"E. L. G." AND THE PROOF OF THE DELUGE.

[4290.]—I too would wish heartily to thank "E. L. G." for his excellent letters on this subject, though I feel my doing so may expose me to our "F. R. A. S.'s" sneer, and to our friend "Sigma's" scorn. But I am one of those old-fashioned people who have not yet been so far "educated" as to have come to regard the Deluge as a "myth," a "grand old legend," and its recorder, though "learned in all the knowledge of the Egyptians," as no more than "a semi-barbarous Hebrew." But, then, I am only an Irishman, trained in the Irish University of good Queen Bess, where the professors, as a rule, do not swallow all Lyell. This may be my misfortune; but, be that as it may, I have not given up "the tales of my childhood," and though I do not go with everything

"E. L. G." says, I think there is a great deal in what he has brought forward which no one has as yet proved false, or shown to be less likely than what they would themselves substitute.

"E. L. G." is right well able to cope with either "Sigma" or "F R. A. S.," who, I regret to think, consider all who differ from their dicta as regards a flood 5000 years ago to be "playing deliberately into the hands of the infidel and scoffer." I cannot see how this need be, and, therefore, I venture to point out one or two weak points in "Sigma's" answers, as it appears to me, and trust he may reconsider what he has written, or found his objections on something else.

In "Sigma's" letter (4086, p. 196), he finds objection to "E. L. G.'s" illustration of the effect of rain in forming "furrow vales" in the earth, and says that its effect on a heap of clay is not at all applicable to the form, past or present, of the earth's surface—for that if any thing is certain it is this, "that the original condition of the earth was not that of a mass of clay, mud, or sand," that "clay, mud, and sand are the products of the wearing away of old solid rocks, and are again passing into solid rock."

Now, from this one would suppose that "E. L. G.'s" theory was based on the supposition that at the time of the flood which he contends for there were no rocks, that the earth was only a lump of clay, sand, and gravel. Starting, then, with this unfounded supposition, which he seems to wish to father on "E. L. G.," he proceeds to smash it to bits, and tells us that the effect of "E. L. G.'s," comet on this earth (which he "Sigma" has made for himself) "would be to reduce it to a true spheroid of rotation covered with water over its whole surface."

Now I have read very carefully all that "E. L. G." has written on this subject, and it seems to me he has never once given ground for such an unfounded supposition to be attributed to him, and has in more than one place stated that it is the "drift gravel," or "boulder clay" (let. 4157, p. 226), the then loose earth or disintegrated rock, which he believed the Deluge had formed and alone could form into "sweep vales."

A flood could not in a few days or hours smooth down a hard jagged rock, and make it into a gentle sweep, but it might gather around it and over it a mass of mud, sand, and gravel, and thus form a gentle slope. If any one doubts this let him walk along the bank of a river where there has lately been a flood, and he will see so many instances of its effects in forming of mud, sand, and gravel, sweep vales and sloping hills in miniature, that I will venture to say he will not regard a flood which once covered the whole earth at the same time as a mythical event, but rather as the most natural way of explaining the numberless facts he cannot shut his eyes to.

"Sigma" seems to think it a more rational explanation to believe that there were a number of local floods—which to produce the effects must have been greater than any recorded in profane history or in the memory or experience of later times, and these great local floods "constantly happening" (let. 4086, p. 196)—than to believe possible the one great flood recorded in a book which some still regard as divine, and of which one flood there is traditional record more or less distinct found amongst all nations even the most barbarous, and not amongst the merely "semi-barbarous Hebrews."

"Sigma" (let. 4193, p. 252) says a mass of water falling over the whole surface would do the reverse of what "E. L. G." states—*i.e.*, the sinking of the highlands, and rising of the sea beds, or as records call it, the breaking up of all "the fountains or foundations of the abyss."

Does "Sigma" forget the little but important fact, that the general surface of the land is somewhat above the sea level, and therefore, though the water might run into the sea, it could only do so if the land did not sink under the weight of water, which, because of the difference of level would first fall on it? The question, then, is, would there not have accumulated on the land and into the hollows a weight of water sufficient to entirely overturn the balance before a single drop had fallen on the sea. I think there is no doubt "E. L. G.'s" comet would effect what he contends for, and not the reverse.

