

IV.—NORTH NORFOLK GEOLOGY: THE CHALK AND ITS DISLOCATION.

By Sir HENRY H. HOWORTH, K.C.I.E., F.R.S., F.G.S.

IN venturing to offer some further criticisms on the interpretation of the phenomena presented by Norfolk geology published in the memoirs of the Geological Survey and elsewhere, I may state that these criticisms are the result of several visits to the county in which I have either drawn or made detailed notes of nearly every critical section along the coast.

Another visit, in which I have recently traversed many miles of country and revisited many important sections, has greatly strengthened the views already published by me in previous papers, and has made me more than ever dubious of the greater part of the *theoretical* explanations of the local phenomena contained in these memoirs.

I have said before, and I wish now to repeat, that the frailty of much of this official work has been largely due to the fact that in East Anglia and Essex the Geological Surveyors have been so badly equipped financially by the Treasury for the work they have been expected to do, and which cannot be done efficiently until a great deal of experimental boring and other expensive modes of testing the surface beds away from the coast, below as well as on the surface, have been carried out, for which funds have not been available. To map the superficial facies of a vast area like Norfolk with no other guides to the surface beds than casual exposures in marl-pits or gravel-pits, most of which are mere shallow scrapings of the skin of the soft strata, is utterly futile, except as a tentative proceeding, and it is misleading beyond measure when, with no better guides to the distribution of these beds and of their possibly disturbed or undisturbed conditions than can be got from such accidental and adventitious exposures, far-reaching theories and explanations are published under official sanction.

This only accounts, however, for a part of the mischief. I cannot help regretting that both in regard to the accessible facts and in regard to the discussion of the theoretical inferences the memoirs in question are so inadequate. It ought not assuredly to have been left to a foreign geologist to first explain the succession of the English Chalk, including that of Norfolk, nor to two amateurs with only casual opportunities to first give a detailed and masterly zonal classification of the same beds, nor to Mr. Brydone to first give us the necessary materials for adequately discussing the problem of the Chalk bluffs at Trimmingham.

Again, it is very unfortunate that in regard to the boulders of the Norfolk drift so little material should be given us in the Survey Memoirs for discriminating the original sources of the stones, the distribution and relative proportions of the different varieties in different districts, and especially the sorting out of the rolled and the unrolled boulders. It is also a pity that in analyzing problems like those connected with the drifts of Norfolk a more systematic effort should not have been made to ascertain rather more carefully from the very intelligent fishermen the nature and constituents of the

adjacent sea-bottom, and that on the other hand in dealing with the question of the origin of the local beds so much reliance should have been placed upon the foreign stones in the shingles on the shore instead of those in the deposits inland, for I am more than ever convinced that a great proportion of the foreigners in the shingles, especially the so-called Scandinavian boulders, are derived from ballast, either from wrecks or discarded from ships, and are entirely misleading in their testimony.

In regard to the *theories* which abound in the memoirs of the Geological Survey dealing with North Norfolk, and which have proved a snare to many credulous people, I hold that they are for the most part quite out of place there. Theories, especially highly polemical theories, ought to find a place in the private publications of the Geological surveyors and not in Government publications where we want facts. We can draw our own inferences.

Can anything, in fact, be more disconcerting than to find the President of the Geological Section at last year's meeting of the British Association (himself a prominent member of the staff of the Geological Survey) completely discarding the theory of interglacial periods which forms the keynote of the explanations of the Norfolk drifts in the memoirs of his own colleagues, Mr. Horace Woodward and Mr. Clement Reid, and which is adduced quite confidently as well-established in their descriptions of these beds. It is perfectly plain that if Mr. Lamplugh's address contains sound reasoning on this subject the theoretical explanation of the surface geology of East Anglia contained in the Survey Memoirs ought to be revised as soon as possible, for these theoretical conclusions are no longer tenable.

That Mr. Lamplugh is right in his views about interglacial beds I have no doubt. The only complaint I have to make is that it should be so belated. The conclusions which he now publishes as if he was the first to generalize in their sense have been pressed for thirty years in many papers and two big works by one Howorth. None of these publications are noticed in his address, which contains, by the way, a good many references to obscure foreign memoirs on the subject, which took me much time to find, and which appear in it as if they had not been previously discovered by other people. It is more to the point, however, that Mr. Lamplugh should have been constrained by the force of the evidence to discard his colleagues' views on interglacial beds so completely, and notably those of Mr. James Geikie, the author of the *Glacialists' Bible*.

