows the worm to be a decided *Lumbricus*, tested by Dr. Benham's definition in "An Attempt to Classify Earthworms"; but it differs from every one of our British species, especially in the backward position and inconspicuous nature of the clitellum. I am unable to refer it definitely to any of the European species, and propose that for the present it should be known as *Lumbricus rubescens* (Temp.), thus retaining the two names from Templeton's synonyms which are most appropriate to what I regard as the species intended by him.

I may add that I have recently found one or two other earthworms in Yorkshire which have not yet been recorded as British, and will form interesting additions to our Annelid fauna.

Idle, near Bradford, July 15.

HILDERIC FRIEND.

Copepoda as an Article of Food.

DURING recent years a good deal has been said amongst marine zoologists of the use, as a food supply, that might be made of the enormous numbers of Copepoda that swarm in the surface-waters of the sea, and the Prince of Monaco has pointed out the value this widely-distributed nutritious matter might have to shipwrecked sailors; but I am not aware that anyone has yet actually made the experiment of cooking and eating Copepoda, so the following record may be of some interest.

While townetting during the last few days about the North Cape, we have had some large hauls of Copepoda; and it occurred to us last night, while watching the midnight sun off the entrance to the Lyngen Fjord, that one gathering might be spared from the preserving bottle and devoted to the saucepan. We put out one of the smaller townets ($3\frac{1}{2}$ feet long, mouth 1 foot in diameter) from 11.40 p.m. to midnight, the ship going dead slow, and traversing in all, say, a mile and a half during the 20 minutes. The net when hauled in contained about three tablespoonfuls of a large red Copepod (*Calanus finmarchicus*, I think), apparently a pure gathering—what Haeckel would call a monotonous plankton. We conveyed our material at once to the galley, washed it in a fine colander, boiled it for a few minutes with butter, salt, and pepper, poured it into a dish, covered it with a thin layer of melted butter, set it in ice to cool and stiffen, had it this morning for breakfast on thin bread-and-butter, and found it most excellent. The taste is less pronounced than that of shrimps, and has more the flavour of lobster. Our 20 minutes' haul of the small net through a mile or two of sea made, when cooked in butter, a dishful which was shared by eight people, and would probably have formed, with biscuit or bread, a nourishing meal for one person. It would apparently, in these seas, be easy to gather very large quantities, which might be preserved in tins or dishes, like potted shrimps.

W. A. HERDMAN.

S.V. *Argo*, Tromsø, Norway, July 13.

Are Seedlings of *Hemerocallis fulva* specially Variable?

I SHALL be grateful to any of your readers who will write and let me know their experiences as to the variability of seedlings of *Hemerocallis fulva*, or who will raise it from seed in fair quantity, and kindly communicate to me their results, which shall be duly acknowledged.

My reason is this: there is in the formation of the pollen in this plant a peculiarity which, according to Weismann's views, should lead to exceptional variability in the seedlings; but, so far as I know, we have no evidence on the subject.

MARCUS M. HARTOG.

Royal University, Dublin, July 9.

The Green Sandpiper.

ON Sunday last, July 12, I saw flying round a large pool in Essex, a specimen of the *green sandpiper*. It flew leisurely round the pool, and seemed as if it were not far from its summer home. I think, therefore, that the bird must be nesting in the county, and probably in the neighbourhood.

Can any of your correspondents inform me whether the nest has been found anywhere, in recent years, in England?

ARGYLL.

Argyll Lodge, Kensington, July 17.

LIQUIDS AND GASES.¹

ALMOST exactly twenty years ago, on June 2, 1871, Dr. Andrews, of Belfast, delivered a lecture to the members of the Royal Institution in this hall, on "The Continuity of the Gaseous and the Liquid States of Matter." He showed in that lecture an experiment which I had best describe in his own words:—

"Take, for example, a given volume of carbonic acid at 50° C., or at a higher temperature, and expose it to increasing pressure till 150 atmospheres have been reached. In the process, its volume will steadily diminish as the pressure augments; and no sudden diminution of volume, without the application of external pressure, will occur at any stage of it. When the full pressure has been applied, let the temperature be allowed to fall, until the carbonic acid has reached the ordinary temperature of the atmosphere. During the whole of this operation, no break of continuity has occurred. It begins with a gas, and by a series of gradual changes, presenting nowhere any abrupt alteration of volume, or sudden evolution of heat, it ends with a liquid.

¹ Lecture delivered by Prof. W. Ramsay, F.R.S., at the Royal Institution, on Friday, May 8.

"For convenience, the process has been divided into two stages—the compression of the carbonic acid, and its subsequent cooling. But these operations might have been performed simultaneously, if care were taken so to arrange the application of the pressure and the rate of cooling, that the pressure should not be less than 76 atmospheres when the carbonic acid had cooled to 31°."

I am able, through the kindness of Dr. Letts, Dr. Andrews' successor at Belfast, to show you this experiment, with the identical piece of apparatus used on the occasion of the lecture twenty years ago.

I must ask you to spend some time to-night in considering this remarkable behaviour; and, in order to obtain a correct idea of what occurs, it is well to begin with a study of gases, not, as in the case you have just seen, exposed to high pressures, but under pressures not differing greatly from that of the atmosphere, and at temperatures which can be exactly regulated and measured. To many here to-night, such a study is unnecessary, owing to its familiarity; but I will ask such of my audience to excuse me, in order that I may tell my story from the beginning.

Generally speaking, a gas, when compressed, decreases in volume to an amount equal to that by which its pressure is raised, provided its temperature be kept constant. This was discovered by Robert Boyle in 1660; in 1661 he presented to the Royal Society a Latin translation of his book, "Touching the Spring of the Air and its Effects." His words are:—

"'Tis evident, that as common air, when reduced to half its natural extent, obtained a spring about twice as forcible as it had before; so the air, being thus compressed, being further crowded into half this narrow room, obtained a spring as strong again as that it last had, and consequently four times as strong as that of common air."

To illustrate this, and to show how such relations may be expressed by a curve, I will ask your attention to this model. We have a piston, fitting a long horizontal glass tube. It confines air under the pressure of the atmosphere—that is, some 15 pounds on each square inch of area of the piston. The pressure is supposed to be registered by the height of the liquid in the vertical tube. On increasing the volume of the air, so as to double it, the pressure is decreased to half its original amount. On decreasing the volume to half its original amount, the pressure is doubled. On again halving, the pressure is again doubled. Thus you see a curve may be traced, in which the relation of volume to pressure is exhibited. Such a curve, it may be remarked incidentally, is termed an hyperbola.

We can repeat Boyle's experiment by pouring mercury into the open limb of this tube containing a measured amount of air; on causing the level of the mercury in the open limb to stand 30 inches (that is, the height of the barometer) higher in the open limb than the closed limb, the pressure of the atmosphere is doubled, and the volume is halved. And on trebling the pressure of the atmosphere the volume is reduced to one-third of its original amount; and, on adding other 30 inches of mercury, the volume of the air is now one-quarter of that which it originally occupied.

It must be remembered that here the temperature is kept constant; that it is the temperature of the surrounding atmosphere.

Let us next examine the behaviour of a gas when its temperature is altered, when it becomes hotter. This tube contains a gas—air—confined at atmospheric pressure by mercury, in a tube surrounded by a jacket or mantle of glass, and the vapour of boiling water can be blown into the space between the mantle and the tube containing the air, so as to heat the tube to 100°, the temperature of the steam. The temperature of the room is 17° C., and the gas occupies 290 divisions of the scale. On blowing in steam, the gas expands, and on again equalizing pressure, it