Now, does "Sigma" think that he is arguing honestly for the truth when he says "the 10ft. on the area of the land would be balanced by the 10ft. per minute on the sea"? Is he so poor a mathematician as to think that the water will run as quickly off an inclined plane, and at the same time do the work of transporting mud, sand, and gravel, uprooting trees, shrubs, and everything, and carrying them along a winding course, and yet only take the same time as it takes to fall through the height of the inclined plane—*i.e.*, in other words, does he believe that there would be accumulated on the land no more than 10ft. water, by the same time there was 10ft. on the sea? How else could the 10ft. on sea balance the 10ft. on land? It would have to balance many times 10ft. on the land.

"Sigma" has only to look again at his reasoning in letter 4193, and he will see he is all wrong, and that a mass of water falling over the whole surface would certainly increase the pressure on the land more than it would on the sea, and first on the land.

Let us take "E. L. G.'s" rainfall of 10ft. per minute; there are, I imagine, many inland plains or valleys, call them what you like, and of large extent, whose level is some thousand feet above the sea. Now the rain which fell on the highest mountains would run down into these valleys and then form lakes, and thus there might be concentrated at one point on the land a pressure many hundred times greater than it had to bear but a minute before, and this might and would in certain places be the case before a drop of rain had fallen on the sea. Of course, till the land began to sink, the water would tend to run off; but still it would be retarded sufficiently to form the elevated lakes I have spoken of. But further, even let us suppose that the rain began to fall on the sea at the same time as on the land, or at a period of time so shortly after as to be inappreciable; and so if it were possible to have 10ft. on sea balancing the 10ft. on land, it would not have the effect "Sigma" supposes, for 10ft. on the sea surface would be equal to the pressure of a column of water 10ft. high distributed equally over all the bottom of the sea, and this it could bear without any sinking. Now, in the second minute, 10ft. more would be added to the pressure on sea's bottom, but it would be equally distributed—it would not be 20ft. at one place and 60ft. or 100ft. at another; it would be 20ft. plus whatever in the second minute might have run down off the land, which would not be more than a foot over the whole sea, if it would be so much. But what state of things would obtain on the land at end of the same second minute? In many places there would be an increased pressure of many hundred times greater than it had to bear but a moment before, and under this it might begin to yield, and if it once began it would take some time to stop it and set it going in the opposite direction. "Sigma" may know this from his chemical balance—if he puts too much in one scale and it begins to descend, a greater weight in the opposite scale will not at once make it commence to rise again. Thus the land would first sink, and could only do so by raising the bottom of the sea, whose waters would rush in on the lands, and thus help to flood and sink them. DERF ERRAC.

P.S.—I trust "E. L. G." will give us some more proofs. He has many a one who sympathises with him in his desire, in this and other matters, to have the truth known and acted on. Let him manfully, yet humbly, fight, and the victory will not be uncertain, though, perhaps, delayed. If he were not so well able to hold his own against all comers, more friends would speak out. Till help is wanted, it is oftentimes better withheld. D. E.

THE DELUGE.

[4291.]—WHILE endeavouring to avoid infringing the law laid down for the exclusion of theological discussions (a law which, however, I much regret, as I long to break a lance with several of your correspondents), I wish to address a few words to "E. L. G." on the subject of his recent letters on the Deluge. Why does he give himself the trouble of trying to explain such an impossible phenomenon as a universal Deluge, deep enough to submerge the hills all over the earth, when there is at hand a far easier solution of the question? There had not been time enough since the Creation for men to have spread themselves very far from their first dwelling place, and, in all probability, their wanderings were confined to the level valleys, where the existence of themselves and their herds could be maintained with little or no exertion; therefore, what is more natural than to suppose that the Deluge, which was really universal as regards man and his possessions, should have been only partial as regards the whole earth? Provided that it accomplished the destruction of every human being, this awful instrument of the Divine wrath had done its work, and further ravage was unnecessary. God's wisdom in adapting means to an end will not allow us to suppose that in order to destroy man, He drowned the whole earth, where man had never yet come. We may, therefore, conclude, until it is proved to the contrary, that the Deluge was not universal, but partial, and confined to those regions inhabited by man.