Let us, however, proceed. I have said that until the boring rod has been used in many parts of Norfolk and Suffolk we shall never know what the true history of the later geological changes in these counties has been. The only part of the work for which adequate material is in fact available, until a great deal of such *experimental* digging has been done, is the mapping of the coast sections, which do afford exceptionally rich materials where the evidence is not hidden or distorted by the occurrence of long stretches of fallen débris forming a sloping talus. This talus, however, on a coast where the sea is very active is periodically cleared away, and fresh clean surfaces are therefore

being daily exposed, so that every part of the coast may at one time or other be critically analyzed and mapped.

The heartbreaking result of this renewal of the cliff sections, however, as Mr. Horace Woodward and others have frequently deplored, is that the constitution and the physical arrangement of the greater part of the beds in question differs so completely at different times and almost in every yard of these very famous cliffs that no section, however carefully drawn, is of more than ephemeral interest. This year's section will inevitably be of no use to us as a picture of what the kaleidoscopic beds will be like when a fresh collapse has taken place and two or three yards have been shaven off the front of the cliff. This baffling inconstancy is a continual source of complaint among all students of Norfolk geology.

It is, therefore, quite futile to generalize from any particular section without taking into account its purely transient, temporary, and local character. The kaleidoscopic feature here referred to does not apply to the whole of the sections, of course, but only to their loose materials, to the clays, loams, gravels, and sands, which everywhere overlie the more stable deposits.

I would first speak about these latter beds. From East Runton to the great shingle beds at Weybourne, where the cliffs abruptly end, the chalk, where exposed in or at the foot of the cliffs, is immediately covered by a remarkable bed, quite unmistakable, and occurring quite continuously, except in certain very limited lengths of the coast where it has been forcibly denuded. This bed does not seem to me to have been adequately described by Mr. Clement Reid. He says of it: "At Weybourne, and wherever the surface of the chalk can be examined, there is nearly always a bed of large unworn or little worn flints at the base of the Crag. This is the 'stone bed' of Norfolk geologists, but it does not necessarily belong to any one horizon; exactly similar beds of unworn flints are now being formed from above high-water mark to about 10 fathoms, and are caused merely by the wearing away of the soft chalk. The 'stone bed' is not formed by the subterranean dissolution of the chalk, for among the flints we often find bivalves in the position of life, and beneath them the chalk is here and there bored by *Pholas* and *Saxicava*."

I am bound to say I cannot think this description quite represents the facts as I have seen them in many visits. In the first place, to compare this continuous bed of flints overlying the chalk with the patches and insular areas now being formed on the foreshore occupied in some cases by an almost continuous pavement of paramoudras and in others by a similar pavement of very large unweathered and generally unbroken flints is most misleading. The greater part of the flints in the continuous beds overlying the chalk are rounded and weathered or broken, and it does not contain, except very rarely, large bizarre-shaped flints and paramoudras, but consists mainly of true rounded boulders, mostly not much larger than a cricket ball, lying, not in a kind of pavement, but heterogeneously mixed and forming a rude conglomerate, in which the contents become smaller and more rounded as we travel eastward until they become in places mere fine gravel. This bed ranges from a yard or more in thickness to

a few inches, and is bound together by oxide of iron into its normal condition of a very hard tenacious conglomerate. It seems to me that in its continuity and in its contents and structure it differs entirely from the insular and detached areas covered with unweathered flints on the foreshore with which Mr. Clement Reid compares it, and any theory based on an analogy between them must fail.

Secondly, I cannot agree with Mr. Reid that this bed "does not necessarily belong to any one horizon." In the stretch of coast I am describing it is an absolutely unique feature, which catches the eye at once, from the contrast between its dark ochreous colour compared with the white chalk below it and the grey clay or loam or sand above it, and from its running continuously along the coast separating the clays and sands above from the chalk below and forming a most unmistakable horizon. Indeed, I do not well see how such a bed could be formed at any other horizon. Its great peculiarities are the shells it contains in many places and the iron oxide which has coloured it so deeply and has indurated it into a mass of 'hard pan.' This iron oxide, it seems to me, has been added to the bed, since it lay under the sea when its shell contents were accumulated, for it could hardly have accumulated in it when it was submerged and washed continually by the sea. Its accumulation in this bed is merely due to the fact that the rain-water has percolated through the Crag and drift sands above it, whose redness and yellowness are caused by the iron oxide they contain, and that the flow of this chalybeate water has been stopped by the chalk which has filtered it and compelled it to throw down the iron oxide in the covering layer of rounded and broken flints, and has thus accumulated a very respectable mine of iron-ore in this particular bed. This condition could only happen at the top of the chalk, and it seems to me that such a bed where it exists as it does here, must mark very clearly the horizon separating the chalk from its covering beds of more or less porous sand and loam.