Apologising for having occupied so much of your valuable space with these notions of mine, I leave "E. L. G." to explain to us what became of the water from his comet, and when that is satisfactorily done, how specimens of all the creatures of the earth could have been got into the Ark and preserved from destruction. If he cannot do this his theory falls to the ground. VERTUMNUS.

MORE PROOFS OF THE DELUGE.—II.

[4292.]—"As lakes and river systems," says Darwin, in his famous work on "Natural Selection," chap. xii., "as lakes and river systems are separated from each other by barriers of land, it might have been thought that fresh-water productions would not have ranged widely within the same country; and, as the sea is apparently a still more formidable barrier, that they would never have extended to distant countries." Certainly that seems, on any Lyellist view of Geology, very obvious. Of the three mansions of terrestrial creatures, land, sea, and fresh-water, the second alone has free connection round the globe, or over most of its face, having but a few detached outliers, as the Caspian and minor salt lakes. The first, the land, is mainly in two masses, but neither exceeding its half, and the outliers (of which our Britain is only about the twelfth largest) vastly more numerous than those of salt water. But the fresh-water mansion, widely different in plan from either, is in countless thousands of separate chambers, the largest barely holding a fraction of a tithe of it, and all of them as separate and permanently inaccessible now to their inhabitants, each from each, even when as near as the Thames and Medway, as are the earth and moon. Assuredly, few things could be plainer than that as long as "causes now in action" were uninterrupted, the mansion most uniformly stocked must be the sea; next to it the continents, and possibly some or many islands to which their land creatures and plants (or eggs and seeds of them) might be occasionally carried on driftwood, by birds, or even on ice; but that the fresh water, in its myriad isolated systems, each locked up in its own valley, must have the diverse, localised and peculiar stocks. Now hear Darwin proceed:—"But the case is exactly the reverse. (!!!) Not only have many fresh-water species, belonging to quite different classes, an enormous range, but allied species prevail in a remarkable manner throughout the world. I well remember, when first collecting in the fresh waters of Brazil, feeling much surprised at the *similarity* of the fresh-water insects, shells, &c., and at the *dissimilarity* of the surrounding terrestrial beings, compared with those of Britain."

"With respect to plants," he adds, "it has long been known what enormous ranges many fresh water and even marsh species have, both over continents and the most remote oceanic islands. This is strikingly shown, as remarked by Alph. de Candolle, in large groups of terrestrial plants, which have only a very few aquatic members; for these latter seem immediately to acquire, as if in consequence, a very wide range." That is, in such a group or family the majority of the members, those of *dry* habitat, with their free run of all their continent's uplands and connected frame, these are all *localised*; but the *fewer* branches of the family that need marsh or even constant water, and so are locked in by hills, each to its particular vale or basin, "these latter seem immediately to acquire, as if in consequence (!), a very wide range," or "enormous ranges, both over continents and the most remote oceanic islands"!

Now, surely, if "the distribution of living beings" is to prove anything at all (as "Santalinus" and your other correspondents tell us it must, though I have demolished any connection between their facts and their dogma about deluges), here is a startling world-wide class of facts, according to Darwin and De Candolle, than whom I know of no higher "authorities" on this distribution, that must go to prove *something!* The trouble taken by Darwin, as detailed in the rest of that chapter, is truly worth reading. Nothing less could impress on minds like our "F.R.A.S." M. Paris, "Osa," &c., the transcendent irreconcileableness of the above general facts with his baseless Lyellian dogma. All these elaborate experiments with duck's feet, &c., to prove what? Only barely to make out a *possibility* of some *occasional* transfer of fresh-water seeds or ova from one country to another over sea, or over hills (though the latter, indeed, the commoner problem, is not solved at all)—a bare chance of *some* fresh-water organisms happening now and then to obtain diffusions approaching or distantly imitating what is general to most of the sea and dry land ones. This is the utmost the experiments establish (if so much), when, by Darwin's own account, as you see above, the thing to be explained is not a diffusion of *some*, nay, not even of all or the generality of fresh water species, partly, or even quite *as well* as most sea or land species; but a diffusion of them as a *general rule, wider* and over *more* "enormous ranges" than the sea and land ones!

In short, if such things are evidence at all, while there is absolutely *no* evidence of all or the generality of *land* having ever, in any geologic period, been connected, or even the whole of the salt waters having been so, it is quite otherwise with the fresh. There is just *all* the evidence the nature of the case would seem to admit of, that the *fresh* waters have been connected, and this in quite the modern geologic period, that of the existing (not extinct) plants and animals. No shadow of evidence, indeed, for any of the many "partial deluges" of "F. R. A. S.," and others—(can none of the geologers, by the way, tell us the limits of even the very last? I do not even ask for the last but one of these deluges)—no single fact for any one of these uncaused and, as far as they have shown, sheerly *miraculous* deluges, baseless myths of their imaginations; but, on the other hand, *all* the evidence that could well be imagined (consistently with known law) that once, in recent ages, a layer of fresh water has covered both our continents and oceans at once.

And so, if it be known, as M. Paris tells us, p. 256 (but it is still unknown to me), that mere freshets of Indian rivers kill "millions of sea fish," then, probably, their mourner has to mourn the death, at that deluge (the sole traceable one), of still more of his marine friends; unless there be reasons (as there may be plenty neither known nor knowable) for a vertical accession of fresh water killing fewer than a lateral. But whether more or fewer, billions or tens, I fail to trace any connection of their deaths with our argument.

Now, being in this twelfth chapter of Darwin, we may as well quote his other grand puzzle, that of the beings on "oceanic islands." First, he tells us, "The species of all kinds which inhabit oceanic islands are few in number compared with those on equal continental areas. Alph. de Candolle admits this for plants, and Wollaston for insects." Of this he proceeds to give very striking examples; all New Zealand, for instance (equal in extent and climates to Italy and Sicily together), having fewer species than a bit of most continents of a hundredth its size and but one uniform climate—nay, than a civilised isle as small and uniform as Anglesea! This *fewness* of species (in either kingdom) on each single pelagic island, and, moreover, their *separateness* on islands even within sight of each other (which astonishes Lyell in the case of Madeira and Porto Santo, two very ancient isles, long, long ante-Noachian)—these two facts, especially coupled, as he says, with great *similarity* in their fossil extinct species, are two quite general facts, for which neither he nor any of his school have suggested the slightest explanation; indeed, have tacitly quite given it up; though, of course, there *is* a theory, that of the "semi-barbarous Hebrews," that so obviously fits pat into both, that it would be an insult to the common sense of your readers to give its explanation of either.

The same, of course, applies to the following of Darwin's facts—"Oceanic islands are sometimes deficient in animals of certain whole classes. . . All such differences in number, and the absence of certain whole groups of animals and plants on islands are generally accounted for" (*i.e.*, said in other books to be so) "by supposed differences in their physical conditions; but this explanation is not a little doubtful. With respect to the absence of whole orders of animals on oceanic islands, Bory de St. Vincent long ago remarked that Batrachians (frogs, toads, newts) are never found on any of the many islands with which the great oceans are studded. . . . This general absence of frogs, toads, and newts, on so many oceanic islands cannot be accounted for by their physical conditions. Indeed, it seems that islands are peculiarly well fitted for these animals; for frogs have been introduced into Madeira, the Azores, and Mauritius, and have so multiplied as to become a nuisance. . . . But why, on the theory of creation, they should not have been created there, it would be difficult to explain." Certainly, I agree with the eminent naturalist, very difficult indeed! especially as it seems plenty of fossil extinct species of such absent orders *did* exist even on islands!

"Mammals," he proceeds, "offer another and a similar case. I have carefully searched the oldest voyages, and as yet I have not found a single instance free from doubt, of a terrestrial mammal (excluding domesticated animals kept by the natives) inhabiting an island situated above 300 miles from a continent or great continental island; and many islands situated at a much less distance are equally barren. . . Yet it cannot be said that small islands will not support at least small mammals, for they occur in many parts of the world on very small islands, when lying close to a continent; and hardly an island can be named on which our smaller quadrupeds have not become naturalised and greatly multiplied. It cannot be said on the ordinary view of creation" (what on earth is the ordinary view of creation?—I am at a loss to conjecture) "that there has not been time for the creation of mammals; many volcanic islands are sufficiently ancient." (!!!) What precise antiquity this requires, I cannot find the learned creationist to have anywhere told us. According to Milton, the only other authority at hand, not a great period:—

> The grassy clods then calved; then half appear'd
> The tawny lion, pawing to get free
> His hinder parts.