Thirdly, while I do not dispute the fact that this bed, before it was saturated with iron oxide, lay once at the bottom of the sea, as its contained shells clearly show, I cannot quite understand how it can have resulted from the mere submarine dissolution of the chalk, which must be a very slow process indeed, except when it is being pounded by a shingle beach and must be very largely limited to the area where these waves act and not reaching the laminarian zone. Nor can I quite understand how the rolled and broken flints in it are to be thus explained. Such beds as have clearly resulted from submarine dissolution and still remain on the foreshore have hardly any rolled or broken stones in them, but the great uncouth flints and paramoudras are held tight and do not move, and are therefore not rolled or broken except in very exceptional cases.

I believe myself that the rounded flints and occasional quartzites in this bed of hard pan (as I will call it) are in their present form much older than the shells it contains and were rolled or broken possibly in the time of the Red Crag sea or perhaps even earlier, and that the bed in question must have a very different origin from the mere submarine dissolution of the chalky matrix of the flints. This, however, is a matter which must not detain us at present.

The point I wish to insist upon is that this bed is not, as Mr. Reid argues, matched by similar beds at other horizons elsewhere, but is, on the contrary, a unique feature, and it marks most graphically for us the present upper limit of the chalk as it is seen in the cliffs and on the foreshore and marks also the base of the Crag.

This hard pan is remarkable, as I have said, for containing in many places a large number of bivalves. Quite a large proportion of these are complete valves, and a large number again have both valves united, showing they are still *in situ* in the position of life. They are jammed in among the stones, and are most clearly, as Mr. Reid says, where they actually lived. This again shows how different in essence the bed is from the insular areas of flints on the foreshore, which contain no whole shells and very few fragmentary ones.

It is perfectly plain, therefore, that since this bed was formed out of rolled materials, etc., and since it was lying at the bottom of the sea in a *quiescent condition* (otherwise the shells would have been reduced to powder), the level of the hard pan with its underlying chalk has been entirely and forcibly altered. These shells could not clearly live at a height of nine or ten or more feet above high-water mark as they now occur in many places in the cliff, but must have lived below low-water mark where the so-called laminarian zone of marine life is now found. The beds have clearly been thus dislocated in the latest geological period.

In more than one paper on the recent dislocation of the chalk in the Eastern Counties published in the *GEOLOGICAL MAGAZINE* I have, in fact, pointed out how recent and how very potent this dislocation of the chalk has been. No evidence of the fact could be plainer than that to which I am now drawing attention. For a long distance from the cliff end at Weybourne, as far indeed as near Sheringham, the hard pan and chalk are now several feet above high-water mark. East of Sheringham the chalk with its covering, after sinking for a short distance, is again raised several feet above high-water mark. It then sinks down again to the level of the shingle, and can be traced along the foot of the cliffs to beyond West Runton. This level of the shingle, again, is several feet above the level of the laminarian zone, so it is quite plain that from the end of the cliff at Weybourne at least to West Runton the chalk and its covering have been raised many feet, or rather yards, since the shells contained in the hard pan were living, which means since the greatest part of the living mollusca existed in the adjoining sea.

This, again, is quite plain from the outline and structure of the chalk beds below the hard pan. Their surfaces are in places meandering and not level, and in places they are arched up, as shown by the curved lines of the flints in the chalk, and as figured by Mr. Reid in his memoir. At intervals, where gaps occur in the cliffs, and their surface sinks down to that of the shingle beach, the chalk for a while disappears, as at Weybourne Mill, Hithe, etc. In these cases the chalk has either disappeared through a fold forming a synclinal dip, or from some disconnection and complete breach of a more violent kind. Apart from this the level of the chalk gradually sinks to a lower point as we move eastwards. East and west of Sheringham its

surface is very irregular and broken. This is immediately before it disappears entirely from the cliff, and where it is found only on the shore at its foot.

The same irregularity in the surface contour of the chalk and the same broken and dislocated character of the same bed is found in the inland parts of the county, as has been shown by earlier writers whose observations I have collected in a previous memoir.