But Darwin proceeds: "Although *terrestrial* mammals do not occur on oceanic islands, *aërial* mammals *do* occur on almost every island. Why, it may be asked, has the creative force produced bats, and *no other* mammals on remote islands? On my view this can be easily answered, for no terrestrial mammal can be transported across a wide space of sea, but bats can fly across." Certainly, that is a ready explanation on *one* view, but I was not aware Darwin's view was the one to yield it. "No terrestrial mammal can be transported across a wide space of sea"! But what need, O Darwin, on *your* view, to be *transported* to the island any more than bats? Why not be *developed* there? Since *you* say, "it cannot be said there has not been time," and "many volcanic islands are sufficiently ancient, as shown by the stupendous degradation which they have suffered, and by their tertiary strata." You explain indeed "easily" (and by nothing peculiarly Darwinian) the presence of the bats, but not the larger fact, the absence of all the other mammals!

But we have not exhausted, we have not yet come to the climax of, Darwin's diluvian proofs. "He who admits the doctrine of the creation of each separate species will have to admit that a sufficient number of the best adapted plants and animals have not been [later edition, "were not"] created on oceanic islands, for man has unintentionally stocked them far more fully and perfectly than has Nature" (later edition, "than did Nature"). Assuredly! Then you see, Nature, or Creation, or natural Selection (which ever of the three terms you prefer), if acting in regard to islands, has been a stupendous blunderer indeed! Widely different from what it has done on continents, and in seas, and even the now infinitely separated bits of fresh water, more numerous and even smaller than islands on the whole! Just observe! In one or two centuries only, our ships merely happen, by blind chance, to carry into most islands, plants or animals so vastly fitter to their physical conditions than the native species that have been there, according to Darwin's present faith—faith in the Lyellian priesthood—for thousands of centuries at least, as to starve out, crowd out, and exterminate, in a single century, these ancient, million-year-settled possessors of the soil! Oh, blundering natural process! With all this unlimited bank of "Time, time, time" (as Scrope hath it), to draw upon, natural selection is unable (according to the very prophet thereof), unable in all these million ages, either to develop *in*, or get *into* these islands, a stock half so fitted to them as man unintentionally (*i.e.*, man's ships, by pure chance) happens to introduce in one short century!

And this same process observe, infallible, as all admit, in stocking regions where it once *operates*, even for a generation or two! A crux indeed for Darwin, under Lyellian bonds, all this insular misdistribution! But taken in connection with all else, with the uniform age of deltas, and waterfalls, and peat-bogs, the universal diluviation, the flood-scoured surface, the boulder-drift, swept and lodged in every isle and every river-vale alike, the iceberg-dropped trains of "grey wethers," and other erratic blocks—what can be plainer? Between land and land, *no* such cause as natural selection, no *regular* or *continuing* cause, *has operated* at all in the present distribution of life. It is found, as regards islands, to be mere *mis*-distribution; therefore, the work of nothing regular or long continued, but of sheer *accident*, the sudden accidents of some quick, sharp, sudden catastrophe.

E. L. G.

A BATCH FROM MR. BOTTONE.

[4296.]—(4265.)—SULPHUR AS A BLEACHING AGENT.—Sulphurous acid and its congeners are powerful reducing agents, but they require certain favourable circumstances to bring their deoxidising powers into play. Sulphurous anhydride *cannot* of itself absorb oxygen from the air, but in contact with steam and an oxide of nitrogen it does so readily. A strong solution of sulphurous acid in water is tolerably permanent, *if air be excluded*, but after the lapse of some weeks part of the water is decomposed, hydrogen is set free, and sulphuric acid is formed, thus:—

$$H_2SO_3 + H_2O = H_2SO_4 + H_2.$$

The presence of organic matter seems particularly favourable to the production of this effect, and in many cases the organic matter absorbs the hydrogen liberated. Cases however occur in which the hydrogen is *not* absorbed. It by no means follows, however, that the result of the deoxidation is *insoluble*, as "A. E. S." seems to infer. The following equations may be acceptable, as illustrative of the effect of sulphurous acid in several cases:—

Blue Indigo.	Sulphurous Acid.	Water.	Sulphuric Acid.	White Indigo.
$2C_8H_5NO$	+ H_2SO_3	+ H_2O	= H_2SO_4	+ $C_{16}H_{12}N_2O_2$

Rosaniline.	Sulphurous Acid.	Water.	Sulphuric Acid.	Leucaniline.
$C_{20}H_{19}O_3$	+ H_2SO_3	+ H_2O	= H_2SO_4	+ $C_{20}H_{21}O_3$

Alloxan.	Sulphurous Acid.	Sulphuric Acid.	Alloxantine.
$2C_4H_2N_2O_4$	+ H_2SO_3	= H_2SO_4	+ $C_8H_4N_4O_7$

NEW METHOD OF OBTAINING POTASSIUM (p. 273).—This is by no means a novelty. In 1865, while at Novara, I prepared both potassium and sodium by the action of iron filings on the respective sulphides. The mode of operation quoted last week is, however, far from being the best. I found that a straight gun-barrel, with the nipple plugged, and inclosed in fire-clay, is the best retort to be got. This is charged with the alkaline sulphide and iron filings, laid transversely in a furnace, while a short U-shaped tube, carrying a globule of mercury, and fitted with a cork, is kept in readiness. More than three-quarters of the gun-barrel must project from the furnace, and be kept cool by artificial means. When the body of the gun-barrel has attained a cherry red heat, the bent tube and cork is to be inserted into the cold end of the barrel. The heat must now be increased and kept up for about an hour. The glass tube may now be corked and the gun-barrel allowed to cool. When *quite* cold the mercury tube is to be removed, and the potassium, which will be found condensed at the cool end of the gun-barrel, quickly removed by a bent scraper, and preserved under naphtha. If the heat be sufficiently high all the potassium will be expelled from the sulphide.

S. BOTTONE.

ANALYSES FROM GEORGE E. DAVIS.

[4294.]—I DARE say "S. S.," who inserted a query in the ENGLISH MECHANIC some time since, must have thought I did not mean to reply, seeing that I had let such a long time slip over without taking any apparent notice. I promised to perform the analyses when I had sufficient leisure, and now that time has arrived, I hasten to publish the results, so that "S. S." may publish the analyses he has received from the "leader" already spoken of.

A qualitative analysis only has been made, and therefore to say how the bases and acids are combined would be simply an absurdity. I give them separately; but if "S. S." particularly requires a qualitative analysis made, I will do so for a consideration, which, in the present instance, will be rather high.

To proceed with the bottle marked "No. 1 Liquid," I found as follows:—*Acid Radicles:* Chlorine, phosphoric, sulphuric, carbonic. *Basyls:* Aluminium, sodium, trace of iron. *Neutral Substances:* Water, with a very small quantity of a fatty acid.

The following is the analysis of the liquid contained in the bottle marked "No. 2 Liquid:"—*Acid Radicles:* Chlorine, hyposulphurous, sulphuric, carbonic, phosphoric, silicic. *Basyls:* Aluminium, sodium, potassium, traces of iron. *Neutral Substance:* Water.

The tin canister containing a paste, and labelled "Composition Paste for Analysis," gave, on examining the contents, the following result:—Partly decomposed organised substance, sodium humate, with traces of copper sulphate.

Having now given the results of the qualitative examination, I should like to know what my friend "W. R." has done. I should like him to write me personally, especially if "W. R." should happen to be "R. W."

GEORGE E. DAVIS.

Radcliffe, near Manchester, May 30.

AN IMPROVED BEEHIVE.

[4295.]—THOUGH "Bee-Keeper," in his interesting letter (4187, p. 251) does not expressly say so, no doubt one reason for making the sides of his hive so thick and durable is to protect his bees from cold, which is advantageous not only to save them from danger, but for economy of food. Mr. Pagden, of Alfriston, in his clever little book, called "£70 a year: How I Made it by my Bees," says at p. 29 that he has proved by weighing hives containing few and many bees that nearly as much honey is consumed in winter by a small as by a large number, a fact mentioned by many writers, though "no one," he says, "has been able satisfactorily to explain how this mysterious fact is to be accounted for."