This last conclusion is attested by the various borings for wells which have been sunk in Norfolk, and by the exposures of chalk in chalk and marl pits. They show conclusively that the level at which chalk is reached in sinking varies very greatly, and varies in areas near one another, showing that it has been greatly disturbed and broken over a wide extent since it was deposited. Of this variation in depth at which the chalk is found in the county the Survey Memoirs afford ample evidence. The same conclusion is forthcoming again from the considerable number of great angular masses of detached chalk occurring among the drifts in various parts of the county, which in places are big enough to have permitted of chalk being quarried out of them; phenomena which have been noted by many observers, and the impressions to be drawn from which have been enlarged upon in my previous papers on the dislocation of the chalk in the Eastern Counties already referred to. In the country round Cromer and Sheringham I have lately again visited several of these pits. There is a fine one behind the mill at Weybourne; another near the road in a cutting through the hill between Weybourne and Salthouse; another behind the hill east of Beeston Hill; another near the upper gate of Mr. Upcher's park on the Holt road; a small one east of Cley, on the road to Sand; and more than one at Northrepps. In all these cases the irregularity of level at which the chalk occurs is remarkable. It is not easy to determine in many cases whether the chalk exists in a detached mass or forms the nucleus of the hill in which it is found, and has been merely raised up, but in either case the proof of some violent action is patent.

The occurrence of great masses of shelly Crag at Norwich, far from the sea and overlying the Chalk, and of other portions also at a considerable elevation in the Bure valley, shows even more forcibly how great has been the elevation and disturbance of the chalk in these latitudes since the Norwich or Weybourne Crag was deposited.

The same conclusion follows inevitably from the conditions under which the vast cakes and huge angular masses of chalk occur in the drifts at Beeston, West Runton, and elsewhere, a phenomenon which has attracted the attention of geologists for a long time and been the cause of endless discussion. From Weybourne to near Sheringham nothing is more remarkable in the cliffs than the scarcity of large flints or of masses of chalk in the clays and sands overlying the hard pan. So scarce are they that they may virtually be said not to exist at all except occasionally in the gravel sometimes capping the cliffs, and which is sometimes contained in pockets, more or less great, in hollows formed on the upper surface of the sands.

East of Sheringham the whole condition of things is changed, and thence to Cromer the cliffs are marked in several places, as the cliff

sections displayed in the Survey Memoirs show, by the vast long cakes of chalk containing flints, which have been ridiculously called chalk boulders, and which, as I have said, have been the puzzle of every inquirer, and the cause of many fantastic theories on the part of those who dislike induction but love paradox and sensation in science.

In the sections illustrating the geology of Cromer cliffs attached to the memoir on the geology of Cromer, these long cakes of chalk between East and West Runton gap are duly represented, although on much too small a scale to show their real features. In the stretch of cliff between West Runton and the brickyard in Beeston cliff there is a particularly interesting and notable enclosure in the drift which is not noticed in the sections in question, nor, so far as I know, in the literature on the subject. It may be that it has only recently been exposed in the cliff, but this seems very improbable. Like the tabular cakes of chalk just mentioned, this included mass is also remarkable for its great length and small thickness, but what makes it much more notable is the disintegrated materials out of which it is composed. When I first saw it I was particularly puzzled, because I could see from the foot of the cliff only a long serpentinous line of chalk (true chalk and not made up) curved into a sinuous shape and extending for a great many yards, nowhere apparently more than a foot thick and thinning down to 4 or 5 inches. On climbing up the cliff I found that this enigmatical ribbon of chalk was covered with a stratum of hard pan formed of small flint pebbles and full of Weybourne Crag shell fragments, and over this again was a stratified bed of sandy and gravelly Crag, the whole united together and forming a continuous series of beds. The entire mass is more or less lenticular in shape in its thickest part, but stretches out on either side into a kind of ribbon and has been detached and transported *en masse*. Although the pebbles of flint are indurated by iron oxide, they are much more loose than elsewhere where the iron pan occurs at the foot of the cliff. There cannot be any doubt that we here have a case of a surface layer or rather skin of chalk having been stripped off violently from the chalk matrix, and with it the usual covering of the chalk as it occurs when *in situ* and undisturbed on the foreshore, that before it was moved this cake of mixed materials formed an integral part of the uppermost bed of the chalk with the Weybourne Crag attached, and that the detachment and portage of the whole mass took place after the deposition of the Weybourne or Norwich Crag.

I ought to add that this remarkable cake of conjoined chalk and crag is matched in the case of the longer and better known tabular chalk masses between East and West Runton Gap. On portions of two of these the same bed of hard pan or consolidated gravel occurs. In these cases the gravel is represented in the sections of the Survey Memoir lying on the chalk, but its significance is entirely overlooked, and, *mirabile dictu*, the gravel is labelled *glacial gravel*. What there is glacial about it defies conjecture. It is formed of small rounded flint pebbles with occasional quartzites, like all the other gravels of the upper crag, and, so far as I know, of nothing else, and is the exact equivalent of the hard pan as it exists similarly planted upon the chalk when the latter occurs with the crag in juxtaposition along

great stretches of the coast. It is, in fact, a definite proof that these chalk cakes, like the serpentinous mass of chalk last mentioned, were violently detached after the deposition of the Weybourne Crag.

I may say that these tabular chalk cakes, like the mixed mass above named, have meandering or arched outlines, and that the once horizontal beds of flints in them have been bent into the same curves, which imply the exercise of similar forces as those in the chalk still *in situ* and were doubtless induced at the same time.

It would be a great mistake to suppose that these enormous masses of detached chalk lying in and surrounded by later beds are a peculiar feature of the cliff sections only. They also occur in several sections far inland, as far, in fact, as Leicestershire and Northamptonshire, in several places having actually been used as chalk-quarries. What is perfectly plain is that the phenomenon they witness to, is that of a violent and notable dynamical movement, however caused, and which is to be put alongside the other proofs of similar dislocation and violent breakage already mentioned in this paper.

We have no reason to suppose that the movements involved belonged to different periods and occurred at different times. On the contrary, such evidence as is available, as we shall see, goes to show that it took place at one time. We have seen that on the coast this is shown to have been after the deposition of the so-called Weybourne Crag, whose shells are found in the red pan. The fact of the Norwich Crag and the so-called Bure Valley Beds (which are merely the local representation of the Weybourne Crag) occurring so far from the coast and at such a comparatively high level, and the fact that the chalk beds in the former place correspond to those between Weybourne and Ranton, as Mr. C. Reid says (see Memoir on Cromer, etc.), is strong testimony to the fact that the dislocations of various kinds here referred to, and so notorious to every student of the district, were contemporaneous and occurred at the same geological epoch, namely, after the deposition of the Norwich or Weybourne Crag.

This view is, to some extent at least, that of the Geological Surveyors. Thus, speaking of the bent and distorted chalk at Trimingham, Mr. C. Reid says: "That this contortion is of Pleistocene date is proved by the similar disturbances of the overlying beds, and by the intrusion of tongues of Boulder Clay into the Chalk. Lyell was fully aware of this unconformity and gave illustrations of it; he mentioned the mixture of Chalk and Boulder Clay on the fore-shore, and considered that the contortion must clearly have been formed subsequently to the deposition of the Drift" (Geology of Cromer, etc., p. 95).

What Mr. Reid here says of the local disturbances at Trimingham I would apply to the Chalk dislocations of all North Norfolk and its borders, which I claim to have all been contemporaneous and posterior to the deposition of the Newer or Norwich Crag. To sum up the case as far as we have gone, the evidence of violent change and movement and dislocation in the Chalk of Norfolk after the deposition of the latest Crag beds is very widespread and very cumulative. It would appear further incontrovertible from the available facts that when these newer Crag were being deposited the contour of Norfolk

was quite different to what it is now, and that a considerable part of it was occupied by beds, not of curved and broken chalk as now, but of horizontally bedded chalk lying at the level of the laminarian zone upon which the Crag shells lived and the Newer Crag beds were deposited, and that it was after this time that the whole of it was subjected to violent forces which dislocated and broke it and gave it its present contour. These conclusions seem inevitable.

The next question that arises is what was the force or what were the forces which caused these dislocations and movements, and which occurred on this great scale not in remote geological time but, geologically speaking, only yesterday, at an epoch, in fact, when the possibility of such movements has been so continuously scouted by the champions of orthodox geology.