Though it is unsafe to conclude that a general rule is established by one experiment, however apparently satisfactory, I believe that it is true that many bees in a hive do require much less honey in proportion to their numbers than few, and that the explanation is that when many are together they keep each other warm with less proportionate consumption of food as fuel (which honey chiefly is) when not used up in the secretion of wax. It is well known that all animals eat less whenever they are less exposed to cold, and bees when warm and not needing wax require but little heat-producing food. If this be the correct explanation, it follows that hives should be very well protected from cold in winter, and fortunately the same means will be effectual in protecting them from heat in summer if sufficient ventilation be provided for, but they will at all times need a great deal more air than "E. L. G." queerly thinks enough for us—namely, "just as much as is breathed, neither more nor less," and if they do not get far more, they will soon want none at all, neither should we if silly enough to confine ourselves to his allowance.

I do not know whether the experiment has ever been tried, but it is, I think, worth trying, if it would not pay to reduce the consumption of honey by bees in winter, by very gently warming their hives, or the air entering them artificially. It would have to be done very cautiously, lest warmth should stimulate them to unnatural activity. Possibly some of your correspondents may know if the experiment has been tried, and with what result. I know that hives are sometimes warmed to remove damp, but can heat be used to save food?

PHILO.

REVOLVING PUDDLING FURNACE.

[4296.]—THE question as to Mr. Danks being the original inventor of the revolving puddling furnace will from present appearances have to be decided in a court of law; but as to who has the credit of perfecting the invention there is no doubt Mr. Danks is entitled to it. A similar manner of puddling was tried years ago at Dowlais and failed; since then Mr. Danks informs the trade that he has succeeded in what the Dowlais Company failed in, which in a great measure he has done by using a new description of "fettling." This has been proved by the commission sent out by the Iron and Steel Institute (of which "G. S." seems inclined to speak disparagingly), one of the leaders of the commission being from the very place where the invention was originally tried. If Mr. Danks's invention is adopted generally, and the economical working which is claimed for it proved to satisfaction, then the Steel Institute has performed a service for this country, which, perhaps, "G. S." may probable in the future acknowledge. Trevithick invented and patented the first locomotive made, but George Stephenson made the locomotive a success.

The remarks of "G. S." about the new Institute are very undeserved; they are doing a good work, which is acknowledged by foreigners of the highest distinction in the iron trade, such as M. Schneider and others; and as to his remarks about "gushing votes of thanks from lordly lips," when "G. S." can benefit mankind by writing such an inaugural address as the late noble President did at the first meeting of the Institute, he will be listened to as an authority. "G. S." must remember he belongs to an honourable profession, which deservedly boasts of an institute in connection with it similar to the Iron and Steel Institute to the iron trade.

A MEMBER OF THE IRON AND STEEL INSTITUTE.

ENTOMOLOGICAL.—(II.)—ON LARVÆ.

[4297.]—THE apparatus required in collecting larvæ is a metal larva-box, a large cotton umbrella, a strong wire ring-net, a stick, and a box to put food in to bring it home. Some collectors prefer chip boxes to metal ones, but they are liable to break in and kill the larvæ.

Thus equipped, the collector may set out. When a suitable locality is reached, he should proceed to beat the trees and bushes, holding the umbrella underneath to catch the larvæ that fall, which may then be boxed with some of their food plant. Many larvæ will contrive to elude observation by being quite still, and others, such as that of the Brimstone Moth, look more like pieces of stick than anything else.

Another method of collecting larvæ is to sweep over the herbage with the net, and so obtain many larvæ which could be found in no other way.

Concerning the best time of the year for collecting larvæ, I think all seasons are good, but early in the spring, in February or March, many otherwise unobtainable larvæ may be found on grassy banks. The next best season is about the end of May or beginning of June, when the trees have been in leaf some time. Lastly, many hybernating larvæ may be found just before the leaves drop off. Just before and

just after sunset are the best times, though the collector will meet with success all day long. Some larvæ are great wood borers, such as *Cossus ligniperda* (Goat), but the collector may be deceived by the borings of beetles, when he expects a good lepidopterous larva; the different appearance of the holes must, of course, be learnt by experience. Other larvæ, such as that of *Catocala nupta* (Red Underwing), rest during the day on the bark of their food tree (willow), artfully concealed from general observation by the similarity in their colour to the bark of the tree, and by their filling up the hollows in it.

Lastly, the presence of birds, such as tits and creepers, shows that larvæ—which are their food—are close at hand, and this ought to make the collector even more active in his search. ENTO.