There are only two ways in which this widespread dislocation and destruction could take place. It must have been either by the application of force from the outside and from above, or by the exercise of subterranean energy. In regard to the former notion, which is the orthodox one, the difficulty is stupendous. We must not forget that the total thickness of the Chalk in this part of Norfolk must be very great, probably largely exceeding 500 feet, and further that chalk is a very tough material, offering great resistance to pressure. How we are to secure an instrument acting from above which could twist and curve this material into meandering curves, arching it here and depressing it into synclinal folds elsewhere, and could further break off with ragged edges and deeply wounded surfaces great angular masses and cakes of chalk, passes my comprehension. Yet the postulate has been gaily appealed to by most of the orthodox geologists without any attempt whatever being made to justify it.

The Rev. O. Fisher, who generally argues in a strictly inductive way, writing as far back as 1868, attributed phenomena such as we are discussing to the pressure exercised by large masses of material on the underlying beds. Thus he says *inter alia*: "in attributing contortions in the underlying beds to the deposition of masses of matter upon the surface, I would go to the extent of suggesting that the remarkable bluffs of chalk at Trimmingham may have been upraised by some such action." Surely here we have a whole series of unverified premises! Where can we find a single instance of such contortions and breakage as we are discussing as the result of placing great weights on the surface of the earth? Take the Pyramids, or buildings like St. Paul's or St. Peter's, among human experiments, or turning to purely natural phenomena, take the innumerable examples we can find of perfectly horizontal beds lying under tremendous loads of superincumbent material much greater than any that can be appealed to as overlying the Chalk in Norfolk. Surely the idea is a purely transcendental one. How, again, is the pressure of such a mass, even if effective for the purpose of producing dislocations, to cause meandering contours and alternate arches and hollows in chalk beds several hundred feet thick, or how is it to break off vast masses from their matrix with raw and angular edges? Assuredly, if the pressure is sufficiently great, crushing will ensue; that is true enough, but has anyone attempted to calculate the amount of mere pressure that would

be needed to crush the chalk, a problem which Mr. O. Fisher might easily have faced before offering an hypothesis so remote from probability. But even if he had faced it we should be no nearer. There is no sign whatever of mere crushing in these chalk cliffs and great chalk masses. The chalk in them and the lines of flint in them are quite intact, and so are the beds of consolidated gravel and finely laminated sand which in many places are adherent to them. We should expect if the pressure had been sufficiently great to be efficient at all that it would have crushed these chalk beds into powder, and not curved them and twisted them in this way, or broken them off with sharp edges. The whole process seems to me utterly fantastic and impossible. I am not alone in thinking so. In this instance I quite agree with Mr. C. Reid when he says: "The Rev. O. Fisher's theory of the forcing up of the beds by irregular deposition of masses of material on their surface seems inadequate to the formation of contortions on so vast a scale. It is doubtful whether anything less than a mountain piled on the surface at Trimmingham would be sufficient for the contortion of 200 feet of underlying strata." This is quite judicious, although it understates the difficulty, but in the face of such a statement what are we to say of Mr. Reid's own theory? After thus demolishing the Rev. O. Fisher's notion that the chalk dislocations were due to the differential pressure of superincumbent masses of strata as quite inadequate, he proceeds, without any attempt at a physical analysis of the conditions of the problem, to apply the very same kind of explanation himself, only substituting ice for beds of rock or sand or clay. Assuredly, nothing can well be more inconsequent, for it merely adds to the difficulties instead of diminishing them. Let us analyze his argument. The distinction in Mr. C. Reid's mind seems in some way to rest on a notion that while the postulated superincumbent rocks, sands, or clays here referred to would be *ex hypothesi* stationary, ice is in a measure mobile, and he says the explanation of the broken and contorted condition of the chalk is only possible on the hypothesis of "a lateral thrust, or of a sliding pressure from above."

(To be continued.)

V.—TWO NEW SPECIES OF *EURYPTERUS* FROM THE COAL-MEASURES OF ILKESTON, DERBYSHIRE.

By HENRY WOODWARD, LL.D., F.R.S., V.P.Z.S., F.G.S.

(PLATE XIII.)

BY the kindness of Mr. Henry A. Allen, F.G.S., of the Geological Survey of England, three examples of *Eurypterus*, in clay-iron-stone nodules, showing impression and counterpart, together with a fragment of a fourth example, all from the Coal-measures to the north-west of Ilkeston, have been most obligingly lent me for description by their discoverer, Dr. L. Moysey, M.A., of St. Moritz, Ilkeston Road, Nottingham.

Remains of *Eurypterus* are extremely rare in the Coal-measures. The earliest Carboniferous *Eurypterus* discovered and